

S.455.

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

VOL. II.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

I MEET my Readers and Correspondents at the completion of this Second Volume, with the satisfaction of directing their notice to the still greater increase of communication, and the means of rendering this work most eminently interesting and useful. It is not necessary that I should recapitulate the outlines of the plan and rules of conduct by which I have endeavoured to deserve the encouragement of the Public; but I think it truly justifiable to exhibit the marks of that approval at the same time that I express the sensibility with which I accept them, and the spirit they must afford to my labours.

Without entering into any estimate of the quantity of original matter in this work, which from the high value of its correspondence is now become the authentic repository of the researches of our philosophers; I will only notice, that the number of original writers in this Volume is more than double that of the respectable list prefixed to the first Volume, at the same time that the value and importance of the Memoirs from foreign and domestic publications have continued to increase.

The authors of original Papers are Mr. F. Accum; A. B. C; John Bostock, M. D. J. W. Boswell; Count de Bournon; H. Campbell, M. D.; R. Chenevix, Esq.; Mr. John Clenel; C. P.; Mr. Wm. Close; W. Cruickshank, Prof. at Woolwich; John Cuthbertson; D. H.; J. Fletcher, Esq. G. H; John Gough, Esq.; Mr. Olinthus Gregory; J. C. Hornblower; Rob. Jameson; J.; Rev. W. Pearson; N. N. Joseph Priestley, L. L. D. F. R. S. &c.; H. Sarjeant, Esq.; Mr. Tho. Sheldrake; Dr. J. H. Schroeter; Tho. Thomson, M. D.; Mr. Trevithick; Troughton; Ez. Walker; Rev. James Wilson, D. D. James Woodhouse, M. D.; Mr. Arthur Woolf; Thomas Young, M. D. Prof. P. R. I.; Baron von Zach; and W. N.—Of foreign works, Carcel; Carreau; Coulomb; Descroizilles; Guillot; Guyton; Hassel Lachenaie; Lalande; Piçtet; Proust; Valentine; Vauquelin.—And of English Memoirs abridged or extracted, Mr. Banks; Wm. Bullock; Sir H. Englefield, Bart.; Mr. Gilpin; J. Gough, Esq.; Ch. Hatchett, Esq.; Wm. Herschel, L. L. D. Ed. Howard, Esq.; Hulme, M. D.; E. Jones, Esq. H. Sarjeant, Esq.

The novelty and excellence of the communications with which this Journal has been honoured, have been productive

PREFACE.

tive of an effect which I have seriously meditated to remedy. In the limited extent of every work of this nature, when the new and interesting Memoirs demand a larger portion of its capacity, a smaller must of course be devoted to less striking, though doubtless very important business of selection. Foreign and domestic matter must be more fastidiously sorted out; articles must be abridged instead of being given at full length; and some must be rejected altogether that would have been highly acceptable, if the original productions could have allowed room.

Two remedies present themselves. The first is to print a supplementary number to each volume; and the other to give a greater number of pages without adding to the price. I shall be happy to adopt the latter as soon as the increased sale shall have rendered it practicable, without diminishing the ordinary remuneration the Work affords; and in case the former should prove necessary or advisable, I am confident my Readers will see the advantages and approve the proceeding. In the mean time, the private recommendation of men of merit to their friends, is the best means of accelerating that circulation which will eventually benefit its patrons, by the greater quantity of them that could in that case be afforded.

I conclude this Preface as usual, by mentioning the subjects of the sixteen Plates which illustrate the present Volume. 1. Improvements in Hydraulic Engines, by Mr. Boswell. 2. Guyton's Improvements of the Swedish Stove. 3. Mr. Sarjeant's cheap Engine for raising Water. 4. Strong framed Levers for Steam Engines, by Mr. Hornblower. 5. Dr. Young's Diagrams to illustrate the Theory of Light. 6. Mr. Gregory's Figure for Mr. Pearfon's Analogy. 7. Mr. Gough's Illustration of the Doctrine of Sound. 8. Mechanical Lamp of Carcel and Carreau. 9. Mr. Woolf's Apparatus for heating Water by waste Steam. 10. Mr. Terry's improved Mill. 11. Mr. Bullock's Lock. 12. Count Bournon's Figures of Anhydrous Sulphate. 13. Mr. Trevithick's Application of a temporary Forcer. 14. Lachenaie's Apparatus for claying Sugars. 15. Mr. Jameson's Illustration of the Formation of Granite. 17. Mr. Banks's Instruments for determining the Pressures and Velocities of effluent Air or Gas. 18. The Spirit Hydrometer and Scales of Atkins. And, 19. Compound Condensers of Electricity, by Mr. Read and Mr. Cuthbertson.

Sto Square, London, September 1, 1802.

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MAY, 1802.

ARTICLE I.

Improvements in the Hydraulic Engine of SCHEMNITZ, and that of Mr. GOODWYN; with comparative Remarks on the most useful Applications of each, and some Facts relative to the Invention of the pressure Engine. In a Letter from Mr. JOHN WHITLEY BOSWELL.

To Mr. NICHOLSON.

S I R,

London, March 14. 1802.

HAVING, with much satisfaction, found that the method of making the Schemnitz hydraulic engine work itself, which I gave you for your excellent Journal in 1800, (IV. 117.) has been since found of considerable utility in * other works of a similar nature, I am induced to send for your approbation a draft of the † application of the same principle to Mr. Good-

Method of working engines by the bucket and syphon.

* Vide Mr. Close's papers in the same Vol.

† The paper signed L in the quarto Journal, page 343, though it professes to shew how Mr. Goodwyn's engine may work itself, has only hinted at this method, but has not shewn how it may be effected.

wyn's engine, and another of a method of causing the Schemnitz to raise water *above* the level of the prime reservoir, together with a comparative view of the advantages of both engines and their powers.

Concise explanation of the Schemnitz engine.

After a perusal of my former Paper on the Schemnitz engine, (IV. 144.) a mere inspection of the figure given here (Plate I.) will be sufficient to shew the manner in which this now proposed will operate. The moving power is the pressure of the column of water from the reservoir R, (Fig. 1.) to D in the lower air chamber A, which forces the air contained in it into the chamber B, which air so compressed in B will impel the water contained in it upwards through the pipe to a height, and in a quantity proportionate to the relative height of the column of water contained in the pipe R D, compared with that contained in S B, or supposing the length of R D given, the greater the length of the pipe S B is, (so as not to exceed R D,) the less will be the quantity of water delivered at S, and *vice versa*.

Mr. Goodwyn's engine; constructed for heavy work, and made to operate without attendance.

In the draft of Mr. Goodwyn's engine, Fig. 2. I have endeavoured to exhibit it as it should be if executed on a large scale, and made all the pipes detached from each other, because though the plan of making one pipe pass through another and through the reservoirs, made use of in Mr. Goodwyn's model, is very convenient and neat in an apparatus that may be placed on a table, yet it would be found to produce an unnecessary trouble, complication, and difficulty of repair in a large engine. The method shewn in this draft of causing the engine to work without attendance, is the same as for the Schemnitz, and causes the cocks G and H to open at intervals, (which may be regulated at pleasure by the hand cock I, letting the water flow more or less quick into the syphon vessel E,) while at the same time it closes the cock at D, and *vice versa*. Self-moving valves are placed at the delivering pipes of the chambers C and B, and also at the air vent of A, because wherever they can be used they are preferable to cocks, or valves used by external power; some doubt may arise, whether there should not be a passage for the air let at intervals into A, as well as one for it to escape: but as great quantities of air are contained in water, which the mode of working of this engine will particularly tend to separate from it, I think it would be needless and that the self-moving valve

valve opening outwards at the air vent of A will be sufficient. The pipe at K is to conduct water from the bucket F to that of the cock D. There are two ranges of reservoirs represented, to shew the method of raising water by this engine about thirty feet high: more would be useless, and even a second would in very few instances be found necessary, except when the fall of water from R to D was very short, in which case it would be better to use some other engine for raising water to the required height.

In comparing these two engines, it will be found that their powers and capabilities are nearly similar. Comparison of the powers of these two engines; numerically stated.

1. In both the greater the height of the original fall of water, denoted by the pipe R D, and the greater the quantity of water which it can supply in a given time, the greater quantity can be raised by these engines in a given time.

2. Both engines can be constructed so as to raise water above the original level, and from below to the surface, or from a pit.

3. By a successive number of reservoirs both engines can be brought to raise water to any height; but as they will raise a smaller quantity as the height is increased, the quantity wanted in a given time, and the expence of construction, will limit the extent of their elevation.

4. In both engines the distance from one reservoir to another, must always be less than that of the original fall R D. The circumstances in which these engines differ arise from the difference of their manner of action.

5. The Schennitz engine operates by causing a fall of water to *compress* air, which re-acting on other water forces it to rise in a pipe to a certain height. Mr. Goodwyn's engine acts by causing a fall of water to *rarefy* a certain quantity of air, into whose space the pressure of the atmosphere forces, when permitted, a quantity of water.

6. Hence in the Schennitz engine, the pressure acting from *within outwards*, tends to *burst* the vessels used in the structure, and to *open* and *extend* any fissures which may chance to be in them.

7. In Mr. Goodwyn's engine the pressure acting from *without inwards*, closes all the parts of which it is composed more together, tends to make its pipes and vessels more staunch, and in any fissure makes its sides operate like valves to shut it up.

8. The Schemnitz engine will always raise water to a height nearly equal to that of the original fall, from one reservoir to another, supposing the original fall of any height whatsoever, as 100 feet.

Mr. Goodwyn's engine will not raise water from one reservoir to another so high as thirty feet in any case whatsoever, as there cannot be a complete vacuum formed by it in the air chamber, but only an approximation to one

Mr. Goodwyn's engine will be preferable, and least costly in small elevations.

From this comparison it will follow, that wherever the original fall of water is less than thirty-two feet, Mr. Goodwyn's engine will be much preferable to the Schemnitz; as, from the 7th article of the comparison, it may be made of the *cheapest materials*, of strong *wooden casts* and *woolen pipes*; whereas the Schemnitz engine, from the 6th article, must be made of the strongest, and of course most costly materials, of cast iron at least, and that of considerable thickness.

The Schemnitz engine for greater depths.

But wherever the original fall exceeds the height of thirty feet much, and it is required to raise the water to nearly the same height, then the Schemnitz engine appears to be preferable; as, in all probability, the fewer number of parts which it will, in this case, require in its construction, will more than compensate for its costly materials.

Piston engine best for very great depths.

When it is required to raise water to a height, much greater than that of the original fall, above the first level, or from a greater depth; either from the original fall being short, or the required height being great, an engine in which the pressure of the water is made to act by a piston in an apparatus similar to that of the steam engine, (one of which is described in your Journal for March) will be preferable to either of the above.

Comparison of the three engines.

The comparison of those engines may be brought to this one point: wherever Mr. Goodwyn's engine can be used, with a single continued pipe for elevating the water, and without a succession of reservoirs, it seems to be the cheapest.

Where Mr. Goodwyn's engine cannot be used without violating this condition, but the Schemnitz can, it promises to be the next in point of cheapness, from its simplicity, absence of friction, and small number of working parts.

But when neither Mr. Goodwyn's engine nor the Schemnitz can be used without a number of reservoirs, then the piston pressure engine probably ought to be preferred; but this will much depend on the number of reservoirs, for perhaps one or

two in addition to the Schemnitz might cost less, than boring the cylinder of the piston engine perfect, and its additional machinery: for merely raising water the powers of each are nearly equal, depending all on the height of the original fall of water.

The great advantage of the piston pressure engine is, not as a cheap engine for raising water, but as that in which a fall of water can be applied without any waste to work mills or machinery for any purpose; which is of very great consequence when the fall of water is of considerable height, and the stream, or supply, small.

The pressure engine works without waste of water.

Conceiving it of very great importance to have it determined in what situations each of the principal engines, worked by water pressure, is to be preferred, I have commenced this comparison, and if this shall be acceptable, will send another paper on a similar subject; that is, on the comparison of the common mill water wheel, with another mode of applying the water to turn mill work, which, I think, I can demonstrate to be much preferable. I hope what I have thus began will excite some others to the same enquiry; and that by this means the multiplicity of water pressure engines will be at last arranged, and their comparative utility ascertained, so that in every different case of fall or supply of water, an engineer may know at once which he should use.

Great importance of comparing the different methods of applying water as a force.— Wheel work, &c.

I beg leave to add here some remarks on the piston pressure engine in your Journal for March: Mr. Trevithack will, I hope, excuse my taking from him the honour of his being the first inventor of this mode of applying a fall of water, to give it back to Messrs. Denifard and Deuille of France, when I confess that for a long time I was in the same error with him, and thought it had first occurred to me, and proposed it with that idea to Mr. Carnac in Nov. 1796, to draw the water from his copper mine, (by which he would have saved the daily labour of twenty men, as he had a fall of water very proper for this engine—a fact which I have Mr. Carnac's signature to prove;) but I since found out that in Belidor's Hydraulic Architecture, published at Paris 1739, in the fourth book and first chapter, there is a method described at large, with very well executed plates, by which a fall of water operating in a cylinder on a piston may work a pump to force water to a greater height; and what is remarkable, Belidor

Mr. Trevithack's pressure engine was executed in 1731 by Denifard and Deuille—and imagined by Mr. Boswell in 1796.

Described in Belidor.

proposes the very same method of working the valves by a tumbling weight, as that called the tumbling bob, in Mr. T's description. In Belidor's engine the piston cylinder and pump are both horizontal, which is the most material difference, but the principle is entirely the same as Mr. Trevithack's.

Mr. Belidor does not claim the honour of this invention, but only proposes the engine I allude to, as an improvement on one executed on the same principle at Seve, between Versailles and Paris, in the year 1731, by Denifard and Deuille, for which they obtained a patent from the king of France for twenty-one years; of which engine there is also a description in the same chapter of the above work.

Proposal of air chambers in the piston.

I beg leave to suggest, that it might be a considerable improvement to this engine to have its action made elastic, by the addition of an air chamber, on the same principle as that used in engines for extinguishing conflagrations; such a one, seems to me, might be best effected by making the piston hollow, and of a larger size, to serve for this purpose, as the air spring would then act both on the upper and lower pressure of the water; Figure 3 is a sketch of this method, in which A represents the hollow piston.

I hope the length of this paper will be excused by the circumstance of my not having received your Journal for March till I had written the most of it, and having had of course, to add the remarks on the piston pressure engine to the rest.

I am, SIR, &c.

W. H. B.

II.

*Remarks of the present State of Paper-making in England and France**. By H. CAMPBELL, M. D. Communicated by the Author, March 26, 1802.

Whether the additional duty on paper has principally injured book trade.

THE additional duty on Paper has not been the chief cause of the diminution in the book and paper trades of this country. After

* The importance and authenticity of the chemical, mechanical, and other facts stated in this Memoir, and of the object itself in general

After establishing this fact, it will be equally evident that the paper and book trades will not be *effectually* relieved by giving up a part, or the whole of the last additional duty.

The former administration of this country by imposing an additional duty, did not teach the French the new art of paper making; nor did it supply them with abundance of raw material; or shew them the art of equalizing the different qualities of rags by Chaptal's mode of bleaching;—neither was any new light thrown by the additional duty upon the mode of printing and type casting, as now practised in France.

France has succeeded by improvements in the arts of paper making and printing.

Before the commencement of the late war, paper-making in this country, strictly speaking, was a mechanical art. The superiority of English paper arose from the superior linen worn by English people. Their rags were superior to the rags collected on the continent. The decency of the English populace, compared with the populace on the continent, could not be better shewn than by an exhibition of English and foreign rags. Rags called *London fines*, the refuse of Irish linen, &c. could scarce be equalled in any other country. The original linen of these rags had been highly bleached. They were consequently calculated to make the finest and whitest paper. Rags at an English paper mill previous to the war, were *sorted* according to their colour and fineness. Pains were taken to cut off the seams and offal parts; and these parts were destined to make inferior paper. This statement alone is almost sufficient to give a clear notion of what I am about to establish.

The British fine paper formerly excelled from the fineness of our *linen rags*;

and our careful sorting them.

I conceive that a representation of the *mechanical necessaries*, as the duster, the knives placed in the engine roller, the plate, the vat, the moulds, and lastly, flannels and presses, although indispensable in a paper mill, are necessary to be noticed in this account; as an imitation of them in France, could not contribute to the present alarming change in our paper trade.

Our machinery,

The English staple, superior rags, English cleanliness in cutting and assorting, and better engines and knives, con-

attempted to be transferred to France.

general to the progress of science and literature, leave no doubt respecting the propriety of inserting it in our Journal, though its most prominent feature relates to political regulation. On this last subject my conduct as a Journalist cannot be supposed to express any opinion as an individual.---N.

tuted

tuted our superiority. About the year 1789 and 90, *certain respectable* English paper makers endeavoured to take these advantages to France. France apparently required them; her coarse foul cotton rags were not made better by separation and assortment; her engines were deficient, and her mills exhibited no mark of prosperity, but every symptom of slovenly neglect.

French rags are coarse cotton.

French paper for copper plates.

One sort of paper, notwithstanding the want of colour and cleanliness, she excelled in, upon necessity. I mean paper for copper plate prints; she necessarily excelled because her staple cotton rag being more bibulous, received better impressions from the plate. This was the state of her paper trade and mills in the year 1789. Before that period, she did not consume all her own rags. We received a considerable part from the cellars of Dunkirk and Ostend, and from countries in her southern vicinity, Leghorn, &c.

New æra in paper making. The bleaching process by ox. mur. acid.

About this time a new æra in paper making commenced. Chemistry by her disciples, Scheele, Berthollet, and Chaptal, from a metallic oxide, solicited and directed the concentrated and pure part of the atmosphere, oxygen, to remove with expedition the colouring part of cloth, or rags made from vegetable substances, such as flax, cotton, hemp, &c. An attention to the best bleaching process ought certainly to form a material part of the considerations in this Paper; because this object, connected with a knowledge of the sorts of rags suitable for bleaching, and making *printing* paper, would give that comprehension of the evil from which might be drawn—not steps of temporary and unavailing expedience, but solid foundations of relief.

Coarse cotton rags are brought to an equality with fine linen rags by that process.

The business under consideration is more intimately connected with printing than with writing paper. The bleaching gas is much better adapted to coarse cotton rags than coarse linen or hempen rags; because the former is without ligneous particles, and the latter abounds with them, and these particles, called by paper makers *sheaves*, are made more conspicuous by bleaching. The staple of our opponents the French, consists in coarse cotton rags. I can, if required, point out places where depots should be established for affording an unlimited supply of similar *cheap* materials, and where each ship, by way of finishing her lading, will take on board a convenient number of bags.

A plentiful and cheap supply of cotton rags.

In the year 1793-4, I imported a parcel of cotton rags at 9l. per ton; bleaching them added 8 per cent. to the 9l. The bleached stuff was worth more than 40l. per ton: the beautiful paper produced now exists in a public work. If paper makers, or stationer-paper makers, had paid attention to this new and growing improvement; (or would pay attention) instead of soliciting a remission of duties, they might be able with their *capital* and mills ready formed, to counteract the French, and contribute still more to the state, than is paid at present. A relaxation of part of the duties, cannot be a radical relief. To forego an object of finance without remedying a complaint, can only be a compliance with the government, to participate in misfortune with the petitioners.

Instance in point. The true remedy, stated to consist in the adoption of the improvements, and the proper employ of our capital and works.

The necessity of abolishing improper combinations and power among journeymen paper makers, and the injury done to paper mills by London stationers *importing and regulating the price of rags*, and monopolizing mills, are evident directions to relieve the suffering part of the trade, and how to obtain revenue from the wealthy part, by way of granting licences, I shall be happy, if required, to communicate.

Combinations and monopolies. Hints at *regulations by the State.*

The arts, commerce, and navigation of this country, are justly considered by the French government, to be the *sinews* of England. Chaptal, Berthollet, and other enlightened men are encouraged to affect us, and benefit their own country in these particulars: that they have not been unsuccessful is manifested by the state of the paper trade in France.

General observations. The arts, commerce, navigation.

The maxim of trade finding its level, is too supine a maxim in the present state of discoveries. Factories are altering, and markets must vary. Goods formed by mere *mixture*, such as saline and other bodies, require neither capital, nor machinery. Many of these are in a state of great cultivation in France. In arts resting on capital, machinery, and aptitude of hand, we shall long remain unrivalled.

Level of trade.

What manufactories can be easily transferred; and what are the most permanent in a wealthy country.

At present I shall check these observations, that my remarks on the paper trade may remain distinct and unmixed with any other matter.

H. CAMPBELL.

No. 11, Fleet-Street, London.

III.

Remarks on Combustion, by THOMAS THOMSON, M. D. Lecturer on Chemistry in Edinburgh.

Phenomenon of combustion is very striking to every class of men. **N**O operation of nature has a better claim to our attention than COMBUSTION. The irresistible devastation which it sometimes occasions is calculated to strike the ignorant with terror; the extraordinary changes which it produces naturally attract the inquisitive eye of the philosopher, while its subserviency to almost every branch of domestic economy renders it a familiar and necessary agent in the hands of every individual. This familiar acquaintance with combustion seems, however, to have retarded the investigation of its nature; for it was not till the seventeenth century that philosophers made it a serious object of enquiry. The labours of Bacon, Boyle, Hooke, and Mayow are well known; and the success with which these labours were attended, must, if we recollect the difficulties to be overcome, give us a very high idea of the genius of these investigators of nature. But the philosophers of our own age, especially Lavoisier, have gone far beyond their predecessors; and have explained some of the most intricate and important phenomena of combustion.

Eminent investigators of its theory.

Lavoisier's theory

Mr. Lavoisier's theory of *combustion*, improperly termed his theory of *chemistry*, is so generally known, that it is unnecessary to enter into any detail concerning it. Its merit is indisputable, and raises its author to the very first rank among philosophers. Many chemists seem to think that it explains the whole phenomena of combustion; but an attentive examination must convince every impartial observer, that the theory of Lavoisier, ingenious and satisfactory without doubt, as far as it goes, leaves yet several parts of that very complicated process as unaccountable as ever. He has corrected the errors of his predecessors, and made a very important new step; but many new steps are still wanting to render the theory complete. I hope, therefore, that the following remarks will not be considered as altogether improper; they will at least exhibit the subject in a new point of view, and may perhaps contribute to call the attention of chemists to certain phenomena

leaves much to be still elucidated.

phenomena which have not hitherto been classified, nor examined with that precision to which they are entitled.

1. Though the French chemists have lately given the term *combustion* a new meaning, and made it stand for the general combination of a body with oxygen, I mean, for reasons which will appear hereafter, to employ it in the sense usually affixed to the term by the generality of mankind. Now when a body undergoes combustion, in the common sense of the word, two things always take place. 1. The body gradually wastes away, and often disappears altogether; it is then said to be *consumed* or *burnt*. 2. During the whole of this process it emits *heat* and *light*; the heat and light thus emitted are usually denominated *fire*, and the waste of the body is considered as the effect or consequence of its combustion. If either of these two phenomena be wanting, we do not say in common language that a body is undergoing combustion, or that it is burning. Every theory of combustion then must explain, 1. Why the burning body is wasted and altered. 2. Why during the progress of this alteration heat and light are emitted.

The French confine the term combustion to the act of oxygenation.

Usual acceptation of the term preferred, *consumption* or *waste with heat and light*.

The theory must explain these effects.

2. If we take a view of the different bodies which occupy the attention of chemists, we shall find, that as far as combustion is concerned, they may be arranged under three classes; namely, 1. Combustibles. 2. Supporters of combustion. 3. Incombustibles.

Relative to combustion bodies are 1. combustibles, or 2. supporters of combustion, or 3. incombustible.

I. The **COMBUSTIBLES** are those bodies, which are said in common language to *burn*. During combustion they appear to emit light and heat, and at the same time gradually waste away. When this change has reached its maximum, the process of combustion is at an end. The class of combustibles is very numerous; but all the bodies belonging to it may be subdivided into three sets; namely,

I. *Combustibles* or the bodies consumed.

1. Simple combustibles,
2. Compound combustibles,
3. Combustible oxides.

The *simple combustibles* are twenty-four or twenty-five in number, namely,

They are simple combustibles; or

1. Sulphur,
2. Phosphorus,
3. Carbon,
4. Hydrogen gas,
5. All the metals*.

* Except perhaps gold, silver, and mercury.

The

—compound combustibles,

The *compound combustibles* consist of compounds formed by the simple combustibles uniting together two and two; and are of course much more numerous than the simple combustibles. They may be arranged under the five following heads :

- | | |
|-----------------|---------------|
| 1. Sulphurets, | 3. Carburets, |
| 2. Phosphurets, | 4. Alloys, |

5. Sulphurated, phosphorated, and carbonated hydrogen.

or combustible oxides,

The *combustible oxides* are composed of one or more simple combustibles, combined with a dose of oxygen. Though the French chemists have given to these bodies the name of *oxides*, we shall see afterwards that they differ essentially from *metallic oxides* and from water, which is considered at present as an oxide of hydrogen. The combustible oxides may be arranged under two heads: 1. Those which contain only a single base combined with oxygen, and which therefore may be termed *simple combustible oxides*. 2. Those which contain more than one base combined with oxygen, and which therefore may be termed *compound combustible oxides*.

and these last are either simple, having a single base, or compound, having more than one base.

Simple comb. oxides.

The simple combustible oxides are only four in number; namely,

- | | |
|-------------------------|------------------------|
| 1. Oxide of sulphur, | 3. Charcoal, |
| 2. Oxide of phosphorus, | 4. Carbonic oxide gas. |

Unless sulphur, phosphorus, and hydrogen gas, bodies at present considered as simple, belong to this class. All the simple combustible oxides are by combustion converted into acids.

Compound comb. oxides.

The compound combustible oxides include by far the greater number of combustible bodies; for almost all the animal and vegetable substances belonging to them. The double base is usually carbon and hydrogen: alcohol, ether, resins, gums, &c. are instances of compound combustible oxides*.

Phlogiston.

It was believed by Stahl and his disciples, that all combustible bodies contain one common principle, to which they owe their combustibility. But in consequence of the discoveries of Lavoisier this theory has been laid aside.

II. Supporters of combustion

II. The *supporters of combustion* are a set of bodies which are not of themselves, strictly speaking, capable of undergoing combustion, but which are absolutely necessary for the process; for no combustible body can be made to burn unless

* To this class of bodies also must be referred all the vegetable and animal acids,

some one or other of the *supporters* be present. Whenever they are excluded the process stops. All the supporters known at present are the following six :

- | | |
|----------------------------|----------------------|
| 1. Oxygen gas, | 4. Nitrous gas *, |
| 2. Air, | 5. Nitric acid, |
| 3. Gaseous oxide of azote, | 6. Oximuriatic acid. |

There are indeed certain substances besides these, which possess nearly the same properties; these I shall enumerate afterwards under the title of *partial supporters*.

All the supporters contain one common principle, namely, universally contain oxygen. oxygen. The first of them consists of oxygen uncombined with any base; but in the other five the oxygen is united to a base. It is very remarkable, that in four cases out of five, the base to which the oxygen in these compound supporters is united is *azote*. Is it not probable from analogy, that oximuriatic acid, the remaining compound supporter, contains azote likewise as a component part. N. B.

III. The *incombustible bodies* are neither capable of under-III. *Incombustibles.* going combustion themselves, nor of supporting the combustion of those bodies that are; of course they are not immediately connected with combustion. At present we are acquainted with about 13 incombustible bodies, not reckoning the compounds which they are capable of forming with each other. These are,

- | | |
|------------------------|----------------|
| 1. Azotic gas, | 3. The earths. |
| 2. The fixed alkalies, | |

The first of these substances constitutes the base of almost all the compound supporters. Some of the alkalies and earths possess certain properties in common with combustibles, and are capable of exhibiting phenomena somewhat analogous to combustion; phenomena to be described afterwards under the Semi-combustion. title of *femi-combustion*.

3. From the preceding observations it is obvious, that in every case of combustion there must be present a *combustible* and a *supporter*. Now during combustion the combustible always unites with the oxygen of the supporter. It is this combination which occasions the apparent waste and alteration of the combustible. The new compound thus formed I shall call a *product of combustion*. Combustion requires a combustible and a supporter. Now every product of combustion is Product of combustion is either water or an acid, or a metallic oxide.

* Mr. Davy first proved that this gas is a supporter.

either, *oxide*.

either, 1. *water*, or 2. *an acid*, or 3. *a metallic oxide*. It is true indeed, that other bodies sometimes make their appearance during combustion, but these will be found upon examination not to be products, nor to have undergone combustion.

Thus one of the two characteristic marks which distinguish combustion, namely, the apparent waste and alteration of the combustible body, has been fully explained. For the explanation of it we are indebted to Lavoisier. It constitutes what is usually, but absurdly, termed the *new theory of chemistry*, and is the most important step which has been made towards a complete theory of combustion,

Facility of combustion is not proportioned to the attraction for oxygen.

But though the combination of the combustible with oxygen be a constant part of combustion, yet the facility with which combustibles burn is not proportional to their apparent affinity for oxygen. Phosphorus, for instance, burns more readily than charcoal; yet charcoal is capable of abstracting oxygen from phosphorus, and of course has a greater affinity for it. The combustible oxides take fire more readily than some of the simple combustibles; thus charcoal burns more easily than carbon or diamond: alcohol, ether, and oils, are exceedingly combustible, whereas all the metals require a very high temperature when the supporter is air. This greater combustibility of combustible oxides is probably owing to the weaker affinity by which their particles are united. For the cohesion of heterogeneous particles, when oxygen constitutes a part of them, is usually weaker than the cohesion of homogeneous particles. Hence they are more easily separated than homogeneous particles, and of course combine more readily with oxygen; those simple combustibles which melt easily, or which are in the state of elastic fluids, are also very combustible, because the cohesion between their particles is easily overcome.

But chiefly depends on the facility of destroying the cohesion.

Hence compound supporters are more readily burned.

It is owing to the same inferiority in the cohesion of heterogeneous particles, that some of the compound supporters occasion combustion in circumstances when the combustibles would not be acted on by simple supporters. Thus phosphorus burns in air at the common temperature; but it does not burn in oxygen gas, unless the temperature exceed 90° . In oximuriatic acid gas phosphorus burns rapidly at the common temperature of the air, and so do several of the metals;

though they cannot be made to burn in air except at a very high temperature. Thus also oils burn rapidly when mixed with nitrous acid. Nitrous gas and the gaseous oxide of azote constitute exceptions to this rule.

4. None of the *products* of combustion are combustible according to the definition of combustion which I have given. This want of combustibility is not owing to their being saturated with oxygen; for several of them are capable of combining with an additional dose of it. But during this combination no caloric nor light is ever emitted; and the compound formed differs essentially from a *product* of combustion; for by this additional dose of oxygen the *product* is converted into a *supporter*.

Products of combustion are never combustible.

Oxygenation of a product converts it into a supporter.

Hence we see that combustion ought not to be confounded with the combination of a body with oxygen, as is done by the French chemists. Combustion indeed cannot take place without the combination of oxygen; but oxygen may combine without combustion. Thus when iron is burnt, it always combines with 0.27 of oxygen, and is converted into the *black oxide*, a product of combustion, and altogether incombustible; capable, however, of combining with an additional dose of oxygen, and of being converted into the *red oxide*. But during this last combination, how rapidly soever it takes place, no heat nor light is emitted. Now the *red oxide of iron* is not a product of combustion, but a supporter; as the following experiments demonstrate: Mix it with phosphorous, and put the mixture into the bottom of a long glass tube, shut at one end, and filled with azotic gas. Close the mouth of the tube, and apply heat to that part in which the mixture is. At a certain temperature a violent detonation takes place, which shatters the tube in pieces. It is needless to remark, that the tube must be sufficiently long to prevent the effects of expansion in the gas included.

Difference between combustion and oxygenation.

Detonation of phosphorus with oxide of iron.

When antimony is burnt, it always combines with 0.20 of oxygen, and is converted into the *white oxide*. Now this white oxide, which is a product of combustion, and of course incombustible, is capable of combining with an additional dose of oxygen, and of being converted into the *acidulous oxide* of antimony. In like manner, lead, when burnt, is converted into the *white oxide* of lead, a product; but this product combines with

Other products converted into supporters.

with additional doses of oxygen, and is converted into the red and brown oxides, both of which are supporters. When the *They act by part of their oxygen ;* supporters, thus formed by the combination of oxygen with products, are made to support combustion, they do not lose all their oxygen, but only the additional dose which constituted them supporters. Of course they are again reduced to their original state of products of combustion. Hence it follows, that they owe their properties as supporters, not to the whole of the oxygen which they contain, but to the additional dose which constituted them supporters. We may therefore call them *partial supporters*, indicating by the term, that part only of their oxygen is capable of supporting combustion, and not the whole. It is very possible that both *azote* and *muritic acid* may be products of combustion; and in that case both the compound and partial supporters would agree with each other in every respect. In the present state of our knowledge, however, it is necessary to distinguish them.

and are *partial supporters*.

The bases of all known *partial supporters* are metallic.

All the partial supporters with which we are acquainted, contain a metallic basis; for metallic oxides are the only products at present known capable of combining with an additional dose of oxygen. It is a circumstance highly deserving of attention, that when metals are capable of combining with several doses of oxygen, the product or oxide formed by combustion is seldom or never that which contains a maximum of oxygen. The following oxides are products of combustion:

Enumeration.

- | | |
|------------------------------|-------------------------------|
| 1. Black oxide of iron. | 8. Oxide of copper*. |
| 2. White oxide of zinc. | 9. Oxide of cobalt*. |
| 3. White oxide of lead. | 10. Oxide of nickel*. |
| 4. Yellow oxide of tin. | 11. Oxide of bismuth*. |
| 5. White oxide of antimony. | 12. Purple oxide of gold? |
| 6. White oxide of arsenic. | 13. Yellow oxide of silver? |
| 7. White oxide of manganese. | 14. Black oxide of mercury †? |

* The particular oxide of these metals, which is the product of combustion, has not been ascertained; but they are all combustible in oximuriatic acid gas.

† I doubt much whether gold, silver, and mercury, be combustible at all. They do not burn in air, how high soever the temperature is; neither do they detonate with red hot nitre, nor exhibit any appearance of combustion in oximuriatic acid gas; though this last body oxidates them with great rapidity.

The

The following oxides, on the other hand, are partial supporters of combustion:

- | | |
|-----------------------------|-------------------------------------|
| 1. Red oxide of iron * | 6. Red and brown oxides of lead **. |
| 2. Yellow oxide of gold †. | 7. Black oxide of manganese. |
| 3. White oxide of silver ‡. | 8. Acidulous oxide of antimony? |
| 4. Red oxide of mercury §. | 9. White oxide of tin ? |
| 5. Arsenic acid . | |

This list would doubtless be increased by an accurate examination of all the metallic oxides not included in either of these tables.

Thus it appears that several of the products of combustion are capable of combining with oxygen. The incombustibility of products, therefore, is not owing to their want of affinity for oxygen, but to some other cause.

5. No *product* of combustion is capable of *supporting* combustion. This is not occasioned by any want of affinity for combustible bodies; for several of them are capable of combining with an additional dose of their basis. But by this combination they lose their properties as products, and are converted into *combustibles*. The process therefore differs essentially from combustion. Thus sulphuric acid, a *product* of combustion, by combining with an additional dose of sulphur or its oxide, is converted into *sulphureous acid*, a substance which, from several of its properties, I conclude to be combustible. Thus also phosphoric acid, a product of combustion, is capable of combining with phosphorated hydrogen, and of forming *phosphorous acid* a combustible body. When this last acid is heated in contact with a supporter, it undergoes combustion; but it is only the additional dose of the combustible which burns, and the whole is converted into phosphoric acid. Hence we see that it is not the whole basis of these compounds which is combustible, but merely the additional dose. The compounds, therefore, formed by the union of a product and combustible,

Incombustibility of *products* is not owing to want of affinity for oxygen.

Nor can they support combustion, though they can combine with combustibles.

Instances. This combination affords *partial combustibles*, acting by part of their basis.

* Fulminates with phosphorus.

† Forms fulminating gold.

‡ Forms fulminating silver.

§ Forms fulminating mercury.

|| Occasions combustion when heated with several combustibles.

** Occasions the combustion of sulphur.

Since *products* can combine with oxygen, but never exhibit combustion unless they be *partial combustibles*, combustibles must contain a substance which they lose in burning.

Products can give oxygen to *combustibles*, and convert them into *products*; but they do not, like *supporters*, cause combustion.

The oxygen of *supporters* differs from that of *products*.

Combustibility is restored to *products* only by *combustibles*.

may be termed *partial combustibles*; indicating by the name, that a part only of the base is capable of undergoing combustion. Now since the products of combustion are capable of combining with oxygen, but never exhibit the phenomena of combustion except when they are in the state of partial combustibles, combustible bodies must contain some principle which they lose during combustion, and to which they owe their combustibility; for after they have lost it, they unite to oxygen without exhibiting the phenomena of combustion.

Though the products of combustion are not capable of supporting combustion, they not unfrequently part with their oxygen just as supporters do, give it out to combustibles, and convert them into products; but during this process no heat nor light is ever evolved. Water, for instance, gives out its oxygen to iron, and converts it into *black oxide*, a product; and sulphuric acid gives out its oxygen to phosphorus, and converts it into phosphoric acid. Thus we see that the oxygen of products is capable of converting combustibles into products, just as the oxygen of supporters; but during the combination of the last only, are heat and light emitted. The oxygen of supporters then contains something which the oxygen of products wants.

6. Whenever the whole of the oxygen is abstracted from products, the combustibility of their base is restored as completely as before combustion; but no substance is capable of abstracting the whole of the oxygen, except a *combustible* or a *partial combustible*. Water, for instance, is a product of combustion, whose base is hydrogen. To restore the combustibility of the hydrogen, we have only to mix water with iron or zinc filings; the metal is oxidated, and the hydrogen gas is evolved as combustible as ever. But no substance, except a combustible, is capable of separating hydrogen gas from water by combining with its oxygen. In the same manner phosphorus absorbs the oxygen from sulphuric acid, and is converted into a product, while sulphur is separated in its usual state of combustibility. Thus we see that combustibles are capable of restoring the combustibility of the bases of products, but they themselves lose their combustibility by the process, and are converted into products. Combustibility, therefore, may be thrown at pleasure from one body to another. This fact was first set in a proper light by Stahl, and was the great step in the theory of combustion

combustion for which we are indebted to that philosopher. Some mistakes into which he fell were afterwards corrected by Lavoisier*.

From these facts it is obvious, that the products of combustion may be formed without combustion; but in these cases a new combustible is always evolved. The process is merely an interchange of combustibility; for the combustible is converted into a product only by means of a product. Both the oxygen and the base of the product having undergone combustion, have lost something which is essential to combustion. The process is merely a double decomposition. The product yields its oxygen to the combustible, while at the same time the combustible gives out something to the base of the product; the combustibility of that base then is restored by the loss of its oxygen, and by the restoration of something which it receives from the other combustible thus converted into a product.

When products are thus formed without combustion, a new combustible is evolved.

There is indeed another method of forming the products of combustion without actual combustion in certain cases; but the phenomena are much more complicated. This method is to expose them to the action of some of the supporters dissolved in water; especially nitric acid. Thus most of the metallic oxides may be formed without combustion by the action of that acid on the metals. But in that case a new supporter is always evolved, namely, nitrous gas; ammonia, a new combustible, is usually also formed; and not unfrequently the product is converted into a *partial supporter*.

Complicated case of products formed by nitric acid, &c.

7. No *supporter* can be produced by combustion, or by any equivalent process. Now as all the supporters, except oxygen gas, consist of oxygen combined with a base, it follows as a consequence, that oxygen may combine with a base without losing that ingredient, whatever it is, which gives occasion to combustion. The act of combination of oxygen with a base, therefore, is by no means the same with combustion. If we

Oxygen may combine without losing the ingredient which occasions combustion.

* When sulphate of iron is dropt into a solution of muriate of gold, nitrate of silver, or nitrate of mercury, the gold, silver, and mercury are precipitated in the metallic state. This is an additional reason for suspecting that these three metals are not combustible. Every person, however, must have observed, that the metals in question have not the metallic lustre when first precipitated, and that they acquire it slowly when allowed to remain exposed to the light.

All supporters
procured either
from other
supporters, or by
electricity.
Oxygen.

take a view of the different supporters, we shall find that all of them which can be obtained artificially, are procured either from other supporters, or by the agency of electricity.

I. OXYGEN GAS may be procured from nitric acid and oximuriatic acid, two supporters; and from several of the partial supporters, as the black oxide of manganese, the red oxides of lead and of mercury. The action of heat is always necessary; but the process is very different from combustion.

Air.

II. AIR, as far as is known at present, cannot be formed artificially. The gas indeed which comes over during part of the usual distillation of nitre and sulphuric acid to obtain nitrous acid, resembles air very closely. But it is obtained from a supporter.

Oxide of azote.

III. THE GASEOUS OXIDE of azote, or nitrous oxide of Davy, has hitherto been only procured from nitrous gas and nitric acid (nitrate of ammonia), both of which are supporters.

Nitrous gas.

IV. NITROUS GAS can only be procured by the decomposition of nitric acid, a supporter.

Oximuriatic
acid.

V. OXIMURIATIC ACID can be formed by combining muriatic acid with the oxygen of nitric acid, a supporter; or with the oxygen of the black oxide of manganese, the red oxides of lead, iron, mercury, &c. all of which are partial supporters.

Nitric acid
formed spontane-
ously.

VI. NITRIC ACID is formed spontaneously upon the surface of the earth by processes with which we are unacquainted.

(To be continued.)

IV.

*Some Account of a new Planetary Body, discovered by
DR. OLBERS, on the 28th of March, 1802.*

Discovery of a
new planet.

A LETTER from Dr. J. H. Schroeter, of Lilienthal, to Mr. Best, London, was lately read at the Royal Society, in which he gives an account of this remarkable discovery of a second planetary body of small apparent magnitude, discovered by Dr. Olbers, of Bremen, on the 28th of last March, when it formed the south point of an equilateral triangle with the stars Nos. 20 and 19 of the constellation Virgo. During the comparisons he was induced to make, from his conviction that it was not there in January when he re-discovered the Ceres

near

near the same spot, he found its change of place to be very perceptible. The following observations were transmitted by him to Dr. Schroeter :

d. h. m. s.

1802. Mar. 28	9 25 10	M. T.	App. R. Af. $184^{\circ} 56' 49''$	Its place.
			App. Dec. 11 33 30 N.	
29	8 49 14	M. T.	App. R. Af. $184^{\circ} 46' 36''$	
			App. Dec. 11 52 59 N.	

Dr. Olbers had not the means of observing any disc, but Dr. Schroeter, from his information, began to observe it on the 30th with his thirteen feet reflector, while his assistant, Mr. Harding, determined its place. With a power of 288 of this large instrument, it appeared round and less hazy than Ceres, with a diameter of 4.635 seconds, which is much larger than those of Ceres and the Georgium Sidus; the former of which, on the 28th March, measured 4.021 sec. and the latter, on the 20th, measured 3.973 seconds. Its light, though pale and white in comparison with that of Ceres, was nevertheless more intense upon the whole, as appeared by its projection on the disc micrometer; but the Georgium Sidus was much brighter.

Its apparent diameter larger than that of Ceres or the Geo. Sidus.

Its light.

A minute star was seen near it on two several days, which Dr. Schroeter seems to suspect as a satellite.

Suspicion of a satellite.

The position of the Olberian planet (or comet), at March 30 d. 8 h. 20 m. 50 sec. mean time, was

App. R. A. $184^{\circ} 35' 52''$ and $12^{\circ} 15' 8''$ app. N. declin.

On the 1st of April it was again observed with the great reflector, and appeared to exhibit no disc, but was less in a brighter light with the power of 288, and could not be distinguished from a fixed star. When it sometimes appeared with a disc, its diameter was only $3''.244$. The Doctor is disposed to ascribe this to the heavy dew.

No disc on April 1.

Letter from Mr. W. WALKER, Lecturer on the Eidouranian.

To Mr. NICHOLSON.

41, Conduit Street, Hanover Square, London,
April 26, 1802.

SIR,

THE account you did me the favour of inserting in your valuable Journal of the last month, respecting the situation of the planet Ceres, will be rendered additionally interesting at present, from the circumstance of a still more recently observed

Present situation, April 26.

Definable disc with power of 100. Pale red and less brilliant than Ceres. Supposed to be nearer the Earth.

moving star being situated very near to the situation there pointed out as the place of the Ceres on the 25th of March. The planet or comet discovered by Dr. Olbers, at Bremen, on the 28th ult. is at present in a very small degree to the S. E. of the situation of the Ceres on the 25th of March, and will readily be seen by a night-glass or telescope. It is invisible to my naked eye, but appears of a definable disc with a magnifying power of a 100 times. Its light is pale red and very faint, and through the night-glass is less brilliant than the Ceres, although no magnifying power that I can use will give the latter any apparent diameter. It seems probable that its distance is about as far again as the Earth is from the Sun, whilst the Ceres is near three times the distance, and Mars about once and a half as far off. As my object is merely to enable any person to find it, I do not trouble you with any more full account at this late period of the month.

I remain, SIR,

With much respect,

Your constant reader,

W. WALKER.

Present situation of Ceres.

The Ceres Ferdinandia will be found a little to the north-east of the star Beta in the Lion's Tail, being the easternmost point of a right angled triangle formed by Beta, a double star due north of it and itself.

V.

On Bradley's Method of observing Transits, and another Method by which the Thickness of the Wire is rendered of no Importance, In a Letter from Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

SIR,

Dr. Bradley's method of making transit observations described.

THE method of taking transit observations introduced by Dr. Bradley is still used by astronomers. This consists in noting the proportional distance of the star from the wire at the two beats of the clock, one immediately preceding, and the other immediately following its passage across the wire. If the wire be so thick as to cover the star, and the star happens to be behind it when the clock beats, the situation of the star's

star's centre cannot be exactly known, which makes the introduction of fine wires very desirable. The finest wires still proving too thick for very small stars, an astronomical friend of mine hinted to Mr. Troughton that he might probably receive assistance in this delicate branch of his business from some of his spiders. This hint was not lost,*—and hence Bradley's method of observing seems to be carried to the highest degree of perfection by the assistance of a harmless insect which is persecuted without mercy by every *fille de chambre* throughout his Majesty's dominions.

Spider's webs in the foci of telescopes.

There is, however, another method of observing which precludes the necessity of very fine wires. This consists in noting the time when the centre of the star comes to the side of the wire. But before this method is used it is necessary that one side of the middle wire should be brought into the meridian; suppose it be that side which appears to the west when the telescope is turned to the south, then the observations are to be taken on that side of all the wires.

Another method of observing one side of the wire.

It is a *line drawn by the strength of imagination* down the middle of the wire, parallel to the sides, which is used in Bradley's method, but in my method a *real line* is presented to the eye of the observer and which he sees very distinctly, although as fine as if it had been drawn through a geometrical definition.

Which is a real line.

This method of observing seems to be more simple than the other, in consequence of its being, in many cases only necessary to attend to one side of the wire: for example: Should the clock beat when half the star is covered by the wire, it is evident that the centre of the star passes the side of the wire at that time. And by making use of the apparent diameter of the star, and the thickness of the wire as two measures, the fractional part of a second may be estimated, by an experienced observer to a very great degree of precision.

Its advantages.

How far this small alteration in the method of using the transit telescope may be found convenient to others experience must determine; but for my own part, I am certain that I can observe, not only with more ease to myself, but with greater exactness by this method than by Bradley's.

I am, Sir,

Your very humble servant,

Lynn, April 19, 1802.

EZEKIEL WALKER.

* See a further account of this invention in the last vol. of this Journal, pa. 319.

VI.

*Description of a Stove on the Principles of the Swedish Fire-place, with Heat-openings, by CITIZEN GUYTON.**

THE true principles of constructing fire-places, so as to obtain the greatest heat with the least consumption of fuel, have been known for some time in France; but they have been much less generally adopted, than the necessity for economising fuel demands. We see many fire-places so deep as to consume double the quantity of fuel necessary, and yet heat the apartment but faintly, where half the expence might be spared by altering the fire-place according to count Rumford's plan.

Fire-places in France not generally constructed on good principles.

Often too deep.

Smoky chimneys how usually remedied.

If a chimney smoke, instead of reducing the tunnel to proper dimensions, so that descending currents cannot take place in it, scarcely any remedy is thought of but air-holes, which require the sacrifice of a certain quantity of fuel, to counterbalance the effect of the cold air continually entering.

The use of the Swedish stoves is probably yet rare, from their not having been constructed on just principles, or in the best proportions, at their first introduction. As I have had one made, which appears to many of my friends to produce an astonishing effect, in compliance with their request I shall give an exact description of it, premising however a few principles with regard to fires.

General principles with regard to fires for domestic purposes,

1. The heat produced is proportionate only to the air consumed by the fuel.

2. The quantity of heat produced by a given quantity of fuel is greatest when the combustion is most complete.

3. The combustion is most complete when the filiginous part of the fuel is retained longest in pipes in which it may undergo a second combustion.

4. Of the heat produced none is of use, but what is diffused through the space to be heated, and retained in this space.

5. The temperature in this space will be higher, in proportion as the current of air, which is to renew and keep up the combustion, is less disposed to absorb the heat of this space in passing through it.

* Abridged from the *Annales de Chimie*, vol. xli. p. 79. C.

Hence we deduce the following obvious consequences:

Corollaries deduced.

1. The fire-place must be kept separate from all bodies that conduct heat rapidly.

2. As heat can be produced only by combustion, and combustion can be maintained only by a current of air, this current should be attracted into pipes, where it preserves the requisite velocity, without going away from the place to be heated; so that the heat it deposits in it gradually accumulates in the whole of the isolated stove, to be afterward given out slowly, according to the laws of its equilibrium

3. When the wood is consumed to such a point as to afford no more smoke, it is of advantage to stop the outlets of these pipes, to keep in the heat, which would be carried into the chimney by the continued current of fresh air, which would necessarily be of a lower temperature.

4. We shall obtain a higher temperature, and preserve it longer, under similar circumstances, if we construct within the stove, or under the hearth and round the fire-place, pipes in which the air derived from without is warmed before it enters into the apartment to support the fire, or to replace what has been consumed.

Apertures for heated air.

These pipes are what have been called heat openings, (*bouches de chaleur*,) because instead of considering their principal object, it is commonly supposed, that they are made to give a more rapid passage to the heat produced. This is not totally without foundation, since the temperature of the air issuing from them is increased by the heat it absorbs from the stove; and on this account some might be disposed to neglect them, as contrary to the most essential object, that of retaining the heat in it; but it is to be observed, that we can shut these outlets when we please; and that we may even cut off all communication with the external air by means of a simple slider; so that every advantage may be derived from them without any inconvenience. It must be added, that they are necessary in very close apartments, unless we would expose ourselves to currents of cold air. These reasons have induced me to employ the heat openings in the Swedish stove, to which they had not been applied.

Necessary in very close apartments.

The Swedish stoves are constructed strictly according to the truest principles, and the pipes in which the smoke circulates are disposed in the best manner for effecting its complete combustion.

Swedish stoves constructed on the best principles.

combustion.

- So useful, that they are become general in Sweden ;
- and are employed in different works.
- Construction ;
- and method of using them.
- Description of the Swedish stove constructed by Guyton.
- Its height,
- breadth,
- depth.
- Height may vary.
- Proportions of the circulatory pipes.
- buſtion. Their utility has been found ſo great, that they have become general in Sweden, where the winters are very ſevere, and where they have diminiſhed the conſumption of wood one third, ſo that there is no country where the inclemency of the weather is guarded againſt at leſs expence. They have likewiſe been employed advantageouſly, with the neceſſary variations of form, in dye-houſes, breweries, &c.
- Their conſtruction is by no means expenſive ; they ſave iron-work, and require only bricks or tiles. Theſe are recommended to be placed edgewiſe, and choſen as thin as poſſible for the inner walls. The circulating pipes are to be placed ſo, that rain falling down the chimney can never get into them. The method of uſing them is ſo eaſy, that in the largeſt public buildings one perſon is ſufficient to light all the fires. All the wood that can be contained in the fire-place, which is very ſmall, is to be put in at once ; it is to be ſawn into pieces of equal lengths ; and as ſoon as it is burned, the ſlider that ſtops the communication of the circulating pipes with the chimney is to be thruſt in. By theſe means all the heat, which the fuel is capable of producing, remains in the pipes, and iſſues out ſlowly, and only to diſſuſe itſelf in the apartment ; while a ſingle piece of wood, that had not burned at the ſame time with the reſt, would oblige the ſlide to be left open, and the current of air neceſſary for its combuſtion would carry off into the chimney the greater part of the heat produced.
- The following is a deſcription of the ſtove conſtructed under my directions.
- Fig. 1, Plate III. represents a front view of the ſtove: its height is 164 centimetres (about 61 inches French), excluſive of the vaſe, which is a ſeparate ornament, merely placed on the top.
- Its breadth is 85 centimetres (about 31½ inches.)
- Its depth 58 centimetres (about 21½ inches.)
- The height may vary according to the ſize of the apartment, and be extended without inconvenience to two metres (about 6 feet, 2 inches.) It may likewiſe be reduced, as I have done for ſtoves in a laboratory, which were to ſupport a ſand bath as high as the hand.
- The other two dimenſions are determined by thoſe of the bricks employed to form the interior circulatory pipes, which ſhould be in certain proportions, that the ſmoke may paſs through

through them freely, without so much air entering with it as would condense it, or sink the temperature below the degree necessary for combustion.

V V are the external parts of the two heat openings.

Heat openings.

m m Apertures of the stove, by which the air, that is to issue through the heat openings, enters. These are closed when the air is drawn from without through a pipe passing under the floor; which is much more advantageous for renewing the respirable air of the apartment, and prevents the danger of currents of cold air attracted by the fire; and which is necessary, as I have observed, whenever the volume of air in the apartment is not sufficient, to supply both the consumption of the fire, and the circulation in the heat pipes.

Fig. 2. is a plan of the foundation of the hearth at the height of the line A B, fig. 1. *ll* are empty spaces, to receive the air, and convey it into the compartments, where it is to be heated before it issues by the heat openings, whether the air be obtained from without, or simply by the apertures *m m*, fig. 1.

Fig. 3. pl. IV. plan at the height of the line C D, fig. 1; that above the door of the fire-place, *n n n n* are the double plates of cast iron, forming the compartments in which the air is to receive the effect of the heat of the fire,

oo The empty space between these plates.

Fig. 4. Front section at the line I K, fig. 3. The arrows indicate the direction of the smoke in the circulatory pipes of the front part.*

In this the plates of iron *n n* are seen in their perpendicular situation, with the tongues which form their compartments on each side of the fire-place. One of these plates is represented in front fig. 7.

T is an opening left at the bottom of the fourth circulatory pipe, to restore the draught of air in the fire-place, if neces-

* Among the number of Swedish stoves described and delineated in the collection published by baron Cronstedt there are several, the circulating pipes of which pass under the hearth. This gives them a little more extent no doubt, but as soon as the hearth is covered with ashes, the air passing beneath can receive but a very slight impression of heat; it obliges the fire-place to be raised higher; and it renders the construction more complex and expensive. For these reasons I have adopted the most simple plan.

fary, by burning there a few slips of paper, or other light combustible, I say *if necessary*, because I have found by experience, that this precaution may be neglected, as soon as the stove has been heated so as to have lost all its internal dampness.

The door of this sort of blower, or air-vent, ought to shut very close. For this purpose it is sufficient, to cut a piece of brick of the proper size, to make a hole in it to receive a handle, and to fasten upon it a piece of plate iron projecting a little all round it.

Fig. 5. Plan at the height of the line E F, fig. 1.

Fig. 6. Transverse section at the line G H of fig. 3, which shews the height of the fire-place, and the first direction of the flame.

V points out the arrangement of the heat pipes.

The dotted lines give the profile of the party walls, which form the four grand circulating pipes.

R the pipe which conveys the smoke from the circulatory pipes into the chimney, and in which is the register that cuts off the communication. It is a common stove tunnel of plate iron; but it would be better to use a substance more slowly conducting heat, as an earthen tube made on purpose, for that part in which the slider or stop plate acts.

The elbow made by this pipe to reach the chimney renders it unnecessary to repeat, that it is a point of the first importance for the body of the stove to be completely separate from the wall. That which I have described is 25 centimetres (about 9 inches) distant from the nearest point of the nich in which it is placed.

S is an elongation of the perpendicular pipe that enters into the chimney. It is intended to receive the water that might condense in the upper part, to prevent it from getting into the stove. The cap at the end of this elongation allows the pipe to be cleaned without taking it down.

The dotted lines forming the square space Q mark a place where a nich may be made, or a sort of little stove, as is done in some of the Swedish stoves, and would advantageously supply the place of the brick-work, with which it must otherwise be filled up.

All these figures being drawn on the same scale, there will be no difficulty in preserving the proportions of the parts.

The construction of this stove is neither difficult nor expensive. For the outside nothing is wanted but Dutch tiles, such as are used for common stoves, that is to say thin in the middle, and having a border all round, which serves to give them more stability. They are fixed in like manner by a band of metal. The hind part may consist entirely of bricks. The vase placed on the slab of marble or stone, which covers the stove is a mere ornament.

Materials of which the stove is constructed.

If it be thought proper to have no heat openings, all the interior structure may be made of bricks of proper sizes, laid with loamy earth moistened, and set on edge for the circulatory pipes, without any iron except a cast plate over the fire-place, and a door and frame in the usual manner.

The heat openings may be omitted.

The expence of the heat openings however consists only in four cast iron plates with tongues and grooves to form the compartments represented at fig. 7. All the rest is done with plate iron, bent round and rivetted, which, when once enclosed in the masonry, will not admit the escape of the air.

The expence however not great. How are they made.

Cast iron plates with grooves are well known, since Franklin's stoves have been adopted. If it were found difficult to procure them, their place might be supplied in two ways. First by portions of pipes of cast iron, which might be placed vertically side by side, serving as the inside walls of the fire-place, and communicating with each other by little channels at top and bottom formed in the masonry. Secondly, by common plain cast plates, soft enough to admit of being bored, so as to rivet on bent slips of plate iron, which would perfectly answer the purpose of the tongues and grooves. As these would never be exposed to the action of the flame, there is no reason to fear their casting. The latter of these two methods is obviously the most advantageous, as it occupies less room, and yet affords more surface to receive the action of the heat, and communicate it to the circulating air.

Substitutes for the cast-iron plates.

In concluding my description of this stove I ought not to omit saying, that nearly two years experience has convinced me of the good effects of its proportions.

Its utility confirmed by two years experience.

It is placed in a room fronting the north, the floor of which measures 47 metres square (about 12 toises $\frac{1}{3}$) and which is 42.5 decimetres (13 feet) high.

Account of its effects.

Every day a log of wood 28 or 30 centimetres (10 or 11 inches) round, sawn into three pieces, or an equal quantity of smaller wood, is burned in it at once. The slider of the door

of the fire-place is shut, and the key R, fig. 6 is turned, as soon as the wood is reduced to charcoal. Ten hours after the air throughout the room is at a temperature above the mean; and the centigrade thermometer, placed 36 centimetres (above 13 inches) from the stove, rises rapidly to 16 or 17 degrees.

To shew still more plainly to what degree the economy of fuel and preservation of heat may be carried by this construction, I shall relate another experiment, which I have repeated on several occasions, and which has always afforded me very nearly the same results.

The thermometer in the room, in which there was no fire the day before, being between 9 and 10 degrees, a log sawn in three as usual was put into the fire-place about eleven in the morning; and at three in the afternoon a similar quantity of fuel was put in.

At four o'clock the thermometer, placed at the distance above-mentioned, was at 42 degrees.

At five, at 37 degrees.

At seven, 34.

At nine, 31.

At midnight, 26.

You could not bear to touch with the hand the iron rim of the heat openings. The bulb of the thermometer being placed opposite one of these openings, at the distance of 8 centimetres (about 3 inches) rose in four minutes to 35°.

The next morning at 9 o'clock the thermometer, which had been again placed at the distance of 35 centimetres, was at 22°.

Finally at noon, that is to say twenty-one hours after the last wood was put in, and eighteen hours after the key had been turned, all the wood being reduced to charcoal, the thermometer stood between 18° and 19°. It was then placed two centimetres only from one of the heat openings, and in less than six minutes it rose to 26°.

These effects are so different from what we commonly obtain by the consumption of three or four times as much fuel, that I may expect more than one reader to suppose them exaggerated; but I hope a sufficient number will be found disposed to make a trial of these stoves, that their testimony and example may at length triumph over our habits, and produce a general conviction, that, without suffering any privation ourselves, we may preserve for our offspring what useless waste is daily robbing them of in an article of the first necessity.

VII.

*A Continuation of the Experiments and Observations on the Light which is spontaneously emitted from various Bodies *; with some Experiments and Observations on solar Light, when imbibed by CANTON's Phosphorus. By NATHANIEL HULME, M. D. F. R. S. and A. S.*

SECTION XI.

The Effects of various aerial Fluids on spontaneous Light.

INTRODUCTION.

THE apparatus employed for experiments with any kind of Apparatus made air, unless otherwise expressed, consisted of the following ^{use of,} parts: 1. A tea-faucer, holding about three ounces of water. 2. A wide-mouthed phial, which would contain about ten ounces of liquid. 3. A small wooden stand, composed of a slender pillar or pin, nearly four inches high, fixed into a round base, a little more than an inch in diameter, and half an inch thick. This stand was fastened by strong thread to the middle of a piece of flat lead, such as lines Chinese tea-chests, having holes in it to admit the thread; the lead was about three inches square, and doubled, to give it weight and stability. The top of the pillar was made pointed; and a round piece of cork, about an inch in diameter, and half an inch thick, was fixed upon it, by means of a superficial hole bored in its under part with a gimlet.

When the whole apparatus was put in use, the phial was ^{and its use.} filled with cold pump water, in a pneumatic tub, then inverted, and the species of air to be employed was let up into it, to the quantity of about eight ounces. The subject for experiment being applied to, or fastened upon, the top of the cork, the stand was placed on the tea-faucer, and then introduced, under water, into the phial containing the air. The whole apparatus, being now supported by the tea-faucer,

* Phil. Transf. 1801. p. 483. The former part is in the Phil. Transf. for 1800, page 161, and also in the Philos. Journal, Quarto, IV. 421, 451,

with

with water in it, was deposited in the laboratory for experiments on light. By this contrivance, the experiments were made in about eight ounces of air, by measure, confined above two ounces of water.

§ 1.

The Effects of common or atmospherical Air on spontaneous Light.

EXPERIMENTS.

The exclusion of atmospheric air prevents the appearance of spontaneous light.

Fish.

Experiment 1. Two fresh herrings were hung up together in the laboratory, so as to touch each other at their flat sides; and it was observed that the parts in contact remained dark, while those exposed to the open air became very luminous.

Exp. 2. Another fresh herring was laid upon a piece of thick brown paper, and placed in the laboratory. On examination, the next evening, the upper part, which was exposed to the air, was very lucid; but the under-side, lying upon the paper, remained quite dark.

Exp. 3. A luminous herring was divided transversely quite through its middle fleshy part; but the inside was perfectly dark. On the following night, that which before was dark had become luminous.

Exp. 4. At 9 P. M. a piece of fresh herring, of about three drams in weight, was introduced above water, into about eight ounces of atmospherical air. On the second night it was become luminous; on the third and fourth, it continued shining; and on the fifth the light was extinguished. This experiment was frequently repeated, with both the flesh of herring and of mackerel, and nearly with the same result.

Exp. 5. The cork of the apparatus was well smeared with the luminous matter of a mackerel, and then introduced above water. It continued to shine finely all that evening; and the light was not quite extinct on the succeeding night.

Exp. 6. Another cork was illuminated with herring-light, at half an hour past six P. M. and introduced above water. It remained very bright at eleven; and retained a glimmering light the next evening. The two last experiments were often repeated, and, in general, with similar effects. It may not, however,

however, be improper to observe, that the illumination of the cork did not always continue so long as twenty-four hours; for it must, of course, vary according to the quantity of luminous matter applied, and its degree of brilliancy.

Exp. 7. A large piece of rotten wood was received from **Rotten wood.** the country, which shone only in one place. The luminous portion was sawed off for use, and the dark part left in the laboratory. On going into the laboratory, the second night after this operation, I was surprised to see the dark piece, which had been left there, very lucid in several places where small splinters had been broken off in sawing; many shining fragments also lay scattered on the floor.

Exp. 8. A quantity of rotten wood, moderately shining, was blown upon for some time with a pair of bellows; but I could not perceive that this had any effect on the light, so as to render it more vivid.

Exp. 9. A small piece of shining wood was tied upon one of the corks of the apparatus, and introduced above water, where it continued lucid until the fifth night. In another experiment, the light was extinguished on the fourth night; and and in a third much sooner.

Exp. 10. A living glow-worm, in a shining state, was **Glow-worm.** submitted to the action of a pair of bellows; but the con- These are not tinuance of the blast did not apparently increase its glowing ^{excited by bel-} _{lows.} quality.

Exp. 11. A very luminous dead glow-worm was fixed upon a cork of the apparatus, by means of a small pin, and then put into the phial, above water. It continued to shine as vividly as it did when in the open air, forming a pure white light, of a circular shape.

OBSERVATIONS.

Observation. 1. These experiments prove, that objects which abound with spontaneous light in a latent state, such as the herring, mackerel, and the like, do not emit it when deprived of life, except from such parts as have been some time in contact with the air.

Obs. 2. They likewise show, that the blast of a pair of bellows does not increase this species of light, as it does that which proceeds from combustion.

§ 2.

The Effects of oxigen Gas or vital Air on spontaneous Light.*

EXPERIMENTS.

Fish, shining wood and Glow-worms in common air and in oxigen gas exhibited no difference of illumination.

Experiment 1. A piece of fresh herring, of about three drams weight, was introduced above water, into eight ounces of oxigen gas. On the second night it was observed to be faintly luminous; on the third, the quantity of light was increased; on the fourth, it continued nearly in the same state; and on the fifth the light was diminished.

Exp. 2. A piece of very fresh mackerel, of the same magnitude, was also put above water. On the subsequent evening it was pretty lucid, and continued the same on the night following.

Exp. 3. At 9 P. M. a cork, finely illuminated with mackerel-light, was introduced above water: it continued very lucid at eleven. On the next evening it was dark.

Exp. 4. Another cork, rendered luminous with the same kind of light, was put above water at 10 P. M. The next morning, at six o'clock, only a glimmer of light was perceived and at 10 P. M. it was extinct.

Exp. 5. At 9 P. M. a fragment of shining wood was introduced above water; it was observed to be still very luminous at eleven; but the light was not quite so vivid, nor so extended in breadth, as when the wood was put in. On the succeeding night, at eight o'clock, it remained faintly lucid.

Exp. 6. A little after 8 P. M. another fragment of wood, shining very brightly, was introduced above water, into the same air that was used in the last experiment: it continued very luminous at eleven; but the light was diminished in quantity. On the next evening it was found to be extinguished.

Exp. 7. The same air was employed again at 8 P. M. with a pretty large and thick fragment of wood, uncommonly lucid: its light continued vivid and broad at half an hour past eleven. The following night, at eight o'clock, the light was still somewhat extensive and bright.

Exp. 8. In three other experiments with shining wood, in fresh oxigen gas, the light was totally extinguished in the space of twenty-four hours.

* The oxigen gas made use of was obtained from manganese, by means of heat

Experiments were made, at the same time, and in the same manner, with atmospherical air and shining wood; but it was not very evident that the wood shone more vividly in the latter air than it did in the oxygen gas.

Exp. 9. A living glow-worm was put into a two-ounce phial, with a glass stopple, containing pure oxygen gas, and kept therein for some time. It was then taken out, and exposed to the open air; but no difference, either in the brilliancy or the quantity of its light, could be discovered.

Exp. 10. A luminous dead glow-worm was then inclosed in about five ounces of the gas; but no increase of its shining quality could be perceived.

Exp. 11. At six o'clock P. M. a shining dead glow-worm was introduced above water into oxygen gas: it continued very lucid therein at 7 P. M. shewing a pure white light. It was then taken out, and put above water into atmospherical air, where it shone, to all appearance, as splendidly as it did when it was in the oxygen gas.

OBSERVATION.

It appears, from these experiments, that oxygen gas does not act upon this kind of light, so as to render it much more vivid than it is in atmospherical air; which is quite contrary to what some authors have alledged.

§ 3

The Effects of azotic Gas on spontaneous Light.

1. *Azotic Gas, obtained from lean muscular Flesh and diluted nitric Acid, in a very low Heat, as recommended by M. de FOURCROY.*

EXPERIMENTS.

Experiment 1. A piece of fresh mackerel, weighing about three drams, was introduced above water, into about eight ounces of this azotic gas; and it was retained therein five days, without emitting any light.

Exp. 2. About the same quantity of fresh herring was then put above water, into the same gas used for the last experiment, and remained in it for the space of three days, in a dark state. This experiment was repeated, and with a similar result.

Exp. 3. At 45 minutes past 7 P. M. a cork, finely illuminated with mackerel-light, was put above water into the gas, and it was found pretty luminous at eleven. On the next evening, at eight o'clock, it still exhibited a faint degree of light.

A fimilar experiment was made, at the same time, in atmospheric air. At 11 P. M. the cork was but moderately luminous; and on the next evening it was dark.

Exp. 4. At 40 minutes past 7 P. M. another cork, rendered very luminous with herring-light, was introduced above water. This cork, at 11 P. M. was not found so lucid as that in the third experiment. On the next evening, a glimmer of light was still perceptible.

Exp. 5. A fragment of very flining wood was introduced above water, into this gas; and it was rendered dark in about 15 minutes.

Exp. 6. The experiment was repeated; and the light was again extinguished in about 15 minutes. In another experiment it was extinguished in about 25 minutes.

II. *Atmospheric Air rendered azotic, by burning Spirit of Wine in it, when confined above Water*

Atmospheric
azote.

Exp. 7. A portion of fresh herring, of about three drams, was put above water, into this azotic gas, at 5 P. M. On the second evening, a spark of light was observable; on the third, the quantity of light was increased; on the fourth, it was again diminished.

Exp. 8. At 3 P. M. the usual quantity of herring was introduced above water. On the second night, it remained dark; on the third it was moderately luminous; on the fourth, it was less so; on the fifth, the light was extinct.

Exp. 9. A piece of fresh mackerel was next put above water, at 11 A. M. On the second evening, it was found to be slightly luminous; it remained so on the third; on the fourth, it was dark.

Exp. 10. Another piece of fresh mackerel was introduced above water, at 3 P. M. On the second night, it was found to be slightly luminous; but on the third, it was dark; and no more light was emitted, though it was kept in the gas for the space of four days.

Exp. 11. A cork, made very luminous with herring-light, was put above water, into this gas, at 20 minutes past 8 P. M. and it continued very lucid at eleven. The next evening, at ten o'clock, the light was nearly extinguished.

A fimilar experiment was made, at the same time, in common atmospheric air, and with the same result.

Exp.

Exp. 12. Another cork was introduced above water, with herring-light, at 40 minutes past 7 P. M. and it remained pretty luminous at eleven. On the following night, it was nearly extinct.

III. *The last mentioned azotic Gas, after being washed with Lime Water.*

Exp. 13. A piece of herring, of about three drams weight, was put above water, into this azotic gas, at 5 P. M. On the second night, it was dark; on the third, very lucid; and on the fourth, the same.

Exp. 14. The experiment was repeated, on a piece of herring, at 3 P. M. On the second evening, it was dark; on the third, pretty luminous; on the fourth, it was less so; and on the fifth, only a faint light remained.

Exp. 15. A portion of fresh mackerel was then put above water, at 11 A. M. On the second night, it was observed to be moderately shining; on the third, the light was extinct.

Exp. 16. Another piece of fresh mackerel was introduced above water, at 3 P. M. On the second evening, it was slightly luminous; on the third, it was dark, and continued so during the four succeeding nights.

Exp. 17. A cork, finely illuminated with herring-light, was next introduced above water, into this gas, at 20 minutes past 8 P. M. The light was much diminished at 45 minutes past 8; at 11 the cork had become almost dark. On the following night, a glimmer was still apparent.

Exp. 18. Another cork, made very luminous with herring-light, was put above water, at 40 minutes past 7 P. M. and it continued pretty lucid at eleven. On the next evening, the light was merely visible.

A similar experiment was made, at the same time, in atmospheric air, and with nearly the same effect.

OBSERVATION.

It is a remarkable circumstance, that azotic gas, which is incapable of supporting light from combustion, should be so favourable to the spontaneous light which is emitted from fishes, as to preserve its existence and brilliancy for some time; when applied upon a cork; yet that it should prevent the flesh of the herring and the mackerel from becoming luminous, and also extinguish the light proceeding from rotten wood.

§ 4

The Effects of hidrogen Gas or inflammable Air on spontaneous Light.*

EXPERIMENTS.

Hidrogen gas does not admit the production of light.

Experiment 1. At 9 P. M. a piece of fresh herring, weighing about three drams, was introduced above water, into hidrogen gas. It was retained therein three days and three nights, without emitting any light. It was then taken out, and exposed to the action of atmospherical air. On the following night it was found to be luminous; but was dark again on the next night.

Exp. 2. Another piece of fresh herring was put above water, at 6 P. M. This was also kept in the gas the same length of time, without producing any light. It was then exposed to the open air, and inspected two successive nights, but it remained dark.

Exp. 3. The same experiment was then made with a piece of mackerel, which was taken out on the fourth night, without producing any shining appearance. The next evening, it emitted a very faint light, which did not continue twenty-four hours.

but extinguishes it. (Fish)

Exp. 4. A cork, brilliantly illuminated with mackerel-light, was introduced above water; and the light was extinguished in about the space of an hour.

Exp. 5. At 39 minutes past 9 P. M. another luminous cork was put above water; it lost some of its light pretty soon, but was not extinct at twelve.

Exp. 6. A cork, with herring-light, was introduced above water, at 23 minutes past 6 P. M. The light gradually diminished, and was only faintly visible at eleven.

Exp. 7. A fragment of very shining wood was put above water, at 9 P. M. and was dark at eleven.

Common air again revives it.

Exp. 8. Another fragment was put above water, at 40 minutes past 8 P. M. at 50 the light was much diminished, and at 8 minutes past 9 the shining ceased. The wood was then taken out, and exposed to the open air, when the light revived in a very beautiful manner.

* This gas was obtained from zinc and diluted sulphuric acid;

Exp.

Exp. 9. A piece of uncommonly shining wood was introduced above water, at 58 minutes past 8 P. M. it remained for a short time very luminous, but at 25 minutes past 9 the light was greatly diminished; at 20 past 10 it was nearly extinguished; and at 29 past 10 was quite dark. It was then exposed to atmospheric air, and the light revived very brightly.

Repetition of
extinction and
refuscitation.
(Wood)

Exp. 10. The same experiment was repeated, at 35 minutes past 8 P. M. the shining property was much diminished at 9; and at 10 it was very faint. The next evening, it continued merely visible. The wood was now taken out, and the light soon revived very strongly. The following night, it was still moderately lucid; but on the next evening nearly extinct.

Exp. 11. Finding, by the above experiments, that the light of shining wood was extinguished by this species of gas, and restored by atmospheric air, the following three trials were made, to discover, in some degree, how long its light might be kept in a latent state, and then be revived. At 9 P. M. several fragments of shining wood, tied up in a piece of gauze, were introduced above water, into the hydrogen gas, and the light was gradually extinguished during that evening. They were kept there in that dark state 48 hours, were then taken out, and exposed to the open air, when, after a little time, the light re-appeared.

Extraction for
48 hours.

Exp. 12. On the 2d of October, another fragment of exceedingly shining wood, two inches and a half long, and pretty thick, was put above water in the evening, and its light was gradually extinguished. On the second night, it was taken out perfectly dark, but its light recovered by degrees, and became brilliant. It was introduced again, that evening, into the same gas, and its light disappeared. On the third night, it was again exposed to the open air, and the light revived as before. It was then reinstated and extinguished, and continued in a dark state, from the third to the fifth night, when, being again taken out, it soon shone in a pretty vivid manner. It was again introduced and extinguished as usual; and no observation was made of it, from some accidental circumstance or other, until the 10th of November in the evening, when it was taken out, and exposed to the open air for a length of time, but the light did not revive.

Three repetitions
of extinction
and renovation.

Exp.

No renovation
after a week

Exp. 13. A third fragment, somewhat larger than the former, and equally luminous, was put above water, at the same time as the one in the last experiment, where it was soon deprived of its light. It was retained there, in a dark state, from the 2d of October till the 10th of November; it was then taken out, and exposed to the action of atmospherical air, for several days, but there was no return of light.

Three extinc-
tions and reno-
vations of a
glow-worm

Exp. 14. About 7 P. M. a shining dead glow-worm was introduced above water into the gas, and its light was soon extinct. It was then exposed to the open air, where, in a very short time, it shone as brightly as before.

Exp. 15. At half an hour past 9 P. M. the same glow-worm was again introduced above water; when its light in a short time disappeared. It was taken out for exposure to common air at 11, and its glowing property was immediately restored. It was again replaced in the gas, where it soon lost all its light a second time, and was kept in that dark state for 24 hours; when taken out, it continued dark for a little time, and then the insect gradually recovered its pristine splendour.

OBSERVATION.

From these experiments we learn, that hidrogen gas, in general, prevents the emission of spontaneous light, and also extinguishes it when emitted; but, at the same time, it does not hinder its quick revival, when the subject of the experiment is again exposed to the action of atmospherical air; although the light may have been a considerable time in an extinguished state.

(To be concluded in our next.)

VIII.

*Caution against the great Danger of keeping Phosphorus in Bottles without particular Caution. By DESCROZILLES the Elder.**

I have narrowly escaped falling a victim to an accident occasioned by the effect of the frost upon a bottle, which contained a hectogramme of phosphorus, with a quantity of water sufficient for covering this highly inflammable substance. This morning, before day-light, some books and a chest suddenly caught fire in the apartment in which I lay, and which is not occupied during the night. It was still fortunate that the two hours of my accustomed sleep were long since elapsed. Some seconds later, I should have been suffocated by the deleterious vapours of the phosphoric acid. Quickly gaining the door, I called for assistance, and we succeeded in extinguishing the fire before it had made any progress.

Dangerous
accident of fire
from phosphorus.

According to all appearance, the frost which had taken place some days before in this piece, had caused the bottle to break ; but it was surrounded with paper, which prevented its falling to pieces. By this means, in proportion as the water ran off by the effect of the thaw, the bundle of phosphorus, exposed to the atmospheric air, was situated under circumstances the most favourable to ignition. The sides of the bottle performed the office of a small furnace, in which the cylinders of the combustible were propped against one another. Soon the gradual combustion which produces phosphorus acid was succeeded by the rapid deflagration, the result of which is phosphoric acid.

How it happened.

Independently of the effects of frost and of blows, a bottle frequently breaks without any apparent cause, and as it were spontaneously : It appears therefore to me, that in order to obviate an inconvenience which in some cases may prove very serious, the best means would be to use cases of copper, strongly soldered, and lined internally with paper or bran, for inclosing the bottles filled with phosphorus and water. Cases of tinned iron would be destroyed much sooner by oxidation, and their soldering would be susceptible of detaching itself by the effect of a moderate heat.

Other cautions.

Finally, it appears to me that this report ought to be as public as possible.

* In a letter to the Editors of the *Annales de Chimie*, No. 123.

IX.

Observations in Answer to DR. PRIESTLEY'S Memoir in Defence of the Doctrine of Phlogiston.* In a Letter from MR. WILLIAM CRUICKSHANK.

To Mr. NICHOLSON.

SIR,

Woolwich, March 22, 1802.

Observations of Dr. Priestly, that metallic calces contain little addition except water.

But when reduced by mere heat they afford pure oxygen, very nearly equal to their loss of weight, and no water.

If calces contain water only, then charcoal must produce the same

IN your Journal published the 1st of this month, I find a letter from Dr. Priestley, in which he still defends the old doctrine of phlogiston, but with very little success; for having additional difficulties to struggle with, he has been under the necessity of adopting new, and sometimes contradictory opinions, in his explanations and defence; For example, it is now his opinion that all metallic calces contain water and little or nothing else; and that charcoal, uniting with water, forms both fixed and inflammable air; for as this substance contains the elements of both kinds, nothing but water is wanted to enable them to take the form of air. It is almost impossible to argue against such strange suppositious and loose reasonings as these. However, let us suppose water to be the only substance contained in oxides; if so, heat alone ought to revive at least some of them, and in this case nothing but water should be separated. Now that heat alone revives several, particularly those of mercury and the perfect metals, is a fact sufficiently ascertained; but, instead of water, we obtain the purest oxygen gas, the quantity of which added to the revived metal, amounts, as nearly as possible, to the weight of the original oxide; even from the ore of manganese, so difficult to reduce, a prodigious quantity of the purest oxygen gas may be procured by a moderate heat; and when the oxide has been previously reduced to powder and well dried, by a heat nearly red, no water whatever can be perceived: in this case, too, the quantity of the oxygen gas disengaged, will be found to correspond very nearly with the loss of weight in the oxide.

How is it possible, then, that these phenomena can be explained on the supposition, "That all the calces contain little

* Philof. Journal I. 181.

“ or nothing else but water?” when not a particle of water thing by heat
 is to be seen during the revival, or partial revival, of the me- with calx as with
 tals from their oxides in close vessels. Let us, however, con- water;
 sider this supposition in another point of view; which is, that
 if water only were contained in these oxides, then the gas ob-
 tained from a mixture of them and charcoal, should be the very
 same as that procured from moistened charcoal; but the con- contrary to fact.
 trary of this I have clearly proved to be the case (see Phil.
 Journal, vol. v. quarto, p. 6.); and this was one of the princi-
 pal faults which led to the discovery of the gaseous oxide.

I shall here just enumerate a few of the properties by which Enumeration of
 these gases may be readily distinguished, being deduced from a differences; spe-
 number of experiments, often repeated with uniform results. cific gravity of
 First, then, the specific gravity of the gaseous oxide of carbon gaseous oxide of
 is no less than double that of the gas obtained from moistened carbon double
 charcoal, being as 30 to 14.5. Secondly, The proportion of that of hydro-
 oxygen necessary to saturate the gaseous oxide, is to that re- carbonate;
 quired by the hydrocarbonate as 15 to 44.8, or 1 to 3, nearly saturating oxygen
 estimating by quantity. And, thirdly, which is the most dif- very different;
 finguishing property of the whole, the same quantity of oxygen, and products of
 suppose 14 parts, which, when combined with the gaseous ox- carbonic acid
 ide of carbon, produces from 36 to 44 parts of carbonic acid with like por-
 (according to the purity of the oxide), will, when combined to tions of oxygen.
 saturation with the hydrocarbonate, produce only 12 parts of
 the same acid, accompanied, however, with much water, pro-
 ceeding in part from the hydrogen in the charcoal, of which all
 common charcoal, it would appear, contain a certain propor-
 tion (see P. Jour. No. 55. p. 210 and 211, and also the Table
 of Analysis, &c. p. 8. No. 59.) Surely gases having proper-
 ties so essentially different, can never be considered as the same.

Dr. Priestley remarks, that there is a considerable difference Differences in
 in the qualities of heavy inflammable air, depending not only heavy inflam-
 on the substance employed, but also on the stages or periods of mable air noticed
 the process itself*. Now I have found that these variations by Dr. Priestley,
 never take place in any remarkable degree but when charcoal are found only
 is employed in some form, and that even then the differences when charcoal is
 chiefly depend upon the imperfect state of the charcoal; for if present,
 good charcoal be exposed to a red heat in close vessels during and more or less
 10 or 15 minutes, and then mixed whilst hot with the substance impure;

* P. Journ. No. 3. (Oct.) p. 183.

to be employed, likewise hot and completely dried, the variations in the qualities of the gases will be much less, the principal difference depending upon the proportions of the carbonic acid and the inflammable gases; but the gases from pure charcoal, either alone or moistened, never have any of the distinguishing characters of the gaseous oxide, being always much lighter, yielding bulk for bulk when saturated with oxygen, not more than one-third of the carbonic acid gas afforded by the other. What, however, distinguishes them still more completely, is the large proportion of water generated by the combustion of the hydrocarbonate in oxygen gas; for the gaseous oxide when pure, or burned under the same circumstances, never produces the least sensible quantity of this fluid.

the hydrocarbonates are much lighter than the gaseous oxide, and they afford water by combustion with oxygen.

Erroneous quotation and remarks.

In mentioning the circumstances of the production of the gaseous oxide of carbon from the metallic calces and charcoal, &c. Dr. Priestley has somehow mis-stated both the meaning and words in what he calls a quotation from the first paper; this passage in his letter is as follows (see p. 182.): "After repeating my experiment, which he found to be just, Mr. Cruickshank did the same with the calces of the other metals, as zinc, copper, &c." and then concludes (p. 4.) "that in ALL these cases the air must come from the partial decomposition of the carbonic acid by the calx, when raised to a high temperature." Then the Dr. goes on and adds: "But the inference that I think is more naturally drawn from them is, that all these calces contain much water, and little or nothing else." Now the passage from which this quotation appears to have been taken (for there is nothing in p. 4. exactly similar to it), does not follow the account of the experiments with the calces of the metals, &c. but is an inference drawn from the first experiments made with the carbonates and the iron scales. I shall insert this passage, and leave the philosophical reader to judge for himself of the accuracy of the statement and the justness of the conclusion. "Conceiving that in these experiments * the gaseous oxide must proceed from the partial decomposition of the carbonic acid by the iron when raised to a high temperature, I thought I should succeed better by employing iron filings in place of the grey

* Alluding to these made with the carbonate of barytes and iron scales, or imperfect grey oxide.

“oxide, as these would have a greater affinity for oxygen.” From the above statement the misrepresentation both of the explanation and words must be manifest, and the insertion of the word *all* is curious enough. (Essay on Gaseous Oxide, &c. Philos. Journal, vol. v. quarto, p. 4.)

In the experiments made with the mixtures of carbonate of lime dried as much as possible and metallic filings, the Dr. conceives that the gas must have been produced by the water which, still remaining in the carbonate, had united with the phlogiston of the metal, and passed over in the form of inflammable gas, mixed with the fixed air separated from the chalk.

Fillings and chalk heated, supposed by Dr. Priestley to afford a gas consisting of phlogiston and water.

If the inflammable gases were produced in these cases by the water separating from the earth, and passing over the red-hot metal in the form of steam, then they should be the very same as that obtained by passing the vapour of water through a red-hot iron tube; but it is well known, the gases thus produced are so far from being the same, that they are extremely different in all their properties; for the hydrogen, or light inflammable air, produced by the decomposition of the water in passing through the tube, is the lightest of all aeriform fluids, and, when combined with oxygen, there is not the least appearance of carbonic acid, and nothing formed but water. On the contrary, the gaseous oxide procured by heat from a mixture of the driest earthy carbonates with metallic filings, is the heaviest of all known inflammable gases, and when united with oxygen, produces nothing but carbonic acid, there not being the least appearance of water. It is impossible, therefore, that these heavy inflammable gases should be produced by water alone, in any state, acting upon pure metals; for the gases obtained from the decomposition of this fluid, whether from the solution of metals in dilute acids, or from their action on its vapour when raised to a red heat, are always of the same nature, being pure hydrogenous gas.

If so, the gas ought to be the same as with fillings and water;

but this is the lightest of elastic fluids, and forms water by combustion;

whereas the gaseous oxide is the heaviest inflammable gas, and produces carbonic gas by combustion, with no water.

Dr. Priestley observes, that before I admitted that the iron or its calx, raised to a high temperature, could decompose the carbonic acid in this experiment, I should have tried whether it would do it in any other. This remark clearly proves that he had not seen the second essay on the gaseous oxide, &c. in your Journal for the month of August, in which a process is described, where this acid is decomposed, even in its gaseous form, by passing it repeatedly through a red-hot iron tube filled

Recapitulation of the experiment wherein carbonic acid gas passed over ignited iron gave oxygen to the metal, and was itself converted into carbonic oxide.

in

in the middle with iron wire. Bladders filled with this gas were attached, by stopcocks, to the extremities of the tube. After the middle of the tube, placed in a furnace, had been made red hot, the gas was repeatedly passed, and very slowly, from one bladder to the other. During this passage it was exposed to a very extensive surface of red-hot iron; and in one of the last experiments made in this manner, after forcing the gas through the tube backwards and forwards 30 times, four parts in five of the carbonic acid gas was converted into gaseous oxide: the gas upon the whole was a little diminished. The iron wires, after the operation, were covered with the same shining crust as if the steam of water had passed over them; and in fact they were oxidated or calcined to a certain extent, in consequence of seizing from the carbonic acid a proportion of its oxygen sufficient to convert it into the intermediate state of an oxide*.

Dr. P. could not decompose carbonic acid by the burning glass with iron, because the contact of ignited surface was insufficient.

The method by which Dr. Priestley attempted to decompose the carbonic acid gas, by heating pieces of iron with a lens placed in it, could not possibly have succeeded, at least to any sensible degree; for the heated air when left at liberty to ascend, would not remain in contact with it, nor even near it, for a moment: thus every portion of air, being unconfined, would expand prodigiously, so that very little of it could come or remain in contact with the heated metal. In the experiment above related, the gas was computed to pass several times over a surface of red-hot iron of some extent; yet, notwithstanding this, a considerable time was required to produce any remarkable decomposition in the acid; a circumstance which proves that, in the method employed by Dr. Priestley, no sensible decomposition could have been produced. There is likewise, in my second Essay, another method still simpler for decomposing the carbonic acid in its gaseous state †. All the oxidible metals are, when raised to a high temperature, capable of decomposing it more or less; but zinc is by far the most powerful, owing no doubt to its greater affinity to oxygen. Dr. Priestley asks, if, in the experiment with the finery cinder and charcoal, the oxygen to form the carbonic acid should come from the calx, how is it to be expelled, as heat alone will not

Simpler method of decomposing carbonic acid gas.

Though mere heat does not expel oxygen from finery cinder; yet heat and the attraction of charcoal may;

* For the particulars of this experiment, and the mode of conducting it, see this Journal, quarto s. No. 55. p. 209.

† No. 55. p. 209.

do it, &c. &c. ? But here he has forgotten that the affinities of bodies are remarkably varied by change of temperature, and that, in all reductions of metallic oxides, the carbon of the charcoal, when raised to a high temperature, unites with the oxygen of the calx in consequence of increased affinity, and forms with it carbonic acid gas and gaseous oxide. That this is the case is remarkably proved by distilling the red oxides of mercury and lead with charcoal; for these oxides we know contain a large proportion of oxygen, which may be separated by heat alone, but when heated with charcoal, nothing but carbonic acid and gaseous oxide come over, because, in this case, the whole of the oxygen being in its nascent state, combines, even at a low temperature, with the charcoal, and passes over in the form of carbonic acid gas mixed with the gaseous oxide. There are some circumstances accompanying the distillation of charcoal with these metallic oxides, which are easily reduced, and at low temperatures, not altogether uninteresting, and, in my opinion, unanswerable by the supporters of phlogiston. In these processes the proportion of gaseous oxide to carbonic acid gas is but small, being in general about one-third or one-fifth of the whole, and for the most part is obtained just before or at the time the mixture becomes red: It is always accompanied with a torrent of carbonic acid gas. At the instant the metal is revived, the gas either ceases entirely, or comes over very slowly; but on increasing the heat, it again makes its appearance, and is now so far from containing gaseous oxide, that it is peculiarly light, not mixed with any sensible quantity of carbonic acid gas, and yields, when saturated with oxygen, but a very small proportion of this acid gas.

The following facts may be drawn from these experiments: First, It would appear that a much greater degree of heat is necessary for the proper production of the gaseous oxide than for the carbonic acid. Secondly, That oxygen, in its nascent state, may unite with carbon at less than a red heat, and form carbonic acid; as is clearly proved by the process with the red oxide of mercury and charcoal. The gaseous oxide appears likewise to be produced at a very low heat.

I have now taken a view of all the arguments which Dr. Priestley has brought forward in defence of his former opinions, but shall at present make no further observation on this subject, leaving the argument and fact to be decided by your philosophical and chemical readers.—I am, SIR, &c.

X.

On the new Planet CERES.

To Mr. NICHOLSON.

S I R,

Parson's Green, April 3, 1802.

The subject re-
fused.

AT the conclusion of the memoir concerning the new planet Ceres, which you did me the honour to publish in your last number of the Philosophical Journal, a want of leisure, and the length of the communication, were alledged as reasons for my not concluding, at that time, the whole of the observations which I had to offer on the subject: I beg leave, therefore, now to resume the examination and detail of those particulars which remain yet to be treated of.

Discovery of a
method of de-
ducing the great-
est equation from
the eccentricity.

About four years ago, when I was inventing a mechanical contrivance, by which the equation of the center and true distance of a planet, or any number of planets, might be exhibited in an orrery, I discovered that the *natural sine of half the greatest equation of any planet, is equal, or very nearly equal, to the decimal figures which represent the value of a vulgar fraction, composed of the eccentricity and mean distance of that planet*: For instance, if we take the mean distance of the earth from the sun at 100000, and the eccentricity, according to Lalande, at 1681,395, the fraction $\frac{1681395}{1000000}$, converted into a decimal expression of the same value, is 01681395; and, omitting the decimal point and three last figures, we shall have 01681 for the natural sine of $0^{\circ} 57' 47,6''$, which arc differs only about *half a second* from one half of the greatest equation, as given in the tables of the third edition of Lalande's Astronomy.

The process, in the form of an analogy, will be thus: As the mean distance : is to unity :: so is the eccentricity : to the natural sine of $\frac{1}{2}$ the greatest equation.

Tabulated num-
bers to shew its
correspondence
in all the planets.

This analogy will apply to all the other planets, as may be seen in the subjoined table, which I have calculated from the mean distances and eccentricities given in Lalande's Astronomy, and copied by Mr. Vince, except in the instance of Ceres, the data of which planet are taken from the elements of Gauss.

Planets.

Planets.	Vulgar Fractions.	Decimals, or Nat. Sines.	Correspondent Arcs.	Half the greatest Equation, 1750.
Mercury	$\frac{7955,4}{38716}$,20551	11 51 34,9	11 50 0
Venus	$\frac{498}{72333,24}$,00688	0 23 38,3	0 23 40
Earth	$\frac{1681,395}{100000}$,01681	0 57 47,6	0 57 48,2
Mars	$\frac{14183,7}{152369,27}$,09308	5 20 26,9	5 20 20
Ceres	$\frac{8250}{276730}$,02981	1 42 28,96	
Jupiter	$\frac{250137}{320379,2}$,04807	2 45 18,6	2 45 19,15
Saturn	$\frac{53640,42}{954072,4}$,05622	3 13 22,7	3 13 21
Georgian	$\frac{90804}{1918352}$,01733	2 42 45,5	2 43 38

In this table the greatest difference between the arcs contained in the same line of the two last columns, is that in the line of Mercury; but it may be worthy of remark, that half the greatest equation of this planet, according to Dr. Halley's tables, is 11° 51' 18"; and also that Lalande himself made a new determination of the elements of Mercury's orbit, as related in the "Memoires de l'Institute Nationale" of Paris for the "fourth year of the Republic;" in which the grand equation is given 23° 40' 45".

Remark concerning mercury.

Here, then, it appears, that the greatest equation of the new planet Ceres, which corresponds to the eccentricity assigned by Gauss, and copied into the different Journals, is about 3° 25', instead of 9° 27' 41"; so that, as I hinted before, either the eccentricity is almost two thirds *too little*, or the greatest equation almost two thirds *too much*. I mean not at present to enter into a geometrical demonstration of the analogy which I have used in procuring the above tabulated results; but leave it to exercise the ingenuity of your mathematical readers, some of whom will probably be induced to favour you and the public with a demonstration, as a separate article. I will, however, just prove to the reader the accuracy of the inference I have made with respect to Ceres, by means of the *elliptic hypothesis* of Ward, which is generally allowed to be a convenient approximation to be used for finding the equation of a planet, instead of either the direct or tentative methods, which are more accurate, but much more intricate.

Hence the greatest equation of Ceres, as assigned, does not correspond with its eccentricity.

Proof of the accuracy of this inference, by computation on the elliptic hypothesis of Ward.

By the elliptic hypothesis, the analogy for converting mean into equated anomaly is simply this: *As the aphelion distance is to the perihelion distance :: so is the tangent of half the mean anomaly : to the tangent of half the equated anomaly*; and the difference between these two anomalies constitutes the equation itself. Now, it is well known to all who are conversant in the theory of planetary motion, that in the projection of any elliptic orbit, a *circle*, described from the focus in which the sun is supposed to be, with a radius that is a mean proportional between the major and minor semi-axes, will cut the ellipse in two points, which shall be the points of mean distance; or, which is the same thing, the points where the equation becomes stationary, and consequently where it is a maximum. It is also equally well known to practical astronomers, and calculators of an ephemeris, that the equation varies very slowly for many degrees both before and after the points of mean anomaly corresponding to the greatest equation; and likewise that these points fall a little beyond the first quadrant from the aphelion, or three degrees of mean anomaly, by a quantity which depends upon the eccentricity of the orbit. In the orbit of Mercury the point of mean anomaly, when the equation is greatest, is nearly at 105° from the aphelion; in that of Venus it is between 90° and 91° ; in that of the Earth about 91° ; in that of Mars about 97° ; in that of Jupiter and Georgian between 93° and 94° ; and in that of Saturn about 94° . Hence it may be inferred, that if the greatest equation of Ceres be $3^\circ 25'$, the said point of mean anomaly will be about 92° : but that if the equation be $9^\circ 27' 41''$, it will be about 96° ; namely, somewhat short of that of Mars, the greatest equation of which is $10^\circ 40' 40''$.

Let us try now what the greatest equation will be upon both suppositions successively, according to the *simple elliptic hypothesis*.

	<i>Log.</i>
As the aphelion distance (27673+825) 28498	4,45481
Is to the perihelion dist. (27673-825) 26848	4,42891
So is the tangent of $46^\circ (\frac{92^\circ}{2}) \frac{1}{2}$ mean anom.	10,01516
	<hr/>
	14,44407
	4,45481
	<hr/>
To the tangent of $\frac{1}{2}$ eq. anom. $44^\circ 17'$ nearly	9,98926
Then $92^\circ - 88^\circ 34' = 3^\circ 26'$ is the greatest equation.	

Again,

Again, supposing the point of mean distance to be at 96° , we have in that case,

As the aphelion distance,	28498	-	-	-	-	-	-	4,45481
Is to the perihelion dist.	26848	-	-	-	-	-	-	4,42891
So is the tangent of 48°		-	-	-	-	-	-	10,04556
								14,47447
								4,45481

To the tangent of $46^\circ 17'$ nearly - - - - - 10,01966
 Then $96^\circ - 82^\circ 34' = 3^\circ 26'$ is the greatest equation, as before.

Hence it is indubitably proved, that the equation, as given by Gauss, is much too great for the eccentricity; and it appears also, according to what has been already asserted, that the equation at 92° and 96° of mean anomaly is nearly the same; that is to say, the difference will only be in the seconds.

The equation given by Gauss is too great for the eccentricity;

But the greatest equation of a planet is usually determined from a series of observations antecedently to the calculation of the eccentricity: therefore the error which has been detected may be in the eccentricity, and not in the equation; in which case, by reversing the analogy already used, we shall have this calculation, viz. As unity: mean distance 27673 :: natural sine of $4^\circ 43' 50\frac{1}{2}''$ or 03247,4 : 2282,2 for the requisite eccentricity. But it will be most easy to determine in which of the two elements the error has been committed, when the whole period has been accurately ascertained.

or the eccentricity is faulty:

but this will be best known from the period, when determined.

When it was mentioned in the former paper on this subject, that oppositions and conjunctions were of importance to be observed, the reason was omitted to be explained; which is, that when a superior planet is in opposition, or an inferior one in conjunction, the observed geocentric longitudes are also heliocentric longitudes, without calculation or reference to distance and eccentricity; because in such relative situations there is no parallax of the orb: and it is well known to astronomers, that when an opposition happens at the place of mean distance of a superior planet, half the difference between the heliocentric place, by calculation of mean motion, and of the place as observed at that time, is equal to the greatest equation. The 13th of March ult. was the day on which the astronomers on the continent predicted that an opposition of Ceres would

Observations of conjunctions and oppositions, and their use.

occur ; but it must have happened on the 23d, as I calculate from Von Zach's little ephemeris continued forwards ; viz. when the geocentric plane was about 182°. The astronomer who has an observatory, and has noted the *exact time*, will do well to make the observation public.

Application to
the planet Ceres.

The mean time which elapses between two successive oppositions or conjunctions of a planet, as seen from the earth, is called a synodic revolution, and is determined by dividing 360° by the difference of the mean daily motions of the earth and other planets. Thus : taking the mean daily motion of Ceres at 770,7376", according to Gauss, and of the earth at 58' 8,33" according to Lalande, we have $\frac{360^{\circ}}{2777,5924} = 466,6$ days nearly for the whole synodic period, on a supposition that the motions are both equable throughout their orbits ; but their respective distances from their aphelia at the time of opposition must be made the argument of a correction, either additive or subtractive, as the case may be, to determine what a synodic period would be if both motions were equable. Now, if we reverse this process, we can just as easily gain the difference of the daily motions between that of the earth and other planets, and consequently the whole period of the latter, from having only the earth's daily motion, and *observed synodic period* ; for 360°, divided by this period in days, gives the difference wanted at once, which, subtracted from the daily motion of the earth, gives that of the other, if it be a superior planet ; but if an inferior one, that difference must be added ; and the more nearly the two daily motions approximate to each other, the longer will be the respective synodic revolution. In the instance before us, if we suppose the whole corrected synodic revolution of Ceres to be 466,6 days from observation, we shall have $\frac{360^{\circ}}{466,6} = 2777,5924''$ for the *difference* to be subtracted from 3548,33" the earth's mean daily motion, which will leave 770,7376" for the mean daily motion of Ceres, as before ; by which if we divide 360°, we shall have the whole tropical period = 1681^d 12^h 8^m 49^s. But it remains to be observed what a whole synodic period of Ceres *may prove in reality*.

Determination
of the real pe-
riod, &c.

Supposing the epoch, or mean heliocentric longitude of Ceres to have been 2 S. 17° 36' 34" on January 1, 1801, the day of its discovery, as is stated by Gauss, and the place of the aphelion 10 S. 26° 27' 38", the mean anomaly must, on this

this supposition, have been at that time $3\text{ S. } 21^{\circ} 8' 56''$, so that it had passed the place of mean motion about either 19° or 15° , accordingly as we make the greatest equation $3^{\circ} 25'$ or $9^{\circ} 27' 41''$: therefore the daily motion was nearer a mean motion than it has been ever since; and it will be yet some months before it arrives at its place of mean motion in the opposite half of its orbit; which place is either 2° or 6° short of the *ninth* sign of anomaly, accordingly as we take the eccentricity. Let us suppose now the whole period to be upwards of 1681 days, as has been, perhaps prematurely, determined; one fourth of this time had elapsed on the 24th of February last; on which supposition, the *mean anomaly* must then have been advanced just *three signs* from the original situation; namely, it must have been upwards of $6\text{ S. } 21^{\circ}$, at which rate the planet had passed the perihelion by a space of time answering to 21° of mean motion, which is about 98 days: therefore the 18th of November, 1801, must have been the day on which it was at the perihelion, or place of greatest velocity; but at that time the planet *was lost*, and we are not in possession of any observation of it nearer that time than the 7th of December following, when Baron Von Zach re-discovered it.

The continuance of any planet in the first quadrant from aphelion is longer than in the second quadrant, by a space of time which corresponds to the whole equation, taken at three signs of mean anomaly; in which situation, it has been already observed, that the equated or apparent motion is also, as nearly as may be, a mean motion; if therefore the equation at three signs be divided by the mean daily rate of motion, we shall have a space of time, which, added to one fourth of the whole period, and subtracted from another fourth, will give nearly the respective times of continuance in the first and second quadrants of anomaly: Hence arises this rule for finding the two semicircles, respectively bisected by the perihelion and aphelion points, viz. divide four times the equation at three signs of anomaly, (which may be the greatest equation where the eccentricity is small), by the mean daily motion, and the quotient will be the number of days that the planet continues longer in the semicircle from nine to three signs of anomaly than from three to nine. For instance, if we take the equation

tion

tion of Ceres at $3^{\circ} 25'$, we shall have $\frac{3^{\circ} 25' \times 4}{77^{\circ} 7376''} = 63,83$ days for the time of continuance in the semicircle embracing the aphelion, longer than in the semicircle which is bisected by the perihelion: but if we take the equation at three signs $= 9^{\circ} 25'$, somewhat less than the greatest equation, in this case, by reason of the increased eccentricity, we shall have the excess of continuance $\frac{9^{\circ} 25' \times 4}{77^{\circ} 7376''} = 175,93$ days.

Observations from which the position of the apsides are deduced, &c. &c.

This suggestion may be worthy the notice of the practical astronomer; for when a variety of observations are taken of the new planet in the different quadrants of its orbit, and the corresponding times recorded, it will be no difficult task, when equidistant geocentric longitudes are converted into heliocentric longitudes, to observe what semicircle of the ecliptic corresponds to that half of the orbit in which the planet has *continued longest*: the middle of that semicircle will be the *aphelion*, and the two extremities will be three and nine signs of anomaly: Also, the *excess of duration*, above the time occupied by the other semicircle, multiplied by the *mean daily motion*, will be *four times* the equation at three and nine signs of anomaly very nearly; and as this equation is very little short of the greatest equation, the eccentricity may likewise be found by either of the methods already described: Thus the form and elementary points of the orbit may be gained by a series of observations converted into heliocentric places, even by the projection proposed in the last memoir on this subject, and these determinations may be corrected by a comparison of them with the results deduced from the properties of an ellipse, which are here purposely omitted, lest a more minute and scientific description of intricate calculations should rather puzzle than inform the generality of readers*.

Inclination of the orbit, and place of nodes.

It remains yet that some observations be made relative to the *position* of the orbit of a planet. There are many methods of ascertaining the nodes of a planet's orbit, from calculation grounded upon observations; but the simplest, when it is practicable, is to convert the geocentric into the heliocentric place at the time when there is no latitude by observation, for

* See Lalande's and Mr. Vince's Astronomy; and also Professor Robison on the Geo. Sidus, in the Edin. Transf. vol. I. 1788.

the heliocentric place will be the place of the node, ascending or descending, as the case may be, which will appear by a subsequent observation; but when the place of a planet, when crossing the ecliptic, cannot be observed, the *middle point* between two *equal north and south* latitudes, gained by observation, will give the node.

The heliocentric latitude, when a planet is just 90° from each node, is the measure of the inclination of its orbit, and is easily obtained from the observed geocentric latitude, taken in that situation, by the analogy already described; or, otherwise, the greatest heliocentric latitude may be acquired from an observation of a geocentric latitude and longitudinal distance from the node, thus: When the earth is in the line of the nodes, the analogy will be, *as the sine of the difference of the longitudes of the sun and planet seen from the earth : radius :: tangent of the geocentric latitude : tangent of the inclination.*

The two days on which the earth will be in the line of the nodes of Ceres will be June 12. and December 13. this year. But it is beyond the proposed intention of this popular memoir to enter into all the minutiae of calculation, were the requisite data before me; but only to point out the *methods* of applying observations for determining the size, form, and position of a planet's orbit: it may not, however, be unworthy of notice, before I conclude, to remark, that the astronomers on the continent, who availed themselves of the earliest observations *only* for determining an approximate set of elements of Ceres, were enabled to do this from noticing that this planet became *stationary* between the 10th and 11th of January, 1801, when its elongation was known by observations; for it has been shewn by writers on astronomy, that, upon a supposition of circular orbits, *the tangent of the elongation is equal to the semi-diameter of the orbit, divided by the square root of that semi-diameter + 1.*

Days when the earth will be in the line of the nodes of Ceres.

The early determinations respecting the orbit of Ceres were made from its stationary position.

Your's, &c.

W. P.

APPENDIX.

April 10, 1802.

Since the preceding paper on the planet Ceres was written, Mr. Ed. Troughton has put into my hands the duplicate of another letter, sent to him by the Baron de Zach, and addressed to Sir Joseph Banks, Bart. which I understand has been read at a meeting of the Royal Society, and which I shall here transcribe*, on account of some remarks which I have to make upon it. (Copy,)

Seeberg Observatory near Gotha, Feb. 20, 1802.

" DEAR SIR,

Letter from Baron von Zach.

" I had the honour to send to you my observations of the new planet Ceres Ferdinandea made in January, here I take the liberty to send the continuation of them made in February.

Table of observations.

1802.	Mean Time in Seeberg.	Ap. Right Ascen. observed.	App. Dec. observed.
Feb. 3	15 ^h 40' 55" S.	188° 42' 13,05"	12° 40' 5" N.
4	15 36 41,4	188 42 36,30	
5	15 32 45,1	188 42 30,15	12 50 25
9	15 16 43,7	188 38 3,90	13 14 18
19	14 34 46,7	187 58 27,90	14 20 3

Dr. Gauss has corrected his elliptical elements of the orbit upon my observations; here is what he has found since my last letter to you.

Elements.

Epoch for the beginning of the year to the meridian of Seeberg	- - - -	77° 27' 36,5"
Aphelion	} both fidereal,	325 57 15,0
Node		80 58 40,0
Greatest equation of the center	- - -	9 20 8,0
Inclination of the orbit	- - -	10 37 56,6
Logarithm of $\frac{1}{2}$ axis major	0,4424742	
Eccentricity of the orbit	0,0814064	
Mean diurnal heliocentric and tropical motion		769",7924

With these elements of the orbit all the observations made by Mr. Piazzi in Palermo, from Jan. 1, till Feb. 11, 1801,

* The letter was sent open to Mr. Troughton for the express purpose of copying and communicating the same.

W. N.
agree

agree perfectly well, and within a few seconds; and my observations are represented by them thus :

Seeberg observ.	R. A. calculated.	Differ.	Declin. calcul.	Differ.
1801, Dec. 7	178° 33' 29,2"	- 1,4"		
1802, Jan. 11	186 45 47,6	- 2,3		
16	187 27 38,8	-14,4		
22	188 6 18,2	- 7,6	0 ' "	
25	188 20 37,2	- 2,0	11 56 58,4	+35,4"
26	188 24 37,0	-12,5		
28	188 31 25,7	-12,1	12 9 55,6	+14,3
29	188 34 14,1	- 4,0		
30	188 36 38,4	- 5,5	12 19 19,8	+19,1
31	188 38 38,3	- 7,1	12 24 15,3	
Feb. 3	188 42 7,8	- 5,2	12 39 53,6	-11,4

As these elements agree hitherto so well with the heavens, the following ephemeris calculated upon them for the next month, will probably do the same, so I annex it here to point out to the English observers the place where they have to look for the Ceres.

Position of the Ceres for Midnight Mean Time in Seeberg Observatory. Ephemeris.

1802.	R. A. in degrees.	Decl. N.	R. A. in Time.
March 1	186° 41'	15° 30'	12 ^h 26' 45"
4	186 11	15 50	12 24 45
7	185 39	16 10	12 22 36
10	185 5	16 29	12 20 18
13	184 28	16 47	12 17 53
16	183 51	17 4	12 15 24
19	183 13	17 19	12 12 50
22	182 34	17 33	12 10 15
25	181 55	17 44	12 7 40
28	181 17	17 54	12 5 7
31	189 39	18 1	12 2 37
April 3	180 3	18 6	12 0 12
6	178 29	18 10	11 57 54

This planet will come in opposition to the sun, March 17 in the afternoon. At the same time this heavenly body will be in its greatest proximity to the earth = 1,6025, and therefore

Variable light of
Ceres.

fore the most favourable time to look for its satellites, if there are any, to measure its diameter; and to examine its nebulosity. About this time, the planet will also be in its greatest geocentric latitude = $17^{\circ} 9'$, and a little later she will have her greatest retrograde motion, about 13 min. in right ascension per day. The north declination will increase till the beginning of April, and about the 9th of the same month the motion in declination will commence to the south. It appeared to me that the Ceres has some change of light; I imputed it at first to our hazy atmosphere this winter, but Mr. Schroeter of Lilienthal, and Mr. Olbers of Bremen, sent me word that they have observed the same, and they believe that it is the planet which is subject to such changes of light. Mr. Herschel will tell us best whether it is so. I have some hopes to find the planet in ancient catalogues of stars. Mr. Melfier was very near it in the year 1779. The famous comet of that year ran just over the northern wing of Virgo, as now, and the new planet was not very far distant. If the comet had attained two months sooner the completion of Virgo, Mr. Melfier must infallibly have observed the Ceres then, because he determined all the little stars in the vicinity of the comet; the planet would have been in the way of the comet, and so of course he would have caught the little planet in 1799.

If my observations are acceptable to you, dear Sir, only a little hint, and I shall continue with pleasure to give you further intelligence.

I am,
with the greatest esteem and regard,
very respectfully,
most honoured SIR,
Your obedient humble Servant,
FRANCIS BARON DE ZACH.
*Lieut. Col. and Director of Seeberg Observatory
near Gotha Saxony."*

REMARKS.

Remarks.

1. The distance which corresponds to the logarithm of $\frac{1}{2}$ axis major, viz. 0,4424742, is 2,769964, the earth's radius being unity.

2. The whole tropical period from the mean daily heliocentric tropical motion, 769,7924', is $1683^d 13^h 41' 56,5''$.

3. The

3. The synodic revolution corresponding to this motion is $466^d 10^h 22^m$.

4. The time of opposition could not be on the 17th of March as stated in this letter, but about the 23d, as has been mentioned before, because it was on that day that the difference of the right ascensions of the sun and Ceres was 180° , even according to the Baron's own table: the error seems to have arisen from reckoning the point diametrically opposite Ceres to be nearly two degrees *short* of the equinoctial point, instead of the same quantity *over*, when the right ascension of Ceres was about 182° : the other circumstances also dependent on the moment of opposition must therefore be attributed to the 23d instead of the 17th.

5. On receiving these last corrections of Dr. Gauss I was at first surpris'd to find such a trifling alteration made with the greatest equation and corresponding eccentricity, after the error which I was confident I had detected; but I have now found out the cause of the apparent discrepancy, which some stress has been laid upon; the mean distance and eccentricity, I now perceive are, contrary to the usual mode of expression, given in terms of *different denominations*: the mean *distance* has been given in terms which suppose the radius of the *earth's orbit to be unity*, and the *eccentricity* is given in terms which suppose the radius of the *orbit of Ceres to be unity*, instead of its *proportional* radius 2,769964. Professor Robison on the contrary, in his approximate elements of Georgian expressed the mean distance and eccentricity in terms of the same denomination, which is also done by Lalande, Vince, and other eminent astronomers with respect to the other planets. Let us try now what the greatest equation will be by the elliptic hypothesis, when *unity* is made the radius of the orbit:

Cause of the apparent disagreement of some deductions of the author, and the elements of Mr. Gauss.

As the aphelion distance ($1 + ,08140$) $1,08140 - 4,03383$

Is to perihelion distance ($1 - ,08140$) $9186 - 3,96313$

So is tang: of $46^\circ \frac{1}{2}$ mean anom: - $10,04556$

4,00869

To tang: $\frac{1}{2}$ eq. anom: $43^\circ 20' 33,6''$ - $4,03383$

9,97486

Then $48^\circ 20' 33,5'' + 2 = 86^\circ 41' 72''$; and $92^\circ - 86^\circ 41' 7,2'' = 9^\circ 18' 5,28''$ is the greatest equation.

Also by the tabulated method we have $\frac{8140}{100000} = 08140$, which is the natural sine of $4^{\circ} 40' 8,27''$, or half the greatest equation $9^{\circ} 20' 16,51''$, which is not $9''$ above the correction of Dr. Gauss.

6. Hence it appears, that the eccentric point in the projection of the orbit of Ceres should be a little less than $\frac{1}{2}$ of the radius from the central point S (Plate XIII. Fig. 1.) which represents the sun.

W. P.

XI.

*Description of a very cheap Engine for raising Water. In a Letter from Mr. H. SARJEANT of Whitehaven, to Mr. TAYLOR, Secretary to the Society for the Encouragement of Arts.**

S I R,

Introduction.

I AM sensible that the little engine, a drawing of which accompanies this letter, can lay no great claim to novelty in its principle; nevertheless it is respectfully submitted to the consideration of the society, how far its simplicity, and cheapness of construction, may render it worthy of their attention, with a view to its being more generally known and used in similar cases.

Height of Irton-Hall 60 feet above the stream.

Irton-Hall, the seat of E. L. Irton, Esq. is situated on an ascent of sixty or sixty-one feet perpendicular height; at the foot of which, at the distance of about 140 yards from the offices, runs a small stream of water. The object was to raise this to the house for domestic purposes.

To this end a dam was made at a short distance above, so as to cause a fall of about four feet; and the water was brought by a wooden trough, into which was inserted a piece of two-inch leaden pipe, a part of which is seen at A, plate 2.

Description of the engine. A bucket is suspended at one end of a beam and a counter-weight at the other end. The stream fills the bucket and raises the counter-weight;

The stream of this pipe is so directed as to run into the bucket B, when the bucket is elevated; but so soon as it begins to descend, the stream flows over it, and goes to supply the wooden trough or well in which the foot of the forcing pump C stands, of three inches bore.

D, is an iron cylinder attached to the pump rod, which passes through it. It is filled with lead, and weighs about

* From the Transactions of the Society, for 1801, page 255. The silver medal was given to the Inventor.

240 lbs.

240 lbs. This is the power which works the pump, and forces the water through 420 feet of inch pipe from the pump up to the house. and this last in its descent works a small force-pump.

At E is fixed a cord which, when the bucket comes to within four or five inches of its lowest projection, becomes stretched and opens a valve in the bottom of it, through which the water empties itself. The bucket is made to empty by a valve and string.

I beg leave to add, that an engine, in a great degree similar to this, was erected some years ago by the late James Spedding, Esq. for a lead-mine near Kewick, with the addition of a smaller bucket which emptied itself into the larger, near the beginning of its descent, without which addition it was found that the beam only acquired a libratory motion, without making a full and effective stroke.

To answer this purpose in a more simple way, I constructed the small engine in such manner as to finish its stroke (speaking of the bucket end,) when the beam comes into an horizontal position, or a little below it. By this means the lever is virtually lengthened in its descent in the proportion of the radius to the cosine, of about thirty degrees, or as seven to six nearly, and consequently its power is increased in an equal proportion. Contrivance to complete the stroke.

It is evident that the opening of the valve might have been effected, perhaps better, by a projecting pin at the bottom; but I chose to give an exact description of the engine as it stands. It has now been six months in use, and completely answers the purpose intended.

The only artists employed, except the plumber, were a country blacksmith and carpenter; and the whole cost, exclusive of the pump and pipes, did not amount to £5. It is very cheap.

I am, Sir,

Your humble servant,

H. SARJEANT.

Warwick Court, Holborn.

Mr. CHARLES TAYLOR.

In another letter, dated Whitehaven, April 28, 1801, Mr. Sarjeant further observes that the pump requires about eighteen gallons of water in the bucket to raise the counter-weight, and make a fresh stroke in the pump; that it makes three strokes in a minute, and gives about a half-gallon into the cistern. With a fall of 4 feet and consumption of 18 gallons, it raises half a gallon thro' 60 feet. That is to say 12 parts of

tern

water raise 5 tern at each stroke. He adds, "I speak of what it did in the parts. Its rate is dryest part of last summer; when it supplied a large family, about one eighth together with work-people, &c. with water for all purposes, part of one man's work; and it in a situation where none was to be had before, except some throws up 24 bad water from a common pump which has been since removed. hogheads in the But the above supply being more than sufficient, the machine is occasionally stopped to prevent wear, which is done by merely casting off the string of the bucket valve."

XII.

Concerning the Identity of Tellurium and Antimony, the galvanic Effects of Magnetism, and other Philosophical Subjects. By a
CORRESPONDENT.

To Mr. NICHOLSON.

S I R,

History of the
intelligence.

ANXIOUS to learn the particulars as well as the truth of the intelligence which I received from Moravia concerning the *Tellurium* and the *decomposition of water* pretended to have been effected at Vienna *by means of the magnetic fluid*,* I applied to a chemical friend residing there; and it appears that this intelligence was not correct. Whence to prevent farther misinformation I conceive it my duty to hasten in publishing the following extract from my correspondent's answer to my letter; and to request you the favour of inserting it in your excellent Journal, if there be room for it.

I have the honour to be,

Sir,

Your humble servant,

N. N.

London, April 21. 1802.

Extract of a Letter from Vienna, dated 30th March, 1802.

"The intelligence communicated to you from Moravia concerning the *Tellurium* and *decomposition of water* was a little premature, and is not farther true than as follows:"

Account of the
experiments that
render it probable
that Tellurium
is antimony.

"As to the first point Mr. *Tcharfsky*, major in the artillery, (well known by his refutation of *Tondi's* experiments† to-

* See *Philosophical Journal*, new Series, vol. I. pag. 234.

† An account of this is given in *Baron Born's* systematic catalogue of the collection of fossils of Mlle de Raab at Vienna. Transl. wards

wards establishing the possibility of reducing barytes, lime and magnesia to metallic reguli,) had resolved to employ his considerable stock of *Tellurite* and *Uranite* for the purpose of subjecting these two metals to a new and closer examination. He, therefore, first prepared, according to Klaproth's manner † a regulus of Tellurium, weighing several grains. This regulus perfectly agrees with reguline antimony, *only* with respect to its *physical properties*, for instance, in the fracture, colour, hardness, specific gravity; and so likewise its oxyde perfectly resembles the *antimonium diaphoreticum ablutum* (lixivated white oxyde of antimony by nitre.) However, it would be premature, without farther experiments, in which Major Tscharsky is at present engaged, directly to conclude that Tellurium is nothing else but reguline antimony."

"Major Tscharsky has also prepared many ounces of the ^{Uranium.} oxyde of Uranium, and will in a short time reduce it to a regulus of considerable size for the purpose of farther experiments."

"Concerning the second point it relates only to an experiment of Professor *Jordan*, who, as in the galvanic experiment for decomposing water, connected metallic wires immersed in water with the opposite poles of a magnet, and observed that an oxydation of the wires was effected. But as this experiment would not succeed in the several instances in which it was repeated, Major Tscharsky and Captain *Lethenyey* are at present occupied in preparing a large magnetic apparatus in order to institute a regular series of experiments on this subject."

Supposed galvanic effects of the magnet.

J.

Postscript. Chevalier *Landriani* affirms that he has discovered a method of copying old writings in the same manner, as of recently written papers, duplicates are taken by means of copying-machines.—He has also prepared an excellent indelible ink, the composition of which he will soon publish.

Landriani's method of copying old MSS.
Indelible ink.

† The process by which Klaproth established the Tellurium as a new metal has been given in *Nicholson's Journal*, vol. II. 4to. 1799, page 273. Transl.

SCIENTIFIC NEWS, &c.

Notice respecting the Discovery and Situation of chromated Iron in France.

Chromate of iron.

C. Pontier has already found, three years ago, in the lower Alps, some fragments of chromated iron, out of its place; but circumstances and the war prevented him from discovering the true position of this new and curious mineral in the earth. He, however, at last found it in its natural place in a quarry near Gassin, in the road to Cavalaire.

This metal is mixed with a green serpentine rock, which probably owes its colour to the chrome, according to C. Pontier's opinion. It sometimes forms masses of five solid decimeters each.—*Bulletin des Sc. No 57.*

BOOKS OF SCIENCE,

A Treatise on Astronomy, in which the Elements of the Science are deduced in a natural order from the Appearances of the Heavens to an Observer on the Earth; demonstrated on Mathematical Principles; and explained by an Application to the various Phenomena. By OLINTHUS GREGORY, Teacher of Mathematics, Cambridge. Octavo, 522, very full Pages, with nine Copper Plates, exclusive of the Preface, Table of Contents and Index, Kearsley, London.

IT must be admitted that we have few works in our language in which the extensive science of astronomy is fully treated; and though we have several excellent popular compendiums, yet a work was wanted in which a familiar deduction of the effects from the phenomena should be combined with the stricter principles of mathematics, and in which a large quantity of useful and interesting matter should be given in the compass of an octavo volume. These are the leading advantages of the present work, besides that of its recent appearance, which naturally enabled the author to enrich his treatise with the interesting discoveries of later times.

His order consists in an investigation of the figure and dimensions of the earth; the use and meaning of the imaginary circles and points; apparent motions; causes of seasons, &c. the fixed stars; parallax, refraction, equation of time; systems of astronomy; truth of the Copernican system; theory of apparent motions; law of the planetary revolutions; real motions, rotations; phenomena of secondaries; eclipses, transits, cometary phenomena; aberration of light; elements of computation, tables, &c. &c.

L. L.

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

JUNE, 1802.

ARTICLE I.

On the Rev. Mr. Pearson's Analogy for deducing the greatest Equation from the Eccentricity. In a Letter from Mr. OLINTHUS GREGORY.

Cambridge, May 7, 1802.

To Mr. NICHOLSON.

SIR,

THE brilliant discoveries which have been lately made by astronomers in different parts of Europe, have naturally produced a spirit of inquiry in many persons who have hitherto paid little regard to scientific subjects, and, at the same time, have excited an earnest and active solicitude in those who have made considerable advances in the cause of science, that these inquirers be rightly directed in their pursuit. Among those who have thus laudably exerted themselves in the promotion of useful knowledge, your ingenious correspondent the Rev. Mr. Pearson, of Lincoln, (now of Parson's Green) must certainly be enumerated; and his many able communications to your Journal demand the thanks of its numerous readers. In the last of that gentleman's valuable papers on the planet Ceres

Interesting discoveries in astronomy.

In Mr. Pearson's method of deducing the greatest equation,

Demonstration
of its ground and
accuracy.

Ferdinandia, he mentions a simple method of deducing the greatest equation of a planet's centre from the eccentricity, which he discovered a few years ago; and he invites your mathematical correspondents to give a demonstration of this method, respecting the accuracy of which he seems to have little doubt. In consequence of Mr. Pearson's invitation, I take the liberty of addressing you on the subject. It will save much circumlocution to refer to a diagram: let, therefore, the ellipsis $APGQ$ in the annexed figure 1. Pl. VII. represent the orbit of a planet moving about the sun S in one of the foci; and let $ANQE$ be a circle described upon the major axis of the orbit as a diameter. Then, if P be the place of the planet in its orbit, AN will measure the eccentric anomaly; also, if the arc AD be taken proportional to the time from the aphelion, it will represent the mean anomaly; and, letting fall the perpendicular ST from S upon NC continued, it is shewn, by *Keill* and many others, that $AN + ST = AD$. It has also been shewn that, when the equation of the centre is a maximum in any orbit, the distance SP of the planet from the sun is $= \sqrt{SA \times SQ}$, and when this is the case, in all orbits of small eccentricity the point P nearly coincides with G , or the place of the planet will be near an extremity of its orbit's minor axis. In this situation of SP , it is manifest that ST will be almost coincident with SC , and nearly equal to it; SD and CN will be nearly parallel; and $DCN + SNC$ nearly equal to $2DCN$; that is, the greatest equation will be nearly equal to $2ND$, or $2ST$, or $2SC$. But, when GS the mean distance represents the radius of a circle, as the angle GCS is 90° , SC will manifestly represent the *sine* of an angle: therefore, as GS , the mean distance, to SC , the eccentricity, so is 1, or radius, to the sine of $\frac{1}{2}$ the greatest equation, which is the same as Mr. Pearson's analogy.

It is merely an
approximation.

It is obvious from this investigation, that the simple method pointed out by Mr. P. is merely an approximation. It supposes that the orbits are nearly circular, and will therefore produce a result deviating most widely from the truth when the eccentricity is the greatest. Thus in Mr. P's table (p. 49, No. V. N. S.) the greatest error is in the orbit of mercury, where the eccentricity is $\cdot 20551$ in terms of its own mean distance. The next greatest error is in the orbit of Mars, where the eccentricity is $\cdot 09308$. The next error in order is

that

that of Ceres, where the eccentricity is $\cdot 08140$, and error $4\frac{1}{2}''$. (Pa. 60. No. V.) In the case of the Georgian the error or difference, as stated in the table, is much too great to correspond with the eccentricity. But here I conceive there is a mistake in the determination of the eccentricity: for, according to a mean from various astronomers (*Mechain, Hennert, De la Place, Zach, and Robison,*) the proportional mean distance of the Georgian is not 1918352, but 1908352. Hence then, we have $\frac{9^{\circ}08'04''}{1908352} = \cdot 0475824 = \text{nat. sine of } 2^{\circ} 43' 38\cdot 4''$, differing $\frac{4}{10}$ of a second from the determination of *La Lande*. And, in the case of Jupiter, where the eccentricity is $\cdot 04807$, the corresponding difference is $\frac{55}{100}$ of a second; which is a little larger than the former, as it ought to be. So that it appears, as well from Mr. P's table as from the investigation, that this method does not furnish a result sufficiently accurate except when the eccentricity is pretty small.

Before I conclude, I would just beg to remark, that this method, though new to Mr. Pearson, is in reality well known. An analogy which is, in fact, the converse of this, though expressed rather less commodiously, is given with a demonstration, in *La Caille's Astronomy*, art. 147. *Vince's Astron.* art. 231. and my *Astron.* art. 344. And I believe it is also given in the excellent work of *La Lande*; but as I have not that performance at hand, I cannot now refer to the place. Another analogy precisely to the same effect, and deduced from the same principles, may be seen in art. 249. Book V. *Robertson's Navigation*, and under the article **ECCENTRICITY** in *Dr. Hutton's Math. and Phil. Dictionary*. It must afford great satisfaction to a gentleman of Mr. Pearson's candour and liberality, to find that the analogy which he accidentally struck out, is the converse of one which admits of a similar demonstration, and has been long used by astronomers as an easy and excellent approximation.

The converse analogy to Mr. Pearson's long known.

I am, Sir,

Your's with much respect,

OLINTHUS GREGORY.

II.

On the Construction of the Beams of Steam Engines. By Mr. J. C. HORNBLOWER. From the Author.

DEAR SIR,

Historical introduction.

I BEG leave through the means of your Journal, to lay before the public an account of the framed lever mentioned at the close of the article *Carpentry* in the supplement to the *Encyclopedia Britannica*, as it was originally designed for an engine to have been erected at Amsterdam in the year 1776, together with two others, possessing every possible advantage of levers consisting of small scantles.

The framed lever here described needs no hole bored in it. Other particulars.

I know not by what means the lever above referred to come to be constructed with the disadvantages intimated by the writer of that article, but there is no necessity for a hole to be bored, or a bolt to be driven in any part of the framing between the arches, except for the chain stays. The wedges *a*, *b*, Fig. 1, Plate V. thus applied, would be an improvement, for want of which a lever of this sort in the hands of a negligent engineman had one of its joggles forced off, the shoulder of the tennon, which was morticed into the arch, not being a joint by the eighth of an inch, or more; but when it met with the arch it went no further, and continued to work for many years under a great load, and much to its disadvantage in other respects.

Dimensions of a lever of indifferent workmanship.

The length of this lever was 21 feet, the scantles were 12 inches by 6; height of the whole when put together 30 inches, and leverage on the gudgeon as 4 to 3. This last circumstance operated much against its construction, by giving additional force against the joggle at that end, but had it been framed six inches higher, I doubt not but it would have stood to this day under all its disadvantages.

Scantles and load.

The sum of the scantles is 18 by 12, area of the section 216 inches, column of water in four lifts 4800 lb. with 440 fathoms of rods, (pump rods) which with the appendages on the other end, added to the power necessary to overcome the resistance, amount to about seven tons.

Another construction Fig. 2, well made.

But a much simpler, and in some respects a more advantageous mode of framing is shown at Fig. 2, and may be constructed

fructed with or without arches. This, with little variation, is the invention of a Dutch gentleman, and was applied to the load of a 52 inch cylinder (an atmospherical engine) set up with advantages, which in point of workmanship at that time was perhaps not equalled, and therefore may be said to have been fairly tried.

This engine was calculated to raise 60,000 gallons per minute, and the scantles were 18 by 12, and 12 by 8, and where firch timber can be had, it is hardly to be expected to have a lever with greater advantages than this for a single stroke, and where a double stroke is required, it may be doubled for that purpose, retaining all its principles and properties as in Fig. 3. which I suppose needs no explanation.

Fig. 4. is a lever constructed by an eminent engineer in Hungary some years since, which possesses a very great degree of support by the king post and iron braces, but does not, in my opinion, discover so much science as the two preceding ones. I forgot to observe, that inner arches may be attached to Fig. 1. without materially affecting its principle, if they are well let on the whole framing, and bolted to each other without passing through the scantles.

I am, SIR,

Your most obedient Servant,

J. C. HORNBLLOWER.

East Row, City Road, Tuesday, May 11, 1802.

III.

On the Theory of Chemistry. In a Letter from the Rev.

J. PRIESTLEY, L. L. D. F. R. S. &c,

To Wm. NICHOLSON, Esq,

DEAR SIR,

IN October last I sent you a reply to Mr. Cruickshank's observations on one of my arguments in support of the doctrine of phlogiston, in which I think I clearly shewed that he supposed fixed air to be formed in circumstances in which it is impossible that it should be formed, and that it is decomposed by a substance which has no such power. Having just received

Reference to former letter. Vol. 1. p. 181.

Mr. Cruickshank abandons water as the sole source of inflammable air, which Lavoisier thought essential to his theory.

ceived a letter from a friend in Paris, in which I find that great account is made of the observations of Mr. Cruickshank, so that it is now taken for granted that I must accede to the new theory, I beg you would add to my former letter, that Mr. Cruickshank himself abandons the most fundamental principle of that theory, which is, that the only source of inflammable air of any kind is water; and he makes it not necessary for that purpose. Mr. Lavoisier, treating of the inflammable air from charcoal and water, which is similar to that from charcoal and finery cinder, says, (*Elements of Chymistry, p. 87 of the English translation*) "It cannot possibly be disengaged from the charcoal, and must consequently be produced from the water." According to the new theory, the union of oxygen, which is supposed to come from the finery cinder, with carbon from the charcoal, must form *fixed air*, and not any kind that is inflammable. Mr. Cruickshank therefore must abandon the new theory, in order to maintain his peculiar hypothesis.

Inference.

If I do not receive a better defence of this new theory from its able supporters in France, I shall conclude it to be incapable of defence, and that, as becomes ingenuous men, they will abandon it, as Mr. Cruickshank has virtually done.

I am, dear Sir,

Your's sincerely,

Northumberland, Feb. 20, 1802.

J. PRIESTLEY.

P. S. I have not any Number of your Journal of a later date than that for April last.

IV.

Experiments upon the tanning Principle, and Reflections upon the Art of Tanning. By CIT. MERAT GUILLOT, Apothecary at Auxerre.

Proust's process for obtaining tanin tedious.

THE tediousness of the process indicated by Mr. Proust for obtaining the tanin, induced me to make some experiments, and to endeavour to find a more speedy method of procuring it: the following is the result of my inquiries:

1. I infused tan, in the state of fine powder, for some hours in water; I filtrated this solution, and treated it with lime water; I obtained a precipitate in considerable abundance, which I collected upon a filtre, dried, and afterwards treated with alcohol, in order to ascertain whether it were soluble in this menstruum; but the alcohol was not even coloured by it.

Infusion of tan affords a precipitate by lime water.

2. Wishing to ascertain whether the lime had a greater affinity for the acids than for the tannin with which it was combined, I treated four drachms of the precipitate of which I have spoken above, with nitric acid diluted with water, with the acid of a very gentle heat; a pretty brisk effervescence took place with a disengagement of carbonic acid gas; after four hours infusion, I filtrated the liquor, which had assumed a very deep tinge, and there remained upon the filtre a black pulverulent substance, brilliant, having an acerb and very slightly bitter taste; this residuum weighed a little less than two grammes.

Acids disengage a pulverulent matter from the lime;

3. In order to ascertain whether the nitric acid had dissolved any lime, I treated the liquid which I had filtrated with the acidulous oxalate of potash, and I obtained an abundant precipitate; on which account I conjectured, that since the nitric acid had dissolved the lime, the substance which I had obtained upon the filtre must be tannin, as the precipitate obtained by the mixture of the lime-water and of the infusion of tan, was produced by the union of the tanning principle and of the lime. In order to ascertain this point, I treated one portion of it with water, and the other with alcohol; I let these substances infuse in the sand bath for twenty-four hours: the water became strongly coloured, and the alcohol more so; but all the tannin (hitherto I only presume that it is such) was not dissolved; the alcohol dissolved only a little more than half of it, and the water less. I treated these two liquids, after having filtrated them, with a solution of glue, and I obtained a precipitate similar to that which is obtained by mixing infusion of tan with the same solution, but of a much darker colour, and a little less elastic. When I treated them with the muriate of tin, I obtained a precipitate which became gelatinous; when I treated them with lime-water, the tannin combined with the lime, and reproduced the tanate of lime already formed.

by combining with the lime,

and the pulverulent matter is, tannin,

According

According to these properties, could I doubt that this was pure tannin? certainly not, since it presents the same results as that obtained by the process of Mr. Proust.

How obtained
pure.

If we wish to obtain more pure tannin than that which is obtained after the solution of the lime of the tanate of this substance by an acid, the infusion in alcohol may be evaporated, and we shall then have very pure tannin.

The muriatic acid has presented me with the same result as the nitric.

Conjectures that
lime water is ad-
vantageous in
tanning by the
lime combining
with the tan.

The rapidity with which the upper leathers of shoes (*cuir d'empeigne*) are tanned, according to the process of Citizen Lequin, who, in manufacturing them, contents himself with merely subjecting them to the preparations of washing and fleshing by lime water, without suffering them to swell, and afterwards tans them, led me to presume that in this case a combination is effected of tannin with the lime contained in the skin thus treated, besides the combination of the tannin with the gelatine contained in the skin, which accelerates this fabrication. May it not be probable according to this notion, that the fabrication of leather would be accelerated, if after having subjected the skins to the operations of washing and fleshing in lime water, they were left to swell in the spent ooze or water in which the old bark, which has already served for tanning leather, has been infused. In this case, the small quantity of tannin dissolved in this water would combine with the lime with which the skin would be charged in proportion to the working, and would form a tanate of lime. The swelling would perhaps be effected by this means with a little less celerity than by the sulphuric acid, but then it would perhaps be preferable, from the circumstance that the skin in swelling would begin to charge itself with tannin, whereas by the sulphuric acid, the lime with which the skin is impregnated, when worked,—this substance dissolved in water, the lime, I say, combines with the sulphuric acid employed to swell it, which, I presume, must give to the leather a brittle quality that it would perhaps not have if the other process were used. Perhaps also, after the skins have swelled in the ooze, this completion might be hastened by putting them first into the solution of tan, as Cit. Lequin does, and afterwards steeping them alternately in lime water and in infusion of tan, always taking care to leave them but a short time in the lime-water, which

which might alter them if they were left in it too long. In this case, the lime-water with which the leather would charge itself, would determine a more speedy precipitation of the tannin, and its union with the lime as well as with the gelatine contained in the leather. I believe also that by this means the leather would acquire more weight, a quality in request amongst the tanners, and perhaps it would become less permeable to water.

These are only conjectures which I advance, as they appear to me to be dictated by the theory of the art of tanning. In this instance probably, as in many others, the practice will not correspond with the theory. I recommended the trial to manufacturers, and shall make it myself when a favourable opportunity offers.

Annales de Chimie, No. 123.

V.

On the Destruction of the Grub of the Cock-chaffer. By EDWARD JONES, Esq. of Wepre-Hall, in Flintshire.*

THE grubs of the cock-chafers (or brown beetles) are white, about an inch in length, and of the thickness of a turkey's quill. When disturbed they contract their length, and their bodies dilating, appear like lumps of white fat †, somewhat oval.

Description of
the grub of the
cockchaffer.

They inhabit sandy and light loamy soils, lie from about two to six inches deep, and may be found in spring, by paring off the sods.

Situation.

This place was much infested by brown beetles; but about twelve years ago, some labourers removing a bank of earth, exposed a bed of grubs, several paces in length. Many of them were scattered among the fallen soil; and one of the men proposed to strip the surface of the bank, which being done, the grubs were seen lying in irriguous channels, as if the parent insects had dropped the eggs moving in various directions.

* Extracted from a letter to the Society for the Encouragement of Arts. Transf. for 1801.

† Hence the British name "Earth-Lard."

They are the favourite food of moles;

The same man informed me that they were the favourite food of moles; and he desired me to observe an end of the bank not stripped (being covered with mole hills); "for there no beetle grubs would be found." When opened, his remark proved true:—the moles had traced all the labyrinths of the grubs.

which on this account deserves protection.

I took the hint for the preservation of my foliage, and have ever since protected the race of moles. The brown beetles gradually decreased, and are now rarely seen here. I have not observed more than one or two stragglers in the two last springs.

General habits of moles.

Some notice of the habits of moles may be acceptable to the Society, as it has been said "that they penetrate deep into the earth in dry weather; rarely quit their subterraneous dwellings, and have few enemies;"—and "that they do great mischief in gardens and corn-grounds."

They are much on the surface while the grass is high.

I have always found that in hay and pasture grounds, as soon as the grass is high enough to cover them, they run upon the surface, where they find their food in the numerous caterpillars and insects which in the early part of the summer crawl out of the earth; and they continue above ground till the harvest. They are frequently cut by the scythe; and I have seen them at various times come out of deep hay grass into places recently mown, and, perceiving their exposure, endeavour to conceal themselves in the storn grass.

Do not dig deep;

I have also often seen moles on very close mown grass, and bare spots in pasture land, plunge, when alarmed, among the roots; following their path (which was discernible by the heaving of the surface), I have forced them out occasionally, to try the depth of the covering, which was only a few shreds of roots.

except to avoid the plough or spade, &c.

There are two circumstances that may oblige moles sometimes to penetrate deeply:—disturbed soils in summer, such as in gardens; and ploughed light lands, where the moles delve in pursuit of worms; and, in their course, they must unroot and destroy some plants; but a vigilant gardener and husbandman will prevent much damage.

or to escape frost, &c.

The other cause of their digging deep is frost, which they avoid, or it would kill them. I have found them in winter, in peat soil, two and three feet below the surface; and in the hard frost of 1794-5 (cutting deep trenches to separate grounds,)

grounds), I found moles several mornings, that had worked through and fallen into the trenches, frozen to death.

Their summer emerſion is proved by the birds of prey: they deſtroy great numbers of moles. This year there were taken out of one kite's neſt twenty-two moles, and out of another fifteen, ſome of which were putrid; beſides many frogs and unſledged birds.

Birds of prey deſtroy great numbers while above ground.

The rapacity of the kites ſhews that they are deſtructive enemies to the moles, which, if moles are ſervicable to man, ſhould be known, that he may ſtay his arm.

Moles are frequently found dead upon the graſs in ſummer, with marks of having been bitten, as if to ſuck their blood, but with no part of their bodies conſumed. This, I ſuppoſe, is done by weaſels; and the following (not very common) occurrence, which happened in the ſummer of 1789, tends to prove it:—

A kite was obſerved riſing from the ground with ſome prey, and inſtead of flying to an adjoining wood, he ſoared almoſt perpendicularly. After remaining a ſhort time ſtationary, he came gradually down, with his wings extended and motionleſs, and dropt very near the place from which he had riſen*.

Incident of a kite deſtroyed by a weazel, &c.

Several perſons who were near, and ſaw the flight and deſcent, ran immediately to the ſpot, and a weaſel darted from the kite, which they found dead; and they diſcovered, on examination, that the kite had been bit in the throat, and bled to death. Near it they found a dead mole, yet warm, which was bitten in the neck; and they concluded that the weaſel had cauſed the death of both.

In ſeveral parts of the kingdom where I have met with a great number of brown beetles, moles were regularly deſtroyed; and in Staffordſhire, being ſhown ſeveral large trees covered by beetles, and totally deſolated, I enquired whether they deſtroyed the moles? The anſwer was, that they did, or they ſhould be over-run with them.

Brown beetles are abundant where the moles are deſtroyed.

The loſs of foliage not being of great conſequence to the farmer, he is ſatisfied that his turkeys make him amends in other reſpects, by eating the brown beetles, of which they are very fond, and which they ſwallow voraciouſly.

Other remarks.

* A ſimilar circumſtance was mentioned in the Cheſter papers, three or four years ago.

A gen-

A gentleman informed me lately, that rooks also eat the beetles.

But these means are confined to the winged beetle. It appears to me that the mole is the only certain destroyer of the grub.

My hay and pasture grounds are, every spring, thickly studded with mole hillocks. They are scattered in the usual manner; and when the grasses are up, the moles cease to work, and scarce a hillock appears till after harvest.

VI.

Methods of diminishing the Irregularities of Time-Pieces, arising from differences in the Arc of Vibration of the Pendulum. By Mr. EZEKIEL WALKER.

TO MR. NICHOLSON.

S I R,

Lynn Regis, May 17, 1802,

Chronometers, &c. rendered irregular by the oil.

AFTER all the improvements which the mechanism of clocks and watches has received, there are still obstacles which stand in the way of an exact performance. The use of oil in chronometers has long been complained of, but still remains a necessary evil; and that variation which obtains in the arc of vibration of the pendulum, is a source of much error in clocks.

Transit clock at Greenwich varies in its arc of vibration.

The transit clock at Greenwich sometimes varies in its semi-arc of vibration *twenty* minutes in a year, and a compound pendulum which came under my own inspection some years ago, varied in its arc nearly as much.

Old remedy for the inequality of the arcs.

Many years have elapsed since it was discovered, that the short vibrations were performed in less time than the long ones, and methods were used to remove the inconvenience. HUYGENS proposed a method by which the centre of oscillation might be made to vibrate in the arc of a cycloid, and demonstrated that a pendulum moving in that curve, would perform all its vibrations, whether long or short, in equal times: and others proposed to remove the evil by a peculiar form of the pallets.

These

These contrivances, how ingenious soever they may appear in theory, have not answered any good purpose in practice; but, what may be impracticable to perform by one method, may not be found so by another.

The spring by which the pendulum is suspended seems capable of receiving such improvement, as would render the long and short vibrations isochronal, without any other alteration in the mechanism of the clock. This improvement consists in cutting the spring broader at the top than at the bottom, that it may not bend totally at the point of suspension. The pendulum would, in that case, when in motion, become shorter at the ends of the arc, consequently it would no longer vibrate in the arc of a circle, and on this account there is some reason for supposing that the long vibrations would be performed in less time than they were before.

When this method is put in practice, the spring should be made so broad at the point of suspension at first, that the long vibrations may be performed in less time than the short ones, and then made narrower till all the vibrations be performed in equal times. But I supposed that this method would be attended with much trouble, and that it would be better to remove the cause than to counteract its effects.

When I made observations on the pendulum above-mentioned, I found that it was much influenced by the weather. In cold and moist weather it moved through a larger arc, than when the atmosphere was dry and warm.

As all metals alter their dimensions, and consequently their elastic force by heat and cold, it follows that that part of the clock called the *crutch*, would act more forcibly on the pendulum in cold weather than in warm; and it seems probable, that this variation would be greater in a weak *crutch* than in a strong one.

About seven years ago having a regulator made by Mr. James Bullock, of Holborn, and being desirous of putting my theory to the test of experience, I had the *crutch* made exceedingly strong, and firmly connected with the pallets, that the maintaining power of the clock might be communicated to the pendulum uniformly in all states of the atmosphere. The clock case is made very thick of solid mahogany, and upon the *rising board**, which is two inches thick, stands an an-

* The board on which the wheel-work stands.

gular piece of brass (thus Δ) to support the pendulum. This support is made fast with screws to the *rising-board* at the bottom, and to the *frame-plate* of the clock at the top. Who invented this support I know not, but it appears a very excellent method of giving stability to the point of suspension of the pendulum.

Result.

This clock has now been going near seven years. It vibrates $1^{\circ} 49'$ on each side of the perpendicular, from which I have not seen it vary more than $2'$, except once in a very hard frost, and as it seldom varies so much as $2'$ in its semi-arc, this cause of error in the pendulum seems to be very nearly removed. It has been cleaned only once since it came into my possession, but this made no alteration, either in its arc of vibration or its rate of going.

I am, with much respect,

Sir,

Your very humble servant,

EZEKIEL WALKER.

VII.

On the Theory of Light and Colours *. By THOMAS YOUNG, M. D. F. R. S. Professor of Natural Philosophy in the Royal Institution.

Importance of general principles in science.

ALTHOUGH the invention of plausible hypotheses, independent of any connection with experimental observations, can be of very little use in the promotion of natural knowledge; yet the discovery of simple and uniform principles, by which a great number of apparently heterogeneous phenomena are reduced to coherent and universal laws, must ever be allowed to be of considerable importance towards the improvement of the human intellect.

Object of the present dissertation.

The object of the present dissertation is not so much to propose any opinions which are absolutely new, as to refer some theories, which have been already advanced, to their original inventors, to support them by additional evidence, and to apply them to a great number of diversified facts, which have hitherto been buried in obscurity. Nor is it absolutely ne-

* In support of the truth of that hypothesis, which ascribes the phenomena to undulatory motions of an extremely elastic and rare fluid. From the *Philos. Trans.* 1802.—N.

cessary in this instance to produce a single new experiment; for of experiments there is already an ample store, which are so much the more unexceptionable, as they must have been conducted without the least partiality for the system by which they will be explained; yet some facts, hitherto unobserved, will be brought forwards, in order to shew the perfect agreement of that system with the multifarious phenomena of nature.

The optical observations of Newton are yet unrivalled; and, excepting some casual inaccuracies, they only rise in our estimation, as we compare them with later attempts to improve on them. A further consideration of the colours of thin plates, as they are described in the second book of Newton's optics, has converted that prepossession which I before entertained for the undulatory system of light, into a very strong conviction of its truth and sufficiency; a conviction which has been since most strikingly confirmed, by an analysis of the colours of striated substances. The phenomena of thin plates are indeed so singular, that their general complexion is not without great difficulty reconcilable to any theory, however complicated, that has hitherto been applied to them; and some of the principal circumstances have never been explained by the most gratuitous assumptions; but it will appear, that the minutest particulars of these phenomena are not only perfectly consistent with the theory which will now be detailed, but that they are all the necessary consequences of that theory, without any auxiliary suppositions; and this by inferences so simple, that they become particular corollaries, which scarcely require a distinct enumeration.

A more extensive examination of Newton's various writings has shown me, that he was in reality the first that suggested such a theory as I shall endeavour to maintain; that his own opinions varied less from this theory than is now almost universally supposed; and that a variety of arguments have been advanced, as if to confute him, which may be found nearly in a similar form in his own works; and this by no less a mathematician than Leonard Euler, whose system of light, as far as it is worthy of notice, either was, or might have been, wholly borrowed from Newton, Hooke, Huygens, and Malebranche.

Those who are attached, as they may be with the greatest justice, to every doctrine which is stamped with the Newtonian approbation, will probably be disposed to bestow on these considerations

Excellence of the optical observations of Newton.

Phenomena of thin plates.

Newton first suggested the theory of undulation.

Euler.

Reference to the works of Newton.

considerations so much the more of their attention, as they appear to coincide more nearly with Newton's own opinions. For this reason, after having briefly stated each particular position of my theory, I shall collect, from Newton's various writings, such passages as seem to be the most favourable to its admission; and, although I shall quote some papers which may be thought to have been partly retracted at the publication of the optics, yet I shall borrow nothing from them that can be supposed to militate against his maturer judgment.

HYPOTHESIS I.

A luminiferous Ether pervades the Universe, rare and elastic in a high degree.

Passages from Newton.

Hypoth. I.
Universality of
the ether.

“ The hypothesis certainly has a much greater affinity with his own,” that is, Dr. Hooke's, “ hypothesis, than he seems to be aware of; the vibrations of the ether being as useful and necessary in this as in his.” (Phil. Transf. Vol. VII. p. 5087. Abr. Vol. I. p. 145. Nov. 1672.)

“ To proceed to the hypothesis: first, it is to be supposed therein, that there is an ethereal medium, much of the same constitution with air, but far rarer, subtler, and more strongly elastic. It is not to be supposed, that this medium is one uniform matter, but compounded, partly of the main phlegmatic body of ether, partly of other various ethereal spirits, much after the manner that air is compounded of the phlegmatic body of air, intermixed with various vapours and exhalations: for the electric and magnetic effluvia, and gravitating principle, seem to argue such variety.” (Birch. Hist. of R. S. Vol. III. p. 249. Dec. 1675.)

Arguments pro-
posed by New-
ton in support of
the ether.

“ Is not the heat (of the warm room) conveyed through the vacuum by the vibrations of a much subtler medium than air? And is not this medium the same with that medium by which light is refracted and reflected, and by whose vibrations light communicates heat to bodies, and is put into fits of easy reflection, and easy transmission? And do not the vibrations of this medium in hot bodies, contribute to the intenseness and duration of their heat? And do not hot bodies communicate their heat to contiguous cold ones, by the vibrations of this medium propagated from them into the cold

“ ones ?

" ones? And is not this medium exceedingly more rare and
 " subtle than the air, and exceedingly more elastic and active?
 " And doth it not readily pervade all bodies? And is it not,
 " by its elastic force, expanded through all the heavens?—
 " May not planets and comets, and all gross bodies, perform
 " their motions in this ethereal medium? And may not its re-
 " sistance be so small, as to be inconsiderable? For instance, if
 " this ether (for so I will call it) should be supposed 700,000
 " times more elastic than our air, and above 700,000 times
 " more rare, its resistance would be about 600,000,000 less
 " than that of water. And so small a resistance would scarce
 " make any sensible alteration in the motions of the planets, in
 " ten thousand years. If any one would ask how a medium
 " can be so rare, let him tell me, How an electric body can by
 " friction emit an exhalation so rare and subtle, and yet so
 " potent? And how the effluvia of a magnet can pass through
 " a plate of glass without resistance, and yet turn a magnetic
 " needle beyond the glass?" (Optics, Qu. 18, 22.)

HYPOTHESIS II.

Undulations are excited in this Ether whenever a body becomes luminous.

Scholium. I use the word undulation, in preference to vi-
 bration, because vibration is generally understood as implying a
 motion which is continued alternately backwards and forwards,
 by a combination of the momentum of the body with an acce-
 lerating force, and which is naturally more or less permanent;
 but an undulation is supposed to consist in a vibratory motion,
 transmitted successively through different parts of a medium,
 without any tendency in each particle to continue its motion,
 except in consequence of the transmission of succeeding undu-
 lations, from a distinct vibrating body; as, in the air, the vi-
 brations of a chord produce the undulations constituting
 sound.

Hypoth. II.
Light consists
in ethereal un-
dulation.

Passages from Newton.

" Were I to assume an hypothesis, it should be this, if pro-
 " pounded more generally, so as not to determine what light
 " is, further than that it is something or other capable of ex-
 " citing vibrations in the ether; for thus it will become so ge-
 " neral and comprehensive of other hypotheses, as to leave
 " little room for new ones to be invented." (Birch. Vol. III.
 p. 249. Dec. 1675.)

Detail of the
hypoth. by
Newton. Light.

Vibrations compared with those of sound.

“ In the second place, it is to be supposed, that the ether is a vibrating medium like air, only the vibrations far more swift and minute; those of air, made by a man’s ordinary voice, succeeding one another at more than half a foot, or a foot distance; but those of ether at a less distance than the hundred thousandth part of an inch. And, as in air the vibrations are some larger than others, but yet all equally swift, (for in a ring of bells the sound of every tone is heard at two or three miles distance, in the same order that the bells are struck), so, I suppose, the ethereal vibrations differ in bigness, but not in swiftness. Now, these vibrations, beside their use in reflection and refraction, may be supposed the chief means by which the parts of fermenting or putrifying substances, fluid liquors, or melted, burning, or other hot bodies, continue in motion.” (Birch. Vol. III. p. 251. Dec. 1675).

— reflection, refraction;

“ When a ray of light falls upon the surface of any pellucid body, and is there refracted or reflected, may not waves of vibrations, or tremors, be thereby excited in the refracting or reflecting medium? And are not these vibrations propagated from the point of incidence to great distances? And do they not overtake the rays of light, and by overtaking them successively, do not they put them into the fits of easy reflection and easy transmission described above?” (Optics, Qu. 17).

and the alternate fits.

“ Light is in fits of easy reflection and easy transmission, before its incidence on transparent bodies. And probably it is put into such fits at its first emission from luminous bodies, and continues in them during all its progress.” (Optics, Second Book, Part III. Prop. 13.)

HYPOTHESIS III.

The Sensation of different Colours depends on the different frequency of Vibrations, excited by Light in the Retina.

Passages from Newton.

Hypoth. III. Colour depends on the frequency of the ethereal vibrations.

“ The objector’s hypothesis, as to the fundamental part of it, is not against me. That fundamental supposition is, that the parts of bodies, when briskly agitated, do excite vibrations in the ether, which are propagated every way from those bodies in straight lines, and cause a sensation of light by beating and

and dashing against the bottom of the eye, something after the manner that vibrations in the air cause a sensation of sound by beating against the organs of hearing. Now, the most free and natural application of this hypothesis to the solution of phenomena, I take to be this: that the agitated parts of bodies, according to their several sizes, figures, and motions, do excite vibrations in the ether of various depths or bignesses, which, being promiscuously propagated through that medium to our eyes, effect in us a sensation of light of a white colour; but if by any means those of unequal bignesses be separated from one another, the largest beget a sensation of a red colour, the least or shortest of a deep violet, and the intermediate ones of intermediate colours; much after the manner that bodies, according to their several sizes, shapes, and motions, excite vibrations in the air of various bignesses, which, according to those bignesses, make several tones in sound: that the largest vibrations are best able to overcome the resistance of a refracting superficies, and so break through it with least refraction; whence the vibrations of several bignesses, that is, the rays of several colours, which are blended together in light, must be parted from one another by refraction, and so cause the phenomena of prisms, and other refracting substances; and that it depends on the thickness of a thin transparent plate or bubble, whether a vibration shall be reflected at its further superficies, or transmitted; so that, according to the number of vibrations, interceding the two superficies, they may be reflected or transmitted for many successive thicknesses. And, since the vibrations which make blue and violet, are supposed shorter than those which make red and yellow, they must be reflected at a less thickness of the plate: which is sufficient to explicate all the ordinary phenomena of those plates or bubbles, and also of all natural bodies, whose parts are like so many fragments of such plates. These seem to be the most plain, genuine, and necessary conditions of this hypothesis. And they agree so justly with my theory, that if the animadverfor think fit to apply them, he need not, on that account, apprehend a divorce from it. But yet, how he will defend it from other difficulties, I know not." (Phil. Transf. Vol. VII. p. 5088. Abr. Vol. I. p. 145. Nov. 1672.)

"To explain colours, I suppose, that as bodies of various sizes, densities, or sensations, do by percussion or other action excite

Particular development;

as in sound.

Various refrangibility:

Thin transparent plates.

Repetition of the theory.

excite sounds of various tones, and consequently vibrations in the air of different bignefs; so the rays of light, by impinging on the stiff refracting superficies, excite vibrations in the ether, of various bignefs; the biggest, strongest, or most potent rays, the largest vibrations; and others shorter, according to their bignefs, strength, or power: and therefore the ends of the capillamenta of the optic nerve, which pave or face the retina, being such refracting superficies, when the rays impinge upon them, they must there excite these vibrations, which vibrations (like those of sound in a trunk or trumpet) will run along the aqueous pores or crystalline pith of the capillamenta, through the optic nerves, into the sensorium; and there, I suppose, affect the sense with various colours, according to their bignefs and mixture; the biggest with the strongest colours, reds and yellows; the least with the weakest, blues and violets; the middle with green; and a confusion of all with white, much after the manner that, in the sense of hearing, nature makes use of aerial vibrations of several bignesses, to generate sounds of divers tones; for the analogy of nature is to be observed." (Birch, Vol. III. p. 262, Dec. 1675.)

"Considering the lastingness of the motions excited in the bottom of the eye by light, are they not of a vibrating nature? Do not the most refrangible rays excite the shortest vibrations, —the least refrangible the largest? May not the harmony and discord of colours arise from the proportions of the vibrations propagated through the fibres of the optic nerve into the brain, as the harmony and discord of sounds arise from the proportions of the vibrations of the air?" (Optics, Qu. 16, 13, 14.)

Scholium. The parts of the retina being probably capable of vibrating in unison with a limited number of colorific motions,

Scholium. Since, for the reason here assigned by Newton, it is probable that the motion of the retina is rather of a vibratory than of an undulatory nature, the frequency of the vibrations must be dependent on the constitution of this substance. Now, as it is almost impossible to conceive each sensitive point of the retina to contain an infinite number of particles, each capable of vibrating in perfect unison with every possible undulation, it becomes necessary to suppose the number limited, for instance, to the three principal colours, red, yellow, and blue, of which the undulations are related in magnitude nearly as the numbers 8, 7, and 6; and that each of the particles is capable of being put in motion less or more forcibly, by undulations

tions

tions differing less or more from a perfect unison; for instance, the undulations of green light being nearly in the ratio of $6\frac{1}{2}$, will affect equally the particles in unison with yellow and blue, and produce the same effect as a light composed of those two species: and each sensitive filament of the nerve may consist of three portions, one for each principal colour. Allowing this statement, it appears that any attempt to produce a musical effect from colours, must be unsuccessful, or at least that nothing more than a very simple melody could be imitated by them; for the period, which in fact constitutes the harmony of any concord, being a multiple of the periods of the single undulations, would in this case be wholly without the limits of sympathy of the retina, and would lose its effect; in the same manner as the harmony of a third or a fourth is destroyed, by depressing it to the lowest notes of the audible scale. In hearing, there seems to be no permanent vibration of any part of the organ.

it is not to be expected that colours can have a musical effect.

The ear not permanently affected.

HYPOTHESIS IV.

All material Bodies have an attraction for the ethereal Medium, by means of which it is accumulated within their substance, and for a small Distance around them, in a State of greater Density, but not of greater Elasticity.

Hypoth. IV. The density but not the elasticity of the ether, is greater within and near other bodies.

It has been shewn, that the three former hypotheses, which may be called essential, are literally parts of the more complicated Newtonian system. This fourth hypothesis differs perhaps in some degree from any that have been proposed by former authors, and is diametrically opposite to that of Newton; but, both being in themselves equally probable, the opposition is merely accidental; and it is only to be inquired which is the best capable of explaining the phenomena. Other suppositions might perhaps be substituted for this, and therefore I do not consider it as fundamental, yet it appears to be the simplest and best of any that have occurred to me.

Newton supposed the contrary.

PROPOSITION I.

All impulses are propagated in a homogeneous elastic Medium with an equable velocity.

Propos. I. Impulse is propagated uniformly in an homogeneous elastic medium.

Every experiment relative to sound coincides with the observation already quoted from Newton, that all undulations are propagated through the air with equal velocity; and this is further

ther confirmed by calculations. (Lagrange, Misc. Taur. Vol. I. p. 91. Also, much more concisely, in my Syllabus of a course of Lectures on Natural and Experimental Philosophy, about to be published. Article 289.) If the impulse be so great as materially to disturb the density of the medium, it will be no longer homogeneous; but, as far as concerns our senses, the quantity of motion may be considered as infinitely small. It is surprising that Euler, although aware of the matter of fact, should still have maintained, that the more frequent undulations are more rapidly propagated. (Theor. mus. and Conject. phys.) It is possible, that the actual velocity of the particles of luminiferous ether may bear a much less proportion to the velocity of the undulations than in sound; for light may be excited by the motion of a body moving at the rate of only one mile in the time that light moves an hundred millions.

Law of the velocities in different mediums.

Scholium 1. It has been demonstrated, that in different mediums the velocity varies in the subduplicate ratio of the force directly, and of the density inversely. (Misc. Taur. Vol. I. p. 91. Young's Syllabus. Art. 294.)

Undulations do not mix.

Scholium 2. It is obvious, from the phenomena of elastic bodies and of sounds, that the undulations may cross each other without interruption. But there is no necessity that the various colours of white light should intermix their undulations; for, supposing the vibrations of the retina to continue but a five hundredth of a second after their excitement, a million undulations of each of a million colours may arrive in distinct succession within this interval of time, and produce the same sensible effect, as if all the colours arrived precisely at the same instant.

PROPOSITION II.

Prop. II. Nature of undulation.

An Undulation conceived to originate from the Vibration of a single Particle, must expand through a homogeneous Medium in a spherical Form, but with different Quantities of Motion in different Parts.

For, since every impulse, considered as positive or negative, is propagated with a constant velocity, each part of the undulation must in equal times have past through equal distances from the vibrating point. And, supposing the vibrating particle, in the course of its motion, to proceed forwards to a small distance in a given direction, the principal strength of the undulation

ation will naturally be straight before it; behind it, the motion will be equal, in a contrary direction; and, at right angles to the line of vibration, the undulation will be evanescent.

Now, in order that such an undulation may continue its progress to any considerable distance, there must be in each part of it, a tendency to preserve its own motion in a right line from the centre; for, if the excess of force at any part were communicated to the neighbouring particles, there can be no reason why it should not very soon be equalised throughout, or, in other words, become wholly extinct, since the motions in contrary directions would naturally destroy each other. The origin of sound from the vibration of a chord is evidently of this nature; on the contrary, in a circular wave of water, every part is at the same instant either elevated or depressed. It may be difficult to show mathematically, the mode in which this inequality of force is preserved; but the inference from the matter of fact, appears to be unavoidable; and, while the science of hydrodynamics is so imperfect that we cannot even solve the simple problem of the time required to empty a vessel by a given aperture, it cannot be expected that we should be able to account perfectly for so complicated a series of phenomena, as those of elastic fluids. The theory of Huygens indeed explains the circumstance in a manner tolerably satisfactory: he supposes every particle of the medium to propagate a distinct undulation in all directions; and that the general effect is only perceptible where a portion of each undulation conspires in direction at the same instant; and it is easy to show that such a general undulation would in all cases proceed rectilinearly, with proportionate force; but, upon this supposition, it seems to follow, that a greater quantity of force must be lost by the divergence of the partial undulations, than appears to be consistent with the propagation of the effect to any considerable distance. Yet it is obvious, that some such limitation of the motion must naturally be expected to take place; for, if the intensity of the motion of any particular part, instead of continuing to be propagated straight forwards, were supposed to affect the intensity of a neighbouring part of the undulation, an impulse must then have travelled from an internal to an external circle in an oblique direction, in the same time as in the direction of the radius, and consequently with a greater velocity; against the first proposition.

Theory of Huygens.

proposition. In the case of water, the velocity is by no means so rigidly limited as in that of an elastic medium. Yet it is not necessary to suppose, nor is it indeed probable, that there is absolutely not the least lateral communication of the force of the undulation, but that, in highly elastic mediums, this communication is almost insensible. In the air, if a chord be perfectly insulated, so as to propagate exactly such vibrations as have been described, they will in fact be much less forcible than if the chord be placed in the neighbourhood of a sounding board, and probably in some measure because of this lateral communication of motions of an opposite tendency. And the different intensity of different parts of the same circular undulation may be observed, by holding a common tuning fork at arm's length, while sounding, and turning it, from a plane directed to the ear, into a position perpendicular to that plane.

PROPOSITION III.

Proposition III.
Lateral undulations explained.

A Portion of a spherical Undulation, admitted through an Aperture into a quiescent Medium, will proceed to be further propagated rectilinearly in concentric Superficies, terminated laterally by weak and irregular Portions of newly diverging Undulations.

At the instant of admission, the circumference of each of the undulations may be supposed to generate a partial undulation, filling up the nascent angle between the radii and the surface terminating the medium; but no sensible addition will be made to its strength by a divergence of motion from any other parts of the undulation, for want of a coincidence in time, as has already been explained with respect to the various force of a spherical undulation. If indeed the aperture bear but a small proportion to the breadth of an undulation, the newly generated undulation may nearly absorb the whole force of the portion admitted; and this is the case considered by Newton in the Principia. But no experiment can be made under these circumstances with light, on account of the minuteness of its undulations, and the interference of inflection; and yet some faint radiations do actually diverge beyond any probable limits of inflection, rendering the margin of the aperture distinctly visible in all directions; these are attributed by Newton to some unknown cause, distinct from inflection: (Optics, Third

Third Book, Obs. 5. and they fully answer the description of this proposition.

Let the concentric lines in Fig. 1. (Plate V.) represent the contemporaneous situation of similar parts of a number of successive undulations diverging from the point A; they will also represent the successive situations of each individual undulation: let the force of each undulation be represented by the breadth of the line, and let the cone of light ABC be admitted through the aperture BC; then the principal undulations will proceed in a rectilinear direction towards GH, and the faint radiations on each side will diverge from B and C as centres, without receiving any additional force from any intermediate point D of the undulation, on account of the inequality of the lines DE and DF. But, if we allow some little lateral divergence from the extremities of the undulations, it must diminish their force, without adding materially to that of the dissipated light; and their termination, instead of the right line BG, will assume the form CH; since the loss of force must be more considerable near to C than at greater distances. This line corresponds with the boundary of the shadow in Newton's first observation, Fig. 1; and it is much more probable that such a dissipation of light was the cause of the increase of the shadow in that observation, than that it was owing to the action of the inflecting atmosphere, which must have extended a thirtieth of an inch each way in order to produce it; especially when it is considered that the shadow was not diminished by surrounding the air with a denser medium than air, which must in all probability have weakened and contracted its inflecting atmosphere. In other circumstances, the lateral divergence might appear to increase, instead of diminishing, the breadth of the beam.

As the subject of this proposition has always been esteemed the most difficult part of the undulatory system, it will be proper to examine here the objections which Newton has grounded upon it.

“ To me, the fundamental supposition itself seems impossible; namely, that the waves or vibrations of any fluid can, like the rays of light, be propagated in straight lines, without a continual and very extravagant spreading and bending every way into the quiescent medium, where they are terminated by it.

Linear explanation.

Objections of Newton to the undulatory system of light.

it. I mistake, if there be not both experiment and demonstration to the contrary." (Phil. Trans. VII. 5089, Abr. I. 146. Nov. 1672.)

"Motus omnis per fluidum propagatus divergit a recto tramite in spatia immota."

"Quoniam medium ibi," in the middle of an undulation admitted, *"densus est, quam in spatiis hinc inde, dilatabit sese tam versus spatia utrinque sita, quum versus pulsuum ruriora intervalla; eoque pacto—pulsus eadem fere celeritate sese in medii partes quiescentes hinc inde relaxare debent;—ideoque spatium totum occupabunt.—Hoc experimur in sonis."* (Princip. Lib. II. Prop. 42.)

"Are not all hypotheses erroneous, in which light is supposed to consist in pressure or motion, propagated through a fluid medium?—If it consisted in pressure or motion, propagated either in an instant, or in time, it would bend into the shadow. For pressure or motion cannot be propagated in a fluid in right lines beyond an obstacle which stops part of the motion, but will bend and spread every way into the quiescent medium which lies beyond the obstacle. The waves on the surface of stagnating water, passing by the sides of a broad obstacle which stops part of them, bend afterwards, and dilate themselves gradually into the quiet water behind the obstacle. The waves, pulses, or vibrations of the air, wherein sounds consist, bend manifestly, though not so much as the waves of water. For a bell or a cannon may be heard beyond a hill, which intercepts the sight of the sounding body; and sounds are propagated as readily through crooked pipes as straight ones. But light is never known to follow crooked passages, nor to bend into the shadow. For the fixed stars, by the interposition of any of the planets, cease to be seen. And so do the parts of the sun, by the interposition of the moon, Mercury, or Venus. The rays which pass very near to the edges of any body, are bent a little by the action of the body; but this bending is not towards but from the shadow, and is performed only in the passage of the ray by the body, and at a very small distance from it. So soon as the ray is past the body, it goes right on." (Optics, Qu. 28.)

Now the proposition quoted from the Principia does not directly contradict this proposition; for it does not assert that such a motion must diverge equally in all directions; neither

can

can it with truth be maintained, that the parts of an elastic medium communicating any motion, must propagate that motion equally in all directions. (Phil. Transf. for 1800. p. 109—112.) All that can be inferred by reasoning is, that the marginal parts of the undulation must be somewhat weakened, and that there must be a faint divergence in every direction; but whether either of these effects might be of sufficient magnitude to be sensible, could not have been inferred from argument, if the affirmative had not been rendered probable by experiment.

As to the analogy with other fluids, the most natural inference from it is this: “The waves of the air, wherein sounds consist, bend manifestly, though not so much as the waves of water;” water being an inelastic, and air a moderately elastic medium; but ether being most highly elastic, its waves bend very far less than those of the air, and therefore almost imperceptibly. Sounds are propagated through crooked passages, because their sides are capable of reflecting sound, just as light would be propagated through a bent tube, if perfectly polished within.

Sound defects
less than waves
of water.

Crooked pas-
sages.

The light of a star is by far too weak to produce, by its faint divergence, any visible illumination of the margin of a planet eclipsing it; and the interception of the sun's light by the moon, is as foreign to the question, as the statement of inflection is inaccurate.

To the argument adduced by Huygens, in favour of the rectilinear propagation of undulations, Newton has made no reply; perhaps because of his own misconception of the nature of the motions of elastic mediums, as dependent on a peculiar law of vibration, which has been corrected by later mathematicians. (Phil. Transf. for 1800, p. 116.) On the whole, it is presumed, that this proposition may be safely admitted, as perfectly consistent with analogy and with experiment.

(To be continued.)

VIII.

Remarks on Combustion. By THOMAS THOMSON, M. D.
Lecturer on Chemistry in Edinburgh.

(Concluded from Page 20.)

Nitric acid
formed sponta-
neously ;

VI. NITRIC acid is formed spontaneously upon the surface of the earth by processes with which we are but imperfectly acquainted ; but which certainly have no resemblance to combustion. Its oxygen is probably furnished by the *air*, which is a supporter ; at least, it has been observed, that if azote, the instant it is evolved, comes in contact with air, it is capable of combining with its oxygen, and forming nitric acid.

and also by elec-
tricity, through
common air, and
probably by gal-
vanism ;

Nitric acid may be formed also, as Mr. Cavendish has demonstrated, by passing electric sparks through common air, a supporter. In all probability it may be formed also by the galvanic pile, but this may be considered as equivalent to electricity. This formation of nitric acid by means of electricity, has been considered as a combustion, but for what reason it is not easy to say : the substance acted upon is not a combustible with a supporter, but a supporter alone. Electricity is so far from being equivalent to combustion, that it sometimes acts in a manner diametrically opposite ; *unburning*, if I may use the expression, a substance which has already undergone combustion, and converting a *product* into a *combustible* and a *supporter*. Thus it decomposes water, and converts it into oxygen and hydrogen gas ; therefore it must be capable of supplying the substances which the oxygen and combustible lose when they combine by combustion, and form a product*.

Apparently by
decomposition.

There is one process more, during which nitric acid is formed, which must at first sight appear an exception to the general rule ; I mean the formation of nitric acid, which takes place during the combustion of hydrogen gas in oxygen gas contaminated with air. But in this case it is the *hydrogen* only which burns, and not the *air* ; the air indeed combines intimately, and forms nitric acid, just as it does when electric sparks are

Nitric acid in
the process for
composing wa-
ter.

* I do not mean to affirm that electricity never occasions combustion, the contrary of which is well known, but that a combination produced by it is not always the same with combustion.

passed through it. But this process has no resemblance to combustion. We see, however, that a certain temperature is capable of producing this change in air.

8. Several of the supporters and partial supporters are capable of combining with combustibles, without undergoing decomposition, or exhibiting the phenomena of combustion. In this manner the yellow oxide of gold and the white oxide of silver combine with ammonia; the red oxide of mercury with oxalic acid; and oximuriatic acid with ammonia. Thus also nitre and oximuriate of potash may be combined, or at least intimately mixed with several combustible bodies, as in gunpowder, &c. In all these compounds the oxygen of the supporter and the combustible retain the ingredients which render them susceptible of combustion; hence the compound is still combustible: And in consequence of the intimate combination of the component parts, the least alteration is apt to destroy the equilibrium which subsists between them; the consequence is, combustion and the formation of a new compound. Hence these compounds burn with amazing facility, not only when heated, but when triturated or struck smartly with a hammer. They have therefore received the name of *detonating* or *fulminating* bodies. Thus we have fulminating gold, fulminating silver, fulminating mercury, fulminating powder, &c.

Supporters, &c. may combine with combustibles without combustion, and produce *fulminating compounds*.

9. Such are the properties of the combustibles, the supporters, and the products; and such the phenomena which they exhibit when made to act upon each other.

If we compare together the *supporters* and the *products*, we shall find that they resemble each other in several respects. Both of them contain oxygen as an essential constituent part; both are capable of converting combustibles into products; and several of both combine with combustibles and with additional doses of oxygen. But they differ widely from each other in the phenomena which accompany their action on combustibles. The supporters convert these bodies into products, and combustion, or the emission of heat and light at the same time, takes place; whereas the products convert combustibles into products without any such emission. Now, as the ultimate change produced upon combustibles by both these sets of bodies is the same, and as the substance which combines with the combustibles is in both cases the same, namely oxygen, we

Supporters and products resemble in many respects;

but they differ widely in their effect on combustibles. The former only produce combustion.

must conclude that this oxygen in the supporters contains something which the oxygen of the products wants, something which separates during the passage of the oxygen from the product to the combustible, and occasions the combustion, or emission of fire, which accompanies this passage. The oxygen of supporters then contains some ingredient which the oxygen of products wants. Many circumstances concur to render it probable that this ingredient is *caloric*.

The oxygen of supporters contains *caloric*.

Combustibles and products also resemble each other :

But the former emit fire when they combine with oxygen.

The *combustibles* and the *products* also resemble each other in several respects. Both of them contain the same or a similar base; both frequently combine with combustibles, and likewise with oxygen; but they differ essentially in the phenomena which accompany their combination with oxygen. In the one case fire is emitted, in the other not. If we recollect that no substance but a combustible is capable of restoring combustibility to the base of a product, and that at its doing so it always loses its own combustibility; and if we recollect farther, that the base of a product does not exhibit the phenomena of combustion even when it combines with oxygen, we cannot avoid concluding, that all combustibles contain an ingredient which they lose when converted into products and that this loss contributes to the fire which makes its appearance during the conversion. Many circumstances contribute to render it probable that this ingredient is *light*.

Combustibles contain *light*.

When supporters and combustibles combine, the *caloric and light* fly off in the combination called *fire*.

If we suppose that the oxygen of supporters contains *caloric* as an essential ingredient, and that *light* is a component part of all combustibles, the phenomena of combustion above enumerated, numerous and intricate as they are, admit of an easy and obvious explanation. The component parts of the oxygen of supporters are two; namely, 1. a base, 2. *caloric*: The component parts of combustibles are likewise two; namely, 1. a base, 2. *light*. During combustion the base of the oxygen combines with the base of the combustible, and forms the product; while at the same time the *caloric* of the oxygen combines with the *light* of the combustible, and the compound flies off in the form of fire. Thus combustion is a double decomposition; the oxygen and combustible divide themselves each into two portions, which combine in pairs; the one compound is the *product*, and the other the *fire*, which escapes.

Combustion will not ensue if either of these in-

Hence the reason that the oxygen of products is unfit for combustion. It wants its *caloric*. Hence the reason that combustion

tion

tion does not take place when oxygen combines with products or with the base of supporters. These bodies contain no light. The caloric of the oxygen of course is not separated, and no fire appears. And this oxygen still retaining its caloric, is capable of producing combustion whenever a body is presented which contains light, and whose base has an affinity for oxygen. Hence also the reason why a combustible alone can restore combustibility to the base of a product. In all such cases a double decomposition takes place. The oxygen of the product combines with the base of the combustible, while the light of the combustible combines with the base of the product. Thus when iron acts on water, the oxygen of the water combines with the base of the iron, while at the same time the *light* of the iron combines with the hydrogen of the water, and occasions its escape in the state of gas.

But the application of this theory to all the different phenomena described above, is so obvious, that it is needless to give any more examples. Let us rather inquire into the evidences which can be brought forward in its support.

10. Now as caloric and light are always emitted during combustion, it follows that they must have been previously component parts either of the combustible, or of the supporter, or of both.

That the oxygen of the supporters contains either one or both of these substances, follows incontrovertibly from a fact already mentioned, namely, that the oxygen of products will not support combustion, while that of supporters will. Hence the oxygen of supporters must contain something which the oxygen of products wants, and this something must be caloric, or light, or both.

That the oxygen of some of the supporters at least contains caloric as an ingredient, has been proved, I think, in a satisfactory manner, by the experiments of Crawford, Lavoisier, and La Place. Thus the temperature of hot blooded animals is maintained by the decomposition of *air*. Now if the oxygen of one supporter contain caloric, the same ingredient must exist in the oxygen of every supporter, because all of them are obviously in the same state. Hence I conclude that the oxygen of every supporter contains caloric as an essential ingredient.

The light emitted during combustion must either proceed from the combustible or the supporter. Now that it proceeds from the combustible must appear pretty obvious, if we recollect

Ingredients of fire
be absent.

They must
therefore have
previously exist-
ed in the com-
bustible, the sup-
porter, or both.

References to
facts: Caloric.

Light.

lect

lest that the colour of the light emitted during combustion varies, and that this variation usually depends, not upon the supporter, but upon the combustible. Thus carbonic acid burns with a blue flame, carbonated hydrogen with a white, and charcoal with a red; sulphur with a blue or violet, zinc with a greenish white, and phosphorus with a white.

Natural formation of combustibles and supporters: Vegetation.

The formation of combustibles in plants obviously requires the presence and agency of light; for when plants vegetate in the dark, their carbon is not increased, nor is any oily or resinous matter formed in them. The leaves of plants emit oxygen gas when exposed to the sun's rays, but never in the shade, or in the dark. Senebier has demonstrated that this emission is occasioned by the decomposition of carbonic acid. This acid, which is a product of combustion, is decomposed by the leaves of plants assisted by sunshine, and converted into oxygen gas and charcoal, a *supporter* and a *combustible*. This process is exactly the reverse of combustion, and must therefore restore the substances which had been lost during combustion; that is to say, caloric and light. But the sun's rays consist of these two bodies. Thus we see why plants require sunshine. A part of vegetation consists in decomposing, or *unburning*, products, and converting them into supporters and combustibles; but for such a conversion caloric and light are absolutely necessary.

The same effects by art.

Besides vegetation, we are acquainted with two other methods of unburning products, or of converting them into products and combustible; by exposing them, in certain circumstances, to the agency of *fire* or of *electricity*. The oxides of gold, silver, and mercury, when heated to redness, are decomposed, oxygen gas is emitted, and the pure metal remains behind. In this case the necessary caloric and light must be furnished by the fire; a circumstance which explains why such reductions always require a red heat. When carbonic acid is made to pass repeatedly over red-hot charcoal, it combines with a portion of charcoal, and is converted into carbonic oxide gas. If this gas be a combustible oxide, the base of the carbonic acid and its oxygen must have been supplied with light and caloric from the fire; but if it be a *partial combustible*, it is merely a compound of carbonic acid and charcoal: which of the two it is, remains still to be ascertained. Electricity decomposes water, and converts it into oxygen gas and hydrogen gas; it must therefore supply the heat and the light which these bodies lost when converted into a product.

These

These facts, together with the exact correspondence of the theory given above with the phenomena of combustion, render it so probable, that I have ventured to propose it as an additional step towards a full explanation of the theory of combustion. Every additional experiment has served to confirm it more and more*. It even throws light upon many phenomena which have been hitherto considered as altogether anomalous, as will be evident from the following observations.

Application of this theory to other facts.

II. In the year 1793, the associated Dutch chemists drew the attention of philosophers to a curious phenomenon which accompanies the formation of some of the sulphurets; a phenomenon previously noticed by Scheele; but which they first described in detail. When eight parts of copper filings, and three parts of flowers of sulphur are mixed together in a glass receiver, and the vessel placed upon burning coals, the mixture melts, a kind of explosion takes place, it becomes suddenly red hot, and a glow, like that of a piece of red hot charcoal fanned by bellows, rapidly pervades the whole. When this disappears, the mixture is found in the state of solid sulphuret of copper. Iron, lead, tin or zinc, may be substituted for copper. The experiment succeeds whether the vessel be filled with air, or with azotic, or hydrogen gas, or even with water or mercury. What is singular in this experiment is the glowing *red heat*, or the emission of *fire* which accompanies the combination of the sulphur and metal. This emission being the same which takes place during combustion, the process has been considered as a combustion, and stated as such by the German chemists, as an objection to Lavoisier's theory, which supposes that oxygen is a necessary agent in that process: while other philosophers have denied that this operation is a combustion, or that it has any resemblance to that process.

Ignition produced by the fusion of sulphur with a metal,

The same emission of caloric and light, or of *fire*, takes place when melted sulphur is made to combine with pot-ash, or with lime, in a crucible or glass tube, and likewise when

or of sulphur with potash or lime; or phosphorus with lime, &c.

* In the preceding enumeration of facts I have not taken notice of the modifications which the Lavoisierian theory has received from Hutton, Delametherie, Richter, and Brugnatelli; because I suppose them sufficiently known. Every one of these modifications agrees in some particulars with the theory given in this Paper, but differs from it in others.

melted phosphorus is made to combine with lime heated nearly to redness. In all probability barytes and strontian exhibit the same phenomenon when combined with melted sulphur or phosphorus; and some of the metals when combined with phosphorus. In general then the emission of *fire* accompanies the combination of melted sulphur and phosphorus, with several of the earths, fixed alkalies and metals, heated previously to a certain temperature.

Explanation.

Fused sulphur or phosphorus contain *caloric*: metal. &c. contain *light*. They become *solid* in combination and give out *caloric* and *light*; i. e. *fire*.

To explain the phenomenon we have only to recollect, 1. That the sulphur and phosphorus are in the melted state, and therefore contain *caloric* as an ingredient. 2. That the alkalies, earths and metals which produce the phenomenon in question, contain *light* as an essential ingredient. 3. That the sulphuret or phosphuret formed is always in a solid state; these three points once established, the process admits of a very simple explanation. The sulphur or phosphorus combines with the base of the metal, earth or alkali; while at the same time the *caloric* to which the sulphur or phosphorus owed its fluidity, combines with the *light* of the metal, earth, or alkali, and the compound flies off under the form of *fire*.

The process resembles combustion except in its product;

Thus the process is exactly the same with combustion, excepting as far as regards the product. The melted sulphur or phosphorus acts the part of the *supporter*, while the metal, earth or alkali occupy the place of the *combustible*. The first furnishes *caloric*, the second *light*, while the base of each combines together. Hence we see that the base of sulphurets and phosphurets resembles the base of products in being destitute of *light*, the formation of these bodies exhibiting the separation of *fire* like *combustion*, but the product differing from a product of combustion in being destitute of oxygen, we may distinguish the process by the title of *semi-combustion*; indicating by the term, that it possesses one half of the characteristic marks of combustion, but is destitute of the other half.

termed semi-combustion.

Facts to shew that potash and lime contain *light*.

The only part of this theory which requires proof is, that *light* is a component part of the earths and alkalies. But as potash and lime are the only bodies of that nature, which I am certain to be capable of exhibiting the phenomena of semi-combustion, the proofs must of necessity be confined to them. Now that *lime* contains *light* as a component part has been long known. Meyer and Pelletier observed long ago, that when water is poured upon quicklime not only heat but *light* is emitted. *Light* is emitted also abundantly when sulphuric

acid is poured upon lime*. In both cases a *semi-combustion* takes place. The water and the acid being solidified give out *caloric*, while the quicklime gives out *light*; that lime during its calcination combines with light, and that light is a component part of quicklime is demonstrated by the following experiment, for which we are indebted to Scheele.

It is well known that fluor spar (native fluuate of lime) has the property of phosphorescing strongly when heated, but that the experiment does not succeed twice with the same specimen. After it has been once heated sufficiently, no subsequent heat will cause it to phosphorate. Now phosphorescence is merely the emission of light, light of course is a component part of fluor spar, and heat has the property of separating it. But the phosphorescing quality of the spar may be again recovered to it, or which is the same thing, the light which the spar had lost may be restored by the following process. Decompose the fluuate of lime by sulphuric acid, and preserve the fluoric acid separated. Boil the sulphate of lime thus formed with a sufficient quantity of carbonate of soda; a double decomposition takes place; sulphate of soda remains in solution, and carbonate of lime precipitates. Calcine this precipitate in a crucible till it is reduced to quicklime, and combine it with the fluoric acid to which it was formerly united. The fluor spar thus regenerated phosphoresces as at first. Hence the lime during its calcination must have combined with light.

That potash contains light, may be proved in the same manner as the existence of that body in quicklime. Dizé has shown that much light is emitted when sulphuric acid is poured upon potash, but more when it is poured on the carbonate of potash. Now as potash is deprived of its carbonic acid by lime, it is obvious that the process must be a double decomposition; the base of the lime combines with carbonic acid, while its light combines with the potash.

These remarks on semi-combustion might easily be extended much farther. For it is obvious, that whenever a liquid combines with a solid containing light, and the product is a solid body, something analogous to semi-combustion must take place. Hence the reason why water increases the violence of combustion when thrown sparingly into a common fire.

* Dizé Jour. de Phys. 49, 177.

IX.

A Continuation of the Experiments and Observations on the Light which is spontaneously emitted from various Bodies; with some Experiments and Observations on solar Light, when imbibed by CANTON'S Phosphorus. By NATHANIEL HULME, M. D. F. R. S. and A. S.

(Concluded from page 40.)

§ 5.

The Effects of carbonic Acid Gas or fixed Air on spontaneous Light.*

EXPERIMENTS.

Carbonic acid gas extinguishes spontaneous light; but it revives by common air.

Exp. 1. AT 10 P. M. a piece of fresh herring, weighing about three drams, was suspended in a wide-mouthed ten-ounce phial, filled with carbonic acid gas, and closed with a cork and bladder. It was retained there for three successive nights; but emitted no light.

Exp. 2. The same experiment was made with a piece of herring, which was beginning to be luminous. On the next evening, the illumination was found to be extinct: nevertheless the herring was still kept in the gas for three nights longer, but did not become lucid.

Exp. 3. At 7 P. M. a piece of fresh mackerel was introduced above water, into a wide-mouthed bottle, holding 24 ounces, which was completely filled with carbonic acid gas, and supported by a tea-faucer that held about three ounces of water. On the second night it was dark, and continued the same on the third. It was then exposed to the influence of atmospheric air, and, on the next evening, it was pretty luminous, and likewise on the succeeding night.

Exp. 4. At 9 P. M. a cork, smeared with the luminous matter of a mackerel, was put into a five-ounce wide-mouthed phial, filled with carbonic acid gas, and then closed with a glass

* This gas was obtained from powdered chalk, or marble, and diluted sulphuric acid.

stople. It continued to shine pretty vividly for some little time; then the light gradually diminished, so that at twelve, only a small spark remained.

Exp. 5. At 10 P. M. another cork, illuminated with mackerel-light, was introduced above water, into 24 ounces of the gas; and its light was nearly extinct at twelve.

Exp. 6. At 8 P. M. a fragment of shining wood was put above water, into 24 ounces of the gas; and it had not been long there before the light disappeared. It was then taken out, and exposed to the action of atmospheric air, when its shining property soon returned.

Exp. 7. Another fragment of brightly shining wood was introduced above water, into the same quantity of the gas, at 10 P. M. and the light was extinguished in the space of an hour. After this, it was exposed to the open air, and the light gradually revived.

Exp. 8. At 8 P. M. a luminous dead glow-worm was put above water into the gas; its glowing appearance gradually faded, and in a short time became quite invisible. It was then taken out, and the light, by degrees, re-appeared as vivid as before.

OBSERVATION.

This gas, we find, has also an extinguishing property, with respect to spontaneous light: but, in general, the light returns, if the object of experiment be taken out, and exposed to the open air.

§ 6

*The Effects of sulphurated hydrogen Gas * on spontaneous Light,*

EXPERIMENTS.

Exp. 1. At noon, a piece of a very fresh mackerel, with a bright eye, was introduced above water, into 24 ounces of this gas, and was retained therein for three successive evenings, without emitting any light. It was then exposed to atmospheric air; yet it continued dark on the two following

Sulphurated hydrogen extinguishes spontaneous light more effectually and more permanently than carbonic acid.

* This gas was obtained from sulphuret of potash and diluted muriatic acid.

nights:

nights: but, on the third, it was very luminous, and remained so on the fourth and fifth.

Exp. 2. The same experiment was then made with a piece of fresh herring, which was also kept in the above gas, for about three nights, without being luminous. After exposure to common air, it did not emit any light during the first 24 hours. However, on the subsequent night, it began to shine, had a very bright light on the following evening, and continued shining for several succeeding nights.

Exp. 3. A cork, smeared with the luminous matter of a herring, was put above water, into 24 ounces of the gas; and the light was extinguished in less than an hour. The experiment was repeated in the same gas, and with the same result.

Exp. 4. A cork, illuminated with mackerel light, was introduced into the same quantity of gas; and was dark in half an hour.

Exp. 5. A fragment of shining wood, being put into the gas, became dark in eight minutes. A second piece became dark in five minutes. They were then taken out, and continued dark all that evening. On the next evening, one of the pieces was uncommonly lucid.

Exp. 6. At 10 P. M. another fragment of brightly shining wood was introduced above water, into 24 ounces of the gas, and was extinct at eleven. It was then exposed to the open air; but there was no return of light that evening. On the following night, it was found pretty luminous.

Exp. 7. A finely shining dead glow-worm was next put above water, into this gas, and its light was quickly extinguished. In a second experiment, in the same gas, the light was much slower in its extinction. In both instances, after the insect was withdrawn, and placed in atmospheric air, the light gradually revived.

OBSERVATION.

It is apparent, by these experiments, that sulphurated hydrogen gas extinguishes spontaneous light much sooner than carbonic acid gas, and that, in general, the light returns much more slowly, when the subject is exposed to atmospheric air.

§ 7.

*The Effects of nitrous Gas * on spontaneous Light.*

EXPERIMENTS.

Exp. 1. A piece of fresh herring was introduced above water, into this gas, at 3 P. M. and remained there four nights, without emitting any light: it was then withdrawn, and exposed to common air, for the space of three nights; but did not become lucid.

Nitrous gas quickly and effectually extinguishes spontaneous light.

Exp. 2. The same experiment was made with a piece of herring beginning to be luminous; but its light was gradually extinguished: it was detained in the gas for three nights, and taken out dark. It was then exposed to the open air, for the three subsequent nights; but its shining appearance did not return.

Exp. 3. A cork with luminous matter, introduced above water, into this species of gas, had its light, in general, extinguished in from 10 to 30 minutes; and, when taken into common air, its light very seldom re-appeared.

Exp. 4. Fragments of shining wood, above water, in nitrous gas, were likewise commonly rendered dark in a very short space of time, as in three or four minutes; sometimes a fragment, if uncommonly luminous, would not be extinguished in less than six or eight minutes; and very seldom would the light revive, on exposing the wood to atmospherical air.

Exp. 5. A dead shining glow-worm being put above water, into this gas, its light was quickly extinguished; but, after the insect was taken into the common atmosphere, the light gradually returned. The experiment was thrice repeated, and with the same result.

OBSERVATION.

This species of gas, we observe to have totally prevented the emission of light, and to have quickly extinguished that which had been emitted: likewise that the luminous objects which had been under its influence, (except the glow-worm) did not experience a revival of their light, when taken out, and kept for some time in common air.

* This gas was obtained from copper and diluted nitrous acid.

§ 8.

The Effects of a Vacuum on spontaneous Light.

EXPERIMENTS.

The light is extinguished in vacuo, but beautifully restored by admission of air.

Exp. 1. A piece of shining wood, of a moderate size, was put under the receiver of an air-pump, in a dark room; in proportion as the air was extracted, the light was gradually extinguished, and at last reduced to a mere point, just visible, owing most probably to a small residuum of air, which is always left, even in the most perfect machine. Fresh air was then leisurely admitted, and the light was immediately revived in a very beautiful manner. This experiment was frequently repeated, and always with the like effect.

Exp. 2. Some luminous matter of a herring, uncommonly bright, was smeared upon a piece of red blotting paper, and then submitted to the operation of the air-pump. The light became fainter and fainter, as the inclosed air was withdrawn, and at last nearly vanished; but brightened up as before, on the influx of fresh air. The experiment was repeated, and with the same result.

SECTION XII.

Experiments and Observations on solar Light, when imbibed by Canton's Phosphorus.

§ 1.

The Effects of Heat on imbibed solar Light.

I. *The imbibed Light is rendered more vivid by a moderate Degree of Heat.*

EXPERIMENTS.

Canton's phosphorus shines more when heated.

Exp. 1. Having prepared some Canton's phosphorus, and exposed it to the light of the sun, it was carried into the dark laboratory, to separate the illuminated parts from those that remained dark. In doing which, some luminous fragments were placed upon the palm of the hand, and retained there for some time, when it was observed, that the warmth of the hand considerably increased the degree of light.

Exp. 2.

Exp. 2. Some fragments of this illuminated phosphorus were put into a small phial, which was then closed with a cork, and suspended, by a string, in a quart of water heated to about 126° ; by these means, the light was rendered much more vivid than before.

Exp. 3. Some other pieces of the illuminated phosphorus were dropped separately into a glass tube 32 inches long, and $\frac{7}{16}$ bore, filled with water at about 120° . The light of each piece became exceedingly bright, as soon as it entered the hot water; and they all descended, very luminous, from the top to the bottom, some quickly and others slowly, according to their gravity, making a very pleasing experiment.

Exp. 4. A large wooden bowl, about 12 inches wide, was next filled with water heated to about 110° , and then a quantity of illuminated phosphorus, partly in the form of powder, and partly in pieces of different magnitudes, was scattered over the whole surface of the water; all which pieces fell, with increased splendour, to the bottom, where they preserved their light for some time.

II. *The imbibed Light is extinguished by a great Degree of Heat.* but too much heat extinguishes it.

Exp. 5. Some fragments of the phosphorus, rendered luminous, were exposed to a greater degree of heat, namely, by casting them into a tin vessel containing two pints of boiling water. They flashed with increased light, as soon as they came in contact with the water, fell precipitately to the bottom, in a lucid state, and then were gradually extinguished.

Exp. 6. *In which the degree of heat was still increased.* A small bar of iron, of about an inch square, was made red-hot, and laid horizontally in the laboratory, until, by cooling, it nearly ceased to shine. Some pieces of illuminated phosphorus were then put upon it in succession, and the light, in a moment, glowed with uncommon lustre, but was quickly after totally extinguished*.

* Solar light, when received merely on a piece of white paper, may also be rendered more luminous by heat, and then extinguished by it, as appears from an experiment made by the late Mr. B. Wilson, whose book on phosphori I had not seen before this Paper was drawn up.

III. *The*

Latent light is excluded from Canton's phosphorus by heat.

III. *The imbibed Light, after being in a latent State, is excited and rendered luminous by the Agency of Heat.*

Exp. 7. Some small pieces of the phosphorus, after having been illuminated, were deposited in the laboratory; when the light by degrees faded away, and became totally invisible. They were kept in this dark state for the space of ten days, and then placed one after another upon a heated bar of iron, as in the last experiment, upon which they quickly became exceedingly luminous.

From an experiment made by the ingenious Mr. Canton, I observe, that some of his phosphorus, contained in glass balls hermetically sealed, and heated in the above manner, gave a considerable degree of light, after it had been kept in a state of darkness more than six months. *Phil. Transf. Vol. LVIII. page 342.*

§ 2.

The Effects of Cold on imbibed Light.

EXPERIMENT.

Cold extinguishes the solar light of Canton's phosphorus, &c. About 15 grains of the phosphorus were put into a half-ounce phial, containing two drams of cold pump water, that had been deprived of its air by boiling. The phial was then corked, and exposed for some time to solar light, whereby the phosphorus became finely illuminated. In this state, it was immediately put into a frigorific mixture, composed of snow and sea salt, and retained there about 30 or 40 minutes, when it was taken out, and the light found to be totally extinguished. The phial was then placed in some water, at about 60° temperature, and the light gradually revived, and became as brilliant as before it had been exposed to the cold. This experiment was frequently repeated, and always with the same result.

I cannot but remark, that in the course of experiments on this subject, the superior power of solar over that of spontaneous light was very apparent. For, the first trials being made in small phials, containing only atmospheric air with the phosphorus, the light was with some difficulty totally extinguished; and, after the phials were taken out of the frigorific mixture,

mixture, the temperature of the laboratory would commonly soon revive the light, which rendered the experiments not altogether satisfactory. Finding it thus somewhat difficult to extinguish solar light *in air*, recourse was had *to water*, in the manner above described. This answered perfectly well; for the water, when frozen, gave a substantial body, as it were, to the imbibed light of the phosphorus, so as to enable it to retain the excess of cold arising from the frigorific mixture; thereby making the experiments quite satisfactory. When the phosphorus was thus surrounded by ice, only a few minutes stay in the frigorific mixture would generally be sufficient for a total extinction.

OBSERVATION.

From these experiments, compared with those recited in my former Paper on spontaneous light, it appears that solar light, when imbibed by Canton's phosphorus, is subject to the same laws, with respect to heat and cold, as the spontaneous light of fishes, rotten wood, and glow-worms.

P. S. In these experiments with solar light, the phosphorus was sometimes exposed to the direct rays of the sun, at other times to common day-light, in a northern aspect; and it was remarked, that it became somewhat more luminous by mere day-light, than by the rays of the sun.

It may also be proper to observe, that the above experiments were made with an improved preparation of Canton's phosphorus. This improvement, which was first made by Dr. Higgins, consists in omitting the pulverization of the shells. His method was, after calcining the oyster-shells, to put the pieces, both great and small, in layers, into a crucible furnished with a cover, and to sprinkle flowers of sulphur between each layer. After they had remained some time in the furnace, they were taken out, suffered to cool, and then kept in a large bottle with a glass stopple. For this communication, I am indebted to Mr. Lewis of Holborn, near Southampton-street, who has an extraordinary dark room, where, at times, he amuses his friends with some beautiful appearances, arising from solar light imbibed by phosphorus prepared as above directed. A still further improvement of this phosphorus, it appears to me, may be made by substituting precipitated sulphur for the flowers of sulphur; and the experiments of this section were chiefly made with phosphorus so prepared.

Improved preparation of Canton's phosphorus by Dr. Higgins.

Dark room of Mr. Lewis.

X.

Description of a Lamp upon ARGAND'S Principle, with Improvements, in which the Oil is maintained at the same Level by the constant Action of a Pump. By Citizens CARCEL and CAREAU.*

Excellence of the lamp of Argand.

LAMPS with a double current of air are among the inventions of the eighteenth century, which are most honourable to the industry of France, and of which the general use sufficiently bespeaks the value. But it is not enough that a great light should be produced without smell or smoke, but an object of nearly equal utility consists in producing the light steadily, with economy, in the most advantageous form, and best adapted to economical purposes.

Improved by Carcel and Careau.

Citizens Carcel and Careau have succeeded in this object. We shall not dwell upon the obstacles they must have met with in arriving at the degree of perfection they have obtained; it will be sufficient to shew the value of their invention, if we can prove that it surpasses every thing of the kind which has yet been exhibited. This lamp has the double advantage of exhibiting all the good qualities of the lamp we before possessed, without their inconveniences. The nozzle undergoes no alteration by heat, neither is the wick destroyed, but almost constantly preserves its whiteness.

Inconveniences of lamps of the usual construction.

The oil in ordinary lamps is liable to flow out by its expansion when heated; but in this mechanical lamp it constantly preserves its level. By dispensing with the reservoirs of oil in those lamps which are called fountain lamps, these inventors have succeeded in affording light which is not shaded on any side. In this respect the lamp is very economical, because nearly half the light of a fountain lamp must necessarily be intercepted by the receiver, which requires to be placed above the level. But in these the reservoir is in the foot, which renders them more portable; and from this circumstance, as well as the other advantages of their construction, there is no dan-

* From *Les Annales des Arts & Manufactures*, vi. 269. I am much obliged to my correspondent C. D. for directing my attention to this article.

ger of spilling the oil by inclining it, which is an inconvenience of great magnitude, and hitherto constituting one of the greatest objections to the lamps in use.

In other lamps the flame varies in its intensity commonly in about two hours; but in the mechanical lamp it constantly preserves the same supply and the same brilliancy.

The mechanism adapted to the foot in order to cause the oil to ascend, has been reduced to the greatest simplicity; it is firm and durable, and has no communication with the oil of the reservoir.

This lamp may be used in distillations and chemical preparations, as well as in culinary purposes; and in general we cannot do better than transcribe the report made to the National Institute by Guyton, Morveau, and Charles.

“ The Class having charged us to examine the mechanical lamp presented by the Citizens Carcel and Careau, at the sitting of the 21st of last month, as a means of adding a new degree of perfection to lamps with an inner current of air, as well as with regard to the intensity of light as to economy and the convenience of daily use.

“ Lamps excited by an interior current of air, of which the invention belongs to Citizen Argant, and which were announced for the first time in February 1784*, have produced a revolution in the art of illuminating which time has served only to confirm, as it does all those which, being founded on true principles, receive the daily sanction of experience.

“ A short time afterwards Citizen Lange thought of contracting the glass chimney, so as to direct the external current of air nearer to the flame, by which means he determined a still more complete combustion of the oil, and produced a more brilliant light, without either smoke or smell. The union of these inventions seemed to have exhausted the subject, but Citizens Carcel and Careau apprehended that it was possible to render the lamps still more perfect. They considered that the best lamps of this description do not constantly afford the same intensity of light, because the wick not being constantly and alike supplied with oil, is subject to become charred; that it is necessary either to raise it beyond the proper elevation, or to trim the lamp again after some hours; and lastly, that the glass

* Journal de Physique for that month, page 159.

chimney not being capable of being made to that degree of accuracy as to afford the contraction always at the same height, the effect of this contrivance could not be the best possible, excepting when the accidental concurrence of circumstances might render it so.

“ At the same time that they were busied in remedying those inconveniences, they did not neglect the means of rendering the lamp more economical, convenient, and portable; without fear of spilling the oil; without any considerable effect upon the flame by motion; without any interception of the light from a reservoir; and with the addition of an agreeable and ornamental form.

“ The account resulting from our examination will enable the Class to judge of the difficulties they have had to vanquish, the ingenious processes they have used, and the success which has crowned their industry.

Experiments
with the new
lamp.

“ The mechanical lamp was lighted before the Class for a short time. They saw the brilliant light it afforded, and compared that light with the lamps which usually illuminate their place of meeting. It continued absolutely the same five hours after it had been lighted, though the wick had not been altered or touched.

Compared with
the common,
it gives more
than twice the
light.

“ In order to determine more exactly the intensity of the light, it was placed at such a distance that a body interposed between its light produced a shadow of the same obscurity as that of a common Argand's lamp. The distances were— from the mechanical lamp forty-six decimeters, and from the common lamp thirty-three decimeters, which, by the square of the distances, gives the ratio of 2116 to 1089, or 100 to $48\frac{3}{4}$, for the intensities of the light.

“ The common lamps of this construction not always affording a light perfectly uniform, and the glass chimney of that which we employed not being of the most favourable dimensions, we thought it proper to endeavour to obtain a confirmation of this ratio by the comparison with candles, of which the light is less subject to vary.

Comparison with
candles.

“ Six candles (I suppose of wax), of the weight of one hectogram (five to the French pound), were arranged in such a manner that their flames could not mutually intercept each other. The shadow produced by the interposition of an opaque body received on a card, was found to be similar to that of the
mechanical

mechanical lamp when the candles were brought to the distance of 566 centimeters, at the same time that the mechanical lamp was drawn back to 785. The squares of these numbers give the ratio of 100 to 52 nearly. If instead of the latter term we put the quotient of its division by the number of candles, we see that the light of the mechanical lamp is to that of one candle as 100 to $11\frac{1}{2}$, or it would require eleven candles and a half to give the same light.

“ As this lamp may be used for the domestic purposes of affording heat as well as for the operations of chemistry even in the dry way, it becomes interesting to ascertain the degree of heat it was capable of communicating to vessels placed above its glass chimney. For this purpose we took a pyrometric piece of Wedgwood not baked; it was included between two very thin capsules of platina, and placed on a support, first at four centimeters, where, at the end of half an hour, the contraction was three degrees of the pyrometric scale; after which it was again placed at the distance of fifteen millimeters only from the upper extremity of the chimney, where, after having been kept for two hours, it was found to have passed the seventh degree, which, according to the table of correspondence of Wedgwood, would indicate 505 degrees of the centigrade thermometer. The heat it gives to other bodies placed over the flame.

“ A tube of glass of five millimeters, or two lines, in diameter, of the weight of thirty-seven decigrams, or sixty-six grains, the cube decimeter, was easily bended over the chimney, that is to say, at eight centimeters, or two inches, &c. from the upper extremity of the flame. Tin, placed in a small crucible of kaolin, upon a support of the same height, flowed in less than seven or eight minutes. Five grams, or eighty grains, of antimony in small fragments, afforded in a similar crucible, at the end of an hour, a degree of fusion sufficiently advanced to round the lower part of the button, and it is known that the lowest estimation of the heat required to fuse this metal is 431 degrees. Tube of glass.

“ The consumption of oil was determined by several trials, one of which was continued for seven hours: It varied very little; the mean term was 34,648 grams per hour. ($1\frac{1}{4}$ oz. avoid.) Consumption of oil.

“ With regard to the operative means by which these effects are obtained, we shall remark, first, the disposition of the chimney, which can be raised or lowered at pleasure by turning the cylinder Mechanical construction.

cylinder which supports it on its screw, which has nine threads; by which means the true situation to produce the best effect is obtained, whatever may be the difference of size between one glass and another. The screws are pewter, but the reporters observe, that it may be better to make one of them iron.

“ The advantages of raising the oil by a pump moved by a spring are real, and prevent the wick from becoming charred for want of a constant supply of oil; besides which, as the flame is kept at a distance from the socket, it can neither calcine the metal nor deposit that crust of hardened oil which so soon alters the effects of other lamps.

The Commissaries express their approbation of the expediency by which the oil is prevented from leaking through and arriving at the mechanism, and state, that they examined the mechanism, which they find to be well calculated to produce the desired effect, and to prove as durable as could be wished.

Illumination
judged by read-
ing a book.

The brilliancy of effect is spoken of in high terms of approbation. They found that when a white gauze shade was put over the lamp, they could read in the *Anacharxis* of *Didot* at the distance of 78 decimeters, or about 24 feet; and that they could read in the same book at the distance of 89 decimeters with the flame uncovered. They do not mention the type of the book.

Blue chimney.

With a chimney of blue glass they could read at 72 decimeters.

When a ground glass was placed before the flame, they could read at the distance of 75 decimeters. A candle placed behind the same glass, gave a light by which it was difficult to read at 23 decimeters.

Particular de-
scription of the
construction of
the lamp.

In addition to the report of the Commission of the Institute, the Editor of *Les Annales des Arts* gives a more minute description, with an accurate engraving, which is presented to the reader in plate IX.

Fig. 1. View of the under part of the movement of the lamp: *aa* the lower plate of brass, which, with the superior plate connected by four brass pillars, forms the frame of the movement: *b*, ratchet wheel for winding or setting up the spring: It is pinned upon the square of the barrel arbor, and is kept in its place by the click and back spring *c*: The two holes in the face of the wheel are intended to receive the points of a key for winding it up: *d*, a plate which receives the

the pivots: *e*, large wheel, the pinion of which is driven by the wheel of the barrel: *ffff*, ends of the pillars: *h*, the fly and its endless screw.

Particular description of the construction of the lamp.

Fig. 2. Plan of the upper side of the movement: *i*, upper brass plate: *k*, the bottom of the reservoir of tin: *lll*, connecting pillars: *m m*, a circle with its screws, which serves to support the pump and its apparatus: *n*, body of the double force pump: *o*, aperture through which the oil rises through the ascending pipe: The elevation of this pipe is seen at *o*, fig. 3: *p*, the double arm of the pistons of the pump: *q q*, pump rods.

Fig. 3. Lateral elevation of the mechanism: *ss*, the plate which covers the body of the lamp, fixed by four screws, which at the same time connect the lower plate *u*: This plate is raised up in the drawing instead of being screwed down, in order to give a clearer idea of the connection. The middle piece *t* is perforated by two cylindrical holes, in which the pistons move which drive the oil. The piece *t* is also perforated above and beneath, in order to admit of two conical valves opening upwards into each cavity or pump barrel. The lower valve in each suffers the oil to rise into the barrel as the piston is withdrawn, and the upper suffers it to pass upwards when the forcing stroke takes place. The pieces *s*, *t*, and *u*, are secured by a piece of leather put between them before they are screwed together.

The dotted line *vv* denotes part of the reservoir of oil: *w*, part of the lower plate *m*: *x*, one of the pillars: *y*, the barrel containing the spring: *z*, pinion of the large wheel: *A*, pinion of the wheel of the endless screw: *B*, the wheel which drives a crank that works the pumps: *C*, the leading piece which acts on the lever: *D*, pinion of the wheel *B*. *E*, middle wheel, taking in the pinion *D*, and moved by its own pinion, which takes in the great wheel *e*: *F*, lever that works the arms of the pump.

Fig. 4. Elevation of the natural size of the glass globe *r*: 1, A pivot of steel which passes through the glass globe to communicate with the arms of the pump. The lower part of this pivot is square, in order to receive the lever of oscillation; the upper part is round, and has the arms of the pump driven tight upon it. 2, A brass socket in which the pivot 1, 3 turns, and is prevented from descending by a shoulder. The glass

Particular description of the construction of the lamp.

globe, or pearl-shaped vessel, is blown from a piece of barometer tube, and is connected in the socket by sealing wax (or lac), which is insoluble in oil. Citizen Carcel found a great difficulty in preventing the leakage of the oil to the works, which he has obviated by filling the glass vessel *r* with molasses thickened by heat. This fluid is not acted on by oil, and answers the purpose very well, without impeding the motion.

Fig. 5. Elevation of a chandelier to which the mechanism is adjusted. *G*, the foot containing the movement: *H*, reservoir containing oil, and the pump: *I*, the stem of the chandelier, forming part of the reservoir, and traversed by the tube of ascension *o*, which supplies the wick with oil: *K*, the apparatus of the lamp, having apertures through which the air enters. Fig. 6. Another more elegant figure proposed by C. O'Reilly.

The number of the wheels are as follow: Barrel wheel, 108 teeth: It drives a pinion of 12 on the arbor of *e*, which has 84 teeth. The middle wheel *E* has 96 teeth, and a pinion of 12. The crank wheel has 90 teeth and a pinion of 8. The last or endless screw wheel has 20 teeth, and a pinion of 12. And the screw has two threads.

The movement will go twelve hours without winding up.

The use of the lamp is simple and easy. The wheel-work is either locked or set at liberty at pleasure by a stop. In the regular process, the wick is to be first trimmed; then the mechanism is to be wound up; and in the next place, the oil is to be poured in. When the stop is disengaged, the oil is seen to rise up to the wick, and this is the proper time to light it. When it is no longer wanted, the movement is to be stopped of course, at the same time that the lamp is extinguished.

XI.

Note upon a peculiar vegetable Principle contained in Coffee. By RICHARD CHENEVIX, Esq. F. R. S. M. R. I. A. From the Author. May 24, 1802.

Coffee heated in water, then filtered and evaporated, **I**N a vessel calculated to confine the vapour of water, I heated a considerable portion of that liquid upon about a pound of raw coffee imported directly from Martinico, and of

of the quality of which I was well assured. I then filtered the liquor, and reduced it nearly to dryness, in a glass evaporating dish, at a gentle heat. By this means I obtained a small quantity of a clear yellow residuum, like the most transparent horn, and of the consistence of honey. This residuum did not deliquesce, or seem to be subject to change, by exposure to the atmosphere. It was soluble in alcohol. It did not manifest either acid or alkaline properties. By some experiments I perceived it to be a substance differing essentially from all the vegetable principles with which I was acquainted; and, finding that I could obtain it pure by a method which Proust used to procure tannin, I proceeded in the following manner:

I poured a solution of muriate of tin into some water which had been made to boil upon coffee, and obtained a precipitate, which I collected upon a filter, and washed. I then put it into water, and caused a current of sulphurated hydrogen gas to pass slowly through the liquor. By this process the oxide of tin combined with the sulphurated hydrogen gas, and the substance originally contained in the coffee, but which, as I shall immediately shew, had combined with the metallic oxide, was disengaged, and remained in the liquor, while the hydrogenized sulphuret of tin was precipitated. It then remained only to evaporate the liquor to obtain the vegetable principle. In this state it exhibited nearly the same appearance as before it had been combined with the oxide of tin, but seemed of a lighter colour, and more clear and transparent, being freed, as I suppose, from all extractive or other matter.

Imagining it now to be sufficiently pure, I dissolved it in a very small proportion of water, and examined it chemically.

The solution was of a bright horn colour; had a bitter taste, though not unpleasent. It was neither acid nor alkaline.

Solution of potash, of soda, or of ammonia, poured into the liquor, changed its colour to a bright garnet red.

Nitric acid produced a similar effect.

Very concentrate solutions of the alkaline carbonates did not cause a precipitate, as in a solution of tannin.

Sulphuric acid became of a dirty brown colour with the solution, but no other change was apparent.

With muriatic, phosphoric, and the vegetable acids, there was no change but what would naturally result from a mixture of the colours of both liquors.

Solution of iron; beautiful green; precipitate. With any solution of iron in which there was not excess of acid, the liquor passed to a beautiful green; and, if it was concentrated, there was a green precipitate. Salts formed of the red oxide of iron succeeded the best; and the reciprocal action of this principle and iron is almost as delicate as that of either gallic acid or tannin and iron.

Muriate of tin, yellowish precip. With muriate of tin there was a very abundant yellowish precipitate, which was a combination of the new vegetable principle with oxide of tin. Both this precipitate and that with iron are soluble in all the acids, and the liquors lose their colour.

Waters of lime, barytes, strontian. Lime-water did not cause any precipitate in this liquor, nor did strontia-water. Barytes-water gave a fawn coloured precipitate. With lime-water tannin gave a bluish-green precipitate; and nearly the same with strontia-water, as also with barytes-water.

Gelatine gave no precipitate. A solution of gelatine did not give any precipitate with this vegetable principle. The effect of tannin upon gelatine is well known.

Hence this principle is different from any other. By these experiments it is proved that the principle bears sufficient characters to distinguish it from tannin, or any other vegetable principle with which we are acquainted. The only property which it possesses in common with tannin, is its affinity for oxide of tin, while it is clearly distinct from tannin in every other point.

Coffee does not exhibit tannin till roasted. It is evident that coffee, before it is roasted, does not contain tannin. A solution of gelatin poured into a decoction of well roasted coffee, gives, however, an immediate precipitate; and the precipitate is the combination of tannin with gelatin. Messrs. Proust, Seguin, and Davy, have observed, that heat develops the tannin principle in many vegetables. In a commercial point of view, it might be advantageous to examine, whether those vegetables are not such as, before they are heated, contain this new principle. Although I did not perceive that the principle, when insulated from the entire vegetable, was converted by heat into tannin; yet the presence of the other component parts of the vegetable may influence the distribution of elements in such a manner as to produce combinations different from what the separate principles will afford.

Whether this new principle, while in the vegetable, may not be converted into tannin by heat. I have not had an opportunity of extending these researches any further in the vegetable kingdom.

XII.

Account of some Experiments performed upon a Scale of considerable Magnitude, and principally by the Agency of Frost, to produce Sulphate of Soda, Carbonate of Magnesia, and Muriate of Ammonia, from Sulphate of Magnesia, Carbonate of Ammonia, and Muriate of Soda. By H. CAMPBELL, M. D. From the Author.

THE selected saline substances were, Epsom salt, or the sulphate of magnesia; table salt, or the muriate of soda; and concrete hartshorn drops, or ammoniacal carbonate. Of the proper mixtures, calculation was formed upon a supposed acquaintance with the table of agents entering into the composition of each body.

Sulphate of magnesia and mur. soda gave sulph. soda and mur. mag. To the latter was added carbonate of ammonia, and the products were, muriate of ammonia and carb. of magnesia.

TABLE.

	Acid.	Alkaline Earth.	Water.
Sulphate of magnesia,	33 sulphuric	19 magn.	48
Muriate of soda,	52 muriatic	42 —	6
Ammoniacal carbonate	45 carb. acid	43 —	12

The expected and unavoidable products were,

	Acid.	Alkali.	Water.
Glauber salts, or sulphate of soda, containing - - -	27 sulph.	15 magn.	58
Salited or muriated magnesia, containing - - -	34 mur.	41 —	25

And upon the addition of the ammoniacal carbonate to the salited magnesia,

Carbonate of magnesia	- - 30 carbon	48 earth	22
Muriate of ammoniac	- - 52 —	40 —	8

The salited magnesia must only be considered as an intermediate product.

To avoid the detail of various experiments, those found to be most practicable will be only related, connected with the most practicable apparatus.

In iron pans of 150 gallons each, due proportions of sulphate of magnesia and muriate of soda were put in water in proportion to the laws of solubility and crystallization. The saline liquor was pumped into troughs lined with tinned iron plate. The troughs were thirty feet long, one foot wide, and three inches

Detail of the experiment.

inches deep. Some of the liquor was exposed in butcher's trays made of ash, four feet long, two feet wide, and a few inches deep. Thermometer, Fahrenheit, 26 —: Product, Glauber salt, or sulphate of soda, and muriate of magnesia, in quantity corresponding with the proportion of the requisites in the mixture.

Observations.

Contrary to my usual practice, the saline liquor in this experiment was put out in a cold state: The consequence was, a precipitation of the sulphate of soda in the form and appearance of fl. benzoin. The whole of the Glauber salt was thrown down; the mother liquor containing only muriate of magnesia. While the mother liquor contains Glauber salt, it will feel and appear in some measure harsh and saline; but when totally deprived of Glauber salt, new appearances will take place. The tinned iron gutters, instead of shewing a disposition to rust, will be remarkably clean and brilliant; the mother liquor soft and oily to the touch, and will pour smooth. To obtain the Glauber salt in distinct crystals, the product of this experiment was redissolved in a leaden evaporator, in its own water of crystallization, and pure water added, enough to make a light saline liquor. This liquor was put into leaden coolers ten feet long, five feet wide, two inches deep, standing in a room, therm. 57, doors and windows were left open. In the morning I found and took about three or four large baskets of Glauber salts, perhaps the most perfect crystals ever seen; crystals from nine to fifteen inches long, and one inch broad.

Very fine crystals of Glauber's salt.

Crystallization by frost without second solution.

To avoid the trouble and expence of dissolving the sulphate of soda twice, while the frost continued the operations were continued in the large way, in the proportions of sulphate magnesia, muriate soda, and water, as mentioned, with the precaution of pumping the liquor from the pans into the troughs and trays, *ног*, but beneath the boiling point. The whole of the Glauber salt was always formed when the frost was severe, as at thermom. 23, in beautiful and distinct crystals, half an inch broad and four or five inches long.

PRECAUTIONS AND REMARKS.

Various cautions, &c. on the management of salts.

Cast iron pans, heated in frosty weather, frequently burst. Boilers should be made of sheet iron. Jointed wooden vessels will not hold hot saline liquors. In melting salts different steps must be adopted. Salt holding little water of crystallization,

as muriate of soda, should be added to a sufficient quantity of water; without a sufficient quantity of water in a heated pan it will fuse into a mass; surface being lost, the difficulty of solution is almost inconceivable. Various cautions, &c. on the management of salts.

Salts holding an abundance of water of crystallization, as sulphate of magnesia and sulphate of soda, should be dissolved by a gentle heat in their water of crystallization, and what further water may be required, should be then added. Salts abounding in water of crystallization, added to warm water, will conglomerate, the caloric is absorbed, and although a small proportion of the salt is dissolved, the remainder is made colder: it is governed by the same law as operates on the solution of ice. The power of adding water evades a superabundant quantity of mother liquor, and ensures a quantity of crystals.

To obtain the saline liquor white and pellucid, it should not remain long in the iron pans, not more than two hours.

The first liquor pumped from the pans should be equally mixed with the last liquor, to obtain regular crystals in considerable quantity. It is known that the under layers of saline liquors are heaviest.

Very light liquors afford the largest shoots or crystals. Very large crystals appear opaque, and are never of a good colour; hence the necessity of breaking down to make many surfaces, to be acted upon by light.

Heavy saline liquors, holding Glauber salts, produce a quantity of salt in a compound state of crystallization.

A saline liquor, of a proper weight, will give numerous crystals, long and narrow, and these will be found in shape and colour most perfect. Such crystals were formed by the power of frost in the troughs and trays, acting upon *hot* saline liquor, in the *night*.

The *darkest* nights produced the best crystals. Perhaps the night was colder than the day. At any rate, the maxim of the necessity of light towards crystallization is doubtful in this instance. Light is not necessary to the crystallization of sulphate of soda. Light not necessary to the crystallization of salts.

The necessity of air towards crystallization, as shewn by the usual experiment of admitting air to a solution of salt in a vessel which did not previously contain air, is frequently exemplified in the large way. Salts sometimes will not shoot in a liquor covered Air is necessary.

covered by a strong pellicle. The workmen break the pellicle, and the salts instantly shoot. Crystallization, under such circumstances, is often *immediately excited* by gently running a saline liquor from one gutter into another gutter.

Electricity is of consequence.

Saline liquors, under an improper atmosphere, will not form regular crystals, but accrete into shapeless lumps. The electric and non electric states of the atmosphere govern crystallization. Many a maker of Glauber salts becomes fatigued from not understanding the latter circumstance.

Effect of frost in these experiments.

From what has been said, the young chemist must not suppose that the elective attractions are altered, and the new compounds in these experiments created by means of frost. From sulphate of magnesia and muriate of soda, sulphate of soda and muriate of magnesia can be obtained without frost. Frost only possesses a readier power of converting, from what was formed in a large portion of fluid, Glauber salt or sulphate of soda, speedily, from a hot liquid into a cold fluid state. The apparent cause consists in the power of severe frost in suddenly displacing the caloric, which becomes a disaggregative power.

H. CAMPBELL.

21, Fleet Street, May 20, 1802.

XIII.

Note of Citizen VAUQUELIN respecting the Boracite, called Magnefio-calcareous Borate by the French Chemists.

Component parts of the boracite.

THIS fossil, of which the properties have afforded a number of interesting observations to philosophers and mineralogists, has been analyzed for the first time by M. Westromb, who found it to contain,

Boracic acid	-	-	-	68
Magnesia	-	-	-	13,05
Lime	-	-	-	11
Alumine	-	-	-	1
Oxide of iron	-	-	-	0,75
Silex	-	-	-	2
				95,80

Citizen

Citizen Vauquelin having examined this substance some time ago with Mr. Smith, who brought a considerable quantity, thought himself justified in concluding, that the lime is no essential part in its composition, because its powder effervesces with the acids, and the small quantity of lime which chemists find in their analysis, did not appear to exceed that which the degree of effervescence has since indicated. They then attempted, by weak acids diluted with much water, particularly by the acetous acid, to separate the portion of carbonate mixed with the borate; but they did not succeed, because the acetous acid, even though feebly, likewise attacked the borate. They then left the question undecided, for want of transparent crystals which did not effervesce with the acids.

But since that period, Mr. Stromayer having supplied Citizen Vauquelin with crystals of this description, perfectly transparent, he subjected them to new experiments, with the intention merely of ascertaining the presence of lime.

He mixed their powder with muriatic acid, and when the solution was effected by means of a gentle heat, he evaporated to dryness in order to expel the excess of acid, and afterwards dissolved it in a small quantity of cold distilled water. By this method he separated most of the boracic acid, which was in very white brilliant plates. He diluted the solution with water, and mixed a certain quantity of oxalate of ammonia, which, as chemists know, is the best re-agent to shew the presence of the smallest quantity of lime contained in any fluid, provided it contain no excess of acid. Nevertheless, it exhibited no sign by which the existence of that substance could be suspected.

Experiments which shew that lime is not an essential part of this mineral.

In order to ascertain that the small quantity of boracic acid dissolved by water, at the same time as the muriate of magnesia, did not oppose the precipitation of the lime, he mixed a portion of the muriate of lime, which did not certainly amount to a fiftieth part of the borate employed, and a cloud was immediately produced through the whole of the fluid.

On the other part, he decomposed the artificial borate of lime in the same manner as the natural borate, and obtained, by the addition of oxalate of ammonia, a very abundant precipitate.

It is therefore evident, that if the natural borate had contained only one hundredth part of its weight of lime, it would have given some indication by the methods employed by C. Vauquelin

quelin. He therefore concludes, that the natural magnesian borate, when perfectly transparent, does not contain lime, and that that which is found in opaque crystals is interposed in the state of carbonate, and causes their opacity.

This substance must not therefore any longer be considered as a triple salt, under the name of magnesian-calcareous borate, but simply that of magnesian borate.

Bulletin des Sciences, No. 60.

XIV.

Facts and Observations tending to explain the curious Phenomenon of Ventriloquism. By Mr. JOHN GOUGH.*

IN the excellent Paper from which I have extracted the following pages, the author begins by observing, that the power of the ear to distinguish very slight variations of tone has long been a subject of universal notice; but that another very remarkable power, namely that of ascertaining the direction of sound, remains still without explanation. We perceive not only the tone, intensity, and character of sound; but also whether it arrives from the right or the left, from above or below, and this with a degree of precision which is of great and eminent utility in the concerns of life.

To investigate the foundation of this habitual judgment, we cannot have recourse to analogy of the sense of hearing with that of vision. The last direction of the ray of light is physically impressed by the pencil which enters the organ of that sense: but in the ear the undulations of sound are all made to strike the instrument of perception in the same direction, namely that of the auditory passage. The author, therefore, is led to attend to other facts and observations. He found by experiment, with an instrument constructed to afford the same equal sound by blows from the action of a spring, that he could judge of an increase or diminution of distance, to the one hundred and twentieth part of the whole range. He also remarks,

* Partly abridged and partly extracted from his "Investigation of the Method whereby Men judge by the Ear of the position of Sonorous Bodies relative to their own Persons." Manchester Memoirs, vol. V. part 2, page 622. London, 1802.

that

that sound is most effective, when its course is most directly opposed towards the organ; and shews by experience, that the head itself is a sensitive solid, capable of perceiving sounds by their action upon its surface. Hence when the differences of effect of the same sound, from its difference of distance and direction with regard to the two ears, and the two sides or portions of the head are considered, there will be a sufficiently great variety in the sensations to mark the circumstance of direction, and produce an habitual power of discrimination in this respect as in all others, which continually affect our daily operations.

The annexed diagram, Fig. 2, Plate VII. is given to explain the phenomena which arise from the pulses of sound being obstructed by the hearer's head, as they move in the horizontal plane passing through his ears, which case ought to be treated separately from the more complex one that comprizes the angle of elevation, along with the horizontal distance from the axis of hearing. When the sonorous object stands directly in front of the hearer, the semicircle ACB may be supposed to represent the horizontal section of his head, passing through the places of the ears, E and F , and the axis of hearing EF ; also let G be the place of the sounding body, which, according to the conditions of the case, lies in the plane ACB produced, and likewise in the right line GS , which bisects EF at right angles; seeing then EF is bisected by the perpendicular SG , the arch ECF is also bisected by the same in the point C . Draw LG , GK , to touch the circle in T and P , then will the arcs TEC , and CFP be equal. Now all the pulses which do not move in right lines, contained in the angles TGS and SGP fly off without touching the circle; consequently they add nothing to the sound impressed on the ears by the body placed at G , whether the places E and F be supposed to lie in the arcs TC and CP , or without them. But the same number of pulses equal in force will fall in a given time, and in similar directions, on the arcs TEC and CFP as well as on the ears situated at E and F ; and it is equally manifest, that the same reasoning will apply to two equal and similar solids, constructed upon the equal and similar planes, ECS and FCS . Now sound, though it be formed in the ears, is very much increased by the vibrations excited in the contiguous parts of the head by the pulses

Diagram to shew
the effects of
sound on the
head.
Direct hearing.

pulses which fall upon them: therefore, as often as the two portions of the head, which are separated by the vertical plane perpendicular to the axis of hearing, are equally agitated by the pulses of the same sound, the ears are also equally affected from the same cause; which never happens, as we learn from the testimony of the other senses, unless the sounding body be placed somewhere in the right line that bisects the axis of hearing at right angles. In this manner men are taught by experience to draw a general inference from a general observation; they therefore conclude a body to be situated directly before or behind their persons, as often as the sound of it strikes both their ears with equal forces.

Phenomena of oblique hearing.

The phenomena of oblique hearing remain to be explained; which case occurs as often as the sounding body is situated in the horizontal plane, but not in the right line that bisects the axis of hearing at right angles. Let M be the place of the sounding body, and draw MO to the centre of the circle; also let OC bisect the arc ECF , and take OG it equal to OM : also draw WM , MR , PG , GI tangents to the circle. Now suppose a sound equal to that at M , to proceed from G , then the latter would have the same effect on the arc TCP that the former has on WDR , because the arcs are manifestly equal, and alike situated relative to the points M and G . But the sound proceeding from G is a case of direct hearing, consequently the ears placed at E and F receive equal impressions from it, which is not the case with the pulses that flow from M . For though the forces imparted to the two arcs, TCP , WDR are equal, they do not fall equally on the circle in respect to the points E and F , which represent the ears; the sound therefore coming from M strikes these organs with unequal forces, as may be easily inferred from the figure.

Practical delicacy of perception as to the course of sound.

The judgment of the direction of sounds by Mr. Gough's ear was precise to about eight degrees of position as to the horizon, and about ten degrees as to elevation above its plane.

Deception of hearing from echos.

The faculty by which men judge of the direction of sounds must inevitably be liable to deception, whenever the sonorous undulations are made to arrive in a direction which is not from the sonorous object, as in the case of echo; and these deceptions will be the more striking, as we are apt to rely on the testimony of the sense with the most implicit confidence.

Upon

Upon this which constitutes the supplementary part of the present essay, I shall copy my author without farther abridgement or selection, page 644.

“ Any person who has had occasion to walk along a valley obstructed with buildings, at the time that a peal of bells was ringing in it, will assent to the truth of the circumstance here alluded to *. For the sound of the bells instead of arriving constantly, at the ears of a person so situated, in its true direction, is frequently reflected in a short time from two or three different places. These deceptions are in many cases so much diversified by the successive interpositions of fresh objects, that the steeple appears, in the hearer’s judgment, to perform the part of an expert *ventriloquist* on a theatre, the extent of which is adapted to its own powers, and not to those of the human voice.

Instance of the sudden changes of direction of the sound of bells.

“ The phenomenon has often attracted my attention; and the similarity of effect which connects it with ventriloquism, convinces me every time I hear it, that what we know to be the cause in one instance is also the cause in the other: I mean that the echo reaches the ear, while the original sound is intercepted by *accident* in the case of the bells, but by *art* in the case of the ventriloquist. In order that the cause which gives rise to the amusing tricks of this uncommon talent may be pointed out with the greater clearness, it will be proper to describe certain circumstances that take place in the act of speaking, because the skill of the ventriloquist seems to consist in a peculiar management of them. Articulation is the art of modifying the sound of the larynx, by the assistance of the cavity of the mouth, the tongue, teeth, and lips. The different vibrations, which are excited by the joint operation of the several organs in action, pass along the bones and cartilages, from the parts in motion to the external teguments of the head, face, neck, and chest; from which, a succession of similar vibrations is imparted to the contiguous air, thereby converting the superior moiety of the speaker’s body into an extensive seat of sound, contrary to general opinion, which supposes the passage of the voice to be confined to the opening

This effect is similar to that of ventriloquism.

The human voice does not issue from the mouth only.

* Viz. that a sudden change of direction in sound will be perceived when the original communication is intercepted, provided there be a sensible echo.---N.

Instance.

of the lips: there are but few persons, I imagine, who have not some time or other witnessed an incident, which shews the vulgar notion to be erroneous in this particular. For if a man standing in a close apartment should happen to apply his face to a loop hole, or narrow window, in order to speak to some person in the open air, a by-stander in the room with him will hear his voice, not indeed in its natural tone, but as if it were smothered by being forced to issue from a hollow case; but the circumstance of his words being heard distinctly, by one who cannot receive them from his mouth, proves the vibrations requisite for their production to be conveyed through the solid parts of the speaker's body, agreeably to the preceding assertion. The reason why we generally conclude the voice to be confined to the opening of the mouth, appears to be this. Those pulses which escape from the aperture are the strongest, they therefore surpass the weaker vibrations of the contiguous parts; for when a number of sounds moving in different directions strikes the ear at the same instant, the hearer does not notice their several places, but refers all of them to the quarter in which the most powerful is perceived. For instance, when a man stands at a sufficient distance from an extensive obstacle, his words are answered by an echo; but let him make a loud uninterrupted noise, neither he nor any body near him hears two voices whilst his continues, but as soon as the noise ceases the echo is perceived. This does not happen because the one begins the moment the other ends; but the reflected sound being the weaker of the two, it is smothered by that which precedes it.

Effect of undistinguished echos upon the voice or an instrument.

“We have seen in what manner secondary or reflected sounds are smothered by their principals; but though the places of such sounds are not recognized by the ear, their effects do not die away unnoticed: for the reverberated pulses mingle with those which come immediately from the sounding body, and thereby alter the sensation, which, without their interference, would be less compounded. This is the reason why the same musical instrument has one tone in a close chamber, where its notes undergo a multiplicity of reverberations, and another in the open air, where the reflections are few in comparison.

Case of an orator;

“But it is time to apply the preceding facts to the subject in hand; and it will be proper to begin with a familiar example. When an orator addresses an audience in a lofty and spacious room,

room, his voice is reflected from every point of the apartment, of which all present are made sensible by the confused noise that fills up every pause in his discourse; nevertheless every one knows the true place of the speaker, because his voice is the prevailing sound at the time. But were it possible to prevent his words from reaching any one of the audience directly, what then would follow? Undoubtedly a complete case of ventriloquism would be the consequence, and the person so circumstanced would transport the orator, in his own mind, to the place of the principal echo, which would perform the part of the prevailing sound at the instant. This he would be obliged to do, because the human judgment is bound, by the dictates of experience, to regard the person as inseparable from the voice; and the deception in question would be unavoidable, being produced by the same concurrence of causes which makes a peal of bells, situated in a valley, seem to change place in the opinion of a traveller. It is the business of a ventriloquist to amuse his admirers with tricks resembling the foregoing delusion; and it will be readily granted, that he has a subtle sense, highly corrected by experience, to manage, on which account the judgment must be cheated as well as the ear. This can only be accomplished by making the pulses, constituting his words, strike the heads of his hearers, not in the right lines that join their persons and his. He must therefore know how to disguise the true direction of his voice, because the artifice will give him an opportunity to substitute almost any echo he chuses in the place of it. But the superior part of the human body has been already proved to form an extensive seat of sound, from every point of which the pulses are repelled, as if they diverged from a common centre. This is the reason why people, who speak in the usual way, cannot conceal the direction of their voices, which in reality fly off towards all points at the same instant. The ventriloquist therefore, by some means or other, acquires the difficult habit of contracting the field of sound within the compass of his lips, which enables him to confine the real path of his voice to narrow limits. For he, who is master of the art, has nothing to do but to place his mouth obliquely to the company; and to dart his words, if I may use the expression, against an opposing object, whence they will be reflected immediately, so as to strike the ears of the audience from an unexpected

who may become
a ventriloquist.

Conditions of
that art.

The sound must
issue from the
mouth only,

and it must be
conveyed by echo.

unexpected quarter, in consequence of which the reflector will appear to be the speaker. Nature seems to fix no bounds to this kind of deception, only care must be taken not to let the path of the direct pulses pass too near the head of the person who is to be played upon; for, if a line joining the exhibitor's mouth and the reflecting body approach one of his ears too nearly, the divergency of the pulses will make him perceive the voice itself instead of the reverberated sound.

Narrative of a ventriloquist.

“The only ventriloquist I ever attended, acted in strict conformity to the preceding theory of this curious paradox in the science of acoustics. His audience was arranged in two opposite lines, corresponding to the two sides of a long narrow room, The benches on which they were seated reached from one end of the place to the middle of it, the other part remaining unoccupied. The seats exhibited by him were the

Voice of a child from beneath certain benches:

three following. *First*: he made his voice come from behind his audience, but it never seemed to proceed from any part of the wall, near the heads of the people present; on the contrary, it was always heard resembling the voice of a child, who seemed to be under the benches. He stood during the time of speaking in a stooping posture, having his mouth turned towards the place from which the sound issued; so that the line joining his lips and the reflecting object, did not approach the ears of the company. *Second*: advancing into the vacant part of the room, and turning his back to the audience, he made a variety of noises, that seemed to proceed from an open cupboard which stood directly before him, at the distance

noises from a cupboard,

cries from an inverted cup.

of two or three yards. *Third*; he placed an inverted glass cup on the hands of his hearers, and then imitated the cries of a child confined in it. His method of doing it was this; the upper part of the hearer's arm laid close along his side; then the part below the elbow was kept in an horizontal position with the hand turned downwards, which was done by the operator himself. After taking these preparatory steps, the man bent his body forwards in a situation which presented the profile of his face nearly to the front of his hearer, whilst his mouth pointed to the cup; in which posture he copied the voice of a confined child so completely, that three positions of the glass were easily distinguished by as many different tones, *viz.* when he pressed the mouth of the cup close against the palm, when one edge of it was elevated, and

Method of operating.

when

When the vessel was held near the hand but did not touch it. The second and third instances of ventriloquism afford strong proofs, that this delusive talent is nothing more than the art of substituting an echo for the primary sound; for, besides the change perceivable in the direction of the voice, it was found to be blended with a variety of secondary sounds; such as we know by experience are produced as often as a noise of any kind issues from a cavity. I have already made some remarks on this species of knowledge; but it would be improper to dismiss the subject without noticing the accuracy, with which the ear recognizes the finer modifications of sounds, and their causes. I have frequently observed, that a certain waterfall makes a flatter and duller noise when the ground is covered with snow, than that which it affords at other seasons. The human voice also undergoes a similar change within doors, by striking a multiplicity of soft bodies, such as a number of piles of wool, or a crowded congregation in a church.

Remarks and observations.

The method of preventing the vibration of the vocal organs from reaching the external teguments, is still wanting to complete this theory of ventriloquism; and I presume it can only be supplied by an adept in the art. I must therefore dismiss the subject unfinished, because I have no pretension to that character.

Difficulty not solved.

XV.

*An Analysis of a Mineral Substance from North America, containing a Metal hitherto unknown. By CHARLES HATCHETT, Esq. **

IN the course of the last summer, when I was examining and arranging some minerals in the British Museum, I observed a small specimen of a dark-coloured heavy substance, which attracted my attention, on account of some resemblance which it had with the Siberian chromate of iron, on which at that time I was making experiments.

Specimen of mineral observed in the British Museum.

Upon referring to Sir Hans Sloane's catalogue, I found that this specimen was only described as "a very heavy black stone,

Historical particulars.

Philos. Transf. 1802.

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"with

“with golden streaks,” which proved to be yellow mica; and it appeared, that it had been sent, with various specimens of iron ores, to Sir Hans Sloane, by Mr. Winthrop, of Massachusetts. The name of the mine, or place where it was found, is also noted in the catalogue; the writing however is scarcely legible: it appears to be an Indian name, (Nautneauge) but I am informed by several American gentlemen, that many of the Indian names (by which certain small districts, hills, &c. were forty or fifty years ago distinguished,) are now totally forgotten, and European names have been adopted in the room of them. This may have been the case in the present instance; but, as the other specimens sent by Mr. Winthrop were from the mines of Massachusetts, there is every reason to believe that the mineral substance in question came from one of them, although it may not now be easy to identify the particular mine.

§ I. DESCRIPTION OF THE ORE.

Description.

The external colour is dark brownish gray.

The internal colour is the same, inclining to iron gray.

The longitudinal fracture is imperfectly lamellated; and the cross fracture shews a fine grain.

The lustre is vitreous, slightly inclining in some parts to metallic lustre.

It is moderately hard, and is very brittle.

The colour of the streak or powder is dark chocolate brown.

The particles are not attracted by the magnet.

The specific gravity, at temp. 65°, is 5918*.

Experiment 1.

The ore was digested in muriatic acid. Little action.

Some of the ore, reduced to fine powder, was digested in boiling muriatic acid for about one hour.

* The following results of some experiments which I have purposely made, will shew how much the specific gravity of this ore is different from that of Wolfram, and Siberian chromate of iron.

Pure Wolfram, free from extraneous substances, at tem. 65° 6955.

Siberian chromate of iron, containing some of the green oxide 3728.

Pure Siberian chromate of iron - - - 4355.

The Siberian chromate of iron, like all other mineral substances which are not crystallized, and which consequently are not always homogeneous, must evidently be liable to considerable variations in specific gravity.

The

The acid appeared to have acted but slightly upon the powder; as the former remained colourless, and the latter did not seem to be diminished. A portion, however, chiefly of iron, was found to be dissolved; for ammonia formed a yellow flocculent precipitate; prussiate of potash produced one which was blue; and tincture of galls, when the excess of acid had been previously saturated by an alkali, formed a precipitate of a rich purplish brown colour.

Experiment 2.

Another portion of the powder was, in like manner, digested with nitric acid; but, excepting some slight traces of iron, this acid afforded nothing worthy of notice; the action of it upon the ore, was indeed scarcely perceptible.

In nitric acid:
little action.

Experiment 3.

Some of the pulverized ore was digested with concentrated sulphuric acid, in a strongly-heated sand-bath, until nearly the whole of the acid was evaporated; the edges of the mass then appeared blueish, and became white, when boiling distilled water was added.

In sulphuric acid.
Slight action:

This acid certainly acted much more powerfully than those which have been mentioned; but still only a small part of the ore was dissolved. It must however be observed, that a very copious blue precipitate was obtained by prussiate of potash; a plentiful purplish brown precipitate was also produced by tincture of galls, after the excess of acid had been saturated by an alkali; and, lastly, when the yellow ferruginous precipitate formed by ammonia was dissolved in diluted nitric acid, some white flocculi remained, which were completely insoluble in the acid, even when it was added so as to be in considerable excess.

From these experiments it was evident, that the ore could not readily be decomposed by the direct application of the mineral acids; and I therefore had recourse to the following method, which has frequently been employed with success in similar cases.

A N A L Y S I S.

A.

A mixture of 200 grains of the powdered ore with five times the weight of carbonate of potash, was exposed to a strong red heat,

Fusion with carbonate of potash.

heat, in a silver crucible. As soon as the matter began to flow, a very perceptible effervescence took place; and, when this had subsided, the whole was poured into a proper vessel.

The mass, when cold, was grayish-brown.

Solution in water left a brown residue.

Boiling distilled water was poured upon it; and the brown residuum, which was considerable, was welledulcorated upon a filter.

Precip. by nitrous acid: white flakes.

The filtrated liquor had a slight yellowish tinge, and, being supersaturated with nitric acid, afforded a copious white flocculent precipitate, which speedily subsided; but, although a very considerable additional quantity of nitric acid was poured upon the precipitate, it was not re-dissolved.

The residue did not yield to fusion by potash.

The residuum of the ore was dark brown, and was again melted with potash, and treated as before; but scarcely any effect was thus produced; the alkali was therefore washed off, and the powder was digested with muriatic acid, which soon assumed the deep yellow colour usually communicated to it by iron. After half an hour, the acid was decanted, and the residuum was washed with distilled water.

It was therefore digested with mur. acid, which took up iron.

This residue was fused with potash, and precipitated as before.

This powder was now of a much paler colour; and, being mixed with potash, it was melted and treated as before. A considerable precipitate was again obtained by the addition of nitric acid; and the residuum, after being digested with muriatic acid, was again fused with potash, by which means the whole was completely decomposed, after about five repetitions of each operation.

What was left was treated in the same manner.

B.

The muriatic solution saturated with ammonia afforded the iron.

The muriatic solution was diluted, and, being saturated with ammonia, afforded a plentiful ochraceous precipitate; which again was dissolved in cold dilute nitric acid, and afforded a small quantity of a white insoluble substance, similar to that which was obtained from the alkaline solution. From this nitric solution, I then obtained, by means of ammonia, a precipitate of oxide of iron, which, being properly dried, weighed 40 grains.

C.

The nitric precipitates were a white matter.

The different alkaline solutions which had been made subsequent to that which has been first mentioned, were mixed together, and, being supersaturated with nitric acid, afforded the same white insoluble precipitate; the total quantity of which, obtained from 200 grains of the ore, amounted to about 155 grains.

The liquor from which this precipitate had been separated and the fluid by nitric acid, was then saturated with ammonia, and, being gave a little more iron. boiled, afforded about two grains of oxide of iron.

I obtained, therefore, from 200 grains of the ore,

	Grains.	Grains.
Oxide of iron	42	} = 197.
And of the white precipitated substance	155	

But, as I could not repeat the analysis without destroying the remaining part of the only specimen at present known of this ore, I do not wish the above stated proportions to be regarded as rigidly exact; it will be sufficient, therefore, to say at present, that the ore is composed of about three parts of the white matter, and rather less than one of iron.

§ II. PROPERTIES OF THE WHITE PRECIPITATE.

A.

It is of a pure white, and is not extremely heavy.

It has scarcely any perceptible flavour, nor does it appear to be soluble in boiling water; when, however, some of the powder is placed upon litmus paper moistened with distilled water, the paper in a few minutes evidently becomes red.

The white precipitate

B.

1. When examined by the blow-pipe, it is not fusible *per se* in a spoon of platina, nor upon charcoal, but only becomes of a less brilliant white.

2. Borax does not appear to act upon it; for the white particles are only dispersed throughout the globule.

not affected by borax.

3. It produces an effervescence when fused with carbonate of soda, and forms a colourless salt; but, if too much of it be added, then the mass, when cold, appears like a white opaque enamel.

Soluble by fusion with soda,

4. When carbonate of potash is employed, the effects are similar in every respect to those of soda; and it may here be remarked, that the saline combinations thus formed with soda, or potash, are soluble in water; and that these solutions have the same properties as that which was formed when the ore was decomposed by an alkali. The portion of the white precipitate which may be in excess, subsides unaltered, when the globules are dissolved in water.

and with potash, as in the first decomposition.

Phosphate of ammonia forms a deep blue globule.

5. Phosphate of ammonia produces a very marked effect; for, when melted in a platina spoon, if some of the white substance be added, a considerable effervescence takes place, and the two substances rapidly unite. The globule, when cold, is deep blue, with a tinge of purple, but, when held between the eye and the light, it appears of a greenish gray colour.

C.

It resists nitric acid:

It is perfectly insoluble, and remains unchanged in colour, and in every other respect, when digested in boiling concentrated nitric acid.

D.

is soluble in sulphuric acid; which by dilution lets fall a sulphate.

It is dissolved by boiling sulphuric acid, and forms a transparent colourless solution, which is however only permanent while the acid remains in a concentrated state; for, if a large quantity of water be added to the solution, or if the latter be poured into a vessel of distilled water, the whole in a few minutes assumes a milky appearance, and a white precipitate is gradually deposited, which cracks as it becomes dry upon the filter, and, from white, changes to a lavender blue colour, and again, when completely dry, to a brownish gray. It is then insoluble in water, has not any flavour, is semi-transparent, and breaks with a glossy vitreous fracture,

This substance is much heavier than the original white precipitate; and in a very slight degree may be dissolved by boiling muriatic acid, or by boiling lixivium of potash.

Upon examining these solutions, I found that both contained the original white substance, together with some sulphuric acid; so that the precipitate obtained from the sulphuric solution by the addition of water, is a sulphate of the white matter*.

The whole is not however precipitated by water; for a part remains in solution, which may be separated from the sulphuric acid by either of the fixed alkalis, or by ammonia.

The sulphuric solution afforded an olive precipitate by prussiate of potash;

The sulphuric solution is not rendered turbid by the addition of water, until some minutes at least have elapsed; when,

* This sulphate is also precipitated when the sulphuric solution has been long exposed in an open vessel to the air; and, according as this may be moist or dry, the effect is produced sooner or later.

therefore,

therefore, some prussiate of potash was added immediately after the water, the colour of the liquor became olive green, and a copious precipitate, of a beautiful olive colour, was gradually deposited.

Tincture of galls, after a few minutes, caused the liquor to become turbid, and a very high orange-coloured precipitate was obtained. orange by tinct. galls,

A few drops of phosphoric acid were added to a part of the concentrated sulphuric solution; and, after about 12 hours, the whole became a white opaque stiff jelly, which was insoluble in water. jelly by phosphoric acid,

Potash, soda, and ammonia, whether pure or in the state of carbonates, separate the substance in question from the sulphuric solution, in the form of a white flocculent precipitate; and, when these alkalis are added to a considerable excess, they do not redissolve the precipitate, unless they are heated; then, indeed, the fixed alkalis act upon it, and form combinations which have already been mentioned, but which we shall soon have occasion more particularly to notice. white flakes by alkali, alkaline combinations.

E.

1. The white precipitate, when recently separated from potash, is soluble in boiling muriatic acid; and this solution may be considerably diluted with water, without any change being produced. Muriatic solution of the white precip.

2. A part was evaporated to dryness, and left a pale yellow substance, which was not soluble in water, and was dissolved with great difficulty, when it was again digested with muriatic acid. Evap. left a yellow residue.

3. Prussiate of potash changed the colour of the muriatic solution to an olive-green; the liquor then gradually became turbid, and an olive-coloured precipitate was obtained, similar to that which has been lately mentioned. But, olive precip. by prussiate.

4. If some nitric acid was previously added to the muriatic solution, then the prussiate changed the liquor to a grass-green, but did not produce any precipitate. but none if nitric acid first added.

5. Tincture of galls, in a few minutes, formed an orange-coloured precipitate, like that which has been mentioned; but, if the acid was in too great an excess, it was necessary to add a small quantity of lixivium of potash or soda, before the precipitate could be obtained. Orange pr. by galls.

White flakes by
phos. acid,

6. A small quantity of phosphoric acid, being added to the muriatic solution, in a few hours formed a white flocculent precipitate.

and by alkalis.

7. Potash, soda, and ammonia, also produced white flocculent precipitates, which were not redissolved by an excess of the alkalis, unless the liquors were heated; and, in that case, part was dissolved by the fixed alkalis, but not by ammonia.

Muriates of
lime, magnesia,
and strontian,
gave no precip.

8. The muriatic solution did not yield any precipitate, when the muriates of lime, magnesia, and strontian, were added; but muriate of barytes formed a slight cloud.

Zinc threw
down white
flakes.

9. When a piece of zinc was immersed in the muriatic solution, a white flocculent precipitate was obtained*.

F.

Acetous acid
does not dissolve
the white mat-
ter.

The acetous acid has not any apparent effect on the white precipitate, when long digested with it.

G.

Fixed alkalis
readily combine
with it.

The fixed alkalis readily combine with this substance, both in the dry and in the humid way.

We have already seen, that the former method was employed with success in the analysis of the ore; and the experiments made with the blow-pipe may be regarded as an additional confirmation. In each of these cases, the white precipitate combined with the alkali, as soon as the heat was sufficient to cause the latter to flow; and, when a carbonate was employed, a portion of carbonic acid was expelled.

The carbonic acid was in like manner disengaged, when the white precipitate was boiled with lixivium of carbonate of potash, or of soda; and the solutions thus prepared, resembled in every respect those which were formed by dissolving in water the salts which had been produced in the dry way.

It will be proper here to give a more particular account of these combinations.

Humid solution
in potash: a
portion left,

1. Some of the white precipitate was digested, during nearly one hour, with boiling lixivium of pure or caustic potash:

* This appears to indicate the obstinacy with which this substance retains a certain portion of oxygen; for we here see that zinc does not precipitate it in the metallic state, but only reduces it to an insoluble oxide.

about

about one-fourth of the powder was dissolved; and the remainder, which appeared little if at all altered, subsided to the bottom of the vessel.

The clear solution, which contained a great excess of alkali, affords a scaly glittering salt; was decanted; and, by gentle evaporation, yielded a white glittering salt, in scales, very much resembling the concrete boracic acid,

The salt was placed upon a filter, so that the lixivium might be separated. It was then washed with a small quantity of cold distilled water; and, being dried, remained as above described, although constantly exposed to the open air.

This salt had an acrid disagreeable flavour, and contained a small excess of alkali. It did not dissolve very readily in cold water; but, when dissolved, the solution was perfect and permanent.

Some nitric acid was added to part of the solution, and immediately rendered it white and turbid. In a short time, a white precipitate was collected, similar to that which had been employed to neutralise the potash: and the clear supernatant liquor, being evaporated, only afforded nitre.

Prussiate of potash was added to another portion; but did not produce any effect, until some muriatic acid was dropped into the liquor, which then immediately assumed a tinge of olive green, and slowly deposited a precipitate of the same colour.

Tincture of galls did not affect the solution at first; but, when a few drops of muriatic acid had been added, it gradually lost its transparency, and yielded an orange-coloured precipitate.

2. As so large a part of the white precipitate had remained undissolved in the foregoing experiment, it was digested again with another portion of the same lixivium, but without any effect. I therefore washed off the alkali, and boiled some nitric acid with the powder, until the acid was completely evaporated. After this, the powder was exposed to a strong heat in a sand-bath. It was then again digested with the lixivium, and a part was dissolved as before; but still the residuum required to be treated with nitric acid, before the alkaline liquor could again act upon it; so that it was necessary to repeat these alternate operations several times, before the whole of the powder could be united with the alkali.

3. When

Carbonate of soda, or potash, acted very nearly as pure potash.

Precip. by the tungstate and molybdate of potash, and cobaltate of ammonia. Hydro-sulph. am. gave redish precip. The white matter dissolved in alkali, and iron dissolved in alkali, gave, by mixture, a precipitate of the original ore.

3. When the white precipitate was digested with solution of carbonate of potash, or of soda, it was dissolved, much in the same manner as above related; and the properties of the solutions, when examined by reagents, were also similar, excepting that the orange-coloured precipitates produced by tincture of galls were of a paler colour.

Tungstate of potash, molybdate of potash, and cobaltate of ammonia, being severally added to the solution of the white substance in potash, produced white flocculent precipitates.

Hydro-sulphuret of ammonia produced a reddish chocolate-coloured precipitate.

4. As the ore was decomposed by being fused with potash, the following experiment affords a curious instance (among the many already known) of the change in the order of affinities produced by a difference of temperature.

Some of the solution of the white precipitate in potash, was poured into the alkaline solution of iron, which was formerly known by the name of Stahl's *Tinctura Alkalina Martis*. Potash was in excess in both of these solutions; but nevertheless a cloud was immediately produced, and a brown ferruginous precipitate was deposited.

Part of this precipitate was dissolved in muriatic acid; and the solution, being examined in the usual way, yielded a blue precipitate when prussiate of potash was added, and a purplish brown precipitate with tincture of galls.

The other part of the precipitate was digested with dilute nitric acid; which dissolved the ferruginous part, but left untouched a white flocculent matter, perfectly resembling the substance which has been so often mentioned. The precipitate therefore produced by the mixture of the two alkaline solutions, was a combination of the white matter with oxide of iron, very similar to the original ore.

(To be continued.)

XVI.

Remarks on the Mamoth. By LOUIS VALENTINE, Physician in Chief of the Army and Hospitals of America, of several National and Foreign Societies, resident in Nancy.

Bones of a large extinct animal found in America.

SINCE settlements have increased in North America, a great quantity of bones have been found belonging to some extinct animal, which seems to have resembled an elephant, but

was much larger. By digging near the lakes of Canada, where the animal is called by the savages, the Father of Oxen; near the rivers which fall into the Ohio; towards the rivers Miami, Muskingum, in the state of Kentucky, and of Tennessee, &c. &c. but principally near the salt springs, pieces of skeletons and tusks have been found, of an astonishing length and weight.

We have seen a femur and a tibia, which, when united, must have been five feet and a half high; another femur, which was alone five feet long, and thirty-six inches in circumference in its middle or cylindrical part; ivory tusks resembling those of an elephant, which were near seven feet long, and one foot six or eight inches in circumference at the base. It was not till the year 1800 that a complete skeleton of these fossil bones could be procured. Two physicians of Philadelphia, Doctors Barton and Wistar, had in their possession the lower jaw almost entire, with two teeth on either side, in particular, that of the former has five and three points, all quite double; but no one had the entire head.

The state of New York (in the environs of the beautiful river Hudson) has of late years been the theatre of discoveries of fossil bones, a greater quantity of them having been found there than any where else. In 1800, by digging in the low and marshy places of the counties of Orange and Ulster, at three, four, and five feet deep, parts, which had never been before discovered, were found. Some bones, ten feet deep in the earth, were as found and entire as those which had been met with nearer the surface. Some, however, were found broken, particularly those of the head.

In another place, eight miles from the city of New York, an upper jaw was found perforated to receive a tusk like that of an elephant; the connection of the tusks was by *gomphosis*; the tusks were evidently of ivory; the openings for the nostrils were eight inches in diameter; and notwithstanding that the bones of the feet afford reason to conclude that the animal had claws, it is scarcely possible to avoid thinking, from the structure of the head, that it was a species of elephant. Some hair has even been found, three inches in length and of a dark colour, which is said to have belonged to this monstrous quadruped. Doctor Graham, a Senator, in a letter to Doctor Mitchell, Representative in Congress, and Professor of chemistry and

Enormous magnitude by ad-measurement.

Where most plentifully found.

Particular fact

Conjectures as to the species of animal.

Question as to
final causes.

and natural history at the college of Columbia, says on this occasion, that it is difficult to resolve the question; "Why Providence should have destroyed this species, which it was pleased to create?" Yet if it was voracious, it is happy for the human species that it has by any means become extinct.

The animal has
never been seen
within human
memory or tra-
dition.

We may reasonably doubt the assertion of those authors who affirm, that certain savages, Russians and Greenlanders, have seen this animal living, and that it still exists in the north. All that has been transmitted by tradition to the oldest Indians who have communication with the United States, and from whom information has been sought concerning this object, is quite fabulous, and does not offer even a shadow of probability. Mr. Jefferson, now President of the United States, formerly paid great attention to this subject. (See his Account of Virginia.)

Conjectural re-
marks.

It is certain that in Siberia and in Greenland, where similar fossil bones have been found, no one has brought proof of having seen this animal living. We cannot suppose it to be cetaceous or amphibious, of the nature of the hippopotamus, as there are sufficient reasons to prove the contrary. Mr. Cuvier, whose researches into the extinct species of animals are no less curious than scientific, distinguishes the animal of Siberia, which affords fossil ivory, from the mamoth, which latter differs principally from the former on account of its magnitude, and the points of its grinders, &c.

An entire skele-
ton,

Since the time of our having received details on this subject (see the New York Medical Repository, Vol. IV.) and what we have related elsewhere (see the third edition of Guthrie's Geography, in French, Vol. VI. page 225 and 262, published by Langlois at Paris), an account has been published in the American papers in 1801, that Mr. William Peal, proprietor of the museum at Philadelphia, having collected the bones found in the county of Orange, in the state of New York, had succeeded in forming a skeleton, the height of which is twelve feet; the head being four feet and a half in length, and the tusks ten feet; the other parts being in the same proportion.

twelve feet high,
and its head four
feet and a half
long.

Local situation
of these enor-
mous bones.

Almost all these bones have been found in calcareous earth. The bones of the megalonix, or great claw, of which Mr. Jefferson has given a description, were found in caverns of lime stone and chalk in Tennessee. The other enormous bones of the megatherium, found in such great quantities in the county of Ulster, of which Sylvanus Miller, Esq. has given some
detail

detail in a letter to Professor Mitchill, are found in strata of marl, which are dug to procure this calcareous substance as a manure for land. It is, nevertheless, sometimes remarked, that these bones begin to fall in small portions when, after having been from their calcareous inclosure, they are exposed to the atmosphere. Teeth, which were found entire when extracted from the earth, become black in a short time, crack, lose their enamel, and fall into small scales. Without such a preservative we may presume, with our friend Mitchill, that the remains of these animals would have been decomposed many centuries ago.

They sometimes perish by exposure to the air.

SCIENTIFIC NEWS, &c.

Prizes of the National Institute of France.

THE class of mathematical and physical sciences having proposed in the year 8, as the subject of a prize to be awarded at the public sitting of the 15 Germinal, in the year 10, the following question: *What are the characters which distinguish in vegetable and animal matters, those which serve as ferments, from such other bodies as they put into a state of fermentation?* And the memoirs which have been received not answering the conditions of the program, the class proposes the same subject again for the year 12. The prize is of the value of one kilogramme (about one hundred and twenty-five pounds sterling). It will be given at the public meeting of the 15 Germinal, in the year 12. Memoirs must be sent before the 1st Nivose of the same year.

Prize medal for a memoir on ferments and fermentable bodies

Astronomical Prize.—Citizen Lalande has presented the National Institute of France with the sum of ten thousand francs (about 400 guineas) to found an annual prize, to be given by the Institute to the author of such discovery, observation, or work in astronomy, as shall be thought the most remarkable or useful, during the course of the year. The Institute very highly applauded this act of generosity in one of its members, and decreed, that thanks should be assigned in their registers, and commissaries nominated by each of the three classes, to present at the next general meeting the means of execution.

Astronomical prize.

Communications to the Royal Society respecting the Planet Ceres.

Dr. Herschel sent an account of the appearance of the new planet, as viewed through his telescopes. He had sought for it

Extreme smallness of the planet Ceres.

it

it in vain, until he received Dr. Maskelyne's determination of its place. When viewed with powers of 600 and 1200, it could not be decidedly distinguished from a star, until it was found to change its place. Its apparent diameter was not large enough to be directly determined, but it was certainly not larger than one-fourth of that of the Georgian planet, and perhaps equal only to one-sixth. From a rough computation of its magnitude, Dr. Herschel concludes that its real diameter is about $\frac{2}{3}$ of that of the moon: its light is of a reddish hue.

Mr. Gilpin also gave the Society an account of observations on the 8th and 12th of February. He found the planet's right ascension change from $188^{\circ} 41'$ to $188^{\circ} 30'$, while its declination increased. Mr. Gilpin observes that its light resembles that of the planet Mars.

Nebulous atmosphere.

Thursday, 25th February. A letter from Mr. Schroeter of Lilienthal, respecting the planet Ceres Ferdinandia, informed the Society that Mr. Schroeter had observed a nebulosity round the planet, somewhat resembling that of a comet: the diameter of the true disc being $1.8''$, and that of the nebula $2.6''$, but the distinction was not always equally observable. Mr. Schroeter considers this body as of a hybrid nature, or a medium between a planet and a comet; but he imagines the apparent nebulosity to be owing to an atmosphere, and that, according to the different states of this atmosphere, the light reflected from the planet is either white, bluish, or reddish.

A table of observations of the same planet was also communicated by Mr. Mechain, through Sir Henry Englefield.—
(*This article is taken from the Journal of the Royal Institution.*)

Leonardo da Vinci.

The lovers of the polite arts will be pleased to learn, a new translation of Leonardo da Vinci's treatise on painting (for which Pouffin made the figures) will soon be ready for publication. This work has been long in the hands of Mr. Rigaud, R. A. who has paid it particular attention and care; and has given new importance and energy to the work, by arranging the chapters successively under proper heads; by which the student will be much facilitated in understanding the precepts of this great master in the art of painting.

The reader will recollect some particulars of the extraordinary researches and very superior genius of Da Vinci, by the extracts given in the former series of this Journal, from an abridgement of his writings, by J. B. Venturi, quarto, II. 84.

Experiments

Experiments to prove that all Bodies, whatever may be their Nature, are obedient to the Action of Magnetism, and that this Action is sufficiently powerful to admit of being measured. By Citizen COULOMB.*

It has long ago been remarked, that platina, nickel, and several other bodies, acquire a sensible degree of magnetism; but some philosophers attribute this property only to a portion of iron not easy to be separated, and conclude, that by obtaining a greater degree of purity, we might succeed in rendering them perfectly indifferent to the action of the magnetic bar.

The new experiments which Citizen Coulomb has made and repeated before the Institute, lead us on the contrary to think, that the action of magnetism extends through all nature; for none of the bodies he has yet tried was found to resist this power.

But however real this action may be, it is not alike in all bodies, and in most of them it must be necessarily very small, to have escaped the attention of philosophers to this time. In order, therefore, to exhibit and to measure these results, we must begin by placing the bodies in a situation which shall allow them to yield to the weakest action.

For this purpose, Citizen Coulomb fashioned his subjects into the form of a cylinder or small bar, and in this state he suspended them to a silken thread, such as is drawn from the silk worms' cone, and in this state he placed them between the opposite poles of two magnetic bars of steel. The single thread of silk could hardly bear the weight of a quarter of an ounce without breaking, consequently it became necessary to form small bars very light and thin. Citizen Coulomb made them about seven or eight millemetres in length (or less than half an inch), with three quarters of a millimetre (or about an hundredth part of an inch) in thickness, and he gave the metals about one-third of this thickness.

In his experiments he placed the steel bars in the same right line, their opposite poles being five or six millemetres farther asunder than the length of the needle intended to oscillate between them. The result of the experiment shewed, that ever might be the substance of the needle, it always disposed

* Communicated to the French National Institute, and inserted in the *Magazin Encyclopedique*, No. 22, an 10.

itself according to the direction of the two bars; and that if they were turned from this direction, they always recovered it, after oscillations of which the number was often more than thirty per minute. It was therefore easy in every case to determine, from the weight and figure of the needle, the force which had produced the oscillation.

Subjects of experiment.

These experiments were successively made with small needles of gold, silver, copper, lead tin, small cylinders of glass, a piece of chalk, a fragment of bone, and different kinds of wood.

Extreme delicacy of the suspension.

Citizen Coulomb has proved, in a former memoir, that the force of torsion of the silk thread is so slight, that in order to draw it round through the entire circle, it would require a force scarcely equal to the one hundred thousandth part of a grain (or about one seven hundredth of a grain). A quantity so minute cannot therefore sensibly derange the measure of magnetic force in the different bodies, and its effect even, if it were admitted to be of perceptible magnitude, may also be urged in proof of the general conclusion of Citizen Coulomb, because the magnetic power must overcome this resistance of the thread in order to manifest itself. Our author gives, in the third vol. of the Memoirs of Natural Philosophy and Mathematics of the National Institute, a very simple formula to determine the magnetic force of a body from the time of its oscillations, and he means to shew in another memoir, the method of determining this result in different bodies of the same figure placed between the poles of two bars. He thinks it now proved, that all the elements which enter into the composition of our globe, are subjected to the magnetic power, and that the whole mass collectively forms one single magnet.

Observations.

In favour of those who might be desirous of repeating his experiments, and rendering them very sensible, the author remarks, that the method of succeeding consists in diminishing the size of the oscillating bodies. From some essays, of which the results terminate this memoir, it seems to follow, that the accelerating forces are inversely as the masses are very nearly in the direct proportion of the surfaces; but Citizen Coulomb gives this rule only as a first deduction, which requires to be confirmed.

ERRATA.

In Mr. Cruickshank's paper, p. 43, end of the first paragraph, for *faults* read *facts*; and p. 46, l. 26, for *computed* read *compelled*.

A
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OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JULY, 1802.

ARTICLE I.

Composition of Writing Ink, possessing the permanent Colour, and other essential Properties, of the Ink used for Printing. In Letter from Mr. WILLIAM CLOSE.

To Mr. NICHOLSON.

S I R,

Dalton, May 25, 1802.

IN the second volume of the Philosophical Journal, at p. 63, the preparation of indelible ink is announced. Such an article may justly be considered a desideratum. As I have for some time directed my attention to the composition of permanent ink, I send you a short memoir on the subject, which was drawn up previous to the perusal of the above-mentioned notice, and intended for a future communication.

THE welfare of individuals often depends very much on the testimony of writing. An atramentous composition, possessing a permanent colour on paper, and such a degree of insolubility, after desiccation, as not to suffer any injury from exposure to humidity, or the application of such chemical preparations

Permanent ink an important article for the preservation of authentic testimony in writing.

parations as totally eradicate the traces of common ink, would certainly be an article of considerable value in the material composition of those writings, or legal instruments, which are intended to evince the transactions of the day, and to ratify the affairs of the future.

The invention of printing probably produced some remissness about the durability of ink.

The ancients were probably more interested in the composition of permanently coloured ink than the moderns. When every book was in manuscript, it would frequently be requisite to transcribe whole volumes, sometimes merely for the purpose of procuring a copy, at other times for the preservation of the work itself; in either case, before the writer begun a process so tedious, it would be of consequence to select such materials as were the most durable, in order to protract the like necessity, and also for the purpose of enhancing the value of his labour. But since the invention of printing, the necessity of transcribing has been entirely superseded; the manuscripts of modern authors after publication, have been reduced to little more value than objects of curiosity; and writers have been contented with such materials as are the cheapest and easiest to procure.

Ink with vegetable infusions and sulphate of iron,

not durable.

The colour may be restored while the sulphate of iron remains on the paper.

The most serious objection to its use is, that it may be totally discharged.

Means of preventing this by the addition of pigments.

For a great number of years, the ink most generally used by European writers has been the infusion of galls, and other astringent vegetables containing gallic acid, rendered black by sulphate of iron, and thickened by the addition of a little gum or sugar. This composition is well adapted to the common purposes of writing and the dispatch of business, but its colour is liable to fade: a composition of a more permanent colour is therefore desirable, for writings which are required to retain their primitive signatures, and such as cannot be printed. Indeed, since the discovery of the method of totally discharging the traces of common ink by the application of the oxygenated muriatic acid, more serious consequences are to be apprehended from the universal use of the common atramentous fluid, than the decay of its colour from age; for it is well known, that while the sulphate of iron remains on the paper, the colour of the writing may be restored, by washing the manuscript with fresh infusion of galls.

In the quarto series of the Philosophical Journal (Vol. IV. p. 479), there are several useful receipts for composing ink capable of resisting the oxygenated muriatic acid. The most material difference, however, of the compositions there re-

commended from those in common use, consists in the addition of pigments producing an unchangeable colour upon paper.

Ink of a permanent colour may be easily obtained, by suspending various pigments in an aqueous fluid, by the intervention of gum, without the assistance of the common ingredients; but such compositions are liable to one of the greatest inconveniences, for the whole of the writing may be detached from the paper by washing the manuscript with water. Such ink, however, was frequently used by the ancients.

Ancient method of composing ink.

As a permanent colour is certainly a valuable requisite, it appears very promising in speculation for the improvement of this indispensable article, according to the simplest, and perhaps the most ancient method of composition, to substitute, in place of the common mucilaginous fluid, as a compound vehicle for the distribution and protection of the colouring matter, the solution of some gum, or resinous substance, which can be dissolved in only a few liquids. After the dissipation of the thinner part of an atramentous compound properly formed with such a solution, the colouring substance will be left on the paper, combined with a sufficient quantity of tenacious matter to protect it from being injured by friction, or from being discharged by the application of any fluid to which the writing may be exposed, without injuring the paper.

Speculation for the improvement of this kind of ink.

Many of the more volatile kinds of oils may be used in writing, if reduced to a proper consistence by the addition of gum or resin. Tolerable ink may be made by dissolving 30 grains of common resin in 90 grains of oil of turpentine, and tempering the solution with $17\frac{1}{2}$ grains of lamp black, and $2\frac{1}{2}$ of indigo. In a dry state, this composition resists the action of water, but not of spirit. Such, indeed, will be the case with every composition in which the colour is merely suspended in the fluid, and attached to the paper, by a substance of easy solubility; the application of the article which produced the fluidity of the ink, will again penetrate and soften the dry compound. Those compositions which contain a tenacious matter, soluble in a few articles only, and at an high temperature, will be the least exceptionable.

Ink with oil and resin.

Inconvenience to which every composition will be subject where the colour is mechanically suspended.

A more insoluble kind of matter than resin should be used. Copal will dissolve in only a few liquids; it appears well adapted to the purpose of retaining a permanent colour upon paper.

Copal is much superior to resin for the suspension of the pigment;

the paper, if a vehicle can be found which will dissolve a sufficient quantity, and which, when the colouring matter is also added, shall be fluent enough in writing.

but is not altogether free from inconvenience.

Oil of lavender will dissolve copal. I have made some experiments with these articles, and the combination succeeds so well, that I have not been inclined to use any other. The only inconvenience to be apprehended from the use of copal in the composition of ink, is, from its being soluble at a low temperature; but whether any other kind of tenacious matter of more difficult solubility, can be used conveniently in writing, I must leave to future discussion. The solution of a substance which could only be dissolved in an high temperature, would certainly be the most proper vehicle to prevent alteration; for, after the desiccation of the writing, it would be difficult to soften the ink in one part of a manuscript, without exposing the whole to a very injurious process.

Composition of ink with oil of lavender, copal, and lamp black.

Ink may be composed of oil of lavender, copal, and lamp black, according to the following proportions of the ingredients:

Take of oil of lavender, 200 grs. copal in powder, 25 grs. lamp black from $2\frac{1}{2}$ to 3 grs. With the assistance of a gentle heat, dissolve the copal in the oil of lavender in a small glass phial, and then mix the lamp black with the solution upon a marble slab, or other smooth surface. Put the composition into the bottle, and keep it excluded from the air. After a repose of some hours, the ink must be well shaken and stirred with a piece of wire before it is used; if it be too thick, it must be diluted with a little oil of lavender, oil of turpentine, or alcohol. The facility of writing with this composition depends much on the quantity of the colouring matter. Three grains of lamp black to two-hundred and twenty-five of the solution of copal, producing ink of a full body of colour, and is nearly as much as the copal can protect from injury after the dissipation of the oil of lavender. Two grains and an half of lamp black, and half a grain of indigo, produces ink of a paler colour, but which may be distributed upon the paper with the facility of common ink. A piece of sponge*, or other organized

* The custom of putting cotton in an inkstand has been disused, more particularly, I think, since an inkstand, invented by the late worthy and ingenious Samuel More, of the Society of Arts, was made

ganized substance, must be used in the inkstand, chiefly for the purpose of cleaning the pen.

Red ink may be made by tempering the solution of copal with red sulphuret of mercury instead of lamp black. The following proportions of the ingredients produce a red ink which writes very well: Red ink with red sulphuret of mercury.

Take of oil of lavender 120 grs. copal in powder, 17 grs. red sulphuret of mercury, 60 grs. Dissolve the copal in the oil of lavender, and then mix the sulphuret with the solution upon a smooth surface. The recipe.

Both these compositions possess a permanent colour, and other essential properties of the ink used in printing. The oil of lavender being dissipated with a gentle heat, the colour is left on the paper surrounded with copal, a substance insoluble in water, in spirits, in acids, or alkaline solutions. A manuscript, written with these compositions, may therefore be exposed to the process commonly used for restoring the colour of printed books, without the smallest injury to the writing; and in this manner all interpolations with common ink may be removed. A manuscript written with these compositions will not be injured by the process of bleaching.

As

made and brought into fashion about thirty years ago, by the celebrated Wedgwood. These changes appear to be both injurious. For the ink in the cotton is kept blacker by the suspension of the atramentous part; and if no more ink be present than perfectly to fill the cotton, the pen will always receive a fluid black ink, and may be charged at pleasure by a greater or less gentle pressure at the time of taking up, or discharged by lodging the point for a moment upon the cotton. It is also very easy to regulate the oxygenation by the air, so as to increase the blackness without suffering mouldiness to come on, by the simple expedient of turning the cotton upside down every day. As the fibres of the cotton prevent the fluid from circulating as usual by the change of temperature produced from evaporation, the interior mass may be considered as in a closed vessel while not in use.

Mr. More's fountain inkstand, at present so universally in use, is certainly very inconvenient. The ink, it is true, is kept *in a closed vessel*; but its colouring matter is at full liberty to subside, and the consumer is obliged to fill his pen from the muddy bottom instead of the surface; and what is still worse, the conical vessel into which the ink flows, is subject to all the evils of evaporation and mouldiness, so as most frequently to afford an adhesive and clogging fluid to the pen. W. N.

Source of apprehension. The oil of lavender will soften the dry composition, but the writing cannot be easily eradicated.

Amber a proper substance if it could be used.

As copal is soluble in several of the essential oils, it may be expected that the application of these will do much injury to the writing. Such an expectation is well founded. If the writing be rubbed with a smooth surface dipped in oil of lavender, much of the colouring matter will be disengaged, and distributed more diffusely upon the paper; but, as some of it will have penetrated the interior parts of the paper along with the copal, it will be extremely difficult to obliterate the traces of the pen without the erasement being perceptible. This is the principal source of apprehension, but I know of no method of obviating the danger. The perusal of a memoir on the nature and preparation of drying oils, &c. by Mr. Sheldrake (*Philosophical Journal*, 8vo. Vol. I. p. 259), suggests amber as a proper article for the composition of ink, if enough of it can be dissolved in any fluid sufficiently thin for the purpose of writing. The more difficulty there is in effecting the solution of any tenacious substance we use, the better, provided it can then be employed without inconvenience.

I am, Sir,

Yours respectfully,

WILLIAM CLOSE.

II.

Experiments and Observations on the Vegetation of Plants, which shew that the common Opinion of the Amelioration of the Atmosphere, by Vegetation in Solar Light, is ill founded. By JAMES WOODHOUSE, M. D. Professor of Chemistry in the University of Pennsylvania, &c.

To Mr. NICHOLSON.

S I R,

Pater Noster Row, May 27, 1802.

Introductory letter.

I INCLOSE for the *Philosophical Journal*, the results of various experiments, made in Philadelphia in the year 1801, upon the seeds, leaves, &c. of a variety of plants, which seem to prove, that growing vegetables, contrary to an opinion almost universally adopted, do not purify atmospheric air; and that
whenever

whenever they appear to afford oxygenous gas, it is by devouring the coal of carbonic acid gas for food, and leaving its oxygen in the form of pure air.

I have the honour to be,

Dear Sir,

With the greatest respect,

Your most obedient,

And very humble servant,

JAMES WOODHOUSE.

First. *Of the Effects produced by the Germination of Seeds in atmospheric Air.*

On the 3d of June, twelve seeds of zea maiz were planted in earth, and confined over water in a glass vessel, in seventy ounce measures of atmospheric air of the purity of 100, and often exposed to the light of the sun. On the 12th, the corn had vegetated, and was from two to five inches high. The air being examined at this time, by throwing up one measure of it, over lime water, in an eudiometer, gave $\frac{3}{100}$ parts of carbonic acid air. Another measure, after being freed from the fixed air, and mixed with an equal measure of nitrous air, produced an absorption of $\frac{3}{100}$. On the 19th, the corn having grown considerably, and the air being tried again, no carbonic acid gas appeared, and the purity was the same as at first. On the 23d, the plants died, and the airs were found to consist of $\frac{5}{100}$ fixed, and $\frac{95}{100}$ azotic gas.

Germination of seeds in atmospheric air.

Similar experiments were made with the seeds of apium petroselinum, lactuca sativa, cucurbita citrullus, phaseolus sativus, fisybrium, and raphanus sativus, and with the same result.

The atmospheric air, in these experiments, appears to be reduced in purity, by its oxygen uniting to the coal of the cotyledons of the seed, or to that of some animal or vegetable matter contained in the earth in which the seeds are planted, or to that of some decayed portion of the living leaves.

The air lost oxygen by uniting to carbon; afterwards it became more pure; and lastly its oxygen was totally absorbed.

Ingenhouz, Humbold, and Thomson, have observed, that soils have the property of absorbing oxygen; but as it cannot be proved that any pure earth, or mixture of earths, render atmospheric air impure, it is certainly more philosophical to ascribe the impurity of the air to the formation of the carbonic acid, the base of which generally exists in all soils.

The effect of veg. mould on air most probably from its carbon.

II. *Of the Effects produced by the Growth of Plants in atmospherical Air.*

Growth of plants in atmoſ. air.

On the 27th of May, twelve plants of perſicaria polygonum, two inches high, growing in earth, were confined in a glaſs veſſel in fifty-two ounce meaſures of atmospherical air, of the purity of 100, and often expoſed to the influence of ſolar light. On the 4th of June, they had increaſed about two inches in height. The air being examined at this time, was found to contain $\frac{1}{100}$ parts of carbonic acid gas, and to be reduced in purity to 80. Several young plants of raphanus fativus, lactuca ſtramonium, phytolacca decandra, zea maiz, phaſeolus fativus, ſidum telephium, amaranthus hyboidus, cucurbita citrullus, ſtrymbrium, and lactuca fativa, were alſo ſeparately confined in from forty to eighty ounce meaſures of atmospherical air, which was examined at various times, from one hour to thirty days, after the plants had been placed in it. Carbonic acid gas was generally formed, and whenever this circumſtance happened, the purity of the air was diminished.

They produced carbonic acid gas, and diminished the purity of the air.

Growth of plants in oxigen diminished its purity by carbonic acid.

Many of the ſame kind of vegetables were alſo confined in forty ounce meaſures of oxigenous gas, which had been well waſhed in lime water, and the purity of the air was very generally leſſened, fixed air being generated. They turned of a white or yellow colour, and ſoon died, after being placed in atmospherical air.

In confined plants the decaying parts afford carbon and form acid, which the living plant decompoſes.

The ſame effects are produced by the growth of plants as by the germination of ſeeds in common air, and by the ſame cauſes. If the leaves are confined a conſiderable time, part of them decay, and the coal of the dead portion, uniting with the oxigen of the atmospherical air, generates carbonic acid. This acid is decompoſed by the living leaf. Its coal is abſtracted, while its oxigen is left in the form of pure air.

But when the formation is quicker than the decomposition, the plant dies.

When the oxigen unites to the coal of the animal or vegetable matter of the ſoil in which the plants vegetate, or to the coal of the decayed parts of the leaves, and makes fixed air quicker than the living parts can decompoſe it, the plants will ſpeedily die.

When the ſoil contains but little of organized remains, the included plant will live much longer.

When a plant in perfect health, growing in a ſoil which contains little vegetable or animal matter, is confined in atmospherical air, it will live a long time, without producing any change in it. Many of the vegetables which were the ſubjects of theſe experiments,

experiments, did not affect the air in five days: some diminished its purity in three hours; and others altered it in a most slow and gradual manner, causing little change in it in twenty days.

III. *Of the Effects produced by the Leaves of Plants in atmospheric Air impregnated with Carbonic Acid Gas, and exposed to the Light of the Sun.*

Leaves exposed to solar light in a mixture of atmospheric and carbonic acid gas.

A handfull of the leaves of *mimosa virgata*, *euphorbia picta*, *digitalis purpurea*, *franklinia altamaha*, *asparagus officinalis*, *coryllus avellana*, *rhus glabrum*, *aristotichia siphoe*, and *periploca græca*, were separately exposed seven hours to the light of the sun, in thirty-six ounce measures of atmospheric air, impregnated with four ounce measures of carbonic acid gas, from the carbonate of lime and sulphuric acid. The fixed air disappeared, and the atmospheric air was so much increased in purity, as to devour two measures of nitrous air.

The carbonic acid disappeared, and the proportion of oxygen in the mixture was augmented.

The leaves of these plants, kept over night in the same air, gave carbonic acid gas in the morning; and its purity, in every instance, was considerably diminished.

In the dark the leaves produced carbonic acid gas.

The leaves of *mimosa virgata* and *amygdalus persica*, were also separately exposed nine hours to the influence of solar light, in forty ounce measures of atmospheric air, in which fixed air had been formed by leaving a fungus to putrefy it. The carbonic acid gas disappeared, and the purity of the atmospheric air was increased from 50 to 80.

Other leaves exposed to light, with the former result.

IV. *The following Tables will shew the Quantity and Purity of oxygenous Gas, obtained by exposing a small Handful of the Leaves of Plants to the Light of the Sun, in forty Ounce Measures of Pump Water.*

Table of experiments on leaves exposed to solar light under pump water.

This water was taken from a well sunk within a few yards of a necessary, from which it was impregnated with carbonic acid gas, as appeared from an analysis. The leaves were separately exposed in glasses arranged near each other, and from eight to thirteen comparative experiments were made at one time.

Leaves of	Carbonic Acid Gas in 100 Parts.	Oxygenous Gas in Drachm Measures.	Purity with one Measure of Nitrous Air.	Do. with two Mea- sures.	Do. with three Measures.	State of the Ther- mometer.	Time when exposed.
<i>Alcea rosea</i> - - -	From 8 to 9 Parts.	19 $\frac{1}{2}$	122	146	96	105° to 110° of Fahrenheit.	July 2, 1802.
<i>Zea maiz</i> - - -		16	116	140	54		The day was very clear.
<i>Amaranthus spinosa</i>		15	120	140	68		
<i>Melissa officinalis</i> - - -		13	120	130	50		
<i>Hyfopus</i> - - -		16	120	138	70		
<i>Convolvulus purpureus</i> - - -		8	110	110	0		
<i>Malva rotundifolia</i> - - -		17	120	140	86		
<i>Lavendula</i> - - -		16	118	130	55		
<i>Rosa centifolia</i> - - -		15	112	130	46		
<i>Mirabilis dichtoma</i> - - -		16	110	130	40		
<i>Convolvulus purpureis</i> - - -	From 8 to 10 Parts.	13	110	120	40	100° to 115°.	July 3.
<i>Anthemis nobilis</i> - - -		12	114	120	32		Day clear.
<i>Hibiscus Syriacus</i> - - -		12	118	130	65		
<i>Polygonum aviculare</i> - - -		18	114	130	50		
<i>Amygdalus Perfica</i> - - -		10	114	112	12		
<i>Pyrus malus</i> - - -		16	116	120	20		
<i>Platanus occidentalis</i> - - -		12	120	140	20		
<i>Tilia Americana</i> - - -		10	120	138	40		

Leaves of	Carbonic Acid Gas, in 100 Parts.	Oxygenous Gas in Drachm Measures.	Purity with one Measure of Nitrous Air.	Dos. with two Measures.	Dos. with three Measures.	State of the Thermometer.	Time when exposed.
<i>Sriodendron tulipitia</i>	-	14	112	120	25	105° to 110°.	July 4, 1801. Day generally clear.
<i>Populus dilatata</i>	-	14	110	132	60		
<i>Æsculus pavia</i>	-	13	110	130	0		
<i>Apium petroselinum</i>	-	12	115	132	55		
<i>Convolvulus purpureus</i>	-	5	120	120	30		
<i>Helianthus annuus</i>	-	13	112	132	62		
<i>Ruta graveolens</i>	-	10	120	130	40		
<i>Trifolium palustri</i>	-	13	120	140	55		
<i>Datura stramonium</i>	-	14	112	130	80		
<i>Hysopus</i>	-	7	112	132	65		
<i>Blattari verbascum</i>	-	12	112	130	45	95°.	July 5. Day clear and cloudy. Twelve ounce measures of this oxygenous air, after being washed in lime water, to free it from the carbonic acid gas, being exposed to a mixture of iron filings and sulphur, were found to consist of eight ounce measures of oxygenous, and four of azotic gas.
<i>Chelidonium majus</i>	-	18	112	136	80		
<i>Chrysanthimum Indicum</i>	-	14	120	142	63		
<i>Acer glaucum</i>	-	14	120	139	63		
<i>Phytolacca decandra</i>	-	14	120	140	80		
<i>Antirrhinum linaria</i>	-	18	120	140	65		
<i>Arctum cappa</i>	-	12	120	140	53		
<i>Syringa vulgaris</i>	-	8	120	132	40		
<i>Helianthus altissimus</i>	-	12	120	140	55		
<i>Polygonum Percicana</i>	-	12	120	140	80		
<i>Cercis Canadensis</i>	-	12	120	140	60		
<i>Sonicera caprifolium</i>	-	12	120	140	60		
<i>Diospyros Virginiana</i>	-	10	120	120	30	90° to 110°.	July 6. Day clear and cloudy. These leaves were gathered in the evening, and kept until morning, in a cool place.
<i>Franklinia altamaha</i>	-	10	120	102	0		
<i>Chionanthus Virginica</i>	-	8	120	100	0		
<i>Arundo gigantia</i>	-	10	120	130	32		
<i>Asclepias Syriaca</i>	-	9	120	80	0		
<i>Annona triloba</i>	-	10	120	130	40		
<i>Magnolia glauca</i>	-	10	110	102	0		
— <i>tripetala</i>	-	16	116	130	40		
<i>Xanthoriza tinctoria</i>	-	8	120	130	50		
<i>Conferva cicularis</i>	-	10	120	120	30		
<i>Alcea rosea</i>	-	5	110	70	0		
<i>Sophora indica</i>	-	7	110	80	0		
<i>Laurus saffrafas</i>	-	10	120	92	0		

Opinion that plants supply oxygen to the atmosphere,

We are indebted to Dr. Priestley for the discovery, that plants exposed to light yield oxygenous air; and ever since it has been made, an opinion has been adopted, that growing vegetables supply the oxygenous portion of atmospherical air, of which there is a constant consumption, by combustion, fermentation, respiration, and the calcination of metals.

is founded;

If this subject is attentively examined, it will be found that plants have no effect in rendering the air of the atmosphere pure.

because they afford none unless carbonic acid be present. Experiments of Priestley in proof;

First. Whenever oxygenous gas has been obtained from vegetables, carbonic acid gas has been present.

Dr. Priestley exposed plants to atmospherical air, in which spirit of wine and wax and tallow candles had burned out; to air which had been vitiated by the death or putrefaction of mice and fishes, and to air which had been frequently taken into his lungs. He also observed, that there was a slower and less production of air from rain and distilled, than from pump and stagnant water.

and of the author; tabulated.

The difference between the quantity and quality of the gas, obtained from river water and the same water impregnated with carbonic acid, by exposing plants in it to the influence of solar light, will be seen by the following table:

Leaves of	Carbonic Acid Gas, in 100 Parts.	Quantity of Gas in Drachm Measures.	Purity with one Measure of Nitrous Air.	Do. with two Measures.	Do. with three Measures.	State of the Thermometer.	Time when exposed.
Siriodendron tulipifera -	None.	From half a Drachm to one Drachm.	55			110°.	July 7, 1801. Day very clear. The leaves were exposed in the water of the river Schuyltril.
Cercis Canadensis -			70				
Tilia Americana -			50				
Salix Babylonica -			32				
Polygonum Perficaria -			30				
Phytolacca decandra -			94				
Platanus occidentalis -			90				
Alcea rosea -			84				
Helianthus annuus -			83				
Amygdalus Perfica -			82				
Conferva fontinalis -			80				
Zea maiz -			75				
Acer glaucum -			90				
Seriodendra tulipifera -	In some of the vessels none, in others, from 5 to 10 Parts.	6 120	130	40	110°.	July 8, 1801. Day a little hazy, although the sun shone constantly. The leaves of the same plants, in the same river water, impregnated with four quarts of the water, saturated with carbonic acid gas, from carbonate of lime and the fulphuric acid.	
Cercis Canadensis -			6 116	124			30
Tilia Americana -			5 110	160			0
Salix Babylonica -			5 120	100			0
Polygonum Perficaria -			10 120	140			70
Phytolacca decandra -			6 120	140			42
Platanus occidentalis -			3 110	60			0
Alcea rosea -			6 120	132			40
Helianthus annuus -			10 120	110			50
Amygdalus Perfica -			6 120	138			40
Conferva fontinalis -			4 120	134			50
Zea maiz -			4 115	125			20
Acer glaucum -			6 120	140			50

It appears from this table, that the leaves of thirteen different plants, separately exposed in forty ounce measures of the water of the river Schuyltrill, produced about ten drachm measures of air, the principal part of which was azotic gas; whereas the same kind of leaves, exposed in the same quantity of the same water, impregnated with carbonic acid, yielded seventy-seven drachm measures of oxygenous air, of a very high degree of purity.

Count Rumford's experiments to obtain oxygen from water by solar light.

Count Rumford made an attempt, in the year 1787, to overthrow the doctrine of the purification of the air by plants. His arguments were, that leaves confined in water were in unnatural circumstances, and that pure air could be obtained from other bodies, as fine spun glass, raw silk, common cotton, and that of the poplar tree, exposed in water to the light of the sun*.

Remarks on Priestley's

The ingenious author of *Phytologia* also says, it may be suspected that, in many of the experiments of Priestley and Ingenhouz, the production of vital air might be simply owing to the action of the sun's light on the water in which the vegetables were immersed, like that from the silk in the experiments of Count Rumford; and that the fine points or sharp edges of these bodies, contributed only to facilitate the liberation of it when exposed to the sun shine, which thus disoxygenated the water by their united effect.

and Count Rumford's experiments.

The experiments of Count Rumford are far from being satisfactory. Thirty grains of raw silk, at the end of three days, yielded him but $3\frac{3}{4}$ cubic inches of air, and sometimes four days elapsed before a sufficient quantity could be collected for an experiment.

Direct experiments

In order to find how much air could be obtained from the "fine points or sharp edges" of certain bodies acting upon water, the following substances were exposed one day to the action of solar light, in forty ounce measures of pump water.

with fibrous bodies, which gave air less in quantity and purity than leaves.

Filaments of asbestos, baked horse-hair, common cotton, and that of the *asclepias Syriaca*, the flower panicles of *rhus cotinus*, the fine hairy plumes of *climatis crispa*, the spikes of *panicum glaucum*, and charcoal in powder. From each of these substances, from two to four drachm measures of pure air were obtained, which devoured nearly two measures of nitrous air; consequently it was less pure than that procured from leaves exposed in the same water. There was also a much smaller quantity of it; for from eight to nineteen drachm measures may be obtained in a few hours, by immersing the leaves of any plant in the same water, and exposing them to solar light.

Other sources of air.

Some water, without any mixture, will yield oxygenous gas by the combined action of light and heat; and many substances placed in water, appear to act merely by raising its temperature.

* Transactions of the Royal Society for 1787.

The green vegetable matter, which forms on all bodies, immersed a considerable time in water, might also have been one of the sources of pure air, in some of the experiments of Count Rumford.

Secondly. Many philosophers suppose, that vegetables yield oxygenous gas by the decomposition of water. Its hydrogen is said to enter into plants, while its oxygen is set at liberty in the form of pure air. Plants do not decompose water; for they do not operate in pure water.

If this opinion was true, oxygenous gas should be obtained by exposing leaves in boiled, rain, distilled, river, or lime water, but this cannot be done.

Thirdly. Some suppose that vegetables give oxygenous air to animals, and that the latter yield them azotic gas in return, which they devour for food. Plants do not (as has been supposed) emit oxygen and absorb azote;

If this hypothesis were just, atmospheric air would be increased in purity by confining leaves in it when it contained no fixed air; and its purity might also be increased, after being previously diminished, by an additional quantity of azotic air, in the same manner.

A handful of the leaves of *euphorbia picata*, *nicotiana tobacco*, *buxus vulgaris*, *cinna glauca*, *mimosa julibrifescens*, *jactus procumbens*, *coryllus avellana*, *Herculea foetida*, *malva crispa*, *pinus strobus*, *colutea arborefcens*, and *epilobium*, were separately exposed four hours to the light of the sun, in forty ounce measures of atmospheric air, and its purity was found to be neither increased nor diminished. After they had remained sixteen hours in the air, no effect was produced on it. The leaves were fresh gathered, and no decay could be observed upon any part of them. for fresh leaves do not affect atmospheric air,

When leaves are plucked promiscuously, and are placed in atmospheric air either in the day or night, they diminish its purity. Wherever a leaf is perforated, and this is very generally done by insects, let the perforation be ever so small, the part decays, and the coal of this decayed part uniting to the oxygen of the atmospheric air, generates carbonic acid, which lessens its purity. though wounded or decaying leaves do.

Experiments in
solar light ;

The following table shews the effect of the leaves of plants gathered promiscuously, exposed five hours to the light of the sun, in forty ounce measures of atmospherical air, at a temperature of 75° of Fahrenheit.

A small handful of the	Fixed Air.		Atmospheric Air of the St. and Ard. of 100.	
Leaves of <i>Datura stramonium</i> - -	3		96	
<i>Rhododendron maximum</i> - -	5		87	
<i>Apium petroselinum</i> - -	4		86	
<i>Anthemis nobilis</i> - -	0		100	
<i>Sophora australis</i> - -	2		95	
<i>Sedum telephium</i> - -	0		100	
<i>Amaranthus hybridus</i> - -	10		70	

in the dark.

The following table will shew the effects produced in one night, on forty ounce measures of atmospheric air of the purity of 100, by a small handful of leaves gathered promiscuously from a variety of plants.

	Fixed Air.		Atmosf. Air.	
Leaves of <i>Ilex aquifolium</i> - - -	5		88	
<i>Juniperus officinalis</i> - - -	4		93	
<i>Berberis vulgaris</i> - - -	2		86	
<i>Franklinia alata</i> - - -	3		85	
<i>Rhododendron maximum</i> - - -	1		95	
<i>Annona triloba</i> - - -	2		88	
<i>Buxus vulgaris</i> - - -	2		90	
<i>Pinus strobus</i> - - -	2		88	
<i>Mitchella repens</i> - - -	0		100	
<i>Arclepias Syriaca</i> - - -	5		86	
<i>Hamamelis Virginia</i> - - -	0		100	
<i>Bignonia radicans</i> - - -	3		77	
<i>Xanthoriza tinctoria</i> - - -	1		94	
<i>Magnolia tripetala</i> - - -	5		67	
<i>Kalmia latifolia</i> - - -	2		85	
<i>Pinus picea</i> - - -	3		80	
<i>Siriodendron tulipifera</i> - -	10		65	

According to some philofophers, carbonic acid gas is secreted by certain vegetables in the night ; but as the quantity of this air obtained is always in proportion to the decayed parts of plants,

plants, and to the temperature to which they are subjected, it appears more rational to ascribe the generation of it to the coal of the decayed parts uniting with the oxygen of the air in which they are placed.

To determine whether plants would absorb or devour azotic gas, eight ounce measures of this air were mixed with thirty-two ounce measures of atmospheric air, so that its purity was reduced from 100 to 91. A handful of the leaves of *euphorbia picta* and *coryllus avellana* were separately confined in forty ounce measures of this air, and exposed to the influence of a bright solar light five hours. No carbonic acid gas was generated, and the purity of the air was exactly the same as when first tried. No decayed portion could be observed upon these leaves.

Leaves exposed to sunshine in a mixture of atmospheric air and azote, produced no effect.

As it is acknowledged that the leaves, stems, and roots of plants, separate the oxygen from carbonic acid, it may be said, that the oxygenous portion of atmospheric air is supplied by the decomposition of this gas, as it is always found in the atmosphere, and often in water in which vegetables grow.

Leaves do not purify the atmosphere by decomposing its carbonic gas; because the quantity of this gas is very minute,

The quantity of carbonic acid gas in atmospheric air, is reckoned to be about one part in an hundred. It must, however, vary in different places. We would expect to find the most of it in cities, where it is formed by combustion, respiration, fermentation, and putrefaction. If one measure of the air of any large city is thrown up over lime water in an eudiometer, no milky appearance will be produced, so that the quantity of carbonic acid in this air must be extremely small. As this gas is also seized upon by alkalis, earths, and metals, and absorbed by water, the quantity floating in the atmosphere may be less than one part in ten thousand.

When we consider likewise, that the oxygen is never separated from the carbonic acid by leaves, but when they are exposed, in contact with it, to the light of the sun; and that every perforation made in a living leaf, however minute, by an insect, causes the part to decay, and absorb oxygen by day and by night; and that, in the autumn in some countries, all leaves fall on the ground, ferment or putrify, and thus diminish the purity of common air; and that the petals and fruit of vegetables have the same effect, we must pronounce, that the oxygenous portion of atmospheric air cannot be supplied by vegetation.

and plants deteriorate the air much more.

Air bladders of various plants contain air worse than that of the atmosphere.

Dr. Darwin supposes, that the air in the air bladders of vegetables serve to oxigenate the feed. The air of the air bladders of *cardiospermum halicacabum*, *staphylia trifoliata*, *colutea arborefcens*, and *sophora australis* being examined, was found to be a little worse than the air of the atmosphere.

III.

On the Theory of Light and Colours. By THOMAS YOUNG, M. D. F. R. S. Professor of Natural Philosophy in the Royal Institution.

(Concluded from page 90.)

PROPOSITION IV.

Prop. IV. Partial reflection of undulations at the confine of mediums differing in density.

When an Undulation arrives at a Surface which is the Limit of Mediums of different Densities, a partial Reflection takes place, proportionate in Force to the Difference of the Densities.

THIS may be illustrated, if not demonstrated, by the analogy of elastic bodies of different sizes. "If a smaller elastic body strikes against a larger one, it is well known that the smaller is reflected more or less powerfully, according to the difference of their magnitudes: thus, there is always a reflection when the rays of light pass from a rarer to a denser stratum of ether; and frequently an echo when a sound strikes against a cloud. A greater body striking a smaller one, propels it, without losing all its motion: thus, the particles of a denser stratum of ether, do not impart the whole of their motion to a rarer, but, in their effort to proceed, they are recalled by the attraction of the refracting substance with equal force; and thus a reflection is always secondarily produced, when the rays of light pass from a denser to a rarer stratum." (Phil. Transf. for 1800, p. 127.) But it is not absolutely necessary to suppose an attraction in the latter case, since the effort to proceed would be propagated backwards without it, and the undulation would be reversed, a rarefaction returning in place of a condensation; and this will perhaps be found most consistent with the phenomena.

PROPOSII-

PROPOSITION V.

When an Undulation is transmitted through a Surface terminating different Mediums, it proceeds in such a Direction, that the Sines of the Angles of Incidence and Refraction are in the constant Ratio of the Velocity of Propagation in the two Mediums.

Prop. V. Law of refraction of undulations transmitted through the confine of different mediums.

(Barrow, Lect. Opt. II. p. 4. Huygens, *de la Lum.* cap. 3. Euler, *Conj. Phys.* Phil. Transf. for 1800, p. 128. Young's Syllabus. Art. 382.)

Corollary 1. The same demonstrations prove the equality of the angles of reflection and incidence.

Corollary 2. It appears from experiments on the refraction of condensed air, that the ratio of the difference of the sines varies simply as the density. Hence it follows, by Schol. I. Prop. I. that the excess of the density of the ethereal medium is in the duplicate ratio of the density of the air; each particle co-operating with its neighbours in attracting a greater portion of it.

PROPOSITION VI.

When an Undulation falls on the Surface of a rarer Medium, so obliquely that it cannot be regularly refracted, it is totally reflected, at an Angle equal to that of its Incidence. (Phil. Transf. for 1800, p. 128.)

Prop. VI. Total reflection of undulation falling obliquely on the surface of a rarer medium.

Corollary. This phenomenon tends to prove the gradual increase and diminution of density at the surface terminating two mediums, as supposed in hypothesis IV; although Huygens has attempted to explain it somewhat differently.

PROPOSITION VII.

If equidistant Undulations be supposed to pass through a Medium, of which the Parts are susceptible of permanent Vibrations somewhat slower than the Undulations, their Velocity will be somewhat lessened by this vibratory Tendency; and, in the same Medium, the more, as the Undulations are more frequent.

Prop. VII. Undulations are retarded by passing through a medium vibrating with less frequency.

For, as often as the state of the undulation requires a change in the actual motion of the particle which transmits it, that change will be retarded by the propensities of the particle to continue its motion somewhat longer: and this retardation will be more frequent, and more considerable, as the difference

rence between the periods of the undulation and of the natural vibration is greater.

Doctrine of heat consisting in vibrations.

Corollary. It was long an established opinion, that heat consists in vibrations of the particles of bodies, and is capable of being transmitted by undulations through an apparent vacuum. (Newt. Opt. Qu. 18.) This opinion has been of late very much abandoned. Count Rumford, Professor Pictet, and Mr. Davy, are almost the only authors who have appeared to favour it; but it seems to have been rejected without any good grounds, and will probably very soon recover its popularity.

Suppose the parts of all bodies to vibrate;

Let us suppose that these vibrations are less frequent than those of light; all bodies therefore are liable to permanent vibrations slower than those of light; and indeed almost all are liable to luminous vibrations, either when in a state of ignition, or in the circumstances of solar phosphori; but much less easily, and in a much less degree, than to the vibrations of heat. It will follow from these suppositions, that the more frequent luminous undulations will be more retarded than the less frequent; and consequently, that blue light will be more refrangible than red, and radiant heat least of all; a consequence which coincides exactly with the highly interesting experiments of Dr. Herschel. (Phil. Transf. for 1800, p. 284.) It may also be easily conceived, that the actual existence of a state of slower vibration may tend still more to retard the more frequent undulations, and that the refractive power of solid bodies may be sensibly increased by an increase of temperature, as it actually appears to have been in Euler's experiments. (Acad. de Berlin. 1762. p. 328.)

then the more frequent luminous undulations will be most retarded. Blue light will be most refracted, and radiant heat least of all,

and refractive power will increase with the temperature.

Scholium. If, notwithstanding, this proposition should appear to be insufficiently demonstrated, it must be allowed to be at least equally explanatory of the phenomena with any thing that can be advanced on the other side, from the doctrine of projectiles; since a supposed accelerating force must act in some other proportion than that of the bulk of the particles; and, if we call this an elective attraction, it is only veiling under a chemical term, our incapacity of assigning a mechanical cause. Mr. Short, when he found by observation the equality of the velocity of light of all colours, felt the objection so forcibly, that he immediately drew an inference from it in favour of the undulatory system. It is assumed in the

proposition,

This position explains the phenomenon at least as well as the projectile hypothesis.

proposition, that when light is dispersed by refraction, the corpuscles of the refracting substance are in a state of actual alternate motion, and contribute to its transmission; but it must be confessed, that we cannot at present form a very decided and accurate conception of the forces concerned in maintaining these corpuscular vibrations.

PROPOSITION VIII.

When two Undulations, from different Origins, coincide either perfectly or very nearly in Direction, their joint Effect is a Combination of the Motions belonging to each.

Prop. VIII.
Undulations that coincide in direction will combine.

Since every particle of the medium is affected by each undulation, wherever the directions coincide, the undulations can proceed no otherwise than by uniting their motions, so that the joint motion may be the sum or difference of the separate motions, accordingly as similar or dissimilar parts of the undulations are coincident.

I have, on a former occasion, insisted at large on the application of this principle to harmonics; (Phil. Transf. for 1800, p. 130.) and it will appear to be of still more extensive utility in explaining the phenomena of colours. The undulations which are now to be compared are those of equal frequency. When the two series coincide exactly in point of time, it is obvious that the united velocity of the particular motions must be greatest, and, in effect at least, double the separate velocities; and also, that it must be smallest, and if the undulations are of equal strength, totally destroyed, when the time of the greatest direct motion belonging to one undulation coincides with that of the greatest retrograde motion of the other. In intermediate states, the joint undulation will be of intermediate strength; but by what laws this intermediate strength must vary, cannot be determined without further data. It is well known that a similar cause produces in sound, that effect which is called a beat; two series of undulations of nearly equal magnitude co-operating and destroying each other alternately, as they coincide more or less perfectly in the times of performing their respective motions.

Effects when they are of equal frequency.

The beat in sound.

COROLLARY I. *Of the Colours of striated Surfaces,*

Boyle appears to have been the first that observed the colours of scratches on polished surfaces. Newton has not noticed

Colours of striated surfaces explained,

ticed them. Mazeas and Mr. Brougham have made some experiments on the subject, yet without deriving any satisfactory conclusion. But all the varieties of these colours are very easily deduced from this proposition.

by the greater length of ray (or line of undulation) from the depressed portion of surface,

Let there be in a given plane two reflecting points very near each other, and let the plane be so situated that the reflected image of a luminous object seen in it may appear to coincide with the points; then it is obvious that the length of the incident and reflected ray, taken together, is equal with respect to both points, considering them as capable of reflecting in all directions. Let one of the points be now depressed below the given plane; then the whole path of the light reflected from it, will be lengthened by a line which is to the depression of the point as twice the cosine of incidence to the radius. Fig. 2. Plate VI.

which will occasion alternations of intensity and remission in the compound undulation; accordingly as the depression causes the undulations of the succeeding ray to coincide with or oppose those of the preceding ray.

If, therefore, equal undulations of given dimensions be reflected from two points, situated near enough to appear to the eye but as one, wherever this line is equal to half the breadth of a whole undulation, the reflection from the depressed point will so interfere with the reflection from the fixed point, that the progressive motion of the one will coincide with the retrograde motion of the other, and they will both be destroyed; but, when this line is equal to the whole breadth of an undulation, the effect will be doubled; and when to a breadth and a half, again destroyed; and thus for a considerable number of alternations: and, if the reflected undulations be of different kinds, they will be variously affected, according to their proportions to the various length of the line which is the difference between the lengths of their two paths, and which may be denominated the interval of retardation.

Experiment by parallel strokes;

In order that the effect may be the more perceptible, a number of pairs of points must be united into two parallel lines; and, if several such pairs of lines be placed near each other, they will facilitate the observation. If one of the lines be made to revolve round the other as an axis, the depression below the given plane will be as the sine of the inclination; and, while the eye and luminous object remain fixed, the difference of the length of the paths will vary as this sine.

such as the fine micrometers of Coventry at the

The best subjects for the experiment are Mr. Coventry's exquisite micrometers; such of them as consist of parallel lines drawn

drawn on glass, at the distance of one five hundredth of an inch, are the most convenient. Each of these lines appears under a microscope to consist of two or more finer lines, exactly parallel, and at the distance of somewhat more than a twentieth of that of the adjacent lines. I placed one of these so as to reflect the sun's light at an angle of 45° , and fixed it in such a manner, that while it revolved round one of the lines as an axis, I could measure its angular motion; and I found, that the brightest red colour occurred at the inclinations $10\frac{1}{4}^\circ$, $20\frac{1}{4}^\circ$, 32° , and 45° ; of which the sines are as the numbers 1, 2, 3, and 4. At all other angles also, when the sun's light was reflected from the surface, the colour vanished with the inclination, and was equal at equal inclinations on either side.

distance of one five hundredth of an inch.

When the angular position of these is varied they produce colours,

This experiment affords a very strong confirmation of the theory. It is impossible to deduce any explanation of it from any hypothesis hitherto advanced; and I believe it would be difficult to invent any other that would account for it. There is a striking analogy between this separation of colours, and the production of a musical note by successive echoes from equidistant iron palisades; which I have found to correspond pretty accurately with the known velocity of sound, and the distances of the surfaces.

which strongly and exclusively confirm the theory.

It is analogous to the musical echo from iron rails.

It is not improbable that the colours of the integuments of some insects, and of some other natural bodies, exhibiting in different lights the most beautiful versatility, may be found to be of this description, and not to be derived from thin plates. In some cases, a single scratch or furrow may produce similar effects, by the reflection of its opposite edges,

Versatile colours of some insects, &c. probably of this nature.

COROLLARY 2. *Of the Colours of thin Plates.*

When a beam of light falls on two parallel refracting surfaces, the partial reflections coincide perfectly in direction; and, in this case, the interval of retardation, taken between the surfaces, is to their distance as twice the cosine of the angle of refraction to the radius. For, in Fig. 3, Plate VI. drawing AB and CD perpendicular to the rays, the times of passing through BC and AD will be equal, and DE will be half the interval of retardation; but DE is to CE as the sine of DCE to the radius. Hence, that DE may be constant, or that the same colour may be reflected, the thickness CE must vary as the secant of the angle of refraction CED: which agrees

Colours of thin plates explained

by the greater length of ray reflected from the posterior surface, which must produce an effect

similar to that of the striated surfaces; namely colours, agrees exactly with Newton's experiments; for the correction is perfectly inconsiderable.

Let the medium between the surfaces be rarer than the surrounding mediums; then the impulse reflected at the second surface, meeting a subsequent undulation at the first, will render the particles of the rarer medium capable of wholly stopping the motion of the denser, and destroying the reflection, (PROP. IV.) while they themselves will be more strongly propelled than if they had been at rest; and the transmitted light will be increased. So that the colours by reflection will be destroyed, and those by transmission rendered more vivid, when the double thicknesses, or intervals of retardation, are any multiples of the whole breadths of the undulations; and, at intermediate thicknesses the effects will be reversed; according to the Newtonian observations.

which will be transmitted and reflected by turns if the thickness gradually vary, as in the Newtonian rings.

If the same proportions be found to hold good with respect to thin plates of a denser medium, which is indeed not improbable, it will be necessary to adopt the corrected demonstration of PROP. IV. but, at any rate, if a thin plate be interposed between a rarer and a denser medium, the colours by reflection and transmission may be expected to change places.

Breadth and duration of the colorific undulations determined,

From Newton's measures of the thicknesses reflecting the different colours, the breadth and duration of their respective undulations may be very accurately determined; although it is not improbable, that when the glasses approach very near, the atmosphere of ether may produce some little irregularity. The whole visible spectrum appears to be comprised within the ratio of three to five, or a major sixth in music; and the undulations of red, yellow, and blue, to be related in magnitude as the numbers 8, 7, and 6; so that the interval from red to blue is a fourth. The absolute frequency expressed in numbers is too great to be distinctly conceived, but it may be better imagined by a comparison with sound. If a chord sounding the tenor \bar{c} , could be continually bisected 40 times, and should then vibrate, it would afford a yellow green light: this bring denoted by c^{41} , the extreme red would be a^{40} , and the blue d^{41} . The absolute length and frequency of each vibration is expressed in the table; supposing light to travel in $8\frac{1}{2}$ minutes 500,000,000,000 feet.

and compared with those of sound.

Colours.	Length of an Undulation in parts of an Inch, in Air.	Number of Undulations in an Inch.	Number of Undulations in a Second.
Extreme -	.0000266	37640	463 millions of millions.
Red - -	.0000256	39180	482
Intermediate	.0000246	40720	501
Orange - -	.0000240	41610	512
Intermediate	.0000235	42510	523
Yellow - -	.0000227	44000	542
Intermediate	.0000219	45600	561 (= 2 ⁴³ nearly)
Green - -	.0000211	47460	584
Intermediate	.0000203	49320	607
Blue - -	.0000196	51110	629
Intermediate	.0000189	52910	652
Indigo - -	.0000185	54070	665
Intermediate	.0000181	55240	680
Violet - -	.0000174	57490	707
Extreme -	.0000167	59750	735

Table of colorific undulations of the ether.

Scholium. It was not till I had satisfied myself respecting all these phenomena, that I found in Hooke's Micrographia, a passage which might have led me earlier to a similar conclusion. It is most evident that the reflection from the under or further side of the body, is the principal cause of the production of these colours.—Let the ray fall obliquely on the thin plate, part therefore is reflected back by the first superficies, —part refracted to the second surface,—whence it is reflected and refracted again.—So that, after two refractions and one reflection, there is propagated a kind of fainter ray,—” and, “by reason of the time spent in passing and repassing,—this fainter pulse comes behind the” former reflected “pulse; so that hereby, (the surfaces being so near together that the eye cannot discriminate them from one,) this confused or duplicated pulse, whose strongest part precedes, and whose weakest follows, does produce on the retina, the sensation of a yellow. If these surfaces are further removed asunder, the weaker pulse may become coincident with the” reflection of the second,” or next following pulse, from the first surface, “and lagg behind that also, and be coincident with the third, fourth, fifth, sixth, seventh, or eighth—; so that, if there be a thin transparent body, that from the greatest thinness requisite,

Quotation from Robert Hooke exhibiting a similar doctrine,

site to produce colours, does by degrees grow to the greatest thickness, the colours shall be so often repeated, as the weaker pulse does lose paces with its primary or first pulse, and is coincident with a subsequent pulse. And this, as it is coincident, or follows from the first hypothesis I took of colours, so upon experiment have I found it in multitudes of instances that seem to prove it." (P. 65—67.) This was printed about seven years before any of Newton's experiments were made. We are informed by Newton, that Hooke was afterwards disposed to adopt his "suggestion" of the nature of colours; and yet it does not appear that Hooke ever applied that improvement to his explanation of these phenomena, or inquired into the necessary consequence of a change of obliquity, upon his original supposition, otherwise he could not but have discovered a striking coincidence with the measures laid down by Newton from experiment. All former attempts to explain the colours of thin plates, have either proceeded on suppositions which, like Newton's, would lead us to expect the greatest irregularities in the direction of the refracted rays; or, like Mr. Michell's, would require such effects from the change of the angle of incidence, as are contrary to the effects observed; or they are equally deficient with respect to both these circumstances, and are inconsistent with the most moderate attention to the principal phenomena.

which was printed seven years before Newton made his experiments.

COROLLARY 3. *Of the Colours of thick Plates.*

When a beam of light passes through a refracting surface, especially if imperfectly polished, a portion of it is irregularly scattered, and makes the surface visible in all directions, but most conspicuously in directions not far distant from that of the light itself: and, if a reflecting surface be placed parallel to the refracting surface, this scattered light, as well as the principal beam, will be reflected, and there will also be a new diffipation of light, at the return of the beam through the refracting surface. These two portions of scattered light will coincide in direction; and, if the surfaces be of such a form as to collect the similar effects, will exhibit rings of colours. The interval of retardation is here, the difference between the paths of the principal beam and of the scattered light between the two surfaces; of course, wherever the inclination of the scattered light is equal to that of the beam, although in different

rent planes, the interval will vanish, and all the undulations will conspire. At other inclinations, the interval will be the difference of the secants from the secant of the inclination or angle of refraction of the principal beam. From these causes, all the colours of concave mirrors observed by Newton and others are necessary consequences: and it appears that their production, though somewhat similar, is by no means, as Newton imagined, identical with the production of those of thin plates.

COROLLARY 4. *Of Blackness.*

In the three preceding corollaries, we have considered the refracting and reflecting substances as limited by a mathematical surface; but this is perhaps never physically true. The ethereal atmospheres may extend on each side the surface as far as the breadth of one or more undulations; and, if they be supposed to vary equally in density at every part, the partial reflections from each of the infinite number of surfaces, where the density changes, will very much interfere with each other, and destroy a considerable portion of the reflected light, so that the substance may become positively black; and this effect may take place in a greater or less degree, as the density of the ethereal atmosphere varies more or less equably; and, in some cases, particular undulations being more affected than others, a tinge of colour may be produced. Accordingly, M. Bouguer has observed a considerable loss of light, and in some instances a tinge of colour, in total reflections at the surface of a rarer medium.

Blackness produced by innumerable reflections at the confine of two surfaces where the ether gradually varies in density: the effect of which is, that the undulations confound and destroy each other, and consequently the sensation of light.

COROLLARY 5. *Of Colours by Inflection.*

Whatever may be the cause of the inflection of light passing through a small aperture, the light nearest its centre must be the least diverted, and the nearest to its sides the most; another portion of light falling very obliquely on the margin of the aperture, will be copiously reflected in various directions; some of which will either perfectly or very nearly coincide in direction with the unreflected light, and, having taken a circuitous route, will so interfere with it, as to cause an appearance of colours. The length of the two tracks will differ the less, as the direction of the reflected light has been less changed by its reflection, that is, in the light passing nearest

Colours by inflection explained by the different lengths of the bended rays occasioning alternations in the compound undulations as in Corollary 1.

to

to the margin; so that the blues will appear in the light nearest the shadow. The effect will be increased and modified, when the reflected light falls within the influence of the opposite edge, so as to interfere with the light simply inflected by that also.

More particular examination of the consequences.

But, in order to examine the consequences more minutely, it will be convenient to suppose the inflection caused by an ethereal atmosphere, of a density varying as a given power of the distance from a centre, as in the eighth proposition of the last Bakerian Lecture. (Phil. Transf. for 1801, p. 83.) Putting $r = 3$, and $x = \frac{1}{2}$, I have constructed a diagram, (Fig. 4,) which shows, by the two pair of curves, the relative position of the reflected and unreflected portions of any one undulation at two successive times, and also, by shaded lines drawn across, the parts where the intervals of retardation are in arithmetical progression, and where similar colours will be exhibited at different distances from the inflecting substance. The result fully agrees with the observations of Newton's third book, and with those of later writers. But I do not consider it as quite certain, until further experiments have been made on the inflecting power of different substances, that Dr. Hooke's explanation of inflection, by the tendency of light to diverge, may not have some pretensions to truth. I am sorry to be obliged to recall here the assent which, at first sight, I was induced to give to a supposed improvement of a late author. (Phil. Transf. for 1800, p. 128.)

Scholium. In the construction of the diagram, it becomes necessary to find the time spent by each ray in its passage.

Since the velocity was denoted by $x \frac{1}{r}$, on the supposition of a projectile, it will be as $x \frac{1}{r}$ on the contrary supposition, (Phil. Transf. for 1801, p. 27. *Schol.* 2. Prop. I.) and the

fluxion of the distance described being $\frac{\dot{x}}{\sqrt{1-yy}}$, that of the

time will be $\frac{\frac{1}{r} \dot{x}}{\sqrt{1-yy}}$ or $\frac{rs}{1-r} \frac{\dot{y}}{yy \sqrt{1-yy}}$, of which the flu-

ent is $\frac{r}{1-r} \frac{s}{y} \sqrt{1-yy}$. Therefore, with the radius $x \frac{1}{r}$,

describe

describe a circle concentric with the surfaces of the inflecting atmosphere, then the angle described by the ray during its passage through the atmosphere, will always be to the angle subtended by the line cut off by this circle from the incident ray produced, in the ratio of r to $r-i$; and the time spent in this passage, will be in the same ratio to the time that would have been spent in describing this intercepted portion with the

initial velocity. For y , being equal to $sx \frac{1}{r} \frac{1}{r}$, is the sine of the inclination of the incident ray to the radius, where it meets this circle; therefore by the proposition quoted, the angle described is in a given ratio to the angle at the centre,

which is the difference of the inclinations. Making $x^2 = \frac{1}{r}$ or $\frac{s}{y}$ radius, the sine, instead of y , becomes s , and the cosine

$\sqrt{\frac{ss}{yy}} = ss$, or $\frac{s}{y} \sqrt{1-yy}$, and, when $y = ss$, $\sqrt{1-ss}$;

therefore the line intercepted is to the difference of the fluents as r to $r-1$. (See also Young's Syllabus, Art. 372.)

PROPOSITION IX.

Radiant Light consists in Undulations of the luminiferous Ether. Conclusion. Ra-

This proposition is the general conclusion from all the preceding; and it is conceived that they conspire to prove it in as satisfactory a manner as can possibly be expected from the nature of the subject. It is clearly granted by Newton, that there are undulations, yet he denies that they constitute light; but it is shown in the three first Corollaries of the last Proposition, that all cases of the increase or diminution of light are referable to an increase or diminution of such undulations, and that all the affections to which the undulations would be liable, are distinctly visible in the phenomena of light; it may therefore be very logically inferred, that the undulations are light.

A few detached remarks will serve to obviate some objections which may be raised against this theory.

1. Newton has advanced the singular refraction of the Iceland crystal, as an argument that the particles of light must be projected corpuscles; since he thinks it probable that the different sides of these particles must be differently attracted by the crystal, and since Huygens has confessed his inability to

Remarks. Newton without giving a reason rejected the law of Huygens in explaining the phenomena of Iceland crystal.

account in a satisfactory manner for all the phenomena. But, contrarily to what might have been expected from Newton's usual accuracy and candour, he has laid down a new law for the refraction, without giving a reason for rejecting that of Huygens, which Mr. Haüy has found to be more accurate than Newton's; and, without attempting to deduce from his own system any explanation of the more universal and striking effects of doubling spars, he has omitted to observe that Huygens's most elegant and ingenious theory perfectly accords with these general effects, in all particulars, and of course derives from them additional pretensions to truth: this he omits, in order to point out a difficulty, for which only a verbal solution can be found in his own theory, and which will probably long remain unexplained by any other.

Michell's experiments on the momentum of light explained, without supposing any projection.

2. Mr. Michell has made some experiments, which appear to show that the rays of light have an actual momentum, by means of which a motion is produced when they fall on a thin plate of copper delicately suspended. (Priestley's Optics.) But, taking for granted the exact perpendicularity of the plate, and the absence of any ascending current of air, yet since, in every such experiment, a greater quantity of heat must be communicated to the air at the surface on which the light falls than at the opposite surface, the excess of expansion must necessarily produce an excess of pressure on the first surface, and a very perceptible recession of the plate in the direction of the light. Mr. Bennet has repeated the experiment, with a much more sensible apparatus, and also in the absence of air; and very justly infers from its total failure, an argument in favour of the undulatory system of light. (Phil. Trans. for 1792, p. 87.) For, granting the utmost imaginable subtilty of the corpuscles of light, their effects might naturally be expected to bear some proportion to the effects of the much less rapid motions of the electrical fluid, which are so very easily perceptible, even in their weakest states.

They did not succeed with Bennet.

Latent light and heat not inconsistent with the doctrine of vibrations.

3. There are some phenomena of the light of solar phosphori, which at first sight might seem to favour the corpuscular system; for instance, its remaining many months as if in a latent state, and its subsequent re-emission by the action of heat. But, on further consideration, there is no difficulty in supposing the particles of the phosphori which have been made to vibrate by the action of light, to have this action abruptly suspended

suspended by the intervention of cold, whether as contracting the bulk of the substance or otherwise; and again, after the restraint is removed, to proceed in their motion, as a spring would do which had been held fast for a time in an intermediate stage of its vibration; nor is it impossible that heat itself may, in some circumstances, become in a similar manner latent. (Nicholson's Journal, Vol. II. p. 399.) But the affections of heat may perhaps hereafter be rendered more intelligible to us; at present, it seems highly probable that light differs from heat only in the frequency of its undulations or vibrations; those undulations which are within certain limits, with respect to frequency, being capable of affecting the optic nerve, and constituting light; and those which are slower, and probably stronger, constituting heat only; that light and heat occur to us, each in two predicaments, the vibratory or permanent, and the undulatory or transient state; vibratory light being the minute motion of ignited bodies, or of solar phosphori, and undulatory or radiant light the motion of the ethereal medium excited by these vibrations; vibratory heat being a motion to which all material substances are liable, and which is more or less permanent; and undulatory heat that motion of the same ethereal medium, which has been shown by Mr. King, (Morsels of Criticism, 1786, p. 99,) and M. Pictet, (*Essais de Physique*, 1790, also in Saussure's *Voyage dans les Alpes*, 1786.) to be as capable of reflection as light, and by Dr. Herschel to be capable of separate refraction. (Phil. Trans. for 1800, p. 284.) How much more readily heat is communicated by the free access of colder substances, than either by radiation or by transmission through a quiescent medium, has been shown by the valuable experiments of Count Rumford. It is easy to conceive that some substances, permeable to light, may be unfit for the transmission of heat, in the same manner as particular substances may transmit some kinds of light, while they are opaque with respect to others.

Light and heat differ only in the frequency of undulation or vibration,

capable in both cases of the same modifications.

On the whole it appears, that the few optical phenomena which admit of explanation by the corpuscular system, are equally consistent with this theory; that many others, which have long been known, but never understood, become by these means perfectly intelligible; and that several new facts are found to be thus only reducible to a perfect analogy with other facts, and to the simple principles of the undulatory system.

This theory explains all that is solved by the corpuscular system, and much more.

system. It is presumed, that henceforth the second and third books of Newton's Optics will be considered as more fully understood than the first has hitherto been; but, if it should appear to impartial judges, that additional evidence is wanting for the establishment of the theory, it will be easy to enter more minutely into the details of various experiments, and to show the insuperable difficulties attending the Newtonian doctrines, which, without necessity, it would be tedious and invidious to enumerate. The merits of their author in natural philosophy, are great beyond all contest or comparison; his optical discovery of the composition of white light, would alone have immortalised his name; and the very arguments which tend to overthrow his system, give the strongest proofs of the admirable accuracy of his experiments.

Experiment in which the corpuscular theory ought to exhibit distortion.

Sufficient and decisive as these arguments appear, it cannot be superfluous to seek for further confirmation; which may with considerable confidence be expected, from an experiment very ingeniously suggested by Professor Robison, on the refraction of the light returning to us from the opposite margins of Saturn's ring; for, on the corpuscular theory, the ring must be considerably distorted when viewed through an achromatic prism: a similar distortion ought also to be observed in the disc of Jupiter; but, if it be found that an equal deviation is produced in the whole light reflected from these planets, there can scarcely be any remaining hope to explain the affections of light, by a comparison with the motions of projectiles.

IV.

An Analysis of a Mineral Substance from North America, containing a Metal hitherto unknown. By CHARLES HATCHETT, Esq.

(Concluded from Page 138.)

H.

THE white precipitate, when distilled with four parts of sulphur, remained pulverulent, and, from white, was only changed to a pale ash colour.

The white precipitate was little affected by distillation of the sulphuric and nitric acids.

Nitric acid was digested on the powder, and, being heated, afforded some nitrous gas; after this, the powder became white, and in every respect recovered its original properties.

Before

I.

Before I conclude this section, I must observe, that when the olive-green precipitates, obtained by prussiate of potash, were digested in an alkaline lixivium, they were decomposed; for the alkali combined with the prussic acid, and with a small part of the white matter; but the greater part of the latter remained undissolved, in the same white flocculent state which was noticed when the alkaline combinations were mentioned.

The orange-coloured precipitates, formed by tincture of galls, were also decomposed when digested in boiling nitric acid; and the white matter was recovered in its original state.

§ III. REMARKS.

The preceding experiments shew, that the ore which has been analysed, consists of iron combined with an unknown substance, and that the latter constitutes more than three-fourths of the whole. This substance is proved to be of a metallic nature, by the coloured precipitates which it forms with prussiate of potash, and with tincture of galls; by the effects which zinc produces, when immersed in the acid solutions; and by the colour which it communicates to phosphate of ammonia, or rather to concrete phosphoric acid, when melted with it.

Moreover, from the experiments made with the blow-pipe, it seems to be one of those metallic substances which retain oxygen with great obstinacy, and are therefore of difficult reduction.

It is an acidifiable metal; for the oxide reddens litmus paper, expels carbonic acid, and forms combinations with the fixed alkalis. But it is very different from the acidifiable metals which have of late been discovered; for,

1. It remains white when digested with nitric acid.
2. It is soluble in the sulphuric and muriatic acids, and forms colourless solutions, from which it may be precipitated, in the state of a white flocculent oxide, by zinc, by the fixed alkalies, and by ammonia. Water also precipitates it from the sulphuric solution, in the state of a sulphate.
3. Prussiate of potash produces a copious and beautiful olive-green precipitate.

The precipitates by pruss. pot. were decomposed by humid alkali, which took up the pr. acid and a little of the white matter.

Obs. The ore is iron, with three parts of the white matter. The latter is metallic; for it is precip. by prussiate, and by galls, and by zinc, and it colours phosf. acid by fusion.

It is of difficult reduction,

and acidifiable.

Differs from all other the acidifiable metals, in the properties here enumerated.

4. Tincture of galls forms orange or deep yellow precipitates.

5. Unlike the other metallic acids, it refuses to unite with ammonia.

6. When mixed and distilled with sulphur, it does not combine with it so as to form a metallic sulphuret.

7. It does not tinge any of the fluxes, except phosphoric acid; with which, even in the humid way, it appears to have a very great affinity.

8. When combined with potash and dissolved in water, it forms precipitates, upon being added to solutions of tungstate of potash, molybdate of potash, cobaltate of ammonia, and the alkaline solution of iron.

These properties completely distinguish it from the other acidifiable metals, *viz.* arsenic, tungsten, molybdena, and chromium; as to the other metals lately discovered, such as uranium, titanium, and tellurium, they are still farther removed from it.

The colours of the precipitates produced by prussiate of potash and tincture of galls, approach the nearest to those afforded by titanium. But the prussiate of the latter is much browner; and the gallate is not of an orange colour, but of a brownish red, inclining to the colour of blood. Besides, even if these precipitates were more like each other, still the obstinacy with which titanium refuses to unite with the fixed alkalis, and the insolubility of it in acids when heated, sufficiently denote the different nature of these two substances.

The iron is in
state of brown
oxide.

The iron in the ore which has been examined, is apparently in the same state as it is in wolfram, *viz.* brown oxide; and this oxide is mineralized by the metallic acid which has been described, in the same manner as the oxides of iron and manganese are mineralized by the tungstic acid or rather oxide. For, from several experiments made upon a large scale, I have reason to believe that in wolfram, the tungsten has not attained the maximum of oxidation. Several facts in the course of the experiments lately described, seem to prove, that this new metal differs from tungsten and the other acidifiable metals, by a more limited extent of oxidation; for, unlike these, it seems to be incapable of retaining oxygen sufficient to enable the total quantity to combine with the fixed alkalis. In § II. G. 2,

this

this is very evident; for, from the experiment there described, it appears, that when the metallic acid or oxide was digested with lixivium of potash, only a part was dissolved; and that the remainder was insoluble in the same lixivium, till it had received an additional portion of oxygen, by being treated with nitric acid; also that several of these alternate operations were required, before any given quantity of the metallic oxide could be completely combined with the alkali. Now there is much reason to believe, that in this case, when the metallic oxide or acid was digested with potash, the portion which was dissolved received an accession of oxygen at the expence of the other part, which of course was thus reduced to the state of an insoluble oxide, and therefore required to be again oxidated by nitric acid, before it could combine with the alkaline solution; but still it appeared, that an adequate proportion of oxygen could never be superinduced, so as to render the oxide totally and immediately soluble in the alkalies by one operation, or even by two.

We may, therefore, regard this as an instance of the effects resulting from disposing affinity, and as very similar to those observed in respect to copper, which have been noticed by my ingenious friend Mr. Chenevix, in his valuable analysis of the arseniates of copper and of iron*.

My researches into the properties of this metal, have of course been much limited by the smallness of the quantity which I had to operate upon; but I flatter myself that more of the ore may soon be procured from the Massachusetts mines, particularly as a gentleman now in England (Mr. Smith, Secretary to the American Philosophical Society), has obligingly offered his assistance on this occasion. We shall then be able more fully to investigate the nature of this substance; and shall be more capable of judging how far it may be applicable to useful purposes. At present, all that can be said is, that the olive-green prussiate and the orange-coloured gallate are fine colours; and, as they do not appear to fade when exposed to light and air, they might probably be employed with advantage as pigments.

I am much inclined to believe, that the time is perhaps not very distant, when some of the newly discovered metals, and

From the limited researches on the small quantity of ore, the only deduction as to immediate utility is, that the precipitates are fine colours.

The new metals are probably compounds.

* Phil. Transf. for 1801, p. 233.

other substances, which are now considered as simple, primitive, and distinct bodies, will be found to be compounds. Yet I only entertain and state this opinion as a probability; for, until an advanced state of chemical knowledge shall enable us to compose, or at least to decompose, these bodies, each must be classed and denominated as a substance *sui generis*. Considering, therefore, that the metal which has been examined is so very different from those hitherto discovered, it appeared proper that it should be distinguished by a peculiar name; and, having consulted with several of the eminent and ingenious chemists of this country, I have been induced to give it the name of Columbium.

POSTSCRIPT.

It appears proper to mention some unsuccessful attempts which I have lately made to reduce the white oxide.

Reduction. The white powder was only blackened by the strong heat with charcoal.

Fifty grains were put into a crucible coated with charcoal; and being covered with the same, the crucible was closely luted, and was exposed to a strong heat, in a small wind furnace, during about one hour and an half. When the crucible was broken, the oxide was found in a pulverulent state; and, from white, was become perfectly black.

The phosphuret.

In order to form a phosphuret, some phosphoric acid was poured upon a portion of the white oxide; and, being evaporated to dryness, the whole was put into a crucible coated with charcoal, as above described. The crucible was then placed in a forge belonging to Mr. Chenevix, and a strong heat was kept up for half an hour.

The inclosed matter was spongy, and of a dark brown; it in some measure resembled phosphuret of titanium.

After this we wished to try the effect of a still greater heat; but in this experiment the crucible was melted.

The above experiments shew, that the white oxide, like several other metallic substances, may be deoxidated to a certain degree, without much difficulty, but that the complete reduction of it is still far from being easily effected.

V.

On the Effect of Sound upon the Barometer. By Sir HENRY C. ENGLEFIELD, Bart. F. R. S. (From the Journals of the Royal Institution, No. 9.)

DURING the time I spent at Brussels in the year 1773 and 1774, it occurred to me, that the effect of sound on the barometer had not, to my knowledge, been attended to; and that it was by no means certain, whether that instrument was capable of being sensibly affected by those elastic vibrations caused in the atmosphere, by the percussion of a sonorous body. I thought the idea worthy of being pursued, and the means of making satisfactory experiments were most opportunely in my power.

Whether the barometer be affected by sonorous undulation.

The sound of a very large bell appeared to me the most powerful, and at the same time to be approached with the greatest security and ease to the observer. The explosion of artillery, besides the very disagreeable smoke and danger of the recoil, might be objected to, on account of the sudden production of elastic and heated vapour, which might, independent of the sound, instantaneously alter the state of the atmosphere, and thereby lead the observer into very great and unavoidable errors.

The bell preferred for experiments.

Every one who has been in the Low Countries must know, that very large bells, and immense numbers of them, are the pride of their churches; and that they are rung quite out, not tolled, on every great festival. The great bell of the collegiate church of St. Gudula, at Brussels, weighs, as I was told, sixteen thousand pounds, and on this I determined to found my experiment.

Large bells in the Netherlands.

Two objections only could be made to the result of this experiment, the one, that the motion of the bell might cause a vibration in the walls of the building, which would hinder the placing the barometer in a state of repose; the other, that the swinging so large a mass with a considerable degree of velocity, might of itself agitate the air so as to cause vibrations in the mercury totally independent of sound.

Whether these would produce irregularity by agitating the building or the air.

The strength of the walls of the steeple, and the manner of hanging the bell, which was contained in a frame of timber, founded

Observations.

founded

founded on a strong vault, and totally independent of the wall of the steeple, might alone have answered the first of these objections; but happily a most complete and satisfactory answer to both of them, was furnished by the manner in which the bell was rung.

Preparation for experiment.

As the bell was to ring out full in an instant, at a signal given from below, it is necessary to have it in motion some time beforehand; and during that time, the clapper is fixed to one side by a strong stick crossing the mouth of the bell, which, at the signal, is pulled out by the hand of a person placed for that purpose. If then, our barometer shewed no variation during all this time, we were absolutely certain that whatever motion was perceived afterwards, was wholly owing to the sound.

Mr. Pigott, who was then at Brussels, was kind enough to lend me one of his barometers, made by Ramsden, and his son made the following observations jointly with myself:

Narrative.

At two o'clock in the afternoon of the 1st of November 1773, we went into the northwest tower of St. Gudula's church, and having fixed the barometer firmly in the opening of a window, not above seven feet from the bottom of the bell, we waited quietly for its ringing.

The height of the mercury before the bell began to swing, as observed by Mr. Pigott, was 29.478 inches. The bell being in full swing, no alteration whatever was perceptible.

The mercury was elevated by the sound of a large bell.

The instant that the clapper was loosed the mercury leaped up, and continued that sort of springing motion, at every stroke of the clapper, during the whole time of the ringing of the bell. These were our observations:

During the ringing of the bell, Mr. P.	-	29.469
During the ringing, by myself.		
Highest	- - - - -	29.480
Lowest	- - - - -	29.474
Highest	- - - - -	29.482
Lowest	- - - - -	29.472

These observations were made with the greatest attention; and, considering their delicacy and the difficulty of observing, agree very nearly. They appear to give from 6 to 10 thousandths of an inch for the effect of this sound on the barometer. It is to be observed, that Mr. Pigott, in general, estimated the

the

the height of the mercury about 5 thousandths lower than myself, which brings our observations to a very near agreement. The following observations prove this:

On the top of the tower, Mr. P.	-	-	29.424
Ditto, by me	-	-	29.430
At the foot of the tower, Mr. P.	-	-	29.639
Ditto, by me	-	-	29.642
In the court of the English Nuns, by Mr. P.			29.676
Ditto, by me	-	-	29.682

And I should think that the difference of eyes may frequently cause such a variation among different observers; at least in delicate observations, it will be always prudent to make the experiment.

NOTE BY DR. YOUNG.

THESE observations appear to agree too well with each other, to allow us to doubt of their accuracy. It therefore becomes necessary to inquire into the causes of the different heights of the barometer. It is indeed barely possible, that a sudden stroke of the clapper on the bell might produce a greater agitation of the building than the preceding alternate motion of the bell itself: but this explanation cannot be called satisfactory. It is certain, that there was neither more nor less air in the tower while the bell was sounding, than while it was silent; the mean density of the air could therefore not have been changed; and if the alternate motions of the particles of air which constitute sound, had taken place by equal degrees, and with equal velocities, in each opposite direction, there is no reason to suppose that the increase of pressure on the surface of the mercury, at one instant, could have tended to raise it, more than the decrease of pressure, in the opposite state of the undulation, would have depressed it. But the same consequence does not follow, if we conceive the motion of the air, in advancing, to be more rapid, but of shorter continuance, than its retrograde motion. For if the wind blew for one hour with a velocity of 4, and the same air returned, in the course of two hours, with a velocity of 2, an obstacle upon which it had acted in both directions would not be found in its original place; for the action of the wind upon an obstacle is as the square

Observations by
Dr. Young.

The mean density of the air was not changed.

If the motion of advance were more rapid than of return in the undulations, it would have the effect of a pressure.

square

square of the velocity, and the time would not compensate for the difference of force. It is therefore easy to suppose, that the law of the bell's vibration was in this experiment such, that the air advanced towards the barometer with a greater velocity than it receded, although for a shorter time; and that hence the whole effect was the same as if the mean pressure of the air had been increased. Such a law might easily result from a combination of a more regular principal vibration with one or more subordinate ones, in different relations; and similar cases may sometimes be observed in the vibrations of chords. Here we find a slight degree of repulsion, in consequence of the undulations of an elastic medium. Dr. Hooke attempted to explain the phenomena of attraction by means of similar undulations of an ether, which he supposed to be the medium serving for the communication of heat; but it must be confessed, that the conjecture has little appearance of probability.

This law might easily result from subordinate vibrations, combining with the principal.

Slight repulsions from this cause in elastic mediums.

Whether gravity be so caused.

VI.

On the Expansion of Carbonated Hydrogen by Electricity. From a Correspondent.

To Mr. NICHOLSON.

S I R,

Expansion of carb. hydrogen by electricity not yet explained.

I VENTURE to trouble you with a few remarks on a phenomenon in chemistry, the expansion of carbonated hydrogen gas on the electric spark being passed through it. None of the explanations of this circumstance which I have met with, appear to me to be satisfactory; and I have stated a few objections which occurred to me on considering them. If you think them of sufficient weight to fill a corner of your ably-conducted Journal, some correspondent of greater abilities may perhaps give a new explanation of the fact. But if you think them beneath your notice, and unworthy of a place among the excellent communications with which your numbers are filled, I have only to beg that you will pardon the presumption of a very young chemist, who, by this first attempt, has perhaps only exposed his own ignorance.

I am, Sir,

Your's with much respect,

Edin. June 8, 1802.

G. H.

On

On the Expansion of carbonated Hydrogen Gas.

This fact was first observed by Dr. Austin. On passing the electric spark through a quantity of carbonated hydrogen gas, he found that the gas was permanently dilated to more than twice its original bulk. He concluded that this remarkable expansion could only be owing to the evolution of hydrogen gas. Upon burning the air thus expanded in oxygen gas, he found that it required more oxygen for its combustion than an equal quantity of carbonated hydrogen gas, which had not been expanded by the electric spark. *An addition therefore had been made to the combustible matter*; for the quantity of oxygen necessary to complete the combustion of any body, is always proportional to the quantity of that body. He concluded from these experiments, that he had decomposed the carbon which had been dissolved in the hydrogen, and that carbon was composed of hydrogen and azote, some of which was always found in the vessel after the combustion.

Dr. Austin first observed that carbon. hydrogen is expanded to double by electricity.

His theory that carbon is hydrogen and azote.

If Dr. Austin had more attentively considered the circumstances of these experiments, he would probably have been prevented from drawing this conclusion. *The quantity of combustible matter had been increased.* Now, if the expansion of the carbonated hydrogen gas was owing merely to the decomposition of carbon, no such increase ought to have taken place, but rather the contrary; for the carbon, which was itself a combustible substance, was resolved into two ingredients, hydrogen and azote, only the first of which burnt on the addition of oxygen, and the application of heat. And besides, if the carbon had been resolved into hydrogen and azote, the product of the combustion could only have been a greater quantity of water, with a residuum of azote; for the hydrogen evolved by the dilatation of the carbonated hydrogen gas, combining with the additional oxygen, must have formed an additional quantity of water. But it is a fact, which the Doctor does not seem to have attended to, that besides water, *carbonic acid gas* is produced from the combustion of the expanded carbonated hydrogen. Whence, then, comes the carbonic acid gas?

Objections. Hydrogen and azote are not more combustible than carbon: and the product is not mere water and azote, but also carbonic acid gas.

Mr. Henry *, who repeated Dr. Austin's experiments with great accuracy, found that he was correct with regard to the

Mr. Henry found that the expansion is

* Philof. Tranfact. 1797.

produced by hydrogen from decomposed water.

increase of combustible matter. He also found, that the expansion could not be carried beyond a certain point, about twice the original bulk of the gas. Upon burning separately by means of oxygen, two equal portions of carbonated hydrogen gas, one of which had been expanded by electricity to twice its original bulk, the other not, he found that each of them produced precisely the same quantity of carbonic acid gas; a proof, that the carbon in both remained the same, and that the hydrogen could not have been produced from it. He concluded therefore, that the evolution of the hydrogen is produced from the decomposition of the water with which hydrogen is always more or less impregnated; to prove which, he took hydrogen gas, from which he expelled as much water as possible, and found that the electric spark produced a very small dilatation; but on admitting a drop or two of water, the expansion went on as usual. It is easily seen how this decomposition is effected. Carbon at a high temperature has a greater affinity for oxygen than hydrogen gas; when the temperature is raised, therefore, by the electric spark, the carbon unites with the oxygen of the water, forming carbonic acid gas, and the hydrogen is evolved. As to the azote, it must have been produced from the admission of atmospheric air into the process.

But this discovery does not account for the greater quantity of oxygen required to burn the expanded gas.

Although this theory of Mr. Henry seems to prove the production of hydrogen from the decomposition of the water contained in the gas; still, however, it is no more able to account for the increase of combustible matter than that of Dr. Austin. When the gas is dilated, hydrogen and oxygen are evolved from the water. The hydrogen goes to increase the bulk of the gas, and the oxygen unites with the carbon. Expanded carbonated hydrogen gas, then, contains hydrogen, carbon, and carbonic acid gas. In combustion, the evolved hydrogen, in order to form water, requires a quantity of additional oxygen, precisely the same as that with which it was combined before decomposition of the water: and on the other hand, the carbon requires precisely the same quantity less, as part of it is already rendered incombustible, by being combined with the oxygen of the water. Therefore, according to Mr. Henry's theory, a quantity of expanded carbonated hydrogen gas requires the same quantity of oxygen for its combustion, as an equal quantity that has not been expanded: and

and consequently they both contain the same quantity of combustible matter; for in proportion as the combustible matter is increased by the formation of hydrogen, in the same proportion it is diminished by the formation of carbonic acid gas. But this is observed to be contrary to fact; for carbonated hydrogen gas when expanded does actually contain more combustible matter than before its expansion. It would appear then, that a satisfactory explanation of this increase of combustible matter has not yet been given.

G. H.

VII.

A new Process for claying Sugars, proposed by CIT. HAPEL-LACHENAIE, Chief Apothecary of the Military Hospitals of Guadaloupe, to the Agents of the Consuls of the French Republic in the Windward Islands.*

BEING called upon by the scientific mission confided to me Introduction. by the Government, to employ myself in every kind of research which may prove interesting to the cultivation and productions of this colony, I have thought it my duty to endeavour to discover a simple, easy, and cheap method of dispensing with the pottery which is used in the fabrication of clayed sugars.

There was even an urgent necessity that I should direct my Want of Pans attention to these objects, because most of the proprietors of for claying sugars in St. Domingo, these colonies, being at this moment in want of the forms and pots hitherto considered as indispensibly necessary for the claying sugars, are obliged to wait till the small number of pots they possess shall be cleared of the sugar they contain before they can fabricate more.

The delays occasioned by this inconvenience are the more obviously productive of great prejudicial to the inhabitants in general, because they are loss. obliged, on this account, to defer cutting their canes when at the most advantageous time of their growth; and as they cannot perform this at the most favourable season, their losses are incalculable by suffering the canes to grow old, and finishing

* From the *Annales de Chimie*, XL. 73.

their

their gathering in the season when they afford very little sugar, in comparison with what they would have yielded if they had been cut in time.

While this retardation diminishes the immediate product, it has an influence on the vigour of the succeeding shoots, as I have observed, and renders their advancement less speedy. And by this means it exposes the farmer of national plantations to fail in payment of the terms of their location.

Experiments for a new process;

I shall not dwell upon the various experiments I made before my success was complete, but shall confine myself to those means which have best succeeded with me, and to which I have confined myself in the subsequent practice in my sugar-work.

for disposing the sugar to be clayed in large vessels.

This process consists in disposing the sugar in receptacles of 12,937 cubic feet, each containing 26 ordinary forms, and to clay it in these same receptacles, of which I shall proceed to give a description :

First used by Citizens Boucherie at Paris,

The priority of the method which I propose belongs to Cits. Boucherie, who first adopted it in their refinery of Bercy near Paris. They were, as far as I know, the first who constructed vessels to clay the raw sugars they received from our colonies previous to their refining them. But though this invention is undoubtedly theirs, I also have a claim as to the degree of improvement which their vessel did not possess, but which I have given them by rendering them more convenient, advantageous, and economical*.

and improved by the Author.

Description of the apparatus of Citizens Boucherie.

The vessels of Citizens Boucherie were, as nearly as I can recollect, about 15 or 18 inches deep, and five feet wide. They were square, and the bottom of each was perforated with a great number of small holes for the discharge of the syrup, which fell into a second vessel less deep, but of the same dimensions as the others as to their width. This second vessel was lined with thin metallic plates, of an alloy invented by Mr. Hiskerdeau, a Spanish chemist. The upper vessels

* I attended their operations in the year 1784, before I came to this colony. I had also the advantage at that time of giving several lectures in their establishment to some cultivators of St. Domingo and other sugar islands, to facilitate their knowledge of the doctrine and processes of that interesting manufacture which they frequently visited.

were

were supported on their edges by a level and firm framing, the inferior vessels stood on the floor.

The upper vessels were filled with raw sugar intended to be clayed, which was first well divided or crumbled in order to its equal distribution. This was levelled and compressed as equally as possible, to form what we commonly call, the bottom (*les fonds*). Upon this mass the diluted and prepared earth, properly adapted to the operation, was very carefully poured.

Charged with raw sugar.

Though this process may appear very simple at first sight, it nevertheless presents difficulties which could scarcely be overcome in that manufactory, and which would be almost impossible to surmount in ours, where the men employed in this business have neither the understanding nor the skill of those of Citizens Boucherie; and it was not till they had practised for some time that they became able to perform it without difficulty.

This simple process is liable to objections.

The first operation in which a failure may be made for want of skill, is that of levelling with a trowel an elastic surface of 25 square feet. The second is to compress equally that surface in order to give solidity to the bottom, that the diluted earth may extend to the same thickness on all sides, and find that surface so close as to admit the penetration of the water only.

It is difficult to level and press the sugar in the large vessels.

All those who are engaged in the manufacture of sugar must be aware, that if any inequality exists in the levelling of the bottom, the water which gradually leaves the earth naturally flows to the lowest place, where, if the compression be not every where the same, the water insinuates itself into the most porous part; and in either of these cases, this fluid being conducted with the earth towards a single point, is collected in sufficient abundance to dissolve the sugar at that place, and form what is called a fox (*un renard*.) When this happens the operation of claying fails, for it is known that the portion of earth retained at the surface acts little upon the rest of the mass of sugar. This inconvenience, which occasions a great loss in the product, is also found to change its quality, which is worse on this account.

Mischievous consequences of bad levelling or pressing.

The water flows to the lowest place and dissolves the sugar; while the rest is not full purified.

In the construction of the cases or vessels of Citizens Boucherie, which cannot be removed, other inconveniencies are found which do not exist in mine. One of these is the difficulty

The sugar is not easily taken out.

culty of taking out the fugar after its drainage. A second is, that the holes in their bottom are too small, which renders them liable to be obstructed, and the process to be greatly retarded.

(To be concluded in our next.)

VIII.

Description of the Crystalline Forms of the Anhydrous Sulphate of Lime, with some Observations on this Substance. By M. Le COMTE DE BOURNON, Member of the Royal and Linnæan Societies of London. Translated from the Original, communicated by the Author.*

Sulphate of lime containing no water is a new object in mineralogy. **G**YPSUM, or the sulphate of lime deprived of water, is a new object in mineralogy: scarcely has the science as yet cast a glance upon this substance, and none of its crystalline forms have hitherto been determined. Having had opportunities of examining some of these forms, and at the same time of comparing a number of specimens of this substance, procured from different countries, I felt myself induced to lay before the Public my observations upon this subject, thinking they might contribute to throw some light upon the nature of several stones, with which we are not yet perfectly acquainted.

Its primitive form is a right prism with rectangular bases. The primitive form of this substance, which has been designated by the name of Anhydrous Sulphate of Lime by the Abbè Hauy, is a rectangular paraleloiped, as had been presumed by this able mineralogist; but it does not appear that this paraleloiped can be the perfect cube, as is indicated by the habitual form of the crystals, which when most simple, is always a right prism with rectangular bases, having two opposite sides broader than the two others (Fig. 1, Plate XI.), and seems equally to be indicated by the circumstances which accompany this form. The mechanical division is very easy upon all the faces of this crystal; but the longitudinal straight faces present, in this respect, a little more resistance than the others.

Easily divided.

And afterwards revised by him.

The

The broad faces, as well as those which terminate this rectangular prism, have generally a very brilliant lustre; the longitudinal narrow ones faces are duller, and very frequently striated in the direction of their length. The broad sides are besides characterised by a pearly reflection, similar to that which is peculiar to the zeolite stilbite, and this brilliancy remains even after those faces have been divided: they also habitually exhibit the intersection (*entrecroisement*) at right angles of the joints belonging to the other faces.

This crystal presents, along its longitudinal edges, a decrease which replaces each of these sides by a plane unequally inclined upon the adjacent faces: it forms with the broad sides of the prism an angle of 130° , and of 140° with the narrow sides, (Fig. 2.) The prism is therefore octahedral, and has four edges of 130° , and four others of 140° . Secondary forms.
Octahedral prism.

These new planes frequently join each other, upon the narrow sides of the prism, and they then convert the octahedral into a hexahedral form, having four edges of 130° , and the two others of 100° (Fig. 3.) The pearly sides frequently then continue to be the broadest; in other instances, the six sides of the prism are equal, or nearly equal with each other. I have not yet seen any secondary faces placed upon the terminal edges. Hexahedral prism.

The crystals upon which this description is founded are of considerable magnitude: some are more than an inch in length. They are of a fine flesh colour, and formed part of one of the specimens brought from the Tyrol, in which the anhydrous sulphate of lime is mixed with muriate of soda or common salt; but as this salt is intirely foreign to it, and only interposed between its parts, frequently even in a manner that is very perceptible to the eye, the form of these crystals ought to be considered as being really that of the pure anhydrous sulphate of lime. Besides, I have seen in Mr. Greville's cabinet at London, a specimen in which this substance presents exactly the same aspect, and the same colour, and which at the same time is totally destitute of salt. In this, the anhydrous sulphate of lime is confusedly intermingled with actinote of a pale green colour, with some parts of cupreous pyrites, and of the black and very magnetic oxide of iron. The locality of this interesting specimen has not been preserved; but there is every reason to presume that it came from Sweden. Foreign admixture of muriate of soda.

Groups of crystals;

In the Tyrolese specimens, similar to that which afforded the crystals which I have just described, these are for the most part strongly engaged and applied against each other, crossing each other in different directions; some of them, however, are insulated. But as many of them are united by their broadish or pearly faces, they have no very strong adhesion, and it is always sufficiently easy to separate them with the edge of a knife. These specimens frequently exhibit in their mass more or less considerable portions of pure common salt; several of their crystals are considerably transparent.

Peculiarity.

The presence of sulphur of antimony and quartz.

A piece of this substance which I procured for Mr. Chenevix, for the purpose of analysing it, exhibited a peculiarity which deserves to be remarked. On breaking some of its crystals, small needles of sulphuret of antimony were perceived within it, adhering for the most part to small groups of crystals of quartz. Not a trace of either of these two substances was found in the other parts that were subjected to analysis: the same was the case with the carbonate of lime, which Mr. Klaproth has indicated at $\frac{1}{100}$ of the analysis which he made of it, and which, undoubtedly, was likewise only an extraneous or interposed substance.

Quartz in another specimen.

In like manner, in the stone of Vulpino, observed and described very accurately and carefully by Messrs. Fleurieu and Bellevue, in the 2d Vol. of the Journal de Physique for 1798, the anhydrous sulphate of lime is mixed with interposed particles of quartz, which, according to the analysis made of it by Mr. Vauquelin, are in the proportion of $\frac{1}{100}$ to its whole mass. I am indebted to the friendship of Mr. Fleurieu for two specimens, which present two distinct varieties of this interesting stone.

One of them is of a good blue colour, is partly of a very fine sandy grain, and partly coarser and lamellated: it greatly resembles the carbonate of lime known by the name of saline marble. An immense number of small laminæ are observable in it, which cross each other in different directions, and are found by the magnifier to be perfectly rectangular.

The other is of a darker ash coloured grey; it is less pure than the former: some portions of a true gypseous earth are observed in it, containing a small quantity of argill and of carbonate of lime. Its substance is more compact than that of the

the preceding specimen; its laminæ are larger, their rectangular form is more perceptible, and several of them have a pearly reflection.

Of two other pieces of the same substance, in the cabinet of Other specimens. Mr. Greville at London, which appear to me to belong to the anhydral sulphate of lime of the salt-works in the canton of Bern, and both which are perfectly pure, the one is white with a little of a blueish cast, the other has the same blueish tinge, but deeper, and greatly resembles in colour the pale blue sapphire, known by the name of water sapphire. Both have a coarse granulated texture, and are composed of a combination of rectangular laminæ, which cross each other in different directions, as may easily be discerned with the naked eye. But the laminæ of that in which the blueish colour is the most intense, present besides so lively a pearly reflection, that it might easily, at first sight, be mistaken for a mass of zeolite stilbite. Rectangular laminæ, of some thickness, may be separated from it, which, like the crystals before described, are easily divided in every direction.

The specific gravity of the anhydrous sulphate of lime, Specific gravity. mixed with common salt, proved to be 2940. That of the specimens which I mentioned in Mr. Greville's cabinet, namely, that in which the laminæ had the strong pearly reflection, was 2957, and the other 2929. Of the two of Vulpino, the most compact was 2951, and the other 2933.

The hardness of this substance, in all the specimens, is some- Hardness. what superior to that of the carbonate of lime; in all of them also the anhydrous sulphate of lime is fusible by the blow-pipe without ebullition, and affords an opaque glass.

There is a marked difference between them, with respect to Phosphorescence. the phosphorescence, upon an heated shovel. The anhydrous sulphate of lime of Vulpino, gives a pretty strong orange-coloured light; that mixed with common salt, affords a very faint bluish light; that of the salt-works of Bern, none at all; and lastly, that which I mentioned as containing actinote, with attractive oxide of iron, &c. gives a light somewhat more reddish than that of Vulpino.

Gypsum deprived of water, or anhydrous sulphate of lime, The absence of water renders the properties altogether different. therefore, possesses characters altogether different from those of the sulphate of lime which contains that liquid; and as in these two stones the sulphuric acid and the lime exist in the

same proportions, it cannot be doubted but that the presence of the water totally changes the nature of the combination of acid and earth when it comes to be joined with it, which, it appears to me, can only take place so far as this water becomes itself an essential component part of the stone.

Remarks in proof that water is essential to the composition of many substances.

Arseniates of copper.

This is not the only substance which enables us to ascertain that the water, which formerly was considered as a part foreign to the stones which contained it, becomes in reality, in several of them, an ingredient essential to their nature. The analysis which Mr. Chenevix has made of the different species of arseniates of copper, have presented us a striking example of this, especially in that variety of the third species of my description (see Philosophical Transactions, 1801) to which I have given the name of Hematiform. A movement of decomposition, which is confined to the gradual loss of their constituent water, totally changes the colour of these arseniates, and at last completely discolours them, at the same time that it renders those that before had some feeble transparency, perfectly opaque. This loss of the water always commences at the exterior, and in this case the interior part preserves all its transparency as well as its colour, whilst the exterior part is discoloured, and exhibits by the shrinking of its surface, sometimes to a very considerable degree, sensible marks of the loss which it has sustained; the water amounting to about one-fifth of its mass*.

* In my description of the arseniates of copper quoted above, I have considered this hematiform arseniate, as well as those which I have designated by the terms of indeterminate, capillary, and amianthiform, only as being varieties of the species in the acute octahedral form: the copper and the arsenical acid are in fact contained in them in the same proportions; but the water, which adds a new constituent part to them, and which did not exist in those varieties of this third species, which are in perfectly determined crystals, forms with them a real hydrate of this third species. I therefore think that it would be proper to separate these varieties, in order to form with them a fifth species perfectly distinct from the third. The arseniate of copper is one of the most astonishing productions of the mineral kingdom, by the immensity of the aspects under which it presents itself, all which, nevertheless, have certainly a particular cause, which I am very far from pretending to have ascertained.

In the same manner it is, that, in the blue carbonate of copper, Blue carbonate of copper. which, according to the investigations of Mr. Proust, appears to contain a considerable quantity of water, far superior to that which exists in the green carbonate, the crystals seem to pass, at their surface, into the state of green carbonate, by the mere loss of a certain portion of their constituent water. This species of decomposition is sometimes even so considerable in them, that it exists, for example, amongst the specimens in large and superb crystals which come from Siberia, crystals, of which the form having undergone no change, belongs to the blue carbonate, but which have entirely passed, throughout their whole substance, into the state of green carbonate.

I am fully persuaded, that when water is once considered and admitted as a constituent part in the composition of numerous mineral substances, it will soon be acknowledged to contribute greatly, by its presence or its absence, to the difference which subsists between several stones: thus I am strongly inclined to believe that the carbonate of lime of slow solution, the hardness and specific gravity of which are so much superior to the same characters in the ordinary carbonate of lime, differs from this latter perhaps only by a difference in the water of composition. Most certainly this difference cannot proceed either from the presence of argil, or from that of magnesia. I know some dolomies which certainly do not contain any trace of argil, whilst, at the same time, I know carbonates of lime, which are highly charged with magnesia, and which, nevertheless, are very speedily dissolved in the acids: Of the number of these latter are, for example, most of the calcareous spars with a pearly reflection and greasy aspect.

I am much inclined also to think, that the species of Chalcedony, named cacholong, may owe its difference from the common chalcedony only to the circumstance, that the latter contains water of composition of which the chacolong is destitute; and that the transition of the chalcedony into this state, and subsequently into the hydrophanes, depends, in a great measure, upon the loss of this water. The probability of this opinion remains to be settled by future observations.

Water is of great importance as an ingredient which modifies the properties of bodies.

Cacholong probably owes its peculiar properties to the absence of water.

IX.

Analysis of Natural and Artificial Anhydrous Sulphate of Lime, by RICH. CHENEVIX, Esq. F. R. S. M. R. I. A. Communicated by the Author.

Component parts of common sulphate of lime. **T**HE proportion of the elements in common sulphate of lime, such as I have stated them in the Transactions of the Royal Irish Academy, are somewhat different from those given by former chemists. Mons. Fourcroy, in his "*Système des Connoissances Chimiques*," and in his "*Synoptic Tables*," has determined them in the following order: sulphuric acid 45, lime 32, water 22. If we deduct the quantity of water, and reduce the remainder to the quintal, we shall have the following proportions: sulphuric acid 58,5, lime 41,5. As the object of the present Paper is merely the Anhydrous sulphate, I shall not take further notice of the quantity of water that may be contained in common sulphate of lime, whether natural or artificial.

Lime dissolved in muriatic acid and sulphuric acid added, and the whole exposed to violent ignition, left dry sulphate of lime containing 56,3 lime, and 43,6 acid. I took one hundred parts of lime, prepared with the greatest care, and dissolved them in muriatic acid. I then poured sulphuric acid into the solution, and heated the whole in a platina crucible, at first gently, but afterwards to violent ignition. The augmentation of weight in the lime and the crucible, (which had been weighed before the operation) amounted to 78,5, and was combined sulphuric acid. This experiment therefore indicates, that strongly calcined sulphate of lime is composed of lime 56,3, and 43,6 of sulphuric acid.

Sulphate of barytes contains 24 acid in the 100 parts. I then took one hundred parts of calcined sulphate of lime, and decomposed them by oxalic acid, to render them more soluble, and then dissolved them in muriatic acid.

I precipitated the solution by muriate of barytes, and obtained 182 of sulphate of barytes. Hence it is evident that 182 of sulphate of barytes, and 100 of calcined sulphate of lime contain the same quantity of sulphuric acid = 43,6; which proportion gives 24 as the quantity of sulphuric acid, in 100 of sulphate of barytes. This quantity is nearly intermediate between that given by Thenard, and that which I had already stated. By this experiment I established a standard, to which I might refer every kind of sulphate of lime.

It is evident that calcined sulphate of lime, or artificial anhydrous sulphate of lime, contains 43,6 of sulphuric acid. It remains to prove its chemical identity, with the natural anhydrous sulphate.

I took one hundred parts of this substance, in as pure a state as the Count de Bournon could procure it, and submitted it to the same experiments. I obtained from this experiment 187 of sulphate of barytes, which announces 44,88. I shall never expect more uniform results in two analyses, even of the same substance, and do not hesitate to pronounce, that the two sulphates are, chemically speaking, one and the same thing.

The natural anhydrous sulphate of lime was treated like the artificial, and gave 44,88 acid.

The Abbé Hauy in his late work has given the proportions in anhydrous sulphate of lime, according to an analysis of Mons. Vauquelin. This excellent chemist found its elements to be nearly in the inverse order of my statement, and such as Fourcroy has established them for the artificial sulphate, deducting the water of crystallization. I should still have doubted the accuracy of my experiments, if I had not discovered a cause that may explain the difference which exists between his results and mine.

Vauquelin's experiments gave much more acid;

The French chemists have mentioned two varieties of sulphate of barytes, one of which contains 13 per cent. of sulphuric acid, and the other 33. If therefore we estimate the quantity of acid contained in sulphate of lime, by the sulphate of barytes containing 33, and not by that containing 24 per cent. we shall have a much greater quantity of sulphuric acid, as a constituent part of sulphate of lime.

most probably from his assuming a higher proportion of acid in the sulphate of barytes.

Among the specimens which the Count de Bournon gave me for trial, there were some which contained muriate of soda. This salt was easily extracted by water alone; and the proportion of it differed in different specimens. Klaproth had some specimens in which he found carbonate of lime, and even silica. But as I have examined some in which I can positively assert that there was neither the one nor the other, those substances may be looked upon as merely accidental.

Some specimens of the anhydrous sulphate contained muriate of soda;

but it was accidental.

X.

Abridgment of a Memoir of Mr. PROUST on Tanin and its Species.*

Uncertainty of the process of separating tanin by muriate of tin.

THE process which Mr. Proust has given for separating Tanin by muriate of tin is subject to considerable uncertainty when it is used, as an English Chemist has lately done to fix the proportion of this principle in vegetable juices. He thinks, therefore, that it may be of utility to announce these causes of error, in order that philosophers, who are engaged in this department of research, may place less confidence in it, and turn their thoughts to some more perfect method. To these remarks he adds an account of certain varieties which he thinks he has perceived in the genus of Tanin.

When the oxide of tin is seized by the tanin, the muriatic acid instead of being set at liberty dissolves part of the tannate.

1. When this muriate is saturated with an astringent juice, it happens that the muriatic acid takes up in solution a portion of the tannate of tin, so that what is collected on the filter represents, in truth, only a part of the tanin principle contained in the plant. This effect is similar to what happens in the preparation of ink, the black dye, and in every case where a dyeing principle can deprive an acid of the oxide which it held in solution. The affinity of this acid not being capable of remaining inactive exerts itself upon the coloured oxide.

This tannate may be thrown down by careful addition of alkali.

When small doses of alkali are added to the liquor the residue of the tannate may be made to precipitate without even touching the gallic acid if it be present; and if this point be exceeded it may soon be perceived by the green colour which the fluid receives from contact of the air. In that case a few drops of acid will be sufficient to dispel the cloud by seizing the excess of alkali and setting the gallic acid at liberty. But as, on the other part, it is necessary at first to employ an excess of muriate to assure the precipitation of the whole of the tanin, there is danger of loading the tannate with a certain excess of oxide of tin.

But the tannate will be overcharged with oxide if too much of the muriate has been used.

The muriate precipitates extractive matter and perhaps other principles along with the tanin.

2. The presence of tanin does not exclude the extractive principle in the juice of a plant, and as the muriate precipitates this last as well as the former, the extractive principle will therefore become a new source of mistake in the estimate of

* From the *Annales de Chemie* XLII. 89.

the quantity of tanin. It may also happen that the vegetable juices may contain many other substances capable of decomposing the muriate either directly or indirectly, so that this re-agent can never be depended upon with confidence.

There are besides earthy salts in these juices as Vauquelin has shewn. Some, as for example Sumac contain sulphate of lime in abundance. If therefore we use the alkali to complete the separation of the tannate, the precipitate will also be charged with an earthy deposition.

3. On reflecting upon the means of avoiding the errors caused by the muriatic acid, Mr. Proust tried a process which he had successfully used to separate the colouring principle from the gelatinous mucilage which accompanies it in cochineal. It consists in heating, or even simply agitating, the astringent juice with the oxide of tin prepared by nitric acid, and kept under water. The oxide in fact, becomes loaded with tanin in a few days. But if the juice of a plant which is not astringent, or a diluted extract, be treated in the same manner, it will also become deprived of the whole of its extractive matter: and the gum and the salt will remain alone in the fluid. This method cannot therefore lead us to our object.

4. What appears most surprising to this chemist in the present experiment is the destruction of the gallic acid, or probably its transition into a state in which it cannot perform the functions of that acid. The fluid in fact when cleared of the tannated oxide by the filter has no longer either colour or taste, and makes not the slightest impression on the solutions of iron, nor even upon turnsol. When examined by every trial the fluid is found to be mere water.

5. Suspecting, nevertheless, that this acid might be combined with the oxide of tin, he passed the tannate of the last described process into potash. The product was a coloured fluid, in which he found no sign of the gallic acid: for it did not assume by exposure to the air that green shade which it always indicates when saturated with an alkali. He precipitated the tanin by a diluted acid, a portion remaining in solution, as happens in this case and he proceeded to examine that which remained upon the filter; but he soon perceived that it had also advanced towards a state in which, as we shall shew, he found infinitely less tanin than before.

When earthy salts exist in the vegetable, these will let fall their bases when the alkali is added.

If the pure maximum oxide of tin be heated or agitated with tanin a combination takes place and, mucilage, if present, is left:

but this process will not separate the extractive matter.

Singular event in the last experiment. The gallic acid is destroyed.

When the tannate &c. was decomposed by potash no gallic acid was found and much less of tanin.

The tannin that is separated is not totally soluble in boiling water; neither does it precipitate glue, nor has it the taste or smell of tannin &c. It resembles extracts.

6. Boiling water cannot totally dissolve it. Its solution no longer precipitates glue; it has neither the harsh taste nor the odour of tannin. With the red sulphate of iron it affords only a whitish grey precipitate, and lastly it does not afford a magma with the muriate of tin; it is precipitated merely in the manner of extracts, to the taste of which it in some measure approaches though it does not possess their bitterness. These are the alterations to which the tanning principle is subject when combined as before mentioned with tin oxidized to the maximum.

It was suspected that the tannin had taken oxygen from the oxide; but experiments on the latter did not seem to indicate this loss.

7. From these changes he suspected that the tin might have yielded to it that portion of oxygen which constitutes the difference between the oxide at the maximum and the oxide at the minimum, as happens with the oxide of iron in ink, hermetically closed. In order to ascertain this he dissolved in muriatic acid the oxide what had been deprived of tannin by potash; but he discovered no indication of that kind. The solution produced no change in that of gold, nor in corrosive sublimate. It was at the maximum. It is true that with respect to the oxide of tin at the minimum, as well as that of iron, washing and exposing to the air speedily bring it to the maximum.

Theory of this conversion yet unknown.

Whether by oxidation or by whatever other process, the tannin principle did at length pass to the state of ordinary extract. Tannin precipitates glue, but extract does not; this is the difference between them. The influence of some affinity, which the author has not sufficiently developed, must have changed its radicals in their primordial arrangements or in their proportions; and it may be supposed that the gallic acid likewise after having been subjected to these changes became assimilated in the same state and by the same causes.

Concerning the Varieties of Tannin.

The genus *tannin* composed of species.

If in the series of immediate principles which compose the whole of vegetable matter we consider the tannin of galls as a genus, because this in fact possesses the qualities in the highest degree, it is easy to form a notion that this genus may have its species, and may, as well as sugar, gum, starch, &c. affect different modifications. There are various species of sugar, resin, gum &c. and there may be also various species of tannin. Mr. Proust thinks that he has found this to be the case.

Cachou, or Terra japonica.

Cachou is an astringent: it is soluble in alcohol and in water. It precipitates glue abundantly and forms with it a vinous (*q.*) magma which has neither the consistence nor the insolubility of the tannate of galls. Qualities of the various species of tannin.
Cachou or Terra japonica.

It reduces muriate of gold, it precipitates the muriate of tin and affords a violet coloured ink with red sulphate of iron. It is a tannin *sui generis*. It dyes silk.

Dragons Blood.

That which is pure and comes to us in calabashes is soluble both in water and in alcohol; its taste is harsh; it dyes silk of a bad wine colour. It abundantly precipitates glue, the muriate of tin, the red sulphate, and difoxides gold; it is also a species of tannin. Dragon's Blood.

Sumach.

The tannin which this substance contains, abundantly precipitates glue, and affords a white magma without consistence. Like tannin it is separated from the decoction of sumach by the carbonate of potash; its curdled deposition is again soluble in hot water with the exception of a small quantity of chalk. Sumach.

Barites and the oxalic acid demonstrate the existence of abundance of lime and sulphuric acid; but it will be necessary to ascertain by experiments on the green sumach whether sulphate of lime be one of its principles, or an adulteration. The author is not surprised at finding it, since he obtained this salt in considerable quantity from the juice of cabbage, and of *Solanum lycopersicon*, which is cultivated in the gardens under the name of *tomates*. This juice also contains gallic acid; it becomes green in the air when saturated with potash. It reduces gold, decomposes muriate of tin and the red sulphate, with which it affords a thick ink.

Yellow Wood, or Fustic.

It contains a species of tannin. Like tannin it precipitates the solution of glue; a solution of salt is sufficient to precipitate it. It is soluble in water and in alcohol. It reduces gold, decomposes muriate of tin and the red sulphate, by means of which it dyes silk of a greyish yellow. Yellow wood or fustic.

Fustic

*Fustet.***Fustet.**

It is a pure dyeing extract soluble in water as well as in alcohol. It contains a small quantity of gallic acid, but does not change the solution of glue. It reduces gold, precipitates the metallic salts, and has no gummy portion.

*Grains of Avignon, or French Berries.***Gr. d'Avignon, or French berries.**

They afford a dyeing extract of the same nature, without gum or tannin. It reduces gold, &c.

*Brazil Wood,***Brazil wood.**

Affords also a dyeing extract soluble in alcohol, without tannin, or gum, reducing gold and precipitating the metallic salts.

Reduction of gold is no exclusive character of tannin.

Mr. Proust has remarked, that the reduction of gold has ceased to be a characteristic quality, since he observed the muriatic solution abandon this metal to all the tinging substances, such as anise, cochineal, gum guttæ, gallic acid, verjuice, wine, vinegar, the juice of all fruits, manna, gum, and sugar, though somewhat slowly.

Concluding observations.

The author concludes by observing, that tannin has its varieties like the other immediate products; that the property of precipitating glue is the generic indication, by which they are distinguished from extracts, which do not alter that substance; and lastly, that the different species of tannin, particularly those which have been discovered in the barks of trees, cannot be compared together as to their force and their useful qualities, but by observations upon skins which have been submitted to their action.

Tanning matters require to be tried by actual preparation of leather.**Sulphate of lime very common in vegetables.**

Note. Plaster must be infinitely common in vegetables. Mr. Proust has found it in verjuice, grapes, apples, gooseberries, &c.

B. L.

XI.

Description of an Apparatus for heating Water by waste Steam.

Invented by Mr. ARTHUR WOOLF.

THE following apparatus was erected at the extensive Engine for heating water by steam. brewery of Messrs. Meux and Co. in August, 1800, and has been in use ever since. I saw it work a few weeks ago, and observed with great pleasure the facility and precision with which it operates, and I have great satisfaction in presenting it to the reader as a very judicious and useful combination.

Plate IX. A represents a steam pipe from the brewing Particular description. copper.

B a valve with its weight.

C the vessel in which the steam is condensed.

D a pipe that conveys the cold water from a reservoir.

E a conical valve through which the water is injected. It is connected with the lever F.

G is a bended pipe to prevent any of the steam from escaping with the hot water.

H a small receiver from which the hot water may be conveyed to different situations by means of pipes and cocks.

I a pipe open to the receiver to prevent a vacuum in case the water should be made to descend in any of the pipes.

K a small pipe to convey the steam into the regulator.

L the regulator which is composed of three cylinders, the outside and inside being closed together at bottom, leaving a cavity between, which is filled with water; the middle or moving cylinder is inverted and close at top. It serves for a piston, and is connected to the lever M, on which is a sliding weight N, by which the quantity and heat of the water may be varied at pleasure.

O is a valve through which the steam is let out when not used for heating water.

The effect of this engine may be easily understood. The Explanation of the manner in which it acts. weight of the inverted hollow piston L presses down the valve E by means of the levers, and this pressure may be regulated by fixing the weight N nearer or farther from the centre of the upper lever. When the steam through A has acquired a certain degree of strength in the vessel C, it raises the piston by its action through K, and consequently opens the valve E. A sheet of water immediately dashes through, as represented

in the figure, and by condensing the steam, suffers L again to descend; and, after a vibration or two, the effect of the steam to raise the piston and of the injection to depress it, balance each other, so that the levers remain nearly motionless. It is evident that the injection will be less, the steam stronger, and the water which passes off through G hotter the nearer the weight M is to the centre of motion. And in this respect the apparatus is so effectual that the water may be heated to 210 degrees, and the quantity that passes off is from 100 to 180 barrels per hour, according to the temperature, as governed by the position of the weight M.

XII.

Description of an improved Drawback Lock for House Doors. By Mr. W. M. BULLOCK. From the Transactions of the Society of Arts, who adjudged a Reward of Fifteen Guineas to the Inventor.

Copy of a Letter to the Secretary of the Society Mr. CHARLES TAYLOR.

S I R,

Inconveniences
and danger of
the common
drawback lock.

I HAVE herewith sent, for the inspection of the Society, an improved Drawback Lock for House Doors, &c. which improvement is in latching the door; for it is well known, particularly in damp weather, that the air drawing through it, rusts the head or bevel of the bolt, by which means it requires great force to shut the door, and occasions a disagreeable noise, besides shaking the building.

It has frequently happened that the house has been exposed to robbery from the door being left unlatched, when supposed to be fast. This improvement removes all those inconveniences, as it lets the bolt shoot into the staple immediately when the door closes, but not before; and the reliever works so very easy, that the door is made fast with one twenty-fourth part of the force required with locks upon the common construction.

By

By an experiment with the lock sent herewith, it will be proved that two ounces added to the reliever, will shoot the lock with more ease than three pounds will do, applied to the bevel bolt; and if the lock is rusty, the advantage will be much more in favour of the new method. I flatter myself it will be of great utility to the public, as its construction is simple and cheap. It may be added to any old lock, as may be seen from that now sent. It may be advantageously applied to French windows and glass doors, as it prevents the door from being strained, or the glass broke, by the force applied to shut them. I have fixed several locks, upon this new principle, which answer well; and if the invention meets with the approbation of the Society, I hope to be rewarded according to its merit.

Advantages of
the improved
lock.

I remain, with respect,

SIR,

Your most obedient Servant,

WILLIAM BULLOCK.

No. 6, Portland Street, Soho, May 5, 1801.

Plate X. Fig. 2. A. Is the new iron latch here affixed to an old common drawback house lock.

B. An iron pin at one end of the latch, on which pin it is moveable.

C. A projecting part of the latch, which, when the common spring bolt D of the lock is drawn back, in the usual manner, is forced into the nick on its higher part at E, by the spring F, underneath the latch.

The bolt D then remains within the lock, until, on closing the door, the reliever G gently presses on the lock box, fixed in the common way on the door cheek; which pressure draws the projecting part C out of the nick E, and permits the end of the bolt D, by the force of the spring G, to slide into the lock box, and fasten the door.

XIII.

Description of an improved Mill for grinding hard Substances.

By Mr. GARNETT TERRY. *From the Transactions of the Society of Arts, who adjudged the Silver Medal to the Inventor.*

Description of
an improved
mill.

MR. Terry, whose residence is No. 20, City Road, Finsbury Square, has constructed this mill on a large scale, and there is also a model deposited in the Society's collection.

Plate X. Fig. 1. A. The hopper, or receptacle of the articles which are intended to be ground.

B. A spiral wire, in the form of a reversed cone, to regulate the delivery of them.

C. An inclined iron plate, hung upon a pin on its higher end: the lower end rests on the grooved axis D, and agitates the wire B.

D. The grooved axis, or grinding cylinder, which acts against the channelled iron plate E.

F. A screw on the side of the mill, by means of which the iron plate E is brought nearer to or removed further from the axis D, according as the article is wanted finer or coarser.

G. The handle, by which motion is given to the axis.

H. The tube from whence the articles, when ground, are received.

* * * The front of the mill is taken off, in order to show its interior construction.

XIV.

Remarks on Dr. Thomson's Theory of Combustion. By C. R.
(Received June 15, 1802.)

General remarks
on Dr. Thom-
son's theory,
and the distinc-
tion between
combustion and
oxidation.

THE scientific world are so highly indebted to Dr. Thomson for many original communications, and for the very perspicuous manner in which he has explained many of the phenomena of chemistry, that every thing that is presented to the world under the sanction of his name, is intitled to much consideration: if in some instances we are induced to hesitate in the yielding of our assent, we cannot but do justice to the ingenuity of his reasonings, and at the same time acknowledge
the

the very luminous manner in which he conveys information, on every subject that he treats. The Paper under consideration is particularly an instance in point, and if we cannot go the full length with the author, we must at least acknowledge, that in the chief, his distinctions are accurate, and his reasoning just. Nothing can be more evident than the difference which in numberless instances prevails, between the act of oxygenation in bodies, and that of combustion, inasmuch as neither the phænomena attending them, nor the results arising therefrom, are the same. The French chemists, however, seem to have been misled, in their confining the term combustion to the act of oxygenation, by considering, that all bodies during their combustion combine with oxygen, without at the same time recollecting, that this latter effect may be produced without any of the phænomena usually attendant on combustion, and that though certainly all combustion presupposes the combination of oxygen with a base, yet this combination may, and repeatedly is effected where no combustion can possibly take place.

That a distinction therefore prevails between the two is obvious, and the Doctor offers us a theory, which he considers as sufficient to explain the different phænomena produced.— This theory it is the purpose of the following lines to shew however ingenious, and apparently satisfactory it may appear to be, is not wholly adequate to the task that is assigned to it. It will be necessary very shortly to state here the outline of the theory under consideration. In all cases says Dr. Thomson *, when heat and light are extricated during combustion, it will be found, that the light is furnished by the combustible or burning body, and the heat by the decomposition of the oxygen, which forms a component part of the supporter, and which is essential to the combustion, and that the distinction that prevails between the two processes of combustion, and of oxygenation, arises from the difference of the phænomena, which accompany the action of supporters and products upon other combustibles. “ The supporters convert these bodies into products, and combustion, or the emission of heat and light at the same time take place; whereas, the products convert combustibles into products, without any such emission.

Outline of the theory. That the light of combustion is furnished by the combustible body and the heat by the oxygen of supporters;

but that products convert combustibles into products by mere oxygenation without combustion.

* Philof. Journal, New Series, II. 10 and 92.

Now as the ultimate change produced on combustibles by both these sets of bodies is the same, and as the substance which combines with the combustible is the same, namely oxygen, it is evident, that the oxygen of the supporters contains something, which the oxygen of the products wants," and this something the Doctor supposes to be caloric. "In the same manner the combustibles and products resemble each other, the chief difference between them consisting, in the phenomena which accompany their combination with oxygen, in the one case fire is emitted, and in the other not." Now says the Doctor, "if we recollect, that no substance but a combustible is capable of restoring combustion to the base of a product, and that at the time of its doing so, it always loses its own combustibility, and further, that the base of a product does not exhibit the phenomena of combustion even when it combines with oxygen, we cannot avoid concluding that all combustibles contain an ingredient, which they lose when converted into products, and that this loss contributes to the fire, which makes its appearance during the conversion." This ingre-

Leading positions of the theory, 1. that light is originally an ingredient of combustibles, 2. and heat of oxygen.

Many reasons why the heat must come from the oxygen of the supporter.

dient the Doctor supposes to be light. It is evident, that the two leading positions of this theory are, 1. That during combustion, all combustibles emit light, which previously formed a necessary ingredient to their own composition; and secondly, That the heat which is evolved during the process of combustion, proceeds from the oxygen of the supporter, of which it likewise originally formed an essential ingredient.—That the heat given out during combustion comes from the decomposition of the oxygen of the supporter, there are many reasons for concluding. We know very well that no combustion will take place without the presence of oxygen, and that the greater the quantity of oxygen absorbed in a given time, the greater is always the heat that is evolved. Now if the heat be not supposed to come from the oxygen, why should the degree of heat given out, be always proportional to the quantity of oxygen that is absorbed, and upon what other principle can we so satisfactorily explain the effects that are produced by the Argand lamp. These considerations, combined with the argument drawn from the maintenance of the temperature of hot blooded animals by the decomposition of air, seem sufficiently to establish the truth of the foregoing position. There are, however, many difficulties that press against our implicit adoption of the other

Difficulties as to the other position, that the light invariably

other part of this theory, *viz.* that the light emitted during combustion invariably proceeds from the burning body, and that consequently it forms no part of the supporter. It of course then follows from this theory, that light is no essential part in the composition of oxygen gas. Let us however see whether this be the case. Many facts it will be found concur to prove that the contrary is the truth. If nitric acid be exposed to the light, after some time we find that it changes colour, it becomes yellow, green, and then red, and oxygen gas is disengaged, the nitric at the same time being converted into nitrous acid. Now it is evident, that as this decomposition is of a chemical nature, the light that occasions it, either combines with the oxygen to form oxygen gas, or with the acid to form nitrous acid: as we find no dissimilarity between the nitrous acid procured by this means, or that by any other, we are necessitated to conclude that the light has combined with the oxygen, and that the latter by the same means is converted into oxygen gas. Again, it is well known, if oxygenated muriatic acid be exposed to the rays of the sun in a transparent bottle, there is disengaged from it oxygen gas; in proportion as the gas is separated the acid loses its colour and odour, and returns to the state of simple muriatic acid.— Here it is evident, that the oxygen has passed from a concrete into a gaseous state from the combination of light, and we must therefore conclude that light is a component part of oxygen gas.

proceeds from
the burning
body.

For the light
which decom-
poses nitric or
ox. mur. acid is
concluded to
have combined
with the disen-
gaged oxygen.

If phosphorus be inserted in nitric acid, the latter is decomposed, and a product of combustion, namely phosphoric acid is formed, during which process neither heat nor light are given out. This process Dr. Thomson considers as an act of oxygenation, and not of combustion, because, says he, though a product of combustion is formed, a new supporter, namely nitrous gas is evolved, and the formation of a combustible, or new supporter, constitutes one of the characteristic differences between the two processes of combustion and oxygenation. Now it is said, that in all cases of oxygenation a double decomposition takes place, the oxygen of the product combines with the base of the combustible, while the light of the combustible combines with the base of the product. The question then naturally presents itself,—what during this process becomes of the light which made a component part of the

When phospho-
rus is acidified in
nitrous acid,
what becomes of
the light of the
combustible?

phosphorus previous to its conversion into a product? It cannot combine with the new supporter that is evolved, because it is a part of this theory, that light is no constituent part of supporters, but only of combustibles; it should therefore have been made evident to the senses, which we do not ever find to be the case, nor is any heat evolved; this latter effect is no doubt very easily explained, but what becomes of the light yet remains to be shewn.

Sulphurous and
sulphuric acids.

Sulphuric acid, says Dr. Thomson, is a substance which from many of its properties I conclude to be a combustible, and not a product. This conclusion, however, does not appear to be perfectly consistent with the definition the Doctor has given of combustion, for when sulphur is heated in the air to the temperature of 302 degrees, it gives out light and heat, and is converted into an acid, viz. sulphurous acid: this according to the theory under consideration is a complete act of combustion, and therefore only a product of combustion, and not a combustible body ought to be formed. Sulphurous acid, according to La Grange, combines slowly with oxygen, and is converted into sulphuric acid, but as no light and heat are rendered visible, ought it not in this case rather to be considered as an act of oxygenation; for if light and heat were evolved in this process, it should appear that combustibles are capable of giving out a part only of their light in some cases, and the whole in others, which does not appear very probable, for it cannot be doubted but that in sulphurous acid, the oxygen and the sulphur mutually saturate each other, and that sulphuric acid is only sulphurous acid combined with an additional dose of oxygen. Though the Doctor apparently reconciles the decomposition of water by iron or zinc with his theory, it yet appears to be attended with some difficulties which are not easily explained.—“Whenever, says he, the whole of the oxygen is abstracted from products, the combustibility of their base is restored as completely as before combustion, but no substance is capable of abstracting the whole of the oxygen from such products, except a combustible, or partial combustible. Water, for instance, is a product of combustion whose base is hydrogen; to restore the combustibility of the hydrogen, we have only to mix water with iron or zinc filings, when the metal is oxidated, and the hydrogen gas is evolved as combustible as ever.” Let us here attend to the phænomena which

Decomposition
of water by iron
or zinc.

which should take place according to the Doctor's theory; water, which is a product of combustion, is hidrogen without its light, in union with oxigen, without its heat; by adding iron (a combustible containing light) we decompose the water, that is to say, 73 parts of iron unite with 27 of oxigen. Now as to every 27 parts of oxigen in water there are about four of hidrogen, of course these four parts of hidrogen are liberated; but as it does not appear probable that combustibles should be capable of combining with light in all proportions, it may be asked, if the 73 parts of iron which are oxidated contain just light enough, and no more, to restore the combustibility of the four parts of hidrogen; for if there be too much for that purpose, the superabundant quantity ought to become visible, and if too little, a part only of the hidrogen should recover its combustibility, and be converted into gas. And the same reasoning may of course be urged with regard to the decomposition of water by zinc: for it cannot but be acknowledged, that the fact is somewhat singular, that the product of combustion should always contain and give out the precise quantity of light which is sufficient to restore combustibility to the base of the product, and in no case either more or less. Thus it appears that there are many difficulties that attend our implicit assent to the present theory, and many of the phænomena of combustion that do not apparently admit from it of an easy interpretation. Whether Dr. Thomson can reconcile these apparent anomalies to it, remains to be seen; but if it is found equal to their solution, there could then it should seem be little objection to its adoption. At all events, no one will be inclined to dispute, that the Doctor has thrown much light on a subject, which before its investigation by him, was considerably more obscure: and that he has placed it in a new point of view, which bids fair to enable us to approximate much nearer to a true theory with regard to the phænomena of combustion, than any other that has hitherto prevailed.

Numerically considered as to the transition of precisely adequate portions of light from combustible to combustible, &c.

Conclusion.

Dr. T. has thrown great light on this obscure subject.

C. P.

XV.

On certain Points of Nomenclature. By a Correspondent.

To Mr. NICHOLSON.

S I R,

On the use of *y*
in English for
the Greek
vowel *v*.

APPREHENDING that your mode of writing certain newly imposed names of substances in chemistry, arises from inattention, and being misled by the French, I take the liberty of a friend to remind you, that, in English, it is not usual to write the vowel *i* for the *v* of the Greeks, but *y*; hence, in our language, we do not write *oxigen*, *hidrogen*, *oxigenised*, &c. but *oxygen*, *hydrogen*, *oxygenised*, &c. : you write, however, properly, *oxide* instead of *oxyd*, as some persons spell the word; because *oxide* shews the etymology in $\alpha\xi\upsilon\varsigma$ and $\epsilon\iota\delta\omicron\varsigma$ better than *oxide*.

Your's ever faithfully,

A. B. C.

ANSWER.

I DO not profess to have directed much attention to the subject of nomenclature; though I am well aware of its importance to the acquisition, as well as the communication of knowledge. It is, therefore, with considerable diffidence that I state my apprehension, that neither usage in a language, nor the motive of precisely indicating the derivation of a term, are very cogent arguments for adopting any particular mode of structure, if other motives present themselves. To me it seemed at least as forcible a reason for the use of *i* instead of *y*, in the words alluded to, that, together with their derivatives, they are so very numerous, as to make it desirable to accommodate them to the general usage of the modern languages; and this appeared to be promoted by following the change proposed by the framers of the chemical nomenclature.

XVI.

Duplicate Copy of a Letter from BARON DE ZACH to the Right Honourable Sir JOSEPH BANKS, Bart. P. R. S. &c. transmitted to Mr. EDWARD TROUGHTON, and communicated by the Rev. J. PEARSON; on the new Planets Ceres and Pallas, with the Elements of the Orbit of the former.

Seeberg Observatory, near Gothu, May 31, 1802.

MOST HONOURED SIR,

HAVING profecuted Dr. Olber's *Pallas* from April 4 till May 11, in the meridian, Dr. Gauß, upon this set of my observations, calculated the elements of an elliptical orbit of this very remarkable heavenly body, which represent, with great accuracy, all the Seeberg observations.

It appears, in general, by these calculations, that *Pallas* is a planetary heavenly body, that moves between the orbits of Mars and Jupiter, with a very great eccentricity and inclination, and whose orbit comes very near to the orbit of the planet Ceres, perhaps touches it, perhaps even cuts it, like two links of a chain, this way ∞ , which cannot yet be asserted with certainty, the observed arc run over by this planet being too small. Notwithstanding, it appears already that the distances of *Pallas* and *Ceres*, in the line of nodes of their orbits, is very nearly equal. In the descending node, the distance of *Pallas* from the Sun is = 2,86, and the same distance of *Ceres* = 2,93. In the ascending node, these distances are of greater inequality. Another very remarkable circumstance is, that the mean motions of *Pallas* and *Ceres* are very nearly, perhaps absolutely the same; though this cannot yet be asserted, because the error of the observations of both planets is still too great. But as far as yet appears, these mean motions will not differ very much; and in this case, small as the masses of *Ceres* and *Pallas* may be, they will nevertheless exert a very sensible action one upon the other, and therefore give occasion to very curious and interesting investigations in the mechanics of the heavens. The new planet *Pallas* will also call forth the utmost exertion of our analytical powers. Hitherto the two elements of a planetary orbit, viz. the eccentricity and the inclination, had been considered as an infinite little quantity, and so it might be, as these

Orbit of *Pallas* very eccentric, and probably linked in that of *Ceres*.

Interesting remarks on the singularity of the orbit of *Pallas*.

two elements, in all our old planets, are very small, so that the higher powers of them could be neglected without danger, in calculating their mutual action, as they produced no sensible term in the approximating series. But this is no longer the case with Pallas, in which the eccentricity of the orbit and the inclination are so very great.

Elements of
Pallas,

*Elements of the Orbit of the Planet Pallas, calculated by
Dr. Gauss.*

Epocha, March 31, at noon, in Seeberg,	161° 12' 43,"2
Aphelium	300 5 4, 0
Node	172 34 35, 0
Inclination	35 0 42, 0
Mean daily heliocentric and tropical motion	757" 166
Logarithm of half the greater axis	0,4472636
Eccentricity	0,2591096

By these elements the whole series of my observations are represented.

TABLE.

Computed and
observed places.

1802.	R.A. by Calculation.	Difference.	Decl. by Calculat.	Difference.
Apr. 4	183° 44' 5,"7	— 0,"9	13° 54' 55,"4	+ 3,"4
5	183 34 24, 6	+ 0, 9	14 13 22, 6	— 0, 3
7	183 15 39, 6	+ 1, 6	14 49 1, 3	— 0, 7
8	183 6 35, 4	— 2, 4	15 6 15, 2	+ 5, 2
15	182 10 17, 9	+ 1, 4	16 54 35, 7	+ 4, 9
18	181 50 29, 4	— 1, 2	17 34 27, 1	—
19	181 44 30, 7	+ 5, 4	17 46 51, 9	— 2, 5
24	181 19 35, 8	— 2, 0	18 42 27, 9	—
25	181 15 38, 3	+ 6, 1	18 52 18, 9	—
26	181 12 2, 0	+ 0, 2	19 1 44, 9	— 4, 5
27	181 8 46, 6	+ 1, 0	19 10 46, 3	— 0, 5
29	181 3 21, 2	+ 4, 6	19 27 36, 2	— 7, 5
30	181 1 11, 4	+ 1, 5	19 35 24, 9	— 8, 6
May 1	180 59 22, 9	+ 0, 4	19 42 50, 8	+10, 0
2	180 57 56, 7	+ 4, 8	19 49 53, 2	+ 1, 6
3	180 56 52, 4	— 3, 2	19 56 32, 9	+14, 8
5	180 55 49, 6	— 5, 0	20 8 45, 6	+ 6, 9
6	180 55 51, 4	— 2, 7	20 14 19, 4	—
7	180 56 14, 9	— 8, 9	20 19 32, 1	— 3, 2
8	180 57 0, 4	— 9, 6	20 24 23, 7	+ 3, 6
11	181 1 25, 5	—16, 2	20 36 58, 8	—14, 4

The differences applied with contrary sign to the calculated A. R. and Declin. will give the observed A. R. and Decl.

Pallas

Pallas and Ceres are now too near to the sun, and the twilight permits no meridian observations. But astronomers who are provided with equatorials of great perfection, as, for instance, those of Greenwich, Oxford, Richmond, and of Sir George Shuckburg, will be able to follow these two planets a longer time. The observation of Pallas will chiefly be of a very great value, as the series of meridian observations is not for above five weeks. If more observations are not procured, it will be with some difficulty we shall find Pallas again next year; for the elements of an orbit calculated upon so small an arc as $7\frac{1}{2}^{\circ}$, may give an error of several degrees in January 1803. You will do, most honoured Sir, a great benefit to science in general, and to astronomy in particular, if you engage the English astronomers, who have so very excellent and fixed equatorial sectors, to follow Pallas out of the meridian as far as they can. For this purpose, I take the liberty to send you here an ephemeris of this planet's motion, calculated by Mr. Gauss, which will enable astronomers to find it, and pursue their observations.

These planets
are now in the
twilight.

*Ephemeris of the Position of Pallas for Midnight, in Seeberg
Observatory.*

Ephemeris of
Pallas.

1802.	R. Ascens.	Declin.
May 24	181° 57'	21° 1' N.
27	182 18	21 0
30	182 41	20 57
June 2	183 6	20 52
5	183 34	20 46
8	184 5	20 38
11	184 37	20 28
14	185 12	20 17
17	185 48	20 5
20	186 27	19 52
23	187 7	19 37
26	187 49	19 22
29	188 32	19 6

I am, with the greatest esteem and regard,

MOST HONOURED SIR,

Your most humble and
obedient servant,

FRANCIS BARON DE ZACH.

XVII. Method

XVII.

*Method of applying a temporary Forcer to a Pump, so as to produce a constant Stream. By Mr. RICHARD TREVITHICK *.*
From the Author.

Additional forcer to a common pump producing a constant stream.

Description.

THIS contrivance which, on several occasions, may prove useful, consists in fixing a barrel with a solid piston alongside of the common pump, in such a manner, that the lower space of the additional barrel may communicate with the space between the two valves of the pump, and lastly, by connecting the rods so that they may work together. This is shewn in fig. 4, plate XI. and the effect is, that when the pistons are raised, the spaces beneath, A and B, become filled by the pressure of the atmosphere, at the same time that the upper column flows out at E. But again, when the pistons descend, the valve C shuts, and, consequently, the water driven by the piston in B must ascend through A, and continue to produce an equal discharge through E in the down stroke.

XVIII.

Experiments and Observations on certain stony and metalline Substances, which at different Times are said to have fallen on the Earth; also on various Kinds of native Iron. By EDWARD HOWARD, Esq. F. R. S. From the Philosophical Transactions, 1802.

Stony and metalline substances have undoubtedly fallen on the earth.

THE concordance of a variety of facts seems to render it most indisputable, that certain stony and metalline substances have, at different periods fallen on the earth. Whence their origin, or whence they came, is yet, in my judgment, involved in complete obscurity.

The accounts of these peculiar Substances, in the early annals, even of the Royal Society, have unfortunately been blended with relations which we now consider as fabulous; and the more ancient histories of stones fallen from heaven,

* This Gentleman's name was, by mistake, printed Trevithack, in Vol. I. 161.

from Jupiter, or from the clouds, have evidently confounded such substances with what have been termed *Ceraunia*, *Bætilia*, *Ombria*, *Brontia*, &c. names altogether unappropriate to substances fallen on our globe. Indeed some mislead, and others are inexpressive.

Why these facts have been discredited.

The term *Ceraunia*, by a misnomer, deduced from its supposed origin, seems, as well as *Bætilia* *, to have been anciently used to denote many species of stones, which were polished and shaped into various forms, though mostly wedge-like or triangular, sometimes as instruments, sometimes as oracles, and sometimes as deities. The import of the names, *Ombria*, *Brontia*, &c. seems subject to the same uncertainty.

In very early ages, it was believed, that stones did in reality, fall, as it was said, from heaven, or from the gods; these, either from ignorance, or perhaps from superstitious views, were confounded with other stones, which, by their compact aggregation, were better calculated to be shaped into different instruments, and to which it was convenient to attach a species of mysterious veneration. In modern days, because explosion and report have generally accompanied the descent of such substances, the name of thunderbolt, or thunderstone, has ignorantly attached itself to them; and, because a variety of substances accidentally present, near buildings and trees struck with lightening, have, with the same ignorance, been collected as thunderbolts, the thunderbolt and the fallen metalline substance have been ranked in the same class of absurdity. Certainly, since the phenomena of lightening and electricity have been so well identified, the idea of a thunderbolt is ridiculous. But the existence of peculiar substances fallen on the earth, I cannot hesitate to assert: and on the concordance of remote and authenticated facts, I shall rest the assertion.

Thunderbolt.

Mr. King, the learned author of *Remarks concerning Stones said to have fallen from the Clouds, in these Days and in ancient Times*, has adduced quotations of the greatest antiquity, descriptive of the descent of fallen stones; and, could it be thought necessary to add antique testimonies to those instanced by so profound an antiquarian, the quotations of Mons. Falconet, in his papers upon *Bætilia*, inserted in the *Histoire des Inscriptions et Belles-Lettres*; † the quotations in Zahn's

Ancient authorities of stones fallen on the earth. King Falconer, &c.

* Mercati, *Metallotheca Vaticana*. page 241.

† Tom. VI. P. 519. et Tom. XXII. P. 228.

Specula Physico-mathematica Historiana;* the *Fisica Sotterranea* of Giacinto Gemma; the works of Pliny, and others might be referred to.

Dr. Chaldni on
the Siberian Iron
&c.

Doct^r Chaldni, in his *Observations on the Mass of Iron found in Siberia, and on other Masses of the like Kind*, as well as in his *Observations on Fire-balls and hard Bodies fallen from the Atmosphere*, has collected almost every modern instance of phenomena of this nature.

M. Southey a
stone wt.
10lbs 1796.

Mr. SOUTHEY relates an account, juridically authenticated, of a stone weighing 10 lb. which was heard to fall in Portugal, Feb 19, 1796, and was taken, still warm, from the ground.†

Abbé Bachelay
1768.

The first of these peculiar substances with which chemistry has interfered, was the stone presented by the Abbé Bachelay to the Royal French Academy. It was found on the 13th of September, 1768, yet hot, by persons who saw it fall. It is described as follows:

“ La substance de cette pierre est d’un gris cendré pâle; lorsqu’on en regarde le grain à la loupe, on apperçoit que cette pierre est parsemée d’une infinité de petits points brillans métalliques, d’un jaune pâle; sa surface extérieure, celle qui, suivant M. l’Abbé Bachelay, n’étoit point engagée dans la terre, étoit couverte d’une petite couche très-mince d’une matière noire, boursoufflée dans des endroits, et qui paroissoit avoir été fondue. Cette pierre, frappée dans l’intérieur avec l’acier, ne donnoit aucune étincelle; si on frappoit, au contraire, sur la petite couche extérieure, qui paroissoit avoir été attaquée par le feu, on parvenoit à en tirer que’ques-unes.”

The specific gravity of this stone was as 3535 to 1000.

Analyzed,

The academicians analyzed the stone and found it to contain.

Sulphur	-	-	-	-	8½
Iron	-	-	-	-	36
Vitrifiable earth	-	-	-	-	55½
					100.

* Fol. 1696, Vol. I. p. 385. where a long enumeration of stones fallen from the sky is given.

† Letters written during a short residence in Spain and Portugal, p. 229.

Of their mode of analysis, I shall have occasion to speak hereafter. They were induced to conclude, that the stone, presented to the Academy by the Abbé Bachelay, did not own its origin to thunder: that it did not fall from heaven; that it was not formed by mineral substances fused by lightning: and that it was nothing but a species of pyrites, without peculiarity, except as to the hepatic smell disengaged from it by marine acid “ Que cette pierre, qui peut-être étoit
 “ couverte d’une petite couche de terre ou de gazon, aura
 “ été frappée par la foudre, et qu’elle aura été ainsi mise en
 “ évidence: la chaleur aura été assez grande pour fondre la
 “ superficie de la partie frappée, mais elle n’aura pas été assez
 “ long-tems continuée pour pouvoir pénétrer dans l’intérieure
 “ c’est ce qui fait que la pierre n’a point été décomposée.
 “ La quantité de matières métalliques qu’elle contenoit, en
 “ opposant moins de résistance qu’un autre corps au courant
 “ de matière électrique, aura peut-être pu contribuer même à
 “ déterminer la direction de la foudre.”

Conjecture that it might have been pyrites, struck in preference by lightning.

The Memoir is however concluded, by observing it to be sufficiently singular, that M. Morand le Fils had presented a fragment of a stone, from the environs of Coutances, also said to have fallen from heaven, which only differed from that of the Abbé Bachelay, because it did not exhale the hepatic smell with spirit of salt. Yet the academicians did not think any conclusion could be drawn from this resemblance, unless that the lightning had fallen by preference on pyritical matter.*

Another stone from Coutances,

Mons. Barthold, Professeur à l’Ecole centrale du Haut-Rhin, gave I believe the next, and last, † analytical account of what he also denominates *Pierre de Tonnerre*. He describes it thus: “ La masse de pierre connue sous le nom de Pierre de
 “ Tonnerre d’Ensisheim, pesant environ deux quintaux, a la
 “ forme extérieure arrondie, presque ovale, raboteuse, d’un
 “ aspect terne et terreux.

Barthold’s examination of a stone called thunder stone weighing two quintals.

“ Le fond de la pierre est d’une couleur grise bleuâtre,
 “ parsemée de cristaux de pyrites, isolés, d’une cristallisation
 “ confuse, en quelques endroits écailleuses, ramassés, formant

Description.

* See Journal de Physique, Tom. II. p. 251.

† A very interesting detail of a meteor, and of stones fallen in July 1790, was given by Professeur Baudin, in the *Magazin für das Neueste aus der Physik*, by Professor Voigt.

“ des nœuds et des petites veines, qui le parcourent en tout
 “ sens : la couleur des pyrites est doré ; le poli leur donné un
 “ éclat d’acier, et, exposées à l’atmosphère, elles ternissent
 “ et brunissent. On distingue de plus, à l’œil nud, de la mine
 “ de fer grise, écailléeuse, non sulfureuse, attirable à l’aimant,
 “ dissoluble dans les acides, peu oxidé, ou s’approchant beau-
 “ coup de l’état métallique.

Fracture.

“ La cassure est irrégulière, grenue, d’un grain un peu
 “ ferré : dans l’intérieur on voit de très petites fentes. Elle ne
 “ fait pas feu au briquet : sa contexture est si lâche qu’elle se
 “ laisse entamer au couteau. En la pilant, elle se réduit assez
 “ facilement en une poudre grise bleuâtre d’une odeur terreuse.
 “ Quelquefois il se trouve de petits cristaux de mine de fer,
 “ qui résistent plus aux coups du pilon.”

The specific gravity of the piece in Professor Barthold’s possession, was 3233, distilled water being taken at 1000.

Composition.

The analysis of Mons. Barthold, of which I shall also have occasion to speak hereafter, gave in the 100.

Sulphur	-	-	-	-	2
Iron	-	-	-	-	20
Magnesia	-	-	-	-	14
Alumina	-	-	-	-	17
Lime	-	-	-	-	2
Silica	-	-	-	-	42

97

From the external characters, and from his analysis, the Professor considers the stone of Ensisheim to be argillo-ferruginous ; and is of opinion that ignorance and superstition have attributed to it a miraculous existence, at variance with the first notions of natural philosophy.*

Sir W. Hamilton’s account of stones that fell during a thunder storm.

The account next in succession is already printed in the Transactions of the Royal Society ; but cannot be omitted, as it immediately relates to one of the substances I have examined. I allude to the letter received by Sir William Hamilton, from the Earl of Bristol, dated from Sienna, July 12th, 1794. “ In the midst of a most violent thunder-storm, “ about a dozen stones, of various weights and dimensions, “ fell at the feet of different persons, men, women, and “ children. The stones are of a quality not found in any part

* See Journal de Physique. Ventose, An. 8. p. 169.

“ of the Siennese territory ; they fell about eighteen hours
 “ after the enormous eruption of Mount Vesuvius : which
 “ circumstance leaves a choice of difficulties in the solution
 “ of this extraordinary phenomenon. Either these stones
 “ have been generated in this igneous mass of clouds, which
 “ produced such unusual thunder ; or, which is equally incre-
 “ dible, they were thrown from Vesuvius, at a distance of at
 “ least 250 miles ; judge then of its parabola. The philoso-
 “ phers here incline to the first solution. I wish much, Sir,
 “ to know your sentiments. My first objection was to the
 “ fact itself ; but of this there are so many eye witnesses, it
 “ seems impossible to withstand their evidence.” (Phil. Trans.
 for 1795. p. 103.) Sir William Hamilton, it seems, also
 received a piece of one of the largest stones, which weighed Weight.
 upwards of five pounds ; and had seen another, which weighed
 about one. He likewise observed, that the outside of every
 stone which had been found, and had been ascertained to have
 fallen from the clouds near Sienna, was evidently freshly
 vitrified, and was black, having every sign of having passed
 through an extreme heat ; the inside was of a light grey
 colour, mixed with black spots and some shining particles,
 which the learned there had decided to be pyrites.

(To be continued.)

SCIENTIFIC NEWS, &c.

Dimensions and Nature of the New Planets Ceres and Pallas.

By Dr. HERSCHEL.

DR. HERSCHEL'S observations on the two lately discovered celestial bodies were read before the Royal Society, 6th of May.

Dr. Herschel begins with stating the result of his attempts to measure the diameter of the stars discovered by Piazzi and Olbers. He employed the lucid disc micrometer, which consists of an illuminated circle viewed with one eye, while the other compares with it the magnified image formed by the telescope ; and he concludes, that the apparent diameter of Ceres was 22", and of Pallas 17" or 15", at the distance of nearly 1.634, and 1.187 from the earth respectively, whence the apparent diameters at the distance of the earth from the sun would be .35" and .21" or .16" respectively, and that their

Observation of
the diameters of
Ceres and Pallas.

real

Ceres about 163 English miles
and Pallas 95 or 71.

real diameters are about 163 and 95 or 71 English miles. There is no probability that either of these stars can have a satellite. The colour of Ceres is more ruddy than that of Pallas. They have generally more or less of a haziness, or coma, but sometimes, when the air is clear, this nebulosity scarcely exceeds the scattered light surrounding a very small star. From a view of all these circumstances, Dr. Herschel proceeds to consider the nature of the new stars. He thinks that they differ from the general character of planets, in their diminutive dimensions, in the great inclination of their orbits, in the coma surrounding them, and in the mutual proximity of their orbits; that they differ from comets in the want of eccentricity, and of a considerable nebulosity. Dr. Herschel therefore, wishes to call them asteroids, a term which he defines as a celestial body, which moves round the sun in an orbit either little or considerably eccentric, of which the plane may be inclined to the ecliptic in any angle whatever, the motion being either direct or retrograde, and the body being surrounded or not by a considerable atmosphere or a very small coma. This definition is intended to include such other bodies of the same kind as, Dr. Herschel supposes, will, in all probability, be hereafter discovered. Some additional observations show, that the apparent comas surrounding Ceres and Pallas, scarcely exceed those which are caused by aberration round the images of minute fixed stars.

Attempt to constitute a new class of celestial bodies.

J. of the Royal Institution.

Extract of a Letter from the Rev. James Wilson, D. D. Minister of Falkirk.

Falkirk, Stirlingshire, June 18th, 1802.

Durability of silk buried in the earth.

A few weeks ago the sexton of this parish, upon opening a grave in the church yard, found a ribband wrapped about the bone of an arm, which upon being washed was found to be entire, and to have suffered no injury, though it had lain for more than eight years in the earth; and had been in contact with a body which had passed through a state of corruption, and was reduced to its kindred dust. By what means did the silk resist the putrefactive process? it is not a compact substance like hair, horn, or bone, which are frequently found in graves after every other substance is completely changed. As silk is deprived of the gummy matter by the act of cleaning and scouring, and as this seems to be the chief animal sub-

stance

stance which it contains, may not the remaining fibrous part be the better prepared to withstand the power of putrefaction? Accurate experiments tending to illustrate this enquiry might be both amusing and instructive.

ACCOUNT OF BOOKS OF SCIENCE.

Memoirs of the Literary and Philosophical Society of Manchester, Vol. V. Part II. Octavo, 700 Pages, with 9 Plates. Cadell and Davies, London, 1802.

THIS publication of the respectable Society of Manchester is no less interesting than the former volumes of which the scientific world well knows the value; it contains the following memoirs:—1. On tragedy, and the interest in tragical representations: An Essay. By the Rev. George Walker, F. R. S. and Professor of Theology in the new College, Manchester.—2. Experiments and observations to determine whether the quantity of rain and dew is equal to the quantity of water carried off by the rivers and raised by evaporation; with an inquiry into the origin of springs. By Mr. John Dalton.—3. Experiments and observations on the power of fluids to conduct heat; with reference to Count Rumford's seventh essay on the same subject. By Mr. John Dalton.—4. Experiments on the velocity of air issuing out of a vessel in different circumstances; with the description of an instrument to measure the force of the blast in bellows, &c. By Mr. Banks, Lecturer in Natural Philosophy. Communicated by Mr. Dalton.—5. Essay on the beautiful in the human form; and enquiry whether the Grecian statues present the most perfect beauty of form that we at present have any acquaintance with. Communicated to the Society from a Correspondent, through the Rev. George Walker.—6. A defence of learning and the arts, against some charges of Rousseau: In two essays. By the Rev. George Walker, F. R. S.—7. Observations on the nervous systems of different animals; on original defects in the nervous system of the human species, and their influence on sensation and voluntary motion. By John Hill, M. D.—8. Experiments and observations on the heat and cold produced by the mechanical condensation and rarefaction of air.

By

By Mr. John Dalton.—9. Account of some antiques lately found in the river Ribble. By Mr. Thomas Barritt.—10. Experimental essays on the constitution of mixed gases; on the force of steam or vapour from water and other liquids in different temperatures, both in a torricellian vacuum and in air; on evaporation; and on the expansion of gases by heat. By Mr. John Dalton.—11. A review of some experiments, which have been supposed to disprove the materiality of heat. By Mr. William Henry.—12. An investigation of the method whereby men judge, by the ear, of the position of sonorous bodies relative to their own persons. By Mr. John Gough. Communicated by Dr. Holme. 13. On the theory of compound sounds. By Mr. John Gough. Communicated by Dr. Holme.—14. Meteorological observations, made at Manchester. By Mr. John Dalton.—Appendix, I. Explanation of a Roman inscription, found in Castle-field, Manchester. By Mr. Thomas Barritt. With a note on the same subject, by Dr. Holme.—II. Note to Mr. W. Henry's paper on heat.

THE Rev. Thomas Falconer, A. M. of Bath, proposes to print by subscription, the Geography of Strabo, in seventeen books: translated from the Greek text; illustrated by maps, coins, inscriptions, &c. accompanied with the notes of the older editors, and of the later; those of Thomas Falconer, Esq. of Chester, the Oxford editor, entire; of Siebenkees, and Tzschucke, of Germany; and those of the translator.

The conditions are—1. The work will be printed in a handsome manner, with foot notes: 2. It will be contained in three volumes quarto, if possible: 3. The price will depend upon the Rate of paper when the work shall be put to press; but it is hoped that four guineas will be the largest estimate: 4. Two guineas to be paid at the time of subscribing, for which a receipt shall be given, and the remainder when half the work is delivered to subscribers: 5. The work will not be sent to press till three hundred copies are engaged, and only five hundred will be printed. Subscriptions received by Messrs. Cruttwell, and Bull, Bath; Cooke, Hanwell, and Parker, Oxford; Cadell and Davies, London; and Manners and Millar, Edinburgh.

ERRATA.

In Mr. Chenevix's paper, p. 114, for *sulphurated* read every where *sulphuretted*.

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

AUGUST, 1802.

ARTICLE I.

*On Granite. By Mr. ROBERT JAMESON. Communicated by
the Author.*

Sheriff Brae; Leith, July 10, 1802.

THE primitive rocks, of which granite is the oldest, were formed during that period which Werner terms the *chaotic*, when the earth was still covered to a great height with water, and before organization had commenced. Their structure shews that they have been deposited from a state of chemical solution *, and the diminishing level of the newer strata, that

Primitive rocks formed during the chaotic state by deposition from an aqueous solvent.

* To the idea of all fossil substances having been in a state of chemical solution in water, it has been objected, that many of them are intirely insoluble in water. To this, without adducing any of the numerous geological proofs, I answer, that we know not the original state of the different earthy and metallic substances; the artificial means we employ to procure them, may, and certainly has altered many of them from their original state.---J.

It may also be remarked, that many insoluble compounds are deposited by the chemical action of bodies which were soluble before they met. Our earths may be such compounds.---N.

the water has sunk gradually and calmly. They usually occupy the higher parts of the globe, but when covered with newer strata may form the lowest. The rocks which Werner considers as belonging to this great class are, granite, gneiss, mica slate, primitive slate, porphyry, and sienite.

Granite.

As granite is in many respects one of the most important of these formations, I shall here detail several interesting particulars respecting it.

On the name granite.

Pliny, and the older writers, describe this rock under different names; the term granite appears to be of modern date, as Montfaucon is the first writer who uses it. This will not surprize us, when we consider, that it was not until the revival of letters when the remains of antiquity began to be studied, that the different rocks received particular denominations. To Werner we are indebted for the most exact description; before his time it was confounded with sienite and grunstone, two rocks that differ both in their oryctognostical and geognostical characters.

Granite is an aggregate, granular, primitive rock, which is composed of felspar, quartz, and mica.

Aggregate component parts of granite; felspar, quartz, and mica.

Felspar is generally the prevailing, and mica the least considerable ingredient. The felspar has a considerable range of colour; the principal colours are white, grey, red, and sometimes, though rarely, green: it is found in all the intermediate states, from very great to very fine grain. The quartz and mica are generally grey, and the first has sometimes a black colour. Several fossils occur in granite besides those we have just mentioned; these are stonr and garnet: such varieties have received particular names, but the geognost views them as accidental, and does not take particular notice of them. The topaz, which is distinguished from all other precious stones (excepting the emerald and garnet) by its occurrence in primitive mountains, is found accompanied with apatite in the tin beds which lie in granite at Ehrenfredersdorf.

Structure. Porphyritic.

Its *structure* is not subject to much variety. When crystals of felspar are immersed in a basis of fine grained granite, it constitutes what is termed porphyritic granite. Of this there are fine examples near to Carlsbad in Bohemia, and in many places in the north of Scotland. It sometimes occurs in glo-

Globular.

bular

bular distinct concretions *, and these are again composed of concentric lamellar, distinct concretions. This structure of granite is only to be discovered, after the softer granite has weathered out; then these concretions which are vastly harder, and are only separated from each other by the looser granite, make their appearance. Upon the road between Dresden and Bautzen I observed many fine examples of this structure of granite: Mr. Barraud, in his interesting description of the Cape of Good Hope, mentions several globular distinct concretions of immense size. In Scotland the island of Arran affords instances of this kind †.

It is frequently observed distinctly stratified: in other instances owing to the thickness of the strata, this structure is difficultly observable, and has given rise to the opinion that such granite is not stratified. The Riesengebirge, which separate Silesia from Bohemia, are for 150 miles composed of granite, disposed in horizontal strata. Last summer I examined these mountains along with a consummate mineralogist, Dr. Mitchell, and we convinced ourselves of the truth of this observation. I have observed similar stratification in Saxony and Lusatia.

It is an interesting fact in the natural history of granite, that it seldom contains extraneous beds, and Werner remarks, that the frequency of such beds increases with the newness of the formation: thus gneiss contains fewer beds than mica slate, and mica slate fewer than primitive slate.

Limestone, which accompanies all the newer primitive formations, is intirely wanting in granite.

Metals which occur in Granite.

This rock is not so rich in metals and their ores, as the primitive rocks of newer formation. It contains, however, a considerable variety, and some of these have been as yet only discovered in granite. Iron, which is remarkable on account of its occurrence in every period of the earth's formation, is found interspersed in the oldest granite. Red iron ore occurs

* These globular distinct concretions used formerly to be considered as bowlded stones, and afforded an invaluable opportunity for the framing of extravagant hypothesis.

† Mineralogy of the Scottish Isles, vol. i. p. 42.

Molybdena hitherto found only in granite.

Bismuth, cobalt, blende, galena, and particularly tin.

More modern granite, alternating with gneifs.

containing slate.

Veins of granite traversing strata of mica slate, and primitive slate.

Characters of the newer granite; usually low, red, fine grained, &c.

in veins in granite, also the brown iron ore, but this is far feldomer than the red. Molybdena has as yet only occurred in granite, either interspersed, or in veins of the oldest formation, as at Schlackenwalde, Geya, and Altenberg.

Bismuth, cobalt, blende, galena, and several ores of copper have been found in granite. Of all metals, however, tin is the one most frequently found in granite, and in the great mining field of Cornwall, it is observed, that copper occurs frequently in primitive slate, but the tin in granite.

The preceding observations refer principally to the old granite, which, as far as our experience goes, is the oldest of all the rock formations. Werner has discovered other granite formations, which are of a newer date.

1st. Upon the Schneekoppe, the most elevated part of the Riesengebirge, which is about 5000 feet above the level of the sea, granite alternates with gneifs, and hence Werner considers it a distinct formation*.

2d. At Greifenstein in Upper Saxony, Werner observed granite which contained pieces of slate lying over strata of primitive slate, hence he justly reckons it to be a distinct formation, which is newer than either of the preceding.

3d. At Auerberg, near Eibenstock in Saxony, and at Fastenberg near Johangeorgenstald, Werner discovered veins of granite traversing strata of mica slate and primitive slate, and this he is at present inclined to consider a new formation. In Scotland granite veins are very common, and several circumstances lead me to believe, that these and the Auerberg are the same formation. They are probably both connected with the Greifenstein formation. Werner mentions a few particulars, which he considers as characteristical for the newer granite formations.

1. Granite, which occurs in low situations, may be suspected to belong to the Greifenstein formation.

2. The newer granite has generally a deep red colour, is more frequently fine than coarse grained, contains garnets, and is not porphyritic.

* I was so fortunate as to have the opportunity of examining this formation in company with Dr. Mitchell. We observed the granite alternations three times with the gneifs.

The late Dr. Hutton of Edinburgh has given us a very different geological view of granite from that contained in the preceding pages. In my outline of the mineralogy of the Scottish isles, I endeavoured to shew the fallacy of Dr Hutton's opinions. Professor Playfair, however, in his illustrations of the Huttonian theory lately published, has stated a number of facts, which he considers as fully confirming Dr. Hutton's ideas respecting granite. I therefore take this opportunity to state, more fully, the reasons that incline me to differ intirely from these gentlemen.

Professor Playfair is of opinion, that the distinction of granite into different formations is hypothetical. To this I can by no means assent; on the contrary, I am fully convinced, that the granite formations are well ascertained, and this will be evident from the following observations: it is a position, the truth of which is acknowledged by every geognost, that of two series of rocks, the newest, is that which covers the other. The Greifenstein granite formation lies over the primitive slate, and is consequently newer than the old granite, gneifs, mica slate, and primitive slate. To render this intelligible to those unacquainted with the Wernerian geognosie, I have represented this formation in the sketch, Fig. 1. Plate XIII.

Professor Playfair's opinion that the preceding distinctions are hypothetical.

The order of date in granite formation, illustrated from their situations.

From this sketch it is plain, that after the deposition of the old granite, gneifs, mica slate, and primitive slate, the water had again risen, and deposited over the ends of these strata, this newer granite formation.

The *sienite formation* , which has been often confounded with the old granite, lies over the four older primitive formations, and has, interposed, a layer of breccia, composed of fragments of these rocks. This demonstrates, as we have already remarked, that the water must have risen and deposited first the fragments of the older strata, (which it had tore off in its rising) and upon this the sienite. The sketch illustrates these appearances, Fig. 2, Plate XIII.

Sienite formation.

These facts and explanations, which are drawn from the Wernerian geognosie, I shall now contrast with those of Dr. Hutton, and Professor Playfair.

Professor Playfair remarks, p. 309. " Veins of granite are also frequent in Cornwall, where they are known by the name of *lodes* , the same name which is, applied in that country to metallic

Remarks of Professor Playfair on the vein of granite in Cornwall.

metallic veins. The granite veins frequently intersect the metallic, and are remarkable for producing shifts in them, and for throwing them out of their natural direction. The mineral veins, particularly those that yield copper and tin, run nearly from east to west, having the same direction with the beds of the rock itself, which is very hard schistus. The granite lodes, as also those of porphyry, called *elvan*, in Cornwall, are at right angles nearly to the former; and it is remarked that they generally heave the mineral veins, but that the mineral veins seldom or never heave the cross veins. In this country, therefore, veins of granite and porphyry are posterior in formation to the metallic veins. These veins of granite may perhaps be connected with the great granitic mass that runs longitudinally through Cornwall, from Dartmoor to the Land's End. This much is certain, that their directions in general are such, that if produced, they would intersect that mass, nearly at right angles." Further, at p. 317. he remarks, "The last instance I have to mention from my own observation, is at St. Michael's Mount in Cornwall. That mount is intirely of granite, thrust up from under a very hard micaceous schistus, which surrounds it on all sides; at the base of it a great number of veins run off from the granite, and spread themselves like so many roots fixed into the schistus: they are seen at low water. In the smaller veins, the granite is of very minute, though distinct parts; in the larger, it is more highly crystallized, and is undistinguishable from the mass of the hill." I agree with Professor Playfair, in believing that the Cornish veins may stand in connection with the granite of the country, but should this be proved, the Cornish granite must then be referred to the Greifenstein or Sienite formation. The appearances observed at St. Michael's Mount demonstrate, that it belongs to one of the formations I have just mentioned; and I have no doubt that if Professor Playfair had examined the situation of the primitive slate, with regard to the granite, he would have found it covered by the granite.

Issuing like roots from St. Michael's Mount into the schistus,

referable to the Sienite formation.

Other granite veins in Galloway,

Again at p. 316, Mr. Playfair informs us, "that another series of granite veins is found in Galloway, which were first discovered by Dr. Hutton, and his friend Mr. Clerk, and afterwards more fully explored by Sir James Hall and Mr. Douglas, the present Earl of Selkirk. The two last traced

the line of separation between a mass of granite and the schistus incumbent upon it, all around a tract of country, about eleven miles by seven, extending from the banks of Loch Ken westward; and in all this tract they found, "that wherever the junction of the granite with the schistus was visible, veins of the former, from fifty yards to the tenth of an inch in width, were to be seen running into the latter, and pervading it in all directions, so as to put it beyond all doubt, that the granite of these veins, and consequently of the great body itself, which was observed to form with the veins one uninterrupted mass, must have flowed in a soft and liquid state into its present position*. I have only further to add, that some of these veins are remarkable for containing granite, not sensibly different, in any respect, from the mass from which they proceed." The Criffle in Galloway, which is one of the most considerable portions of what Dr. Hutton considers as the granite of that country, I found to be sienite, consequently it has no relation to the old granite formation. Professor Playfair, who examined the appearances at Loch Ken, believes with Dr. Hutton, that sienite and granite, in a geognostic point of view, are to be considered as the same †. From this I draw the conclusion, that at Loch Ken we have a portion of the same sienite as that which forms the Criffle.

which Mr. Playfair concludes to have flowed in a soft state to their present situation.

The author observed the Criffle to be sienite,

and infers that the other granites are a portion of the same.

It appears then evident, that wherever granite, in the form of veins is to be observed issuing from granite into the contiguous strata of gneiss, mica slate, &c. it must belong to a newer formation, and probably to that of Greifenstein. Many of the instances where such appearances have been observed, certainly belong to the sienite formation.

The same inference generally stated,

It is therefore demonstrated, that Granite is the oldest Rock with which we are acquainted.

and that granite is the oldest of rocks.

Before I conclude these remarks, I shall notice two objections which have been urged against the possibility of granite veins having been filled from above. Mr. Playfair observes at page 313, "that a strong objection to the supposed origin of granitic veins from infiltration, and indeed to their formation in any way but by igneous fusion, arises from the number of

To Professor Playfair's remark that fragments of schistus could be insulated in granite veins only by igneous fusion;

* Transactions of the Royal Society of Edinburgh, vol. iii. p. 8.

† Illustration of the Huttonian Theory, p. 312.

fragments of schistus, often contained, and completely insulated in these veins. How these fragments were introduced into the fissures of the schistus, and sustained till they were surrounded by the matter deposited by water, is very hard to be conceived; but if they were carried in by the melted granite, nothing is more easily understood." To this objection,

it is answered, that they are not insulated.

it is only necessary to answer, that the apparent insulation of masses of stone in veins, is occasioned by our only seeing part of the mass; for when the whole is seen, we invariably find, that it rests upon another fragment, or upon the sides of the veins. At p. 314, when describing the junction of the granite with the other primitive strata in the island of Arran, Professor Playfair remarks, "Along this line, particularly on the south, wherever the rock is laid bare, and cut into by the torrents, innumerable veins of granite are to be seen entering into the schistus, growing narrower as they advance into it; and being directed in very many cases, from below upwards, they are precisely of the kind which the infiltration of water could not produce, even were that fluid capable of dissolving the substances which the vein consists of. From the south face of this mountain, and from the bed of a torrent which intersects it very deeply, Dr. Hutton brought a block of schistus, of several hundred weight, curiously penetrated by granite veins, including in them many insulated fragments of the schistus."

And with regard to the granitic veins or numerous masses in schistus in the isle of Arran;

it is remarked that they are often insulated, and have other signs of being nearly coeval with the gneiss.

In the island of Arran I have had frequent opportunities of examining these kind of veins; they are generally found in gneiss, are very much incorporated with it, and often both extremes of the vein are to be observed in the same bed. From this it is evident that these cannot be considered as granite veins, since they are nearly of coeval formation with the gneiss, and have no communication with any formation, older or newer than that in which they occur*. Werner remarks, that veins which are nearly coeval with the vein rock, are

* Although we find in gneiss veins filled, and layers composed of a rock which, oryctognostically considered, has every character of granite, yet the geognost justly views them as varieties of gneiss. It often happens in a mountain of gneiss, that strata occur which are not to be distinguished from mica slate; these, however, are merely accidental, and the whole is therefore to be referred to gneiss. This admirable mode of investigation, which was first discovered by Werner, has been of the greatest utility in geognosia.

very

very much incorporated with it, have no salband, far less an interposed layer of clay, (besteg) and the vein mass differs very little from the vein rock. On the contrary, the newer the veins, the fewer are the points of their agreement, so that at last there is not the least resemblance. These old veins he supposes to have been immediately filled out of the same solution from which the rock had been precipitated: the contents at least of this fluid had not undergone any considerable change.

R. JAMESON.

II.

Observations on the Conversion of Iron into Steel. In a Letter from JOSEPH PRIESTLEY, L. L. D. F. R. S. &c. &c.

To Mr. NICHOLSON.

SIR,

Northumberland, America, May 22, 1802.

YOUR giving the following article a place in your valuable Journal, will oblige
Your's, &c.

J. PRIESTLEY.

ACCORDING to the antiphlogistic theory iron becomes steel by imbibing carbon; to this is said to be owing the addition to its weight in the process of cementation; and the flakes of black matter which remain undissolved after a solution of steel in diluted acid of vitriol are said to be *plumbago*, or carbon united to iron. Antiphlogistic theory of steel-making.

I cannot, however, help concluding from some late experiments, that iron is converted into steel by imbibing only phlogiston from the charcoal, and that the addition to its weight is not from carbon, but from *finery cinder*. Objection from late experiments and phlogistic theory.

Having made a quantity of iron filings perfectly pure, by first expelling from them all the air that they could be made to yield by heat, then washing out of them any carbonaceous matter they might contain, and exposing them again to heat, I took 120 grains of them, and heating them with a burning lens in inflammable air, found that they imbibed 18 ounce measures 120 gr. of iron filings cleared of air by heat and washed were heated in inflammable air.

which they imbibed and became bright.

These left in vitriolic acid a black matter as steel does.

Its habitudes were those of finery cinder.

The black matter from pure steel was affected in the same manner.

It was therefore finery cinder, and not carbon.

Bright needles afforded much less black matter than watch springs, of which the surface was blued or oxidized.

measures of it. In consequence of this, from being of a dark colour, they became exceedingly bright; and I concluded that they were now become *steel*, though I was not able to ascertain it by a direct experiment. But after dissolving those bright filings in diluted vitriolic acid, a quantity of black matter, as after the solution of steel, remained unaffected by it. This being heated in inflammable air imbibed a considerable quantity of it, and then, by means of diluted vitriolic acid, gave inflammable air very copiously. This black matter had evidently the properties of finery cinder.

I then dissolved 200 grains of *broken watch springs*, which are undoubtedly pure steel, and collected from the solution three grains of black matter. Heating this in inflammable air, a great proportion of it was imbibed; and then, by means of the diluted acid, it gave out inflammable air as copiously as iron or steel filings would have done. This black matter, therefore, from the solution of steel was finery cinder, and not carbon, or plumbago. And as iron acquires weight by becoming finery cinder, and this addition of weight, I think I have proved to be from *water*, it can hardly be doubted, but that the addition of weight to iron, in being converted into steel, is from the same cause. Indeed, I believe it to be impossible to expose iron to a red heat in circumstances in which there is any possible access of water, or of air, which always contains water, without a partial calcination of it; that is without its becoming superficially at least finery cinder.

This was evidently the difference between the result of the solution of the *watch springs*, and that of an equal weight, viz. 200 grains of *broken polished needles*, which had not undergone any calcination. For the black matter that remained from the solution of them would not have weighed a quarter of a grain. Giving colour to steel, which is done to watch springs, is always a partial calcination of the metal; and this appears from the preceding experiments to be the conversion of a part of it into finery cinder, which is the reverse of plumbago; being, according to the new theory, an *oxide* in the highest degree; whereas if plumbago contain any oxygen, it is in the lowest degree.

III.

*An Account of the Art of making Glue. In a Letter from
Mr. JOHN CLENNEL.*

DEAR SIR,

Newcastle, June 21, 1802.

THE following attempt to develope the "art and mystery" of glue-making is at your service, if you think it worthy a place in your very valuable miscellany. The improvement of any manufacture depends upon its easy access to men of science, and a prudential theorist can never be better employed than in attempting to reduce to regularity or to system the manufactures that may fall under his attention. In conformity to the first principle, I made some notes whilst visiting a glue manufactory a few years ago in Southwark, and those, interwoven with the remarks on that subject of some chemists of the first respectability, I take the liberty of sending you: at the same time I must beg of you, or your correspondents, that where it may be corrected in any manner, it may be done, and I shall feel myself obliged by the attention.

Glue is an inspissated jelly, made of the parings of hides or horns of any kind, the pelts obtained from furriers, and the hoofs and ears of horses, oxen, calves, sheep, &c. quantities of all which are imported in addition to the home supply, by many of the great manufacturers of this article: these are first digested in lime water, to cleanse them as far as it can from the greafe or dirt they may have contracted; they are then steeped in clean water, taking care to stir them well from time to time; afterwards they are laid in a heap, and the superabundant water pressed out; then they are boiled in a large brass caldron with clean water, skimming off the dirt as it rises, and further cleansed by putting in, after the whole is dissolved, a little melted alum or lime finely powdered, which, by their detersive properties, still further purge it: the skimming is continued for some time, when the mass is strained through baskets, and suffered to settle, that the remaining impurities, if any, may subside; it is then poured gently into the kettle again, and further evaporated by boiling a second time, and skimming, until it becomes a clear but darkish brown colour: when it is

Great advantages of the visitation and report of manufactories

Glue is made from refuse of hides, &c.

Cleaned from greafe by lime-water; then steeped in clean water; drained; boiled in water; clarified by alum or lime; strained; cleared by subsidence; again boiled to the requisite density; poured into moulds; cut into portions; and dried on a net.

thought

thought to be strong enough (which is known either by the length of time a certain quantity of water and materials have boiled, or by its appearance during ebullition), it is poured into frames or moulds of about six feet long, one broad, and two deep, where it hardens gradually as the heat decreases: out of these troughs or receivers it is cut when cold by a spade, into square pieces or cakes, and each of these placed within a sort of wooden box, open in three divisions to the back; in this the glue, as yet soft, is taken to a table by women, where they divide it into three pieces * with an instrument not unlike a bow, having a brass wire for its string; with this they stand behind the box and cut by its openings, from front to back: the pieces thus cut are taken out into the open air, and dried on a kind of coarse net work, fastened in moveable sheds of about four feet square, which are placed in rows in the glue-maker's field (every one of which contains four or five rows of net work); when perfectly dry and hard, it is fit for sale.

Character of
good glue.

That is thought the best glue which swells considerably without melting, by three or four days immersion in cold water, and recovers its former dimensions and properties by drying. Glue that has got frost, or that looks thick and black, may be melted over again and refined, with a sufficient quantity added of fresh to overcome any injury it may have sustained; but it is generally put into the kettle after what is in it has been purged in the second boiling. To know good from bad glue, it is necessary for the purchaser to hold it between his eye and the light, and if it appears of a strong dark brown colour, and free from cloudy or black spots, the article is good.

Judgement of
the cakes.

I am, Sir,

With great respect,

Your's, &c.

JOHN CLENNELL.

* When the women, by mistake, cut only two, that which is double the size is called a Bishop, and thrown into the kettle again.

IV.

*On the Preparation of Indelible Ink. In a Letter from
Mr. THOMAS SHELDRAKE.*

To Mr. NICHOLSON.

S I R,

AS your correspondent, Mr. Clofe, has alluded to my memoir on the nature and preparation of drying oils, it may not be unpleasant to receive such information as I am able to give respecting the object of his pursuit.

By experiments repeated and varied every way that my imagination can suggest, I am convinced that amber is not soluble in alcohol or any essential oil: it is soluble in expressed oils, by the process described in Lewis's philosophical commerce of the arts, but that solution does not dry well, and therefore will not answer Mr. Clofe's purpose; but when dissolved by the * well-known process for making amber-varnish, it is likely to answer extremely well.

There is another substance which seems likely to answer his purpose very well. † Asphaltum is a bituminous substance, perfectly black when viewed in a mass, but a dark transparent brown when dissolved: it is soluble in spirit of turpentine at a

Amber is not soluble in alcohol or essential oil.

The solution in expressed oil is not good for ink; but the solution for varnish is good.

Asphaltum promises to be a good material.

* The following is the most convenient method: Put small pieces of amber into an iron ladle, set it on a fire till they are melted, then add so much of the best drying oil as will make it liquid, stir them well together, and, when cold, add so much spirit of turpentine as will make it thin enough to flow from the brush. The object in making varnish is to discolour the amber as little as possible; therefore it is but little roasted, and the lightest coloured drying oil is used; but if this solution of amber was used for making ink, the darkness of the colour would be an advantage, therefore the amber should be thoroughly melted, and the darkest drying oils used in preference to the others.

Process for the oily solution of amber.

† Within these few years good asphaltum may be procured in many shops in London: before that period it was unknown; the best of what *was sold, and is still sold in some places*, was the caput mortuum of amber; other compositions of pitch and various resins were likewise sold for asphaltum. If it were used for Mr. Clofe's ink, care should be taken to select the best.

Good asphaltum is now easily procured.

low

low heat, and when dissolved runs freely from the pen: I have known some artists draw with this in preference to ink, because its colour harmonizes better with other materials also used in drawing, and because it is indelible, as it strikes immediately into the paper, and if it is not thick, will strike through it: by this means every stroke made with it is visible through every colour that is washed over it.

Asphaltum in spirit of turpentine, rendered consistent with solution of amber, and coloured with lamp black, would give an indelible ink.

It seems then that if a solution of asphaltum was made in spirit of turpentine, and so much of the solution of amber was added as would give it due consistence, and the finest lamp black to give it colour, a perfect ink would be formed, and possessing those properties Mr. Close seems to desire; for, supposing the other materials could be removed, so much of the colour as depends on the asphaltum would be indelible, except by such means as would destroy the paper or parchment.

The drying oil of the varnish would increase the difficulty of obliteration.

Even the small quantity of drying oil introduced into the varnish, would be useful in this respect; for it is well known; that if oil is dropped upon white paper, though the mark is scarcely visible at first, in a year or two it will become a dark yellowish brown: it seems as though the oil changes the paper so much that its colour can never be recovered, at least those who undertake to restore the white colour of old prints, always make an exception to spots of oil.

If this hint should be thought deserving any notice, you will have the goodness to make what use you please of it.

I am, Sir,

Your's, &c.

T. SHELDRAKE.

No. 50, Strand, July 6, 1802.

Copal would probably be discharged from paper by camphorated spirit.

P. S. I believe Mr. Close will find himself mistaken as to the insolubility of copal when used in his ink. So powerful is the influence of camphor upon it, that if copal be reduced to powder, and a little camphor is rubbed into it, it immediately begins to soften, and the whole soon becomes a coherent mass; and though copal is not soluble in alcohol alone, if camphor is added, it dissolves as easily in the compound as the softest resin would. It is therefore extremely probable, that, if a paper written with his ink was washed with camphorated spirits, the writing would be removed with very little difficulty.

V. Observations

V.

Observations on the Causes why a large Quantity of common Salt prevents Putrefaction, and a small Quantity hastens it. By D. H.

To Mr. NICHOLSON.

S I R,

THERE are few phenomena of nature more interesting, and at the same time more involved in obscurity, than the two opposite actions of muriate of soda, which is known to have very considerable effect, both in accelerating and in retarding putrefaction. The *antiseptic* property of this salt has been known from the earliest ages. It was discovered, however, by Pringle, Macbride, and Gardane, that putrefaction may be hastened by sprinkling the animal substances with water holding a small quantity of muriate of soda in solution. This discovery excited much surprize; and the celebrated chemists who observed it seem to have been fully aware of the difficulties attending its explanation, as they have offered no theory to account for it. It would doubtless appear a presumptuous undertaking, to attempt the solution of a question which has baffled the ingenuity of so many philosophers, did not the subsequent discoveries in chemistry and physiology enable us to speculate on the subject with some degree of probability.

It seems necessary for the decomposition of an animal substance, *1st*, that it be in contact with atmospheric air; *2dly*, that it be exposed to a moderate degree of heat; and, *3dly*, that it be impregnated with humidity. It must necessarily follow, that whatever removes these conditions will check the progress of putrefaction. Of this we have many instances, as in the effects of cold; in covering the substances with sugar, resins, &c. and in preserving them in spirit of wine. I conceive with Gren, that muriate of soda acts only in this way, by abstracting the moisture, and removing the substance from the contact of oxygen; and not by a peculiar innate, and as it were hidden (*vis occulta*) antiseptic power*.

Remarkable opposition of effects of common salt in preventing or accelerating putrefaction; as its quantity is more or less.

Putrefaction requires air, warmth, and moisture;

and is checked by cold and covering from the air.

Crude common salt supposed to preserve by drying and covering the body it is applied to.

* Gren's Chemistry, Chap. VIII.

Putrefaction is hastened by destroying irritability.

With regard to the *septic* property of muriate of soda, it must be referred to another cause. The destruction of muscular irritability appears to be a chief cause of accelerating putrefaction. This was ascertained by Mr. John Hunter, who found, that when death is occasioned by an electric shock, by violent exercise, by a blow on the stomach, or by any thing that destroys the irritability of the muscular fibre, putrefaction quickly ensues. Fontana found that the same effects were produced by the poison of vipers. It has been also found by experiment, that the compounds of potash and soda destroy muscular irritability. Now, is it not a fair inference from these premises, that a small quantity of muriate of soda should possess a septic quality? Upon these grounds, it will not be difficult to reconcile the two opposite actions of muriate of soda. When a large quantity of this salt is applied to an animal substance, it acts merely by removing the indispensable conditions of putrefaction, air and moisture. The particles in contact with the substance may indeed act in destroying the irritability of the muscular fibre; but this being only a secondary cause of putrefaction, cannot operate unless in conjunction with air and moisture. On the other hand, when a small quantity of this salt in solution is applied, it is insufficient either to exclude the air, or to abstract the moisture; its peculiar property, therefore, acts *in conjunction* with the other causes, and these causes united accelerate the putrefactive process much more than any of them separately.

Common salt inferred to produce that effect.

Much salt retards putrefaction by drying and covering, more than it accelerates by the last mentioned quality.

Little salt accelerates it for the contrary reason.

Such is the explanation of these phenomena which occurred to me. Although it may be imperfect in many points, yet it appears to involve no hypothesis, but to be a strict deduction from facts. Should this attempt have the good fortune to meet with your approbation, its insertion in the Philosophical Journal may at least have the effect of drawing the attention of some more eminent chemist to this too much neglected subject.

I am, Sir,

Your's respectfully,

Edinburgh, July 12, 1802.

D. H.

VI.

Account of the Methods by which Soda is at present prepared for the English Market ; with other Observations. By Mr. FRED. ACCUM. From the Author.

SINCE the late new duty on salt, the manufacturers of soda avail themselves of the method of decomposing the sulphate of foda *, by means of what is called American potash. Though this kind of potash contains less pure alkali than the Russian or German potash, it is imported in a state of perfect dryness, whereas the other is always met with in a moist state ; so that the absence of water appears to compensate for the deficiency of the alkali. The American potash sells now from 48s. to 54s. per cwt. it has been considerably higher, until lately it fluctuated between 54s. and 56s.

Soda obtained by decomposing the sulphate of the bleachers with American potash.

I have been employed in a soda manufactory in which the following method answered exceedingly well. Five hundred pounds of sulphate of foda were introduced into an iron boiler containing a sufficient quantity of Thames water. 560 lb. of American potash were likewise dissolved in as little water as possible in an iron boiler fixed near the former. The potash was always previously tried, and if indifferent the quantity taken was 10 lb. more.

Particular description of the manufacturing process.

The solution, as near as I recollect, was made with about thirty pails of water to the alkali here mentioned. Both solutions were then made to boil, and as soon as the ebullition took place the solution of potash was ladled into the boiler containing the sulphate of foda. The mixture was agitated during the transfusion, and the fire raised as expeditiously as possible. As soon as the fluid boiled it was ladled into a wooden gutter, which conveyed it into a cistern of wood lined with sheet lead nearly half an inch thick, which was fixed in a cool place. Sticks of wood were then placed across the cistern, from which slips of sheet lead two or three inches wide were hung into the fluid at four inches distant from each

A boiling solution of potash was added to another of sulphate of foda,

then boiled,

* Sulphate of foda is sold cheap by the bleachers, who have it as the residue of decomposing common salt by sulphuric acid with manganese. In February, 1801, it was worth from 11s. to 14s. per cwt. but I have not the present price.---A.

and drawn off to crystallize in a leaden cistern.

at a temperature under 55°. The soda is then washed with cold water ;

then dissolved and evaporated at a low heat.

Pellicles of sulphate of potash are formed and fall down ;

which ceasing, the fluid is drawn off to crystallize.

100 parts of sulphate of soda afford 138 of carbonate.

The two crystallizations are found to be necessary.

other. When all was cool, which in the winter was generally the case in three days, a plug in the bottom of the cistern was drawn in order to let off the fluid, and the crystallized salt was taken from the slips of lead. The bottom exhibited a rock of salt, which was detached by chissel and mallet. On this account it is that the lead which lines the cistern must be thick, in order to guard against accidents. For if the metal be perforated, the saline solution creeps between it and the wood, and in a very short time detaches the lining, and it is besides extremely difficult to find out the place where the defect really is. The temperature where the soda is left to crystallize ought not to exceed 55° Fah. In this stage of the process the whole of the salt is washed in the same cistern with cold water, to clear it of impurities ; after which it is transferred again into the boiler, dissolved in clear water, and evaporated by heat. As soon as a strong pellicle is formed, it is suffered to cool so far that the hand may be dipped in the fluid without injury, and the heat is kept at that temperature as long as effectual pellicles continue to be formed over the whole surface of the boiler, and then fall to the bottom. When no more pellicles are formed, or at least only by blowing with the mouth upon the surface, the fire is withdrawn, and the fluid is ladled out into the cistern to crystallize. The sulphate of potash, &c. which had been deposited, is then taken out of the boiler and put aside. If the fluid be suffered to cool pretty low before it is suffered to run into the cistern, very little sulphate of potash is found in the soda ; but in general the rocky masses of soda met with in the market contain a considerable quantity. By this process from 136 to 139 lb. of soda may be obtained from 100 lb. of sulphate of soda, if the soda be crystallized in large crystals ; if small crystallized it yields less ; it sells now at 52s. or 54s. per cwt. and is retailed at 8d. per lb.

We might be inclined to suppose that the first operation was unnecessary, and that the soda might be separated at once from the sulphate of potash at the instant of its formation ; but practice will convince the operator otherwise. A considerable loss is manifested if the process be not conducted in this manner ; though the discovery of the cause may perhaps be not so easily accomplished as the proof of the fact.

Other

Other manufacturers grind together 500 cwt. of Glauber's salt of the bleachers, and 100 cwt. of charcoal; they expose this mixture in a reverberatory furnace resembling a baking oven, till the matter when stirred with a rake becomes pasty. It is then withdrawn and transferred into large casks; each provided with a double bottom. Water is then suffered to stand one inch high over it for 24 hours; the cock is then opened, the solution runs through the perforated bottom, over which a stratum of straw had been previously placed; and is thence conducted into the boiler for evaporation and crystallization *.

Another process. The acid of sulphate of soda is decomposed and expelled by heat with charcoal, and the soda extracted by water; &c.

It is a curious fact, that iron plates are absolutely necessary to constitute the surface on which these articles are exposed to heat: fire bricks do not answer. It seems as if the iron assisted the union; though neither iron filings mixed with the articles nor pyrites are found of advantage.

The heat must be applied on an iron, and not a brick hearth.

This method of making soda is extremely uncertain. If the heat be not raised gradually, or if the mixture be not fused enough, or a little too much, it does not succeed. The worst event is, that when the mixture has been made too hot; sulphuric acid is produced, and sulphate of potash is formed:

Uncertainty of this method.

The quantity of soda which may be obtained by this process, is said to be equal to that obtained by any other method.

It is said to be profitable if well managed.

I have been lately informed, that in Germany soda is made by decomposing the sulphate of soda by means of acetite of lime; the acetic acid is obtained for that purpose from wood, and the charcoal is found to pay the costs.

Third method. Sulphate of soda decomposed by acetite of lime, of which the acid is had by distilling wood.

The method recommended by several chemists, of obtaining soda by decomposing Glauber's salt by acetite, or the oxide of lead, does not answer in this country. The mass is by far too bulky; and requires too much time, attendance, and fuel to reduce it to a narrow compass. I have been informed by men well skilled in this department, that it is nearly impracticable in the large way. The muriate of lead which is produced cannot be used as a white pigment, as the inventors pretend.

The method recommended by acetite of lead, or else oxides of that metal is productive of loss in this country.

The muriate of lead thus obtained is not a pigment.

In Prussia they mix muriate of soda and quick lime together; then slake the lime, and form the whole into a thick pulp,

Prussian method by quicklime; then slaking and long exposure to the air. Product sulphate of lime and carbonate of soda.

* This was the process at the salt works.---N.

which is extended about two inches thick over a large surface, and left in that situation for three months. Carbonate of soda is then formed, which is washed out and crystallized in the usual manner. The soda obtained by this process always has a yellowish cast.

VII.

Comparison of the French definitive Metre with an English Standard, brought from London by M. A. PICTET, one of the Editors of the Bibliothèque Britannique.*

Short history of
admeasurements
of the earth.

particularly that
lately made in
France.

and the standard
measure thence
deduced.

THE measurement of the earth, and the investigation of its figure, were the subjects, at various times in the course of the eighteenth century, of the labours of a number of philosophers of the first eminence in different countries. Some Swedish astronomers are now employed in a second measurement of the same degree which was measured sixty years ago by the French Academicians in Lapland, under the polar circle. In France, when the idea of seeking in the dimensions of the globe itself the unit to which all measures and weights might be referred, had once been conceived and adopted, it was necessary to make an effort proportional to the importance of an undertaking which was thus become national. In the midst of a long and sanguinary war, together with difficulties of every other kind, a chain of triangles has been formed between Dunkirk and Barcelona, comprehending the tenth part of the arc of the meridian which extends from the Equator to the pole, and which is equal to one fourth of the circumference of the globe; and the ten millionth part of this arc, thus determined, has been adopted for the unit of the metrical system: it has been fixed by the construction of standards made of substances proper to resist the attacks of time; and by a careful examination of the precise relation of the length of the metre to that of the pendulum vibrating seconds, on the level of the sea, in a given latitude, the determination of this unit has been rendered independent of any accident that might destroy or

* From No. 148 of the Bibliothèque Britanique. I avail myself of the free translation given in the Journal of the Royal Institution; but have very carefully read the proof with the original.---N.

impair the standards representing it; while in the formation of these standards all the precautions have been employed that could be suggested by the present improved state of natural philosophy, and of the arts.

In England, on the other hand, operations have been carried on for these five and twenty years, which are to be the foundations of an exact map of Great Britain. These labours, begun by the late General Roy, have been conducted with much sagacity and precision; and the results are likely to procure very interesting information respecting the figure of the earth. Sir George Shuckburgh, an eminent member of the Royal Society of London, has successfully employed himself in private, in researches intended to fix the precise length of the standards, which have served as bases for the measurements made in Great Britain.

Operations of the same kind in England,

It was therefore to be regretted, that operations so similar, conducted in two neighbouring countries, and capable of acquiring a new interest by comparison, should remain unconnected, for want of an actual standard of the measures of the one country, which might be transported into the other, after the definitive determination of the French measure. This regret we had deeply felt at various times when these objects were laid before our readers; and we may say with truth, that if the hope of procuring this medium of comparison was not the only motive of the journey to England that one of us has made, it at least greatly contributed to induce him to undertake it.

Great advantage of an accurate comparison of the French and English standards.

Our colleague took some steps in his passage through Paris, to obtain an authentic metre, in order to be submitted to the examination of the Royal Society, to which he has the honour of belonging, but he did not remain long enough in Paris to be able to succeed in this attempt. He took advantage of his longer stay in England, in procuring from the hands of Mr. Troughton, an artist celebrated for his accuracy in the construction and division of geometrical and astronomical instruments, a standard rigorously conformable to that which he had made for Sir George Shuckburgh, and with which this philosopher had compared the principal English standards. Our colleague procured also from the same artist the comparative apparatus of Sir George Shuckburgh, composed of two excellent microscopes, the one bearing a micrometer which di-

Means used by M. Picet to obtain a true British standard.

Apparatus by Troughton for comparing linear measures.

vides

Commission of
the French Na-
tional Institute
for comparing.

vides the English inch into ten thousand equal parts. Upon his return to Paris he made haste to exhibit these instruments to the Minister of the Interior, and to the National Institute. This learned body nominated three of its members, in order to proceed to the regular comparison of the definitive metre with the English standard. The undertaking, by no means so easy as it at first appeared, occupied the committee in five different meetings, of nearly four hours each; and it was performed with all the care and precaution that the nature of the subject required. Mr. Prony, who, as the translator of General Roy's memoir on the first trigonometrical operations in England, was particularly interested in these researches, acted as secretary to the Committee, and it was at his house, and with the assistance of a comparative apparatus belonging to him, that the principal experiments were made. He has been so obliging as to furnish us with an authentic copy of the report made to the Institute, which was deemed of sufficient consequence to be read at the public sitting of the last quarter. He adds, that "This report will soon be followed by a memoir, in which he will enter into more circumstantial details of all the observations that he has made; and in which he will give a description and a figure of his comparative instrument." We shall bear in mind this promise, and in the mean time we shall give our readers a copy of the report; informing them that we have bestowed on the correction of the proofs of this important paper all the attention necessary to enable us to affirm that no typographical error has been committed in the numbers.

*National Institute of Sciences and Arts. 6 Nivose, Year 10
(27th December, 1801.)*

Report of the
Institute.

A member read, in the name of a committee, the following report on the comparison of the standard metre of the Institute with the English foot.

M. Picet, Professor of Natural Philosophy at Geneva, submitted to the inspection of the class in the month of Vendémiaire, an interesting collection of objects relative to the sciences and arts, which he collected in his journey to England.

English standard
of 49 inches,

Among them was a standard of the English linear measure, engraved on a scale of brass, of 49 inches in length, divided by very fine and clear lines into tenths of an inch.

It

It was made for M. Pictet by Troughton, an artist in London, who has deservedly the reputation of dividing instruments with singular accuracy; it was compared with another standard made by the same person for Sir George Shuckburgh, and it was found that the difference between the two was not greater than the difference between the divisions of each; that is, it was a quantity absolutely insensible. This standard may therefore be considered as identical with the standard described by Sir George Shuckburgh in the Philosophical Transactions for 1798.

M. Pictet also exhibited to the Institute a comparer, or an instrument for ascertaining minute differences between measures, constructed also by Mr. Troughton. It consists of two microscopes with cross wires, placed in a vertical situation, the surface of the scale being horizontal, and fixed at proper distances upon a metallic rod. One of them remains stationary at one end of the scale, the other is occasionally fixed near to the other end; and its cross wires are moveable by means of a screw, describing in its revolution $\frac{1}{100}$ of an inch, and furnished with a circular index, dividing each turn into 100 parts; so that having two lengths which differ only one tenth of an inch from each other, we may determine their difference in ten thousandths of an inch. The wires are placed obliquely with respect to the scale, so that the line of division must bisect the acute angle that they form, in order to coincide with their intersection. General Roy has described, in the 75th volume of the Philosophical Transactions, a similar instrument made by Ramsden, for measuring the expansion of metals.

Instrument of comparison bearing two microscopes.

M. Pictet offered to the class the use of the standard, with the micrometer described, for the determination of the comparative length of the metre, and the English foot: the offer was accepted with gratitude, and M. M. Legendre, Méchain, and Prony, were appointed to co-operate with M. Pictet in the comparison of the standard metre of platina and the English foot.

The first meeting was on the 28th Vendémiaire (21st of October,) at the house of Mr. Lenoir.

First meeting of the commission.

At first a difficulty occurred from the different manner in which the measures were defined: the English scale was graduated by lines; the French standards were simply formed to the length of a metre: hence the length of the metre could not easily

compare the English scale graduated by lines, and the French, which is of the precise

length without graduation.

easily be taken by the microscopes; nor could the English scale be measured by the method employed for making new standard metres, which consists in fixing one end against a firm support, and bringing the other into contact with the face of a cock or slider, adjusted so as barely to admit the original standard between it and the fixed surface.

Method by an intermediate piece.

Mr. Lenoir attempted to overcome this difficulty by reducing to a thin edge the terminations of a piece of brass of the length of a metre; so that it was compared with the standard metre in the usual manner, and its extremities, when placed on the English scale, constituted two lines parallel to those which were really engraved on the scale, and capable of being viewed by the microscopes.

Experimental comparison by this method.

The standard metre of platina, and another standard of iron, belonging also to the Institute, were thus compared with the English foot; each of these two measures being equal, at the temperature of melting ice, to the ten millionth part of the quadrant of the meridian. At the temperature of 15.3° of the decimal thermometer, or 59.5° of Fahrenheit, the metre of platina was equal to 39.3775 English inches; and that of iron to 39.3788, measured on M. Pictet's scale.

Result,

These first experiments, showed, however, that the method employed was liable to some uncertainty, arising from the difficulty of placing the cross wires precisely at the extremity of the thin edge of the plate of brass employed in the comparison; a reflection or irradiation of light, which took place at that extremity, prevented its being distinctly observed if the optical axis of the microscope was precisely a tangent to the surface exactly at the termination.

in some respects uncertain.

Accurate method of comparison by contacts; a ruler carrying a line was caused to move through the precise length of the metre, under the microscopes.

In order to remove this inconvenience, another arrangement was proposed by one of the Committee. (It was Mr. Prony that suggested this ingenious method, and M. Paul of Geneva, who happened to be present, that executed it. B.B.) A line was traced on a small metallic ruler, perpendicular to its length; the end of the ruler was fixed against a solid obstacle, and the cross wires made to coincide with the line: the standard metre was then interposed between the same obstacle and the end of the piece, and the line traced on it, which had now obviously advanced the length of the metre, was subjected to the other microscope. To the microscopes thus

thus fixed, the graduated scale was transferred; one of the divisions was placed exactly under one of the microscopes, and the micrometer screw was turned in order to measure the fraction, expressing the distance of the other microscope from another division.

The comparison was repeated in the same manner the 4th Brumaire (26th October last) at the house of one of the Committee, and after several experiments, agreeing very satisfactorily with each other, it was found that at the temperature 12.75° centigrade, or 55° of Fahrenheit, the standard of platina was 39.3781, and that of iron 39.3795 English inches.

Precise result at 55° of Fahrenheit.

The two metres being constructed to be equal at the temperature of melting ice, these operations may be verified by reducing their results to that temperature. For this determination we are provided with the accurate experiments made by Borda, and the committee of weights and measures, on the dilatation of platina, brass, and iron; from which it appears, that for every degree of the decimal thermometer, platina expands .00000856; iron .00001156; and brass .00001783; (for Fahrenheit's scale these quantities become 476, and 642, and 990 parts in an hundred millions.) From these data we find that, at the freezing point, the standard metre of platina was equal to 39.38280, and that of iron to 39,38265 English inches of M. Piçet's scale. The difference is less than the 500th of a line, or the 200000th of the whole metre, and is therefore wholly inconsiderable.

Reduction to the temperature of melting ice.

The result of the whole comparison is therefore this. Supposing all the measures at the temperature of melting ice, each of the standard metres is equal to the 10000000th part of the quadrant of the meridian, and to 39.38272 English inches of M. Piçet's scale.

Standard metre at 32° is = 39.38272 English inches.

At the class of mathematical and physical sciences of the National Institute, 6 Nivose, year 10.

Legendre, Méchain, and Prony, *Reporter*.

This report is approved, and its conclusions adopted by the class. Certified in conformity with the original by Delambre. Paris, 26 Nivose, year 10 (16th January, 1802).

[The rest of this Paper is by the learned Secretary to the Royal Institution, Dr. Young.]

On examining the reduction of the standards of platina and iron to the freezing point, it appears that they differ somewhat less

Revision by Dr. Young.

less

less than is stated in the report, and that they coincide within an unit in the last place of the decimals expressing their magnitudes, or one ten thousandth of an inch. The standard of platina at the freezing point becomes equal to 39.37380, and that of iron to 39.37370 English inches on the scale of brass at 55°, and the mean of these to 39.37100 English inches at 62°, which is the temperature that has been universally employed in the comparison of British standards, and in the late trigonometrical operations in particular. This result agrees surprisingly with Mr. Bird's determination of the lengths of the toises sent by Mr. Lalande to Dr. Maskelyne, of which the mean was 76.734 inches: hence the metre, having been found to contain 36.9413 French inches, appears to be equal to 39.3702 English inches: or rather to be either 39.3694 or 39.3710, accordingly as the one or the other of the two toises happens to have been the more correct; we may therefore give the preference to that which measured 76.736 inches.

Allowing the accuracy of the French measurements of the arc of the meridian, the whole circumference of the globe will be 24855.43 English miles, and its mean diameter 7911.73. Taking the ellipticity at $\frac{1}{338}$, the axis will be nearly 7893½, the equatorial diameter 7928, and the diameter of a sphere of equal solid content about 7916 miles; the brass standard being at the temperature of 62° of Fahrenheit.

Standard metre at 62°.

As long, therefore, as the English standard continues to be reduced to this temperature, we must consider the metre as equivalent to 39.3710, and not to 39.3827 English inches.

Upon these joint authorities it may be of use to reprint here a table of the principal measures and weights now used in France, with the very slight corrections which this last comparison has introduced into it. In translating the French terms into English, we are fully at liberty to rescue them, in some measure, from the barbarisms in orthography which have been committed in forming them.

Measures of length, the metre being at 32°, the foot at 62°.

						English inches.
French measures of length,	Millimetre	-	-	-	-	.03937
	Centimetre	-	-	-	-	.39371
	Decimetre	-	-	-	-	3.93710
						Metre

Metre	-	-	-	-	-	39.37100
Decametre	-	-	-	-	-	393.71000
Hecatometre	-	-	-	-	-	3937.10000
Chiliometre	-	-	-	-	-	39371.00000
Myriometre	-	-	-	-	-	393710.00000

	M.	F.	Y.	Ft.	In.
A decametre is	0	0	10	2	9.7
A hecatometre	0	0	109	1	1
A chiliometre	0	4	213	1	10.2
A myriometre	6	1	156	0	6

8 chiliometres are nearly 5 miles.

Measures of capacity.

	Cubic inches E.
Millilitre	.06103 of capacity.
Centilitre	.61028
Decilitre	6.10280
Litre, a cubic decimetre	61.02800
Decalitre	610.28000
Hecalitre	6102.80000
Chiliolitre	61028.00000
Myriolitre	610280.00000

A litre is nearly $2\frac{1}{8}$ wine pints. 14 decilitres are nearly 3 wine pints. A chiliolitre is 1 tun, 12.73 wine gallons.

Weights.

A gramme is the weight of a cubic centimetre of pure water at its maximum of density. It has been found equal to 18.827 French grains, of which 576 made 472.5 English; and 489.5058 grammes make a pound of the standard of the mint at Paris.

	E. grains.
Milligramme	.0154
Centigramme	.1544
Decigramme	1.5444
Gramme	15.4440
Decagramme	154.4402
Hecatogramme	1544.4023
Chiliogramme	15444.0234
Myriogramme	154440.2344

A deca-

A decagramme is 6 dwts. 10.44 gr. tr.; or dr. iiss. gr. 4.44 apoth.; or 5.65 dr. avoird. A hecatogramme is 3 oz. 8.5 dr. av. A chiliogramme is 2lbs. 3 oz. 5 dr. av. A myriogramme is 22lbs. 1.15 oz. av. 100 myriogrammes are 1 ton wanting 32.8lbs.

Agrarian measures.

Agrarian mea- sures,	Are, 1 square decametre	- - -	3,95 perches.
	Hecatere	- - -	2 acres, 1 rood, 35.4 perches.

For fire-wood.

for fire-wood.	Decistere, $\frac{1}{10}$ stere	- - -	3.5317 cub. f. E.
	Stere, 1 cubic metre	- - -	35.3171 cub. f.

MONEY. *Copper.* E. grains.

Money.	Centime, 1 gramme	- - - - -	15.4
	5 centimes, or fous	- - - - -	77.2
	Decime	- - - - -	154.4
	2 decimes	- - - - -	308.8

Silver $\frac{9}{10}$ or $\frac{36}{40}$ fine.

Franc, 5 grammes,	- - - - -	3 dwts. 5.2 gr.
6 francs,	- - - - -	16 dwts. 2.1 gr.

The franc is nearly the same with the livre tournois, and worth about 10d. Bolton's penny weighs 435 gr.; his half-penny 165; a shilling nearly 93 gr. and is $\frac{37}{80}$ fine.

Length of pendulum, &c. It appears from Mr. Borda's experiments, that in latitude 45° , a pendulum of the length of a metre would perform in a vacuum 86116.5 vibrations in a day: the length of a pendulum being supposed to increase with the latitude, in the proportion of the square of the sine of the latitude, multiplied by .000567, while the time of its vibration remains unaltered.

VIII.

On the Figure of Sulphate of Barytes, and the Formation of Mandreporæ. In a Letter from Mr. H. SARJEANT,

To Mr. NICHOLSON.

S I R,

IF the following mineralogical notices appear to contain any thing worthy of attention, they are at your service, from

Your humble servant,

Keswick, July 16, 1802.

H. SARJEANT.

THE barytes, or ponderous earth, occurs in a great variety of forms: one of the rarest is described as "resembling a number of small double convex lenses set edgeways in a ground." This singular formation, so different from the angular forms affected by crystals in general, was the cause of my examining it with considerable attention; and I am persuaded, that whoever does the same, will perceive that the shape of these crystals is not, properly speaking, lenticular, but rather a sort of very acute edged rhomboid, inserted by one of the corners as far as the diagonal line, so that the projecting part resembles the corner of a carpenter's chissel, admitting that the angle formed by its edge and side were enlarged to 100 degrees, or somewhat more, and the side bevilled off in the same manner as the edge, but in the opposite direction.

In its most usual form, the sulphate, the barytes is a very abundant production. It is found in this and the neighbouring counties, in great quantities, with lead, with lime (common limestone), with iron, and with gypsum.

That sort of limestone which abounds with the petrefactions called mandreporæ, appears to owe its origin to depositions of calcareous mud, formed in a manner similar to what may be seen in the upper part of most lakes, that of Keswick in particular, in which a species of equisetum grows, often in sheets of several acres, slightly covered with water. The section of the madreporæ is precisely that of the fresh root, and they are generally inclined all in the same direction, as if caused by a stream of water passing over them, which might be the case when the water was drawn off from such places, by the effect of earthquakes, or other great operations of nature.

The subsequent induration of such mud into stone, may be considered as a fact sufficiently known.

IX. *Experiments*

IX.

Experiments and Observations on certain stony and metalline Substances, which at different Times are said to have fallen on the Earth; also on various Kinds of native Iron. By EDWARD HOWARD, Esq. F. R. S. From the Philosophical Transactions. 1802.

(Continued from page 221.)

Stone that fell in Yorkshire and was exhibited in London. 1796, weighing 56lb. Historical account.

IN 1796, a stone weighing 56lb. was exhibited in London, with several attestations of persons who, on the 13th of December, 1795, saw it fall, near Wold Cottage, in Yorkshire, at about three o'clock in the afternoon. It had penetrated through 12 inches of soil and 6 inches of solid chalk rock; and in burying itself, had thrown up an immense quantity of earth, to a great distance: as it fell, a number of explosions were heard, about as loud as pistols. In the adjacent villages, the sounds heard were taken for guns at sea; but, at two adjoining villages, were so distinct of something singular passing through the air, towards the habitation of Mr. Topham, that five or six people came up, to see if any thing extraordinary had happened to his house or grounds. When the stone was extracted, it was warm, smoaked, and smelt very strongly of sulphur. Its course, as far as could be collected from different accounts, was from the south-west. The day was mild and hazy, a sort of weather very frequent in the Wold hills, when there are no winds or storms; but there was not any thunder or lightning the whole day. No such stone is known in the country. There was no eruption in the earth; and, from its form, it could not come from any building; and, as the day was not tempestuous, it did not seem probable that it could have been forced from any rocks, the nearest of which are those of Hamborough Head, at a distance of twelve miles.* The nearest volcano, I believe to be Hecla, in Iceland.

Investigations respecting it.

The exhibition of this stone, as a sort of show, did not tend to accredit the account of its descent, delivered in a hand-bill at the place of exhibition; much less could it contribute to re-

* Extracted from the printed paper delivered at the place of exhibition.

remove the objections made to the fall of the stones presented to the Royal French Academy. But the Right Hon. president of the Royal Society, ever alive to the interest and promotion of science, observing the stone so exhibited to resemble a stone sent to him as one of those fallen at Sienna, could not be misled by prejudice; he obtained a piece of this extraordinary mass, and collected many references to descriptions of similar phenomena. At length, in 1799, an account of stones fallen in the East Indies was sent to the president, by John Lloyd Williams, Esq. which, by its unquestionable authenticity, and by the striking resemblance it bears to other accounts of fallen stones, must remove all prejudice. Mr. Williams has since drawn up the following more detailed narrative of facts.

Account of the Explosion of a Meteor, near Benares, in the East Indies; and of the falling of some Stones at the same Time, about 14 Miles from that City. By John Lloyd Williams, Esq. F. R. S.

Explosion of a meteor near Benares, and the falling of some stones at the same time.

A circumstance of so extraordinary a nature as the fall of stones from the heavens, could not fail to excite the wonder, and attract the attention of every inquisitive mind.

Among a superstitious people, any preternatural appearance is viewed with silent awe and reverence; attributing the causes to the will of the Supreme Being, they do not presume to judge the means by which they were produced, nor the purposes for which they were ordered; and we are naturally led to suspect the influence of prejudice and superstition, in their descriptions of such phenomena; my inquiries were therefore chiefly directed to the Europeans, who were but thinly dispersed about that part of the country.

The information I obtained was, that on the 19th of December, 1798, about eight o'clock in the evening, a very luminous meteor was observed in the heavens, by the inhabitants of Benares and the parts adjacent, in the form of a large ball of fire; that it was accompanied by a loud noise, resembling thunder; and that a number of stones were said to have fallen from it, near Krakhut, a village on the north side of the river Goomty, about 14 miles from the city of Benares.

Large ball of fire with noise like thunder.

The meteor appeared in the western part of the hemisphere, and was but a short time visible: it was observed by several Europeans, as well as natives, in different parts of the country.

In

In the neighbourhood of Juanpoor, about 12 miles from the spot where the stones are said to have fallen, it was very distinctly observed by several European gentlemen and ladies who described it as a large ball of fire, accompanied with a loud rumbling noise, not unlike an ill discharged platoon of musquetry. It was also seen, and the noise heard, by various persons at Benares. Mr. Davis observed the light come into the room where he was, through a glass window, so strongly as to project shadows, from the bars between the panes, on a dark coloured carpet, very distinctly; and it appeared to him as luminous as the brightest moonlight.

Degree of light,
&c.

Investigation on
the spot by Mr.
Davis.

When an account of the fall of the stones reached Benares, Mr. Davis, the judge and magistrate of the district, sent an intelligent person to make inquiry on the spot. When the person arrived at the village near which the stones were said to have fallen, the natives, in answer to his inquiries, told him, that they had either broken to pieces, or given away to the Tefeldar (native collector) and others, all that they had picked up; but that he might easily find some in the adjacent fields, where they would be readily discovered, (the crops being then not above two or three inches above the ground,) by observing where the earth appeared recently turned up. Following these directions, he found four, which he brought to Mr. Davis: most of these, the force of the fall had buried, according to a measure he produced, about six inches deep, in fields which seemed to have been recently watered; and it appeared, from the man's description, that they must have lain at the distance of about a hundred yards from each other.

Account by the
natives.

What he further learnt from the inhabitants of the village, concerning the phenomenon, was, that about eight o'clock in the evening, when retired to their habitations, they observed a very bright light, proceeding as from the sky, accompanied with a loud clap of thunder, which was immediately followed by the noise of heavy bodies falling in the vicinity. Uncertain whether some of their deities might not have been concerned in this occurrence, they did not venture out to enquire into it until the next morning; when the first circumstance which attracted their attention was, the appearance of the earth being turned up in different parts of their fields, as before mentioned, where, on examining, they found the stones.

The assistant to the collector of the district, Mr. Erskine, a very intelligent young gentleman, on seeing one of the stones, brought to him by the native superintendant of the collections, was also induced to send a person to that part of the country, to make inquiry; who returned with several of the stones, and brought an account similar to that given by the person sent by Mr. Davis, together with a confirmation of it from the Cauzy, (who had been directed to make the inquiry) under his hand and seal.

Mr. Maclane, a gentleman who resided very near the village of Krakhut, gave me part of a stone that had been brought to him the morning after the appearance of the phenomenon, by the watchman who was on duty at his house; this, he said, had fallen through the top of his hut, which was close by, and buried itself several inches in the floor, which was of consolidated earth. The stone must, by his account, previous to its having been broken, have weighed upwards of two pounds.

At the time the meteor appeared, the sky was perfectly serene; not the smallest vestige of a cloud had been seen since the 11th of the month, nor were any observed for many days after.

Of these stones, I have seen eight, nearly perfect, besides parts of several others, which had been broken by the possessors, to distribute among their friends. The form of the more perfect ones, appeared to be that of an irregular cube, rounded off at the edges; but the angles were to be observed on most of them. They were of various sizes, from about three to upwards of four inches in their largest diameter; one of them, measuring four inches and a quarter, weighed two pounds twelve ounces. In appearance, they were exactly similar: externally, they were covered with a hard black coat or incrustation, which in some parts had the appearance of varnish, or bitumen; and, on most of them were fractures, which, from their being covered with a matter similar to that of the coat, seemed to have been made in the fall, by the stones striking against each other, and to have passed through some medium, probably an intense heat, previous to their reaching the earth. Internally, they consisted of a number of small spherical bodies, of a slate colour, embedded in a whitish gritty substance, interspersed with bright shining spiculæ, of a metallic or pyritical nature. The spherical bodies were much

Inquiries by Mr. Erskine.

and Mr. Mac-lane.

State of the weather.

Stones of this description which have been seen by the author.

Some account of the same.

harder than the rest of the stone: the white gritty part readily crumbled, on being rubbed with a hard body; and, on being broken, a quantity of it attached itself to the magnet, but more particularly the outside coat or crust, which appeared almost wholly attractable by it.

The author declines any conjecture.

As two of the more perfect stones which I had obtained, as well as parts of some others, have been examined by several gentlemen well versed in mineralogy and chemistry, I shall not attempt any further description of their constituent parts; nor shall I offer any conjecture respecting the formation of such singular productions, or even record those which I have heard of others, but leave the world to draw their own inferences from the facts above related. I shall only observe, that it is well known there are no volcanos on the continent of India; and, as far as I can learn, no stones have been met with in the earth, in that part of the world, which bear the smallest resemblance to those above described.

Iron in the Bornian collection said to have fallen in Bohemia.

IT remains for me to speak of a substance mentioned in the *Lithophylacium Bornianum*, Part I. page 125, described thus: “*Ferrum retractorium, granulis nitentibus, matrice virescentis immixtis, (Ferrum virens, Linn.)* cujus fragmenta, ab unius ad viginti usque librarum pondus, cortice nigro scoriaceo circumdata, ad Plann, prope Tabor, circuli Bechinensis Bohemiae, passim reperiuntur.”

The iron thus described, is moreover made remarkable by a note *, which observes, that credulous people assert it to have fallen from heaven during a thunder storm, on the 3d of July, 1753.

The collection of Baron Born, it is well known, has a place in the cabinet of the Right Honourable Charles Greville, who, from the effect produced by comparing the histories and structure of the Italian and Yorkshire stones with the description of this iron, was induced to search the collection of Born, where he discovered the very substance asserted to have fallen on the 3d of July, 1753. How far these four substances have resemblance to each other, it will soon appear not to be my province to anticipate.

* Quæ (fragmenta) 3 Julii, anni 1753, inter tonitrua, e cœlo pluvisse creduliores quidam asserunt.

The President having done me the honour to submit his specimens of the Yorkshire and Italian stones to my examination, I became indebted to Mr. Greville and Mr. Williams for a similar distinction: and, being thus possessed of four substances, to all of which the same origin had been attributed, the necessity of describing them mineralogically did not fail to present itself. To execute this task, no one could be more eager, and certainly no one better qualified, than the Count de Bournon. He has very obligingly favoured me with the following descriptions.

Other stones of similar origin.

Mineralogical Description of the various Stones said to have fallen upon the Earth. By the Count de BOURNON, F. R. S.

THE stones I am about to describe, are not of any regular shape; and those which were found in an entire state, that is, those which had not been broken, either by their fall or otherwise, were entirely covered with a black crust, the thickness of which was very inconsiderable.

Count Bournon's description of the various stones.

The stones which fell at Benares, are those of which the mineralogical characters are the most striking: I shall therefore begin the following description with them; and shall afterwards make use of them, as objects of comparison, in describing the others.

STONES FROM BENARES.

These stones, as well as the others described in this paper, whatever may be their size, are covered over the whole extent of their surface with a thin crust of a deep black colour: they have not the smallest gloss; and their surface is sprinkled over with small asperities, which cause it to feel, in some measure, like shagreen, or fish skin.

Stones from Benares. Thin black crust; rough like fish skin.

When these stones are broken, so as to shew their internal appearance, they are found to be of a greyish ash colour; and of a granulated texture, very similar to that of a coarse grit-stone: they appear evidently to be composed of four different substances, which may be easily distinguished, by making use of a lens.

Fracture; coarse grained, grayish ash colour; composed of four substances; viz.

One of these substances, which is in great abundance, appears in the form of small bodies, some of which are perfectly globular, others rather elongated or elliptical. They are of various sizes, from that of a small pin's head to that of a pea, or nearly

Opake gray globules, of conchoidal fracture and slight lustre, slightly giving fire with steel, so: and abrading glass;

so: some of them, however, but very few, are of a larger size. The colour of these small globules is gray, sometimes inclining very much to brown: and they are completely opaque. They may, with great ease, be broken in all directions: their fracture is conchoid, and shews a fine, smooth, compact grain, having a small degree of lustre, resembling in some measure that of enamel. Their hardness is such, that, being rubbed upon glass, they act upon it in a slight degree; this action is sufficient to take off its polish, but not to cut it: they give faint sparks, when struck with steel.

Pyrites, not
magnetical.

Another of these substances, is a martial pyrites, of an indeterminate form: its colour is a reddish yellow, slightly inclining to the colour of nickel, or to that of artificial pyrites. The texture of this substance is granulated, and not very strongly connected: when powdered, it is of a black colour. This pyrites is not attractable by the magnet; and is irregularly distributed through the substance of the stone.

Metallic malle-
able iron.

The third of these substances consists in small particles of iron, in a perfectly metallic state, so that they may easily be flattened or extended, by means of a hammer. These particles give to the whole mass of the stone, the property of being attractable by the magnet; they are, however, in less proportion than those of pyrites just mentioned. When a piece of the stone was powdered, and the particles of iron separated from it, as accurately as possible, by means of a magnet, they appeared to compose about $\frac{2}{156}$ of the whole weight of the stone.

Whitish gray,
crumbly earth.

The three substances just described, are united together by means of a fourth, which is nearly of an earthy consistence. For this reason, it is easy to separate, with the point of a knife, or even with the nail, the little globular bodies above mentioned, or any other of the constituent parts of the stone you may wish to obtain. Indeed the stone itself may readily be broken, merely by the action of the fingers. The colour of this fourth substance, which serves as a kind of cement to unite the others, is a whitish gray.

The crust is
probably black
oxide of iron.

The black crust with which the surface of the stone is coated, although it is of no great thickness, emits bright sparks, when struck with steel: it may be broken by a stroke with a hammer; and seems to possess the same properties as the very attractable black oxide of iron. This crust is, however, like the substance of the stone, here and there mixed with small particles of iron

in

in the metallic state: they may easily be made visible, by passing a file over the crust, as they then become evident, on account of their metallic lustre. This is more particularly the case with respect to the crust of those stones which remain to be mentioned, they being much more rich in iron than that I have just described; a circumstance I think it needless to repeat, in the following descriptions of them. The stone now treated of, does not, when breathed upon, emit an argillaceous smell: the same remark may be applied to all the others.

The specific gravity of this stone is 3352.

STONE FROM YORKSHIRE.

This stone, the constituent parts of which are exactly the same as those of the stones from Benares, differs from them, however,

Stone from Yorkshire. Composed like the former; but of finer grain: The globules are less regular:

First. In having a finer grain.

Secondly. That the substance described as being in the form of small globular or elliptical bodies, is not so constantly in those forms, but is also found in particles of an irregular shape; a circumstance that is not met with in the other stones: these bodies are likewise, in general, of a smaller size.

Thirdly. The proportion of martial pyrites, which has precisely the same characters as that in the stones from Benares, is less; on the contrary, that of the iron in a metallic state, is much greater. The quantity I was able to separate by means of the magnet, appeared to me to compose about eight or nine parts, in one hundred, of the weight of the whole mass. I observed many pieces of this iron, of a pretty considerable size; one of them, taken from a portion of the stone I had powdered, in order to separate the iron, weighed several grains.

The pyrites in less quantity; and the metallic iron more.

The part of the stone which is in an earthy state, and which serves to connect the other parts together, has rather more consistence than that of the preceding stones; and its appearance does not differ much from that of decomposed felspar or kaolin. The stone itself, therefore, although by no means hard, is rather more difficult to break with the fingers.

Its earthy part is more consistent.

The specific gravity of this stone is 3508.

It is denser.

STONE FROM ITALY.

This stone was in a perfectly entire state; consequently, its whole surface was covered over with a black crust peculiar to all

Stone from Italy.

coarse grained ;
composed nearly
like the others.

It contained a
yellow transpa-
rent globule,
softer than calca-
reous spar.

all stones of this kind. As the stone was of a very small size, it became necessary to sacrifice the whole of it to the investigation of its nature. Its grain was coarse, similar to that of the stones from Benares: in it might be perceived the same gray globular bodies, the same kind of martial pyrites, and the same particles of iron in the metallic state. The proportion of these last was much less than in the stone from Yorkshire; but rather greater than in the stones from Benares. The same kind of gray earthy substance served to connect the different parts together; and nothing more could be perceived, except a few globules, which consisted wholly of black oxide of iron, attractable by the magnet, and one single globule of another substance, which appeared to differ from all those we have already described. This last substance had a perfectly vitreous lustre, and was completely transparent: it was of a pale yellow colour, slightly inclining to green; and its hardness was rather inferior to that of calcareous spar. The quantity of it, however, was too small to be submitted to such an investigation as might have determined its nature. The black crust which covered the stone, was rather thinner than that of the stones already described; and seemed to have undergone a kind of contraction, which had produced in it a number of fissures or furrows, thereby tracing upon the surface the appearance of compartments, similar in some measure to what is observed in the stones called Septaria.

The specific gravity of this stone was 3.418.

STONE FROM BOHEMIA.

Stone from Bohemia. Composed like the preceding;

The internal structure of this stone is very similar to that of the stone from Yorkshire. Its grain is finer than that of the stones from Benares: in it may be observed the same gray substance, both in small globules and in particles of an irregular shape; also the same particles of metallic iron. The same kind of earthy substance likewise served to connect the other parts together.

This stone, however, differs materially from the others.

but its pyrites
very minute;
the quantity of
metallic iron
much greater;

First. The particles of pyrites cannot be seen without a lens. Secondly. It contains a much larger quantity of iron in the metallic state; inasmuch, that the proportion of that metal, separated from it by means of the magnet, amounted to about $\frac{25}{100}$ of the weight of the whole.

This

This stone has also (owing perhaps to its having remained a ^{and more oxidized} much longer time in the earth than the preceding ones, all of which were taken up nearly at the very instant of their fall) another difference, viz. many of the particles of iron in a metallic state, have undergone an oxidizement at their surface; a circumstance that has produced a great number of spots, of a yellowish brown colour, and very near to each other, over a part of its internal substance. This oxidizement, by adding to the bulk, and to the force of action, of the part we have described as serving by way of cement to the other constituent parts of the stone, has occasioned a greater degree of adhesion between these parts, and has rendered the substance of the stone more compact.

The great quantity of iron in a metallic state which this stone contains, added to its greater compactness, makes it ^{It is harder, and admits a slight polish.} capable of receiving a slight degree of polish; whereas it is impossible to give any polish to the others. When polished, the iron becomes very evident, in the polished part; appearing in the form of small specks, almost close to each other, which have the colour and lustre peculiar to that metal: these specks are, in general, nearly of an equal size.

The black crust of this stone is similar to that of the others.

The specific gravity of the stone is 4281.

It is easy to perceive, from the foregoing description, that these stones, although they have not the smallest analogy with any of the mineral substances already known, either of a volcanic or any other nature, have a very peculiar and striking analogy with each other. This circumstance renders them truly worthy to engage the attention of philosophers; and naturally excites a desire of knowing to what causes they owe their existence. ^{These stones are not in the smallest degree analogous with any other known minerals.}

(To be continued.)

X.

An Answer to Mr. Gough's Essay on the Theory of Compound Sounds. By THOMAS YOUNG, M. D. F. R. S.

To Mr. NICHOLSON.

S I R,

Introductory remarks.

I HAVE already made public, through the medium of your useful Journal, one vindication of my remarks upon Dr. Smith's Harmonics; and I shall now beg you to insert some further observations upon a subject nearly similar. I do not think myself bound to reply to all the arguments that may happen to be advanced against any of my opinions; but when they come from persons of literary respectability, and especially when they convey an imputation of having detracted from the merits of others, I cannot forbear attempting to answer them; and in the present instance a very few words will be sufficient for the purpose. Mr. Gough has published in the last volume of the Manchester Memoirs, a paper in which he professes to defend Dr. Smith's opinions against mine: but he has unfortunately so far mistaken the question between us, that with respect to the principal circumstances, while he imagines he is confuting me, he is completely on my side.

The author's inferences from coalescence of undulations as to the doctrines of sound and light, scarcely to be called a new theory.

Proceeding upon the grounds of the well known facts relative to the grave harmonics, or the third sounds produced by the coalescence of two others, as well as upon the allowed principles of the composition of motion, I had drawn a number of inferences, which appeared to be of some importance in the first place with respect to the doctrine of sound, and which have since furnished me with still more interesting conclusions with regard to light. I was not aware that there was any probability of the justice of my inferences being disputed, or that there was so much novelty in the mode of obtaining them, as to deserve the name of a theory or an invention: but if it be deemed otherwise, I shall always be ready to acknowledge the invention with satisfaction, and to support the theory with alacrity.

Dr. Smith imagined that, when undulations in

I shall now quote the words of Dr. Smith, which gave rise to my animadversions; and this statement is the more necessary,

fary,

ary, as I before omitted to detail the particulars of that author's misconception, and as Mr. Gough appears wholly to have overlooked the passage. "Different particles of the air at the ear" says Dr. Smith, "will keep moving constantly opposite ways at the same time. And in so rare a fluid as air is, where the intervals of the particles are eight or nine times greater than their diameters, there seems to be room enough for such opposite motions without impediment: especially as we see the like motions are really performed in water, which in an equal space contains eight or nine hundred times as many such particles as air does. For when it rains upon stagnating water, the circular waves propagated from different centres, appear to intersect, and pass through, or over each other, even in opposite directions, without any visible alteration in their circular figure, and therefore without any sensible alteration of their motions." Harmonics, 1759, p. 105.

air or in water
crosses each other,
different particles
of the fluid
were at the same
time moved *inter-
se*, according to
the respective
undulations.

It certainly would have "cost me an effort of study" to demonstrate this, although I could not exactly consider it as the "intuitive conclusion of a comprehensive mind." Such a mind appeared to me to comprehend with equal ease the distinct and the confused; and I am only at a loss to conceive how the writer of this passage could ever have composed a valuable treatise on optics. Mr. Gough's theory differs as widely from this statement as mine does. My remark on it was this; "It is surprising that so great a mathematician as Dr. Smith could for a moment have entertained an idea, that the vibrations constituting different sounds, should be able to cross each other in all directions, without affecting the same individual particles of air by their joint forces: undoubtedly they cross, without disturbing each other's progress; but this can be no otherwise effected than by each particle's partaking of both motions. If this assertion stood in need of any proof, it might be amply furnished by the phenomena of beats, and of the grave harmonics observed by Romieu and Tartini, which Mr. Lagrange has already considered in the same point of view." Phil. Trans. 1800, p. 130.

Whereas the
same individual
particles of the
fluid receive and
transmit both
undulations.

I have no objection to admitting the whole of Mr. Gough's propositions, in the particular cases which he has considered; but when he says that the coalescence of two sounds is impossible on mechanical principles, he thinks proper to omit the only case in which I had asserted its existence, that is, when

Sounds may coalesce which arrive at the ear in the same direction;

the sounds arrive at the ear "in the same direction." p. 131. Surely Mr. Gough will not deny the possibility of such a coincidence in direction, so far at least that the physical effect may be the same as if the coincidence were perfect: when, for instance, we listen to two or more sounds passing through a long tube, or to the various subordinate sounds of the same chord or pipe. If he be actually disposed to deny the perfect coincidence in practical cases, I shall only appeal to experience, which shows that the effect of a third sound is most distinctly heard, whenever the theory leads us to expect it; but there will always be a much greater portion of each sound so reflected from the surrounding objects as not to coincide in direction sufficiently for coalescence, and hence the original notes will always be much more audible than the new compound. The ear indeed appears to have greater powers of analysis than one would naturally have expected, it decomposes a "compound" just as if it were a mere "mixture," not only in this case but in many others: how it performs this operation, I do not pretend to determine. When Mr. Gough represents me as maintaining that two musical strings, differing in the times of their vibration, and happening to vibrate in concert, do not occasion two distinct sounds, and that the waves of air are compelled by their mutual interference to coalesce, thereby producing a new succession of pulses, constituting a single sound in the place of the former; his expressions tend to impute to me an opinion which could only be maintained by a person who had never heard a single musical composition, or ever been present at the conversation of a mixed company.

not by losing their respective tones in one single sound; but by producing an additional weaker sound.

All imperfect unisons must produce beats.

I am not solicitous for the application of the term compound by coalescence, to the human voice; but Mr. Gough can scarcely form to himself a distinct conception of it, very different from mine. A mixture of imperfect unisons would inevitably be accompanied by the production of beats; and if he assert that the imperfection is too small to produce this effect, I will only request him to assign any reasonable limit to its magnitude, and by producing the note long enough, I will show that a beat must necessarily ensue. Perhaps a wish to retain the Newtonian theory of the law of the undulations may have led him into these superfluous refinements.

If Mr. Gough will take the pains of examining the phenomena of the grave harmonics, which he seems to have hitherto thought beneath his notice, he will be convinced that the coalescence of musical sounds is not only possible, but of very frequent occurrence, and that the compound sound does actually “acquire sensible properties peculiar to itself;” and if he will explain, in any other manner than I have done, the indubitable fact of the audible impression of the presence of the fourth below the key, in consequence of the coexistence of the sounds constituting the interval of a major third, even when both the notes are freed from their harmonics, and when all echo is avoided; I shall then listen to him, with the attention due to a person who endeavours to regulate his arguments by the results of accurate observation.

The phenomena of the grave harmonics shew that musical sounds coalesce in numerous instances.

I am, Sir,
with great respect,
Your obedient humble Servant,
THOMAS YOUNG.

Royal Institution, May 30, 1802.

CORRECTION.

In your fifth volume (quarto) p. 166, l. 20, *for* “a better third than that,” *read*, “equal to the third.”

The following CORRECTIONS made by Dr. YOUNG in his Paper on the Mechanism of the Eye, which is inserted in our fifth volume (Quarto Series) were omitted to be made in their proper places.

Page 256, line 6, Prop. III. *after e*, insert the base being unity.

Page 257, line 15, Cor. 10. *for* ntu , *read* ntt ; line 16, *for* product &c. *read* square of the cosine of incidence.

Page 258, line 2, Cor. 11. *for* $1 + u^2 - 2u^4$, *read* $2muu$.

Page 258, Prop. V. Cor. See the note in p. 299.

Page 259, Prop. VIII. By a mistake of a sign, the eighth proposition is rendered erroneous; no use having been made of that proposition, it has been inserted without proper revision. It ought to stand thus, with its demonstration:

PROPOSITION VIII. PROBLEM.

To find the path of a ray of light falling obliquely on a sphere, of a refractive density varying as any power of the distance from the centre.

The

The refractive density, in the sense of these propositions, varies as the ratio of the sines, and as the velocity of light in the medium.

(Schol. 2. Prop. I.) Let the velocity at the distance x be $x^{-\frac{1}{r}}$; then, considering the refractive force as a species of attraction, we

have, in Prop. 41. l. 1. Princip. $\sqrt{ABFD} = x^{-\frac{1}{r}}$, $Q = s$, the

sine of incidence, the radius being unity, $Z = s x^{-1}$, $Dc =$

$$\frac{s}{2 x x \sqrt{x^{-\frac{2}{r}} - s^2 x^{-2}}} = \frac{1}{2} s x^{\frac{1}{r}-2} \cdot \left(1 - s^2 x^{\frac{2}{r}-2}\right)^{-\frac{1}{2}}, \text{ and}$$

the fluxion of the area described by the radius $= -\frac{1}{2} s x^{\frac{1}{r}-2}$

$\cdot \left(1 - s^2 x^{\frac{2}{r}-2}\right)^{-\frac{1}{2}}$. Let the sine of the inclination to the radius

at each point be called y ; then $y = s x^{\frac{1}{r}-1}$, $y = \frac{1-r}{r} s x^{\frac{1}{r}-2}$

\dot{x} , and the fluxion of the area $= \frac{r}{2r-2} \dot{y} \cdot \left(1 - y^2\right)^{-\frac{1}{2}}$, of which

the fluent is $\frac{r}{2r-2} Y$, y being the sine of the arc Y ; and the angle

corresponding is $\frac{r}{r-1} Y$. The value of that angle being found for

any two values of x or y , the difference is the intervening angle described by the radius. This angle is therefore always to the difference of the inclinations as r to $r - 1$, and the deviations is to that difference as 1 to $r - 1$.

Corollary. Hence, in the passage to the apsis, and the return to the surface, the deviation is always proportionate to the arc cut off by the incident ray produced: therefore such a sphere could never collect parallel rays to any focus, the lateral density being too small towards the surface.

Page 259, line 9, *for* but the two last, &c. *read* the seventh may either be deduced from the eighth, or may be demonstrated independently of it.

Page 264, last line, *after* internally, *insert* Or, if a lens of equal mean dimensions, and equal focal length, with the crystalline, be supposed to consist of two segments of the external portion of such a sphere, the refractive density at the centre of this lens must be as 18 to 17.

XI.

Experiments on the Velocity of Air issuing out of a Vessel in different Circumstances; with the Description of an Instrument to measure the Force of the Blast in Bellows, &c. By Mr. BANKS, Lecturer in Natural Philosophy.*

THE object of this inquiry may be announced in the following proposition: If an elastic fluid is generated in a given vessel, or any way contained in it, and at liberty to issue out of the said vessel through a given aperture, to determine the resistance which the vessel meets with from its action, or the power which it has of communicating motion to the vessel, as in a sky-rocket, Sadler's steam engine, &c.

Before we proceed to relate the experiments, it may be proper to premise certain principles deduced from theory. If a tube be filled with any kind of fluid, as *air, water, mercury*, &c. and placed in a vacuum, every fluid will flow out with the same velocity. For though the pressure of a column of mercury of a given altitude be much greater than an equal column of water, yet the weight of the particles to be projected is greater in the same ratio. On the other hand, if air is lighter than water, the particles projected are also lighter in proportion. If a tube of 16 feet high be filled with air of any density, that air, like water, would flow into a vacuum with a velocity of 32 feet per second, no corrections being made for resistance †.

And if we take the gravity of air to water as 1 to 840, then a column of one foot of water compressing air, will produce as great a velocity in that air as a column of air 840 feet high, supposing it was of uniform density.

If we take the whole pressure of the atmosphere equal to 53 feet of water, or its height (supposing it to be equally dense,

Proposition. To determine the mechanic force or reaction of an elastic spouting fluid.

All uniform fluids flow from an orifice with the same velocity *in vacuo*, if their heights be equal, however different their densities.

Equal velocities will be produced in one issuing fluid by the pressure of another; if the height of the former to that of the latter, be inversely as its density.

* From the Manchester Memoirs, Vol. V. p. 398.

† In the supposition of a perpendicular tube open at the top, filled with air or any elastic fluid, the author takes the density of the column at the bottom, or where the aperture is made, to arise solely from the weight of the elastic column; and the altitude to be that which would be if the whole column were reduced to the density of that at the bottom.

which

The atmosphere which in this case will make no difference) equal to 33 multiplied by 840, or 27720 feet; then as the square root of 16 is to 33 feet of water, or 27720 feet of air. It would therefore propel air into a vacuum with the velocity due to this fall: *viz.* $\sqrt{16} : \sqrt{27720} :: 32 \text{ feet} : 1332 \text{ feet per second.}$

To prove whether air compressed by 33 feet of water would be impelled into the atmosphere with the above velocity, I have made, amongst many more, the following experiments:

A, plate xiv. fig. 1*, is a vessel of a known capacity, into the top of which is screwed an aperture of a known area. The tube *Td*, recurve at *d*, is soldered or screwed into the top of the said vessel. The hole *a* is stopped, and water poured into the tube at *T* till it is full, at which time a quantity of water will have passed out of the tube at *d*, and condensed the air in the vessel, more or less as the tube *Td* is longer or shorter.

At this time a person who has closed the aperture at *a* with a finger of one hand, and held a half second pendulum in the other, removes both at the same time, while at the same moment an assistant opens a cock over the tube *T*, which supplies it with water as fast as it can descend into *A*. The moment that the water appears at *a*, the time-piece is stopped, and the time of expelling the air is noted, from which, by knowing the capacity of the vessel, the velocity may be obtained.

If the tube *Td* should be continued near the bottom of the vessel *A* while it was filling with water, the length of the compressing column would be gradually diminishing, and of consequence the pressure would be constantly changing; hence the open end of the tube is as near the top of the vessel as is consistent with a free passage for the water.

EXPERIMENTS.

The vessel *A* contained 15 lb. 6 oz. of water, from which we find its capacity is 425.088 cubic inches.

The area of the aperture *a*, through which the air is expelled, .0046 inches.

* There being no figure in the Transactions, I have inserted the drawing referred to. N.

Exper.

Exper. I. The altitude of *T* above the vessel is 30 inches. Experiment I. with a fall of 33 inches. Time of expelling the air, by several trials, is 33 seconds.

Exper. II. The altitude of *T* is six feet. The time of filling, by several trials, is 21.3 seconds. Experiment II. with a fall of 72 inches.

In the first experiment, 425.088, the solidity of the vessel, divided by .0046, the area of the hole *a*, gives 92410.4 inches for the length of the stream of air driven out in 33 seconds; divide that length by 33, and we shall have 233.3 feet, the velocity per second, communicated by 30 inches of water. Computation of the first experiment gives actual velocity per sec. = 233.3 feet;

The second experiment by the same process gives 361.6 feet per second. and of the second gives 361.6 per second. If we would compare these together, we may say, as the square root of 30, the head, is to 233.3, the velocity; so is the square root of 72, the second head, to 361.8 feet, the velocity per second.

Again:—As the square root of 6 feet is to 361.6, so is the square root of 33 feet to 845.2 feet per second, the velocity produced by that head, or the initial velocity with which the atmosphere would enter a vacuum. This velocity, found by experiment, is 487 feet per second less than has been assigned by theory. Whence the velocity of air from the atmosphere into a vacuum is computed, and turns out to be 845.2 instead of 1332 feet, as by theory.

It appears however that the results, as determined by theory and experiment, do not differ more than in the case of effluent water. For, if we would reduce the velocity of effluent water found by theory, to that which experience gives, we must multiply it by .634. Accordingly, if we multiply 1332 feet, the velocity of the atmosphere entering a vacuum as calculated above, by .634, the product is 844.5 per second, differing but $\frac{7}{10}$ of a foot from that just found by experiment. But this result, corrected by the multiplier given by experience for effluent water, proves to be very correct.

I have also made experiments by sinking vessels in water, till their tops were even with its surface, and opening the aperture that the rising water might expel the air, by which I obtained the same velocities as above; but the method of computing is much more intricate, for which reason I shall not insert them. Experiments with rising water.

From the above it appears, that a pressure equal to 33 feet of water, will expel air out of the bellows into the atmosphere with a velocity of 845 feet per second; that one foot of water in depth will produce a velocity of $147\frac{1}{4}$ feet, and one inch a velocity of 42 feet per second, or 20 miles an hour. General result:

Hence we may construct a table shewing the velocity communicated to air by any head of water; for as the square root applied to the construction of tables for effluent air pressed by water;

of 6 feet is to the velocity produced by that head, so is the square root of any other depth to the velocity produced by that depth.

or in bellows, We may also, from the above, construct an instrument which will shew the velocity with which air flows out of any kind of bellows, with as much accuracy, as the experiments have been made on which its construction depends.

Description of the Instrument, &c.

by means of an instrument here described. It is a portable vessel containing water, to be formed into an open gage tube;

The metal box or tube *B*, plate xiv. fig. 2, may be about the size of the figure; the top must be made air tight by the cover *L*; into the bottom is fixed the small tube *AC*, and into the piece *D* is cemented the glass tube *ED*; the instrument is then inverted, and some water poured through the tube *AC*, till, when in its proper position, it is visible at *D*. It is now ready for use, and the end *A* may be fixed in a hole made in the upper board of the bellows, and the water will rise in the glass tube, in smith's bellows, generally from 9 to 12 inches, furnace bellows, generally four feet or more. But where the compression is great, quicksilver may be used instead of water, only in this case the instrument should be made of iron, as quicksilver causes the screws of brass to break. Or, instead of quicksilver, the tube *ED* may be sealed at the top *E*, and then a length of 12 or 18 inches will be enough for any blast. The glass tube needs not be more than one-eighth or one-tenth of an inch in diameter.

Whatever compression there may be in the bellows, there will be the same in the upper part of the tube *B*, which will force the water into the glass tube *DE*, and make the air in its upper part of the same density, deducting from the compressing force the altitude of the water raised above *D*, which however will be of little or no importance; if the gauge is placed in a horizontal position, with the glass tube downward, there will be no difference of density.

or the tube may be hermetically closed.

The computation for the force in the case where a tube hermetically sealed at the top is adopted in the instrument, will be effected by considering that the space occupied by any elastic fluid is inversely as its force. Thus, let the tube be 12 inches long, and suppose the water to be raised one inch, then it will be 11 : 12 :: the force of the atmosphere : the force of the air in the tube :: 1 : $1\frac{1}{11}$. Hence a scale may be adapted to the instrument,

instrument, to express the force of condensation over and above the common atmospheric pressure; which force is signified in the instance above, by the fraction $\frac{1}{11}$, unity being the atmospheric pressure. If we denote the atmospheric pressure by 30 inches of mercury, or 32 feet of water, then the force $\frac{1}{11}$ in the above example, will be expressed by 2.727 inches of mercury, or 2.91 feet of water; and the like for any other instance.

If a mercurial instrument of the above construction be preferred, it becomes necessary to add the height of the mercurial column to the force found as above: thus, if the condensation of air be from 12 into 9 inches, then the addition to the force of the internal air in the tube is equal to $\frac{1}{3}$, or 10 inches of mercury, to which must be added the three inches raised in the tube, and the whole force will be 13 inches of mercury, exclusive of that of the atmosphere.

If mercury be used, its density must be considered in the short instrument.

This sort of instrument or gauge serves equally well for finding the expansive force of any kind of elastic fluid, as for measuring the velocities with which they issue out of the place of their confinement. It may be applied to all kinds of bellows, to condensed steam, and to the air pump.

General application to all gases.

XII.

On the Variation of Rate in a Time-Piece, as indicated by the Changes in the Arc of Vibration. In a Letter from Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

S I R,

Lynn, July 20, 1802.

FROM what I have already mentioned in a former paper *, it follows, that in clocks used in making astronomical observations, it is necessary to observe the arc of vibration very frequently, and when it is found different from that generally described by the pendulum, the rate of the clock must on this account be corrected.

Expediency of frequent observation of the arc of vibration in clocks.

To determine this correction, let x denote the time which a pendulum, vibrating in an arc exceedingly small, will lose when made to vibrate in a larger arc of the same circle, T the

Rule for deducing the variation in time from that of the arc.

* See pa. 76 of this vol.

number of seconds in 24 hours (86400), and D the number of degrees described on each side of the perpendicular *.

Then $x = T \times \frac{D^2}{52480}$ nearly. Consequently $x = D^2 \times 1.6463$ nearly.

From the above theorem the following table was computed, which shews the time lost by a pendulum in 24 hours, by increasing its semi-arc of vibration in the same circle 1' of a degree.

Table of vibrations, and correspondent los from their increase.

Half the arc of vibration of a pendulum in a circle.	Loses per day of a pendulum vibrating the least arc possible.	Difference.	Loses per day on increasing its semi-arc of vibration 1' of a degree.
0°	0,"0		
0 $\frac{1}{4}$	0, 1	0,"1	,006
0 $\frac{1}{2}$	0, 4	0, 3	,020
0 $\frac{3}{4}$	0, 9	0, 5	,033
1	1, 6	0, 7	,046
1 $\frac{1}{4}$	2, 5	0, 9	,060
1 $\frac{1}{2}$	3, 7	1, 2	,080
1 $\frac{3}{4}$	5, 0	1, 3	,086
2	6, 6	1, 6	,107
2 $\frac{1}{4}$	8, 3	1, 7	,113
2 $\frac{1}{2}$	10, 3	2, 0	,133
2 $\frac{3}{4}$	12, 4	2, 1	,140
3	14, 8	2, 4	,160
3 $\frac{1}{4}$	17, 4	2, 6	,173
3 $\frac{1}{2}$	20, 2	2, 8	,187
3 $\frac{3}{4}$	23, 2	3, 0	,200
4	26, 3	3, 2	,213

* Simpson's Fluxions, Art. 464.

The use of this table will be easily understood by the following examples.

EXAMPLE I.

Suppose a clock goes mean solar time when the pendulum Examplest vibrates 2° on each side of the perpendicular, what will it lose per day when the pendulum vibrates through an arc of three degrees?

Against 2° and 3° in the first column, we have 6,"6 and 14,"8 respectively in the second column. Then $14,"8 - 6,"6 = 8,"2 =$ the time lost per day.

EXAMPLE II.

Suppose a clock gains $1''$ per day, when the pendulum vibrates $1^\circ 49'$ on each side of the perpendicular, what would be its rate when the pendulum vibrates $1^\circ 51'$?

Between $1^\circ \frac{3}{4}$ and 2° in the first column, we have ,''107 in the fourth column. Then $1'' - ,''\overline{107} \times 2 = 0,"786 =$ the daily rate of the clock gaining.

I am, with respect,

Sir,

Your humble servant,

E. WALKER.

ANNOTATION.

The arc of vibration in clocks is likely to be affected in practice by changes in the density of the air, in its temperature, and in the action of the first mover; and these causes will most probably afford results considerably different from those inferred from the simple pendulum in vacuo. I take it to be experimentally ascertained by the registers of the performance of astronomical clocks very firmly fixed, that the gridiron pendulum, with a very flexible spring suspension, is not sensibly affected during the different stations of the barometer and thermometer; and I apprehend that the first mover, if a weight, must operate either with a constant force or with periodical variations, occasioned by the train or mechanism through which its action is transmitted, the influence of oil, &c. which periods are rendered shorter by the well known methods of disengaging the escapement, or parts nearest the pendulum. Many other considerations will offer themselves to the learned author, as to

Whether theory can be applied to an engine so compounded as a clock, &c.

those parts of the vibration to which the maintaining power may be applied so as to increase the arc, while the time may be either lengthened or shortened according to the circumstances, the general nature of the escapement, &c. and these will probably lead to a conclusion, that the corrections of time to be applied to a clock, which either in its arc of vibration or otherwise gives symptoms of irregularity, can be gained only from direct observation.

W. N.

XIII.

Description of Atkins's Hydrometer for ascertaining the specific Gravities of spirituous Liquors. By J. FLETCHER, Esq. Communicated by the Author.

Utility of descriptions of philosophical instruments.

AMONGST the various papers which are to be found in the works of those journalists who have so materially contributed to the advancement of our knowledge, there are perhaps but few which have been more effectually conducive to this end than those which are appropriated to the description of the instruments of science. It is indeed much to be lamented, that the merchant, the manufacturer, and the artizan, should continue to be so generally deprived of the advantages to be obtained from the use of such of them as are adapted to their purposes, for want of the necessary information with regard to the mode of applying them.

The instrument which is here intended to be described, is one of those which stand in a great measure in this predicament; and as it appears very well to deserve a description, it is conceived that, imperfect as the following one is, it will not be unacceptable.

Spec. gr. only criterion of the strength of spirits.

It is now universally acknowledged, that the specific gravities of spirituous liquors afford the only tolerable criterion of their strength, and consequently of their comparative values. To ascertain the specific gravity of a liquid with a considerable degree of precision, is an operation of no great difficulty. The most accurate method of performing it is perhaps that which is also the simplest: to weigh the fluid in a vessel in which its bulk is capable of being nicely measured. Much more, however,

ever, remains to be done in order to the estimation of the strengths of spirituous liquors: the contraction of the mass of a compound of alcohol and water on their mixture, and the variation in its measure in respect of temperature, are each of them of sufficient practical importance to render their appreciation necessary when the value of the spirit is to be discovered. Thus for example, 18 gallons each of alcohol and water will produce only 35 gallons of the compound, and a difference of 30° in temperature of Fahrenheit's scale, occasions such a change in the specific gravity of proof spirit, as, if omitted to be taken into consideration, would render the dealer liable to an error of upwards of ten per cent. in the estimation of its strength and consequent value.

Corrections necessary to be applied for concentration and temperature.

18 parts each of alc. and water produce only 35 of the mixture. A variation of 30° of temp. occasions a change of 10 per cent. in the apparent strength of proof-spirit. Commercial estimation of value of spirituous compounds,

In commercial transactions with respect to spirituous liquors, it is necessary to appreciate their strength by comparing it with that of spirit of a certain supposed quality in this respect as a standard. This standard-spirit (which is called *proof-spirit*) is of such a degree of strength that its specific gravity is about .920 at 60° of Fahrenheit's thermometer; and the object of enquiry in all cases is with regard to the quantity of this proof spirit which would be equivalent to a given quantity of any spirit under examination. The language of the spirit-dealers with respect to their terms of "*over-proof*" and "*under-proof*," has in all cases this kind of reference to commercial value.

by comparison with proof-spirit. Definition of terms "over-proof" and "under-proof."

When they say that a certain kind of spirit is 30 per cent. over proof, they mean that if 100 parts by measure be increased to 130 by the addition of water, it will become of proof strength; and when they say that it is 30 per cent. under proof, they mean that 70 parts by measure of proof-spirit will become, when increased to 100 by the addition of water, of equal strength to that of the spirit in question. If therefore a gallon of proof spirit be worth 8s. 4d. the same quantity of the former kind of spirit will be worth 10s. 10d. and of the latter 5s. 10d.

It is not therefore sufficient for the purchaser of a sample of spirit, or the officer who is to collect the duties on it, to be informed of its specific gravity at a given temperature: it is necessary for him to ascertain the quantity of proof-spirit which is capable of producing, or being produced, by the spirit in question by the addition of water only, and which is consequently equivalent to it in value.

The desideratum is to find the equivalent quantity of proof-spirit.

The

Calculation in-applicable to this purpose.

Inconvenience of tables.

Instrumental solution preferable.

The hydrometer with many weights,

with a sliding-rule.

The latter most eligible.

Description of Atkins's hydrometer.

Square stem, with the letters of the alphabet on one face,

and four weights,

When it emerges to the bulb with one weight, the next heavier sinks it to the top of the stem.

Stem and divisions thus in effect multiplied by 5.

With the weights singly applied, indicates specific gravities from .806 to 1.000;

The solution of this problem being almost impossible by any application of mathematical formulæ, it is of course desirable to be enabled to obtain it instrumentally, or by inspection of tables previously laid down from experiment; and the use of the latter, under such circumstances as those in which operations of this kind are generally performed, having been found inconvenient, its instrumental solution has been universally preferred. This has been attempted principally in two ways: by the hydrometer, with a multiplicity of weights adapted to the various corrections for temperature; and by the same instrument more simply constructed, so as to indicate merely the specific gravity of the liquor; the necessary corrections being applied by a scale or sliding-rule.

The instrument here to be spoken of is constructed on this latter principle, which appears for many reasons to be the best: The hydrometer A B (pl. XV fig. 3) is of brass. It is eight inches in length, with an elliptical bulb an inch and a half in diameter, and two inches long. The stem is square, each side being about $\frac{7}{8}$ of an inch wide, and, when the instrument is intended for such liquors only as are specifically lighter than water, is engraved only on one face with the 26 letters of the alphabet, and an O, or zero, at the top and bottom; opposite to each, and between every two of which, is a division for marking the point of the stem which is cut by the surface of any liquor in which it floats, the whole number of divisions being 55, as shewn in the plate. The weight of the instrument is about 400 grains, and it is provided with four weights, marked 1, 2, 3, 4, weighing respectively 20, 40, 61, and 84 grains, to be applied as occasion requires on the shank of the instrument C, on which they are retained by the button or fixed weight B. These weights are so adjusted, that when with any one of them, as for instance No. 2, the stem of the instrument, when floating in a given liquor, just emerges to the lower division O, it will, on changing the weight for the next heavier, No. 3, become immersed exactly to the other division O, towards its superior extremity. The stem is by this means virtually extended to five times its real length, and the number of divisions in effect augmented to 272. Thus without any weight at all, as represented in the figure, it would sink in a liquor whose specific gravity was .806, exactly to the upper division O,

O,

O, and in one whose specific gravity was .843, to the lower division **O**; the intermediate divisions answering to intermediate specific quantities. With No. 1, it indicates specific gravities from .843 to .880; with No. 2, from .880 to .918; with No. 3, from .918 to .958; and with No. 4, from .958 to 1.000; the surface of water in which the instrument is made to float with the latter weight, exactly coinciding with the lower **O**, at 55° of temperature.

The whole interval from .806 to 1.000, comprehending the specific gravities of all spirituous compounds, is in this manner divided into five nearly equal portions, each of which, comprizing from .038 to .040, is measured by the whole length of the stem; so that each of the 54 intervals on its face corresponds to considerably less than an unit in the third place of the specific gravity, and indicates a difference of about one half per cent. or two quarts in 100 gallons, in the quality of the compound, and the instrument is therefore abundantly sensible to very minute variations in these respects.

With regard to the application of the weights, no error can possibly be committed. If the instrument floats in any liquor, so that its surface cuts any part of the stem, it is properly loaded. Any weight but the proper one will either sink it entirely, or suffer the stem to rise totally above the fluid.

The specific gravity indicated by any division on the stem of the hydrometer, is seen instantly by reference to a sliding-rule belonging to it, whose two faces are shewn, plate XV. fig. 2 and 3, on which the lines of divisions **A A**, **A A**, marked with the letters of the alphabet, represent those on the instrument when loaded with the weight, whose number is marked over the **O** at the commencement of each series, and the exterior lines of divisions **B B**, **B B**, near its edges, shew the corresponding specific gravities compared with that of water at 55°.

In order, however, to ascertain the strength and value of the spirit, it is necessary to examine its temperature and apply the proper correction, which is done with extreme accuracy by the same sliding-rule, by an ingenious application of two scales of unequal parts to each other, *viz.* the lines **C C**, **C C**, marked "*Atkins*," and the lines **A A**, **A A**, marked with the letters of the alphabet. The mode of doing this is as follows: the temperature of the liquor being taken by the thermometer accompanying the instrument, the asterisk or index, plate XV. fig. 1, by setting an index on the slider to the degree of temperature as shewn by the thermometer.

each length of the stem answering to about .04; and each division to less than .001 in spec. gr. or $\frac{1}{2}$ per cent. in strength.

Spec. gr. indicated by the divisions on the stem compared with the rule.

Correction for temperature applied,

Division on slider answering to the letter on the stem gives per centage.

Example from the rule set as in the plate.

Uses of the other lines on the slider.

Of the concentration-line.

Concentration to be deducted from the per centage.

Example. Diminution of 3 parts on adding 50 of water to 100 of 50 over-proof.

Concentration on mixture of proof-spirit and water.

on that part of the slider opposite to the thermometrical scale D, is to be set to the degree of temperature so found, and the division on the line C, marked "*Atkins*," which then corresponds on either side of the rule with that letter and division on the alphabet line A, at which the stem was cut by the surface of the liquor, indicates the strength and value in commercial terms. Thus suppose the temperature to be 68° (at which the index appears set in the plate), and the hydrometer, when loaded with its weight No. 1, to float with the surface, cutting the division I on the stem (corresponding to a specific gravity of about $854\frac{1}{2}$), we find, on setting the slider of the rule to 68° on the thermometer scale, that 46 over proof corresponds to that letter and division, and is consequently the strength and value of the liquor.

The lines marked "*Dicas*" and "*Clark*" on the rule, are for giving the appreciation of the values and strengths as shewn by the instruments invented by those artists, and are intended for the use of such as have been in the habit of employing them; the latter being perhaps inserted principally on account of the use of Clark's instrument for the purposes of the revenue.

The figures on the line marked "*concentration*" amongst the *over-proofs*, indicate the diminution in volume which takes place on reducing the given compound to proof; and amongst the *under-proofs*, the diminution which takes place on reducing proof-spirit to that strength. This must in the first case be deducted from the quantity marked on Atkins's line (C), in order to obtain the accurate per centage according to the estimation of commercial men. For instance, 50 over-proof on Atkins's line indicates, that if to 100 parts of spirit of the strength which answers to this division, we add 50 of water, we shall get proof-spirit, of which, however, the quantity will be only 147 parts; the concentration or diminution of bulk by mixture being 3 parts. With regard to the spirit which is under-proof, the figures on the concentration line indicate the diminution of volume in pints for every 100 gallons of the mixture: if, for instance, 70 gallons of proof-spirit be combined with 30 of water, we get a compound whose strength is that which is marked on the line C, 30 under proof; but the concentration being seven pints, we shall get only $99\frac{1}{8}$ gallons.

When the hydrometer is intended only for spirituous compounds, the weights are applied singly as before-mentioned: if, however, in addition to the weight No. 4, the others are successively applied, it becomes applicable to the examination of worts and other liquors, whose specific gravities are from 1.000 to 1.109, or, in the language of the brewery, up to 40 lbs. per barrel heavier than water. In this case the other three sides of the stem are also graduated, and another rule with an ivory slider, carrying a thermometrical scale for comparing worts at different temperatures, is included in the case with the instrument.

Applicable to worts by applying the weights doubly.

Spec. gr. of worts designated by lbs. per barrel.

Additional rule for the brewery.

The writer of this paper having made a considerable number of experiments with this instrument, on the specific gravities of a variety of spirituous liquors, had originally intended to have given their results; but it has already run to such a length, that they must be deferred to a future communication. The errors, even including those which must necessarily arise from the various temperatures of the compounds, and the different quantities of foreign matters with which these fluids, in an impure state, may be supposed to be charged, appear to fall within very narrow limits; and the extreme facility and expedition with which it resolves the questions to the solution of which it is applicable, cannot fail to render it very highly valuable to those for whose purposes it is principally intended.

Errors in results very small, and advantage of facility of use very considerable.

XIV.

An Examination of Sig. VOLTA'S Experiments which he calls fundamental, and upon which his Theory of Galvanism rests; with a Description of a very sensible Electrical Condenser, and an Explanation of the Action of the Electric Fluid in the Galvanic Instrument. By JOHN CUTHBERTSON, Philosophical Instrument Maker, No, 54, Poland Street, London. Communicated by the Author.

IN Vol. I. 8vo, page 136, paragraph three, of this Journal, Sig. Volta affirms, "If two insulated discs, one of copper and the other zinc, be applied together for a moment and then separated, the zinc will be positive and copper negative." I have always had the same result, but some times much weaker than at others.

Volta's expt. of the contact of insulated discs of copper and zinc producing + el. in the zinc, and — el. in the copper. Succeeds.

Page

Expt. of copper and zinc joined, and the copper to touch a zinc condenser, and afford — el. in this last. Page 137, in the last paragraph, it is said, “ If a piece of zinc to which is joined a piece of copper, and the zinc held by two fingers or in any other manner, and the copper made to touch the superior disc of the condenser, which is zinc, while the inferior is in communication with the ground, a moment afterwards raising the upper disc in the air, it will

Did not produce — el. but + el. be negatively electrified.” This I have always found the contrary, *i. e. positive*.

Expt. of zinc touching a copper condenser, and affording no el. Page 138, paragraph three, it is also said, “ If the superior disc of the condenser be copper, and a piece of zinc be made to touch it immediately without any intervening substance, nothing will be obtained, because the zinc being then in contact at the two opposite ends with copper and copper, two equal forces act in opposite directions, and by that

Did not succeed, means destroy or counterbalance each other.” If the superior disc was copper, I always found it negative, and if it was zinc touched by a piece of copper, it was positive; both contrary to Sig. Volta's assertion.

We find then that only one experiment out of three succeeds, which he calls fundamental, upon which his theory of galvanism is founded.

The foregoing expts. were often repeated. Knowing Sig. Volta's abilities both as a philosopher and experimentalist, I own I mention the result of the foregoing experiments (though it is after numerous repetitions) with much diffidence; but the experiments are so simple, that it is almost impossible that I can have erred.

By reasoning upon the phenomena, when copper and zinc are made to touch each other, it becomes easily explicable by the old known laws of electricity.

EXPLANATION.

Explanation. In expt. I. either the zinc attracted or the copper repelled the elect. while in contact. In the first experiment, where zinc and copper are made to touch each other, we find, on separation, that the zinc has acquired a greater share of electric fluid than it had before the touch, and the copper less, by virtue of their mutual action upon each other when in contact. So that the zinc must have either attracted the electric fluid out of the copper, or the copper must have repelled it from itself into the zinc; and that the first is the true cause may be proved by connecting the copper disc with conductors, and then touching it by the insulated zinc, which will be found, on separation, to be much more strongly positive

positive than when the copper was insulated. For whatever quantity of electric fluid may be drawn out of the copper by the zinc, is recovered by its being in connection with continued conductors; whereas, if it had been repelled from the copper into the zinc, it must be stronger when insulated, because when insulated, the copper has no other body but the zinc to receive that which it repels from it; and when not insulated, every other conductor with which it is connected takes a part in proportion to their conducting property. As the zinc disc is found to be much stronger when the copper disc is not insulated, I conclude that zinc has the property of attracting the electric fluid out of copper when they are in contact.

The effect is stronger when the zinc alone is insulated, and less when the copper alone is insulated; hence the zinc is inferred to attract.

In regard to the second experiment, where the zinc is found to be much more strongly positive than in the first, is clear from what I have already said, *i. e.* because the copper disc is not insulated, but is free to act as their mutual action upon each other may require.

In expt. II. the insulation of the zinc increases the effect.

In the third experiment, in which no effect should follow (according to Sig. Volta), the zinc was found to be as strongly electrified as in the last experiment. Neither the zinc nor the copper being completely insulated, they are at liberty to act upon each other as their mutual contact directs; the zinc at liberty to attract and the copper to give, which is in no wise repugnant to the old laws of electricity.

Expt. III. The metals being at liberty to assume the respective states on contact, did so accordingly.

How Sig. Volta could be led to such erroneous conclusions is not easy to understand, unless he was deceived by the condensers he made use of. "To render that feeble electricity sensible and manifest, he recommends flat metallic plates covered with a slight layer of sealing wax or lac varnish," which I was not a little surpris'd at, because I have always found such condensers very equivocal in their results, and shew different signs without any variation or obvious cause, or at least such as I was unable to detect. They are more easily excited by a negative power than by a positive one, and retain it much more tenaciously; and hence, when they happen by any means to be strongly electrified by a negative power, it is almost impossible to discharge them of it, so as to be fit to proceed on immediately with experiments that require nice investigation.

Sig. Volta supposed to have been deceived by his varnished condensers.

Since the invention of the galvanic instrument, various electrometers, condensers, doublers, multipliers, &c. &c. have been used to investigate its electrical properties, all of which appear

Mr. John Read's construction of the condenser.

appear to me to be much inferior to one constructed in the year 1796, by Mr. John Read, an ingenious electrician at Knightbridge; but as he invented it near the time of his retiring from business, he did not publish it, consequently it is known but by very few electricians. I have found it of great use not only in galvanism, but in all experiments wherein small quantities of electricity are to be made evident; and I doubt not but that it will be considered as a valuable acquisition by electricians. I have found it capable of rendering much smaller quantities sensible than any other instrument. By this instrument we are able to learn the positive and negative sides of only one piece of zinc, copper, and wet cloth; and I have not heard that it has been done by any other instrument less than with a series of twenty*.

A Description and Use of Mr. Read's compound Electrical Condenser.

Description of
Read's con-
denser.

Fig. 1. plate XVI. represents a vertical section of the large condenser: *aa* is a round flat plate of brass of about eight inches, standing insulated upon a wooden foot *g*; *gh* is a hollow brass cylinder, and *fh* is a solid stick of glass, with a brass mounting at *f*, to which the plate *aa* is fixed; *bb* is another plate of brass somewhat less in diameter, and has a round hole in the middle of about two inches diameter; *cd* is a hollow cone fixed to *bb* at *cc*; *de* is a hollow brass cylinder fixed to the cone at *d*, and made to slide up and down upon the cylinder *gh*; *i* is a milled head-screw which holds *bb* fast when it is at its proper distance from *aa*, where a stop is made for that purpose; when *i* is loose, *bb* falls down by its own weight, and rests upon the foot *g*.

Altered by the
author.

The above is the original construction, which I found more complex and less portable than I could wish; for which reasons I make them in the following manner; and fixing the condensing plates in a vertical position instead of horizontal, it has all the advantages, is more simple, and perfectly portable, and I have no doubt will meet with the approbation of Mr. Read himself.

Description of the improved Condenser.

Description of
the improved
condenser.

Fig. 2 represents a vertical section of the large condenser standing edgewise to the eye: *aa* and *bb* are two flat round

* Bennet's electrometer alone shewed the state of 40 pairs. See Van Marum in *Philos. Journal*, octavo, I. 174.—N.

brass plates of about six inches; *c* is a glass cylinder fixed at one end into a wooden foot *d*; *e* is a brass ball, and mounting at the other end to which the plate *bb* is screwed fast; *f* is a brass wire with a joint at its lower end, and at the other end a ball to which the plate *aa* is screwed, standing parallel to *bb*. By means of the joint, the plate *aa* may be moved back in the situation of the dotted outline *ag*. The joint has a shoulder which stops the plate *aa*, and keeps it at its proper distance from *bb*.

Fig. 3 is the common gold-leaf electrometer, with an addition of two vertical brass plates of about $1\frac{1}{2}$ inch in diameter; one is screwed fast to the brass mounting at the top of the electrometer, and the other to a wire which has a joint fixed to the foot of the electrometer, by which it is moveable, and is set either parallel and at a proper distance from the other plate, the joint being furnished with a stop for that purpose, or may be moved backwards in the direction of *ga*, fig. 2: *l* is a brass cup with a shank at the bottom, which screws into a hole in the top of *e*, fig. 2, serving to examine the state of electricity excited by dropping of metals, &c.; *m* is a stick covered with tinfoil, which screws into the hole at *e*, to examine the electricity of the atmosphere; *n* is a brass wire jointed at *o*, with a shank, which can also be screwed into *e* when required, or into the top of the gold leaf electrometer, fig. 3. These two instruments, fig. 2 and 3, are used separately or combined, as the nature of the experiment may require. When the experiment requires the aid of both condensers, they are combined, as is represented fig. 4, the two fixed plates standing towards the eye. The fixed plate of the large condenser has a small brass pin at one side, which, when the instruments are used together, must touch the fixed condensing plate of the gold-leaf electrometer*.

THE METHOD OF USING THE DOUBLE ELECTRICAL CONDENSER.

To shew the Electric Fluid excited by Effervescence, &c.

Screw *l* into the top of the ball *e* of the large condenser, and set therein a china or glass cup with proper ingredients for that

* Instead of gold leaf Mr. Read uses very fine fibres of flax, which he thinks more sensible than gold leaf; but they are very difficult to be seen, and more easily deranged. If gold leaf be properly managed, I think it preferable.—C.

Bennet's electrometer with a condenser.

Use of the two condensers. To shew the electricity from effervescence, &c.

purpose;

purpose; then join the two instruments as in fig. 4. While the effervescence is going on, turn back the moveable plate of the large condenser into the position of the dotted line, fig. 2, taking care not to touch the fixed plate; then if the excited electricity be very strong, the gold leaf will diverge; but if not, just move the electrometer so that it is quite free from the pin, turn back its moveable plate, and if a sufficient quantity of electric fluid be excited, the gold leaf will diverge.

To shew if there be any sensible Quantity of Electric Fluid in the Atmosphere.

Atmospheric
electricity.

Screw the stick *m* into *e* of the large condenser; a convenient place being chosen not much surrounded by buildings or trees, set the instruments combined as in the last experiment, and proceed in the same manner.

Method of applying the combined Condensers to the Galvanic Instrument.

Method of ap-
plying the con-
denser to the gal-
vanic instru-
ment.

Screw the short end of *no* into *e* of the large condensing plate (the instruments being combined as in the former experiments); bend the end downwards, at such a distance from the table, or whatever it may stand upon, that the two pieces of metal, zinc and copper, as at *n*, can be put under it, and drawn away from under it again, without its touching the table when the metal is drawn away. Take two pieces of metal, zinc and copper, about the size of half a crown or upwards, either separate or soldered together, with their flat sides in contact, and push them under the end *n* of *on*. After remaining a short space of time, a quarter or half a minute, draw them away from under the point, and take notice that the point does not touch the table, or any other conductor; then turn back the moveable plate of the large condenser; move the electrometer so that its plate shall no longer touch the pin of the large plate, and then turn its moveable plate back; the gold leaf will remain undisturbed.

Electricity from
the galvanism of
a single pair of
plates.

Turn up the condensing plates to their first position; place the two instruments together as before, taking particular care that the fixed plate of the electrometer condenser touches the pin proceeding from the large plate. Lay upon the pieces of metal before used, a piece of woollen cloth well soaked in a solution of muriate of ammonia, or any other menstruum com-
monly

monly used for the galvanic experiments either upon the zinc or the copper, and push them under the point of *n* again. Press the point down upon them, that it may be perfectly in contact; after they have remained the time before-mentioned, draw the metals away, and separate or turn back the large condenser plate, and also the small one, after separating it from the pin of the large one, and immediately the gold leaf will diverge. If the zinc was the uppermost, then the gold leaf will diverge with positive electricity; but if it was underneath, the gold leaf will diverge with negative electricity. It makes no difference in the general effect, upon which metal the wet cloth was laid; or whether two pieces of cloth were used, one under the metals and the other above; or only one either above the metals or under them. But if the cloth be only laid upon the copper and not upon the zinc, the electric fluid brought into action will be so weak, that the combined instrument can hardly shew it; if laid upon zinc, the divergency will be about $\frac{1}{40}$ of an inch; sometimes more and sometimes less*.

By reflecting on this phenomenon I found the following explanation, without having recourse to any new hypothesis.

Explanation of the Action of the Electric Fluid in the Galvanic

When flat pieces of zinc and copper are laid in contact, the zinc becomes positive and the copper negative at the moment of the touch; and while they remain in contact, the electric fluid contained in them both is perfectly in equilibrio. The copper has given and the zinc has received such a quantity of electric fluid as their mutual action upon each other required; and in consequence of this property, they present a mutual resistance to any further change being produced upon them. If then any menstruum be added to the opposite side of the metals, capable of producing a change in their metallic property (such as the fluid contained in the wet cloth), a change in their electrical property must of course follow. But as this change in the metallic property is only superficial, it will only be there that its electric property is changed. The other parts of the two metals in contact will remain unaltered, and maintain their property of resistance. The change produced by the action of

Explanation of the facts offered.
Zinc and copper in contact arrive at an equilibrium of electricity; the zinc + and the copper —.

Chemical action alters the electrical property of that part of the metal in which it takes place.

* When the atmosphere is in a favourable state. In this as well as all other experiments where so small quantities are to be made evident, the atmosphere has great influence.

the

This change is asserted to be of an opposite nature to the electric state of the unaltered metal, and therefore produces electric states at the wet surfaces opposite to those of the dry metals: which do not pass from zinc to copper through the metals and become quiescent; but from zinc to copper through the fluid, this passage being easiest. Hence the galvanic current: which is progressive, because the fluid cannot perfectly conduct the electricity as fast as it is extricated.

The shock from the accumulation suddenly transmitted.

the menstruum, with respect to its electrical property upon that part only of the metal whereon it has acted, is exactly the reverse of that of the parts not acted upon. The part of the zinc thus acted upon, must consequently be disposed to throw off its electric fluid, and would give to that part of the copper, which by a like action is disposed to absorb it) so that the two states of the surfaces acted upon would unite and counterbalance each other imperceptibly) if it were not opposed by the still existing property and mutual action of the two metals in their parts not acted upon by the menstruum; hence it follows that the electric fluid is propelled forwards from the zinc through the menstruum to the next adjoining copper in the pile or trough; but this can only happen in a progressive manner, on account of the menstruum being an imperfect conductor, which appears to be an indispensable condition to the maintaining any electric intensity.

The shock or sensation felt on touching the two opposite ends of the galvanic instrument, depends upon the menstruum, together with the resisting property of the two metals in contact. The fluid must be adequately proportioned between being an electric and a perfect conductor. If it were a perfect conductor, the electric fluid would pass from the zinc through the menstruum to the next adjoining copper, as quick as it is given off by the altered part of the zinc; no accumulation would ensue, and consequently no sensation of shock or discharge would be perceived. If it were an electric, the electric fluid given off by the altered zinc would be stopped, and accumulate till it become possessed of sufficient force to overcome the mutual resistance of the two metals in contact, and pass through by a reverse motion to the copper; consequently no sensation would be felt by forming a communication between the two opposite ends. For though there would be an accumulation, yet it cannot be united so as to act in concert with that of the other combined metals; being shut off by the interposed electric menstruum, and too feeble of itself to affect our senses.

ANNOTATION. W. N.

Short history of instruments for shewing minute quantities of electricity.
W. N.

At page 396, vol. I. of the quarto series of this Journal, an outline is given of the history of all the instruments for shewing or measuring minute quantities of electricity, with a statement of their advantages and defects. With regard to the condenser discovered

discovered by Volta in 1780 *, and used for atmospheric electricity and in the grand discovery of the electricity produced by chemical changes, it was applied very early by Cavallo in the compound form; that is to say, by charging a smaller instrument with the contents of a larger. Mr. Bennet before 1787 †, added the condenser to his electrometer, and invented the method called doubling, which had before been used with the electrophore by Lichtenberg ‡ and Klincock §. Our electricians, particularly Mr. Cavallo and Mr. Bennet, were at this time fully aware of the spontaneous electricity of the doubler, and the evils to be apprehended from the varnish and the actual contacts. I think it was Mr. Cavallo who substituted small knobs of sealing wax instead of varnish, which I saw adopted in a mechanical doubler by Dr. Darwin in 1787; and in the year 1788 I communicated to the Royal Society || the revolving doubler, in which the plates approach and recede without touching; and soon afterwards I made the spinning instrument, consisting of a condenser and an electrical well. In the same year Mr. Cavallo published ** his *collector* of electricity, which is a condenser having two uninsulated plates, which separate like plate *a*, fig. 2, on each side of *b*, without contact; and in the third vol. of his "Complete Treatise on Electricity," 1795, he describes his *multiplier*, which consists of two condensers, equally perfect; one of which is made to charge the other by repeated alternations of the compensating plate.

The above short sketch will enable the reader to ascertain to what extent the ingenious contrivers of the instruments in plate XV. have availed themselves of the labours of former operators.

* Journal de Physique for May and August 1783, and Phil. Transf. LXXII. p. 237.

† Phil. Transf. LXXII. p. 32.

‡ Journal de Phys. Jan. 1780, p. 20.

§ Phil. Transf. LXVIII. p. 1029.

|| Phil. Transf. LXXVIII.

** Phil. Transf. LXXVIII.

XV.

Observations on the Phosphorescence of the Tremolite, and of the calcareous Phosphate of slow Solution, known by the Name of Dolomie. By M. LE COMTE DE BOURNON, Fellow of the Royal and Linnæan Societies. Translated from the Original; communicated by the Author.

The phosphorescence of minerals has been little inquired into.

FEW researches have yet been made concerning the phosphorescence of the bodies of the mineral kingdom. No satisfactory explanation has yet been given of this phenomenon, though this knowledge would undoubtedly throw new light upon the study of minerals, and prove a great acquisition to natural philosophy and chemistry.

The methods of exacting it: 1. by friction; 2. by heat.

This property of emitting light, which daily observation shews to belong to many more minerals than had formerly been suspected, requires particular management. In some fossils, such as quartz, blende, corundum, &c. &c. it becomes sensible only by friction. In others it exhibits itself only when the mineral is placed upon a red hot coal, or upon any other body heated to a similar temperature. This is the case with the carbonate of strontian, of barytes, &c. In others again, the phosphorescence is developed both by friction and by heat, as is the case with the phosphates and fluates of lime, as well as with a great number of carbonates of the same earth, particularly those of a brown or yellowish colour.

These facts give occasion for several questions, the solution of which would be extremely interesting.

Does it arise from combined light?

Do these two kinds of phosphorescence depend upon the same cause? In all the stones which exhibit them, and are at the same time coloured, the colour diminishes in proportion to the disengagement of the phosphorescence by the action of heat: and when they cease to be phosphorescent, they at the same time intirely cease to be coloured. Does this phenomenon proceed from the disengagement of the combined or interposed light? Does the colour in these stones always belong, in reality, to metallic oxides, particularly those of iron? May it not rather belong simply to the combined light? In this case, may it not be supposed that the light is decomposed, by combining with these stones, and that it then entered into their composition

composition only by insulated rays, or by combination of two or more, and not the whole of the rays? We are, however, enabled to make an observation with respect to this subject, namely, that these stones constantly exhibit by the action of heat, a phosphorescence of the same colour, whatever may be the colour of their proper substance. For instance, the fluates of lime which exhibit the most lively and variegated colours, constantly give a light inclining to the violet, with the single exception of the Siberian variety, which has been named the Chlorophane, and which, though of a violet colour, gives a phosphorescence of a beautiful emerald green. In others, as in some carbonates of lime, in those of barytes, of strontian, &c. though these stones are perfectly colourless, the phosphorescence is constantly reddish, or orange yellow. What may be the cause of these contrasts?

In some cases the cause which produces the phosphorescence of the stones seems to belong to an essential part of their substance, which is never completely expelled from them. This is the case with the calcareous fluates and phosphates, &c. In others it appears to be purely accidental in the stone, and shews itself only in a certain number of individuals belonging to the same species. In the first case, this property ought to be indicated amongst the specific characters of the stone; in the second, it cannot serve as character of the species, but can merely be used to designate one of its varieties. Such is, for example, that which exists in the tremolite and in the dolomie, respecting which I intend here to offer some observations, which appear to me to deserve the attention of mineralogists.

M. de Saussure and Professor Blumenbach were, as far as I know, the first who observed the two kinds of phosphorescence in the tremolite; and, since them, all the works of mineralogy have placed this property amongst the distinctive characters of this stone. Many tremolites, indeed, are endowed with this double phosphorescence; but this is by no means the case with all, nor can this character be considered as essential to its nature.

The tremolite, both that which is found in different vallies of Mount St. Gothard, and that which is brought to us from a great number of other places, is generally inclosed in a granulated carbonate of lime, the grains of which are of various degrees of fineness, and their adhesion more or less considerable.

The phosphoric colour is the same in all stones of the same species, however themselves may be coloured.

It may arise from a permanent cause, or from some portion that can be abstracted.

The tremolite is not in all specimens phosphorescent.

Those which are in a phosphorescent gangue are themselves phosphorescent; and on the contrary when the gangue is not phospho-

rescent the tremolite is not so.

Among these carbonates of lime, which constitute the gangue of the tremolite, and very frequently belong to the species which is called dolomie, a great number are endowed with the double phosphorescence; but, on the other hand, we meet with several, in which not the slightest trace of this property can be found. The tremolites inclosed in the first partake, though in rather an inferior degree, of their phosphorescence, whereas those which are inclosed in a non-phosphorescent carbonate of lime are equally void of that property.

That the phosphorescence arose from interposed carbonate of lime, was proved by taking it up by nitric acid, which destroyed the luminous quality.

From the very first moment when I observed this fact, it occurred to me, that the phosphorescence of the tremolite might very probably proceed merely from that of the carbonate of lime, which undoubtedly is interposed between its parts. I therefore selected some crystals that were inclosed in phosphorescent carbonate of lime, and after having satisfied myself that they were themselves phosphorescent, both by friction and by the action of heat, I digested them for some time in nitric acid. When I took them out, their surface was perforated with small cavities, occasioned by the solution of the portions of carbonate of lime, and friction was then incapable of making them display the slightest phosphorescent light. This light, however, was emitted, at the point immediately after ignition, though in an extremely weakened degree. I afterwards reduced some of the same crystals to a coarse powder, and this powder having remained again for some time in the acid, was intirely deprived of all its phosphorescent property.

This quality therefore belongs to the variety, and not the species.

I could no longer doubt that the carbonate of lime, interposed between the particles of the tremolite was the real cause of the phosphorescence of this substance, when its gangue was endowed with the same property. It therefore appeared to me to be at the same time perfectly proved, that phosphorescence cannot be ranked amongst the characters of the species in this substance, and that it ought to be considered merely as distinctive of one of its varieties.

Whether the large portion of lime said to exist in tremolite can be admitted as a constituent part?

A new doubt, which was a very natural consequence of this observation, now presented itself. Could it be true, that the lime which chemists have reckoned to amount to $\frac{1}{100}$ amongst the constituent parts of the tremolite, does exist in it in so large a proportion. In order, if possible, to satisfy this doubt, I selected some crystals of non-phosphorescent tremolite, which

had only an argillo-martial substance for their gangue, and requested Mr. Chenevix, who has already rendered such useful services to mineralogy, to examine them by analysis. I likewise gave him some crystals from amongst those phosphorescent ones which I had broken, and afterwards deprived of their phosphorescence, by digesting them for some time in nitric acid.

My suspicions were verified: Mr. Chenevix found only $\frac{4}{100}$ of lime in each of these two analyses. But what at the same time struck me, was that the tremolite taken from the phosphorescent variety, having for its gangue a carbonate of lime of the species called the dolomie, which is likewise phosphorescent, but whose calcareous part had been taken away by the nitric acid, gave by analysis only $\frac{4}{100}$ of argil, whilst that which was taken from an absolutely argillaceous gangue gave $\frac{14}{100}$ of the same earth. Mr. Klaproth having found no argil at all in the analysis which he had before made of this substance, it is probable that its existence in it, like that of the calcareous carbonate, proceeds merely from its simple interposition.

These two observations appeared to me to be very interesting to the study of mineralogy, especially to that part of it which relates to analysis; since it shews with what care the chemist ought to avoid confounding, with the true constituent parts of a substance, those which are foreign to it, and only interposed between its parts. It is extremely common to find the parts of a mineral, even in the state of crystallization, envelope more or less of the portions of that substance which constitutes its gangue; and what may likewise contribute much to mistakes in this respect, is the kind of constancy with which, (whatever may be its cause, has hitherto been little attended to) the same substance, placed in similar circumstances, admits this interposed extraneous substance, in equal, or nearly equal proportions. It is not therefore sufficient that the chemist should select for his analysis, amongst the crystals of a substance, those which appear to him to be the most pure, (and the perfection of their form and transparency is the strongest presumption which he can have in this respect;) but he should also repeat the same analytical process upon the same substance taken from totally different gangues.

Probability that a portion of the gangue, whether lime or argil, is interspersed in the mineral.

Hence it is of importance that chemical analyses of any mineral should be repeated upon specimens taken from different gangues.

When

The phosphorescence of tremolite being admitted to arise from interposed carbonate of lime, will account for the differing observations of authors.

When it is once admitted that the tremolite is phosphorescent only in proportion as this property is contained in the carbonate of lime interposed in its substance, the variations which some authors have found in its phosphorescence, will be easily accounted for. It must, for example, be the more easily obtained by friction, as the hardness of this stone is less considerable; because the friction, by breaking its surface, will successively arrive at the interposed particles of carbonate of lime. And hence it is quite natural that the fibrous varieties should be more phosphorescent than the others, and that these should be the less so in proportion as they are harder.

Though the phosphorescence of tremolite cannot be admitted as specific, yet the facility with which this very hard mineral is crushed by pressure with the hammer and its elastic resistance, are peculiar specific characters.

As this character, which was represented as specific in the tremolite, must no longer be considered as such, there is one which has been overlooked, and which, in my opinion, ought to be added to those which have been already observed in it; I mean the great facility with which, notwithstanding its hardness, which, in the purest specimens is such that it easily cuts glass, it is crushed by the mere pressure of the hammer, and the kind of flexibility which it then exhibits. If, in breaking it, the pressure be moderate, the crystals of tremolite divide, pretty generally, according to the length of their prisms, into small fibres, which are frequently as fine as those of the amianthus, to which, in this state, they have much resemblance. We may then even increase the pressure without breaking the fibres, which in this case afford by their resistance the same sensation as is felt from a slightly elastic body. This effect, as well as the reduction of the tremolites into small fibres, is more distinct the less pure the mineral. Both properties however are observed, but in a much less degree in the purest tremolites, and consequently in those whose hardness is the most considerable.

Tremolite is found in Scotland, at Mount Vesuvius, and in Bengal.

With a view to add as much as possible to the knowledge already acquired respecting this substance, I shall add to the list of places which have been indicated as the native countries of the tremolite, Scotland, Mount Vesuvius, and Bengal. Mr. Greville's rich cabinet in London contains specimens from each of those different places; a description of these will probably be acceptable to mineralogists.

Description of the tremolite of Scotland.

The tremolite of Scotland is in the fibrous state, its fibres being very fine and close, part of which is disposed in divergent

gent rays, transversely crossed by other fibres, so as to represent a kind of texture, as is sometimes the case with the zeolite mezotipe. This tremolite is of a greenish white colour: it adheres to a granulated but very compact mass of carbonate of lime, mixed in almost equal proportions with the same tremolite, also in a granulated state, on which account it strikes fire with steel. This carbonate of lime possesses both kinds of phosphorescence, but the light which it gives is of a slightly blueish colour; the same is the case with the tremolite which it contains. This carbonate does not belong to the dolomie, and it dissolves in acids in the same manner as the common carbonate of lime.

The tremolite of Vesuvius is likewise in the fibrous state, with fine and close fibres; its colour is a greyish white, it adheres to a gangue composed of carbonate of lime, of an immense quantity of very fine small fibres of the same tremolite, and of a great number of very small crystals of pyroxene, of a beautiful green colour and transparent. Some portions of idocrase are also observable, of which there is a group at one of the extremities of the specimen in pretty large crystals. The carbonate of lime does not belong to that of slow solution; it possesses both kinds of phosphorescence, and the light which it emits is of a very lively deep orange colour. The tremolite exhibits absolutely the same phosphorescence.

We owe our knowledge of the tremolite of Bengal to Sir John Murray. It is in pretty large crystals of a greenish grey colour, bedded separately in a granulated carbonate of lime, the very fine grains of which have a strong adhesion to each other; a character which, joined to the great whiteness of this stone, causes it very much to resemble a piece of double refined sugar. This carbonate of lime belongs to the species of the dolomie; it is even one of those in which I have observed the solution to be the slowest and most insensible, but it is nevertheless completely dissolved in the nitric acid, leaving only a light whitish and clouded residue, which disappears when the acid is diluted with water. Its hardness, which is much superior to that of the ordinary carbonate of lime, is rather inferior to that of the fluuate of the same earth; and this is the case with all the dolomies, not excepting those whose grains have the least adhesion with each other. This dolomie is not phospho-

Tremolite of
Mount Vesu-
vius.

Tremolite of
Bengal.

phosphorescent either by friction or by the action of heat, and the tremolite which it contains is intirely in the same state. There are many other dolomies also which possess no phosphorence, as has been also observed by the Abbé Hauy. This character therefore still belongs to the variety, and not to the species.

XVI.

Outline of the History of Galvanism: with a Theory of the Action of the Galvanic Apparatus. By JOHN BOSTOCK, M. D. From the Author.

To Mr. NICHOLSON.

SIR,

Introductory
letter.

HAVING been lately employed in some experimental inquiries on the subject of galvanism, I found it commodious to arrange the numerous discoveries that have been made in this department of science into the historical form. The facts which have been successively developed begin to assume so elevated a rank among the branches of natural philosophy, that a sketch of the most important and best established amongst them seems desirable, in order that the experimenter may be easily enabled to see what has been done by his predecessors, and may thus be prevented from wasting his time and exertions upon points which have been previously investigated. From reflecting upon the labours of others, and comparing them with my own experiments, I have been led to form a theory of the action of the galvanic pile, which seems to explain in an easy manner most of its singular properties. I am indeed well aware of the undue attachment which every one feels for the offspring of his own imagination, and I shall not be surprized if you perceive in the hypothesis many blemishes which have escaped my notice. I have however sent you both the history and the theory, in order that you may insert them in your Journal, if you think them deserving of a place there.

I am, SIR,

Your obedient Servant,

JOHN BOSTOCK, M. D.

Liverpool, June 1, 1802.

HISTORIC

HISTORIC SKETCH OF GALVANISM.

THE first publication * upon the subject is the work of Galvani himself, which appeared in 1791. It begins by giving an account of the following accidental discovery. A frog had its hinder legs separated from its body, except that the crural nerves were left undivided, and was by chance laid upon a table on which stood an electrical machine. It was observed that when the animal was placed in contact with, or sufficiently near to, an extensive surface of a conductor of the electric fluid, if a spark were taken from the machine in any direction, the legs of the animal were spasmodically contracted. When the frog formed part of the electric circle, so that the fluid passed immediately through it, a quantity almost imperceptible was found to excite contractions of the muscles. A much smaller quantity of the electric fluid produced the effect, when the animal was prepared in the manner described above, than when the body was left entire, because in the former case the fluid was confined to the narrow track of the nerves in its passage along the circuit. A *prepared frog* appears therefore to be a most delicate electrometer, as it exhibited contractions where no marks of electricity could be discovered by the instruments either of Bennet or Cavallo. Galvani afterwards found that contractions could be produced in the limbs of prepared frogs by the electricity of the atmosphere, and it was in consequence of some arrangements which he made for this purpose, that he was led to his great discovery of animal electricity. He found that he was able to produce contractions in the limbs of frogs without the aid of any foreign or artificial excitement, merely by the application of a conducting substance from the nerve to the muscle. These contractions were capable of being produced, of whatever substances the circuit of communication was composed, provided only they were all conductors of the electric fluid. The effects were found to be much increased by applying a metallic coating to the nerve. He found this peculiar species of electricity to exist in a great variety of animals, and that the contractions may be excited

First publication on galvanism by Galvani in 1791. Legs of a frog, separated from the body, were convulsed by extremely minute portions of electricity.

Proper galvanism, or contractions without excited electricity.

* Sultzer in his *Theorie des Plaisirs*, quoted by Fabbroni, *Philos. Journal*, quarto IV. 120, mentioned the taste by contact of two metals.--N.

Theory of *Galvani*; that the interior of the muscle is charged plus, and the nerves form a communication with the outside.
Valli's Letters on Animal Electricity, 1793.
Volta's Letters to *Cavallo*, 1793.

No charge in the animal.

but electricity from the contact of different metals applied to the nerves.

The galv. action supposed to exist in nerves only.

either in the whole body or in particular parts of it, as long as the animal possesses any remains of vitality. Galvani supposed these phenomena to be analogous to the effects of the Leyden phial; that there is an excess of electric fluid in the interior of the muscle or in the nerve, and a deficiency on the outside, or *rv*. The nerve he conceived to act the part of the wire in the Leyden phial. Soon after the publication of Galvani's work, *Valli's* Letters on Animal Electricity appeared in the *Journal de Physique*, vol. 41. & seq.

In the Transactions of the Royal Society for 1796, *Volta's* letter to *Cavallo* was published, which besides giving an account of Galvani's discoveries, contains many original experiments and observations. The analogy of the Leyden phial he shows is without foundation, for he found that he could excite similar contractions in the limbs, when the conducting circuit only touched two parts of the nerve, two muscles, or two parts of the same muscle; in order to accomplish this, it is however necessary to use two different metals. He supposes that in these cases the muscular contractions are produced by a small quantity of electricity which is excited by the action of the metals upon each other; this he conceives to depend upon a general law of the electric fluid, and that its effects are visible in the experiments of Galvani only because the prepared animal is the most delicate species of electrometer. *Volta* endeavours to prove by experiment, that the action is always in the first instance upon the nerves, and that the muscles are only affected through their medium. He imagines that it is not necessary that a communication should exist between the nerves and the muscles according to the opinion of Galvani, he imagines that the contractions will be produced in the limbs, if the influence be only made to pass from one part of a nerve to another part of the same. But we shall find that in this idea *Volta* is probably mistaken, as in his experiments the moisture adhering to the nerve formed a communication between it and the muscle. He found that if different parts of a nerve, or indeed if the body of the animal in general, be laid upon two different metals, and these metals be made to communicate by a conducting substance, muscular contractions are produced. These experiments succeed with more certainty when the skin is removed; this precaution is more especially requisite if the animal have a dry skin, as is the case with birds and quadrupeds.

ped. In a second letter which is printed in the same vol. of the Phil. Transf. Volta pursues his experiments and observations upon the subject of galvanism. If a single muscle, or a part of it be armed with coatings of two different metals, and these be made to communicate by a conductor, the contractions will be excited, but no effect will be produced if two coatings of the same metal be employed. Worms and snails he found could not be excited by this influence; but flies, beetles, grasshoppers, and butterflies, he found were subject to its action. Upon the whole it appears that those animals only which have distinct limbs, with flexor and extensor muscles, are excitable by animal electricity. In those animals which are acted upon by the galvanic influence, Volta found that it was only the muscles which are under the direction of the will that can be made to retract; in his experiments he never perceived that the heart was affected by the two metals, though this organ is thrown into strong contractions by the slightest chemical or mechanical stimulus. The two metals which were found by their union to perform the most powerful effects were zinc and silver. Volta placed these metals one on each surface of the end of the tongue; when they were brought into contact no motion was produced, but a strong sensation of taste was excited. When the metals were applied to the root of a tongue cut from the mouth, contractions were produced.

In the same year in which Volta's letters appeared in the Phil. Transactions, Fowler published an essay on Animal Electricity; these works must therefore be considered as equally original. The extracts given above from Volta's letters, prove that he considered the phenomena of galvanism to depend upon the operations of the electric fluid; other experimenters however conceived it impossible to reconcile the new discoveries with their previous ideas of the nature of electricity, and Fowler commences his treatise by this enquiry. In order to ascertain this point he investigates the circumstances which are necessary to the production of the muscular contractions. These he found to be the contact of the two metals with each other, and their communication with the animal; the contractions may also be produced by bringing the metals in contact with each other in water, without either of the metals touching the animal. When the metals are applied to

Of the various animals, those only were found to be affected which have distinct limbs, &c. and those muscles only which are subject to the will.

Zinc and silver.

Fowler's Essay, 1793, Whether galvanism be electricity.

Conditions of the effect: that two different metals should touch each other and the animal; viz. the nerve and muscle;

either immediately,
or mediately by
water.

Attempt to dis-
prove the theory
of an electric
charge in the
animal system;
and to shew that
galvanism is not
electricity.

Torpedo.

Effect on worms,
and on the in-
voluntary mus-
cles.

Discovery of the
flash of light.

Professor Robi-
son makes a
pile of zinc and
silver.

Darwin in 1794
considers galva-
nism as electri-
city.

the nerve alone, Fowler still considers it requisite that there be a connection between the nerve and the muscle; in ordinary cases this connection is effected by the moisture which adheres to the nerve, a circumstance which Volta seems to have overlooked. Valli had endeavoured to form a theory of animal electricity founded upon the idea that the electric fluid was unequally dispersed through the body, and that the application of the metals produced an equilibrium; Fowler performed a number of experiments to disprove this idea, and apparently with success. He concludes the first part of his essay by giving it as his opinion, that the phenomena of galvanism are not reconcilable to the known laws of electricity, because for the excitement of the electric fluid, motion between an electric and a conductor appears to be necessary, whereas in these experiments two metals only are the substances employed. He also failed in causing charcoal to conduct this influence, though it is a better conductor of electricity than the animal fluids; in this particular, however, he appears to have been mistaken. He considers the influence as very similar in its effects to the action of the torpedo, though not altogether the same. He was unable to produce the muscular contractions in worms, yet he found that when a worm or a leech lying upon silver, put its mouth upon zinc, it appeared to suffer great uneasiness. Dr. Fowler in the second place proceeds to enquire, whether all the muscles of the body be subject to this new influence. He found it difficult to excite any contractions in the heart, though at length, by using proper precautions, he succeeded; but he was not able to excite contractions in the stomach or intestines. He discovered that when the nerves of vision were acted upon by the two metals, a sensation of a flash of light was perceived in the eye. When inflammation was excited in a limb, it appeared to acquire additional sensibility to the galvanic influence. Fowler's work concludes with a letter from Professor Robison, who made some new observations upon the production of the galvanic flash, and first noticed the effect produced by applying the tongue to a number of pieces of silver and zinc alternately piled upon each other.

In 1794 the first volume of the *Zoonomia* was published; Darwin speaks of the phenomena of galvanism, and considers them as electrical. The muscular contractions he supposes depend

depend upon the sensibility of our nerves to small quantities of the electric fluid. Bennet discovered by means of his electrometer, that zinc, when separate from other metals, always is in the minus state, and silver in the plus state; when therefore they are brought nearly together, a small plate of air is charged like a Leyden phial; when the metals are brought into contact this is discharged.

Bennet's early discovery of the plus state of zinc (1789.)

In the Phil. Transf. for the year 1795, is a Paper by Dr. Wells on the subject of galvanism. He proposes for consideration the three following enquiries; Do the contractions observed by Galvani depend upon any property peculiar to the living body? What are the conditions necessary for the excitement of the influence? Is it electrical? With respect to the first question, he supposes that animals act only on account of their moisture. With respect to the second, he found that one metal and charcoal excited the contractions as readily as two metals; in this he corrects the mistake of Fowler noticed above. He however found that all charcoal will not act as a conductor of the influence, in which opinion Volta agrees with him. Wells does not agree in the hypothesis adopted by Volta and others, that the contact of the metals produces an alteration in the disposition of the electric fluid, for he very properly asks, why should not the natural moisture of the animal afford a communication between the two metals before they are connected by any other conductor? He farther discovered, that contractions could be excited by one metal only when it had been rubbed upon another metal, or even upon the hand. Charcoal may by the same means be made to produce the same effect; he proved by a variety of experiments that the friction does not in these cases communicate electricity to the metal or the charcoal; we may conjecture that an incipient oxidation, or some other chemical change was produced upon the surface of these substances. With respect to the third question, Wells is decidedly of opinion, that the phenomena are electrical; the influence being conducted by all conductors of electricity, and by them alone.

Dr. Wells, 1795, stated from experiments that animals are affected only by means of humidity; that the state of the metals is not altered by contact; but that the phenomena are electrical.

Besides the works here mentioned a number of communications appeared in the different scientific journals of Italy, France, and Germany, and several distinct treatises were published on the subject of animal electricity. Humbolt particularly

Various other papers, Humboldt, Dr. Monro, the Institute of France, &c.

ticularly distinguished himself by his assiduity in varying the experiments. Dr. Monro wrote upon the subject, and the National Institute of France published an elaborate report, drawn up by a Committee composed of several of its most learned members. As however these works do not appear to contain any facts which materially illustrate the nature of the galvanic influence, or lead us to form any more accurate notions respecting its operations, we shall in this brief sketch only notice the papers of Fabroni, of which an account may be found in the 4th Vol. of Nicholson's Journal. He deduces from his experiments that galvanism is intirely a chemical phenomenon; he finds that metals become oxidated when in contact with each other in circumstances where this would not take place if they were kept separate. This idea he confirms by many observations and experiments, and supposes that when the galvanic influence is excited by the action of two metals that a chemical affinity is exerted; he does not however point out very clearly in what manner the chemical action which is exerted by the metals can be connected with the phenomena of galvanism.

Fabroni: that galvanism is merely a chemical phenomenon.

History of this power in Cavallo's Electricity, and the Supplement to the Encyclopædia Britannica.

Grand discovery of the pile by Volta.

Description.

Galvanic shock.

In the year 1789, Cavallo published a new edition of his Electricity, and he added a good account of the principal facts in galvanism; an ample history of this science may be also found in the Supplement to the Encyclopædia Britannica.

In this state our knowledge of galvanism continued until the beginning of the year 1800, when Volta made his discovery of the apparatus used called the Galvanic Pile. In the Phil. Transf. for that year is a letter from Volta containing an account of the pile, and a detail of many curious experiments which he had performed with it. This instrument consists of a number of circular pieces of two different metals, laid alternately upon each other, with a piece of moistened pasteboard or skin interposed between each pair. The metals which answer the best for this purpose are zinc and silver, which were found to be the most powerful in exciting the muscular contractions in the former experiments. If the two pieces of metal which form the extremities of the pile be grasped firmly in the hands previously moistened, a shock will be felt through the hands and arms, more or less powerful in proportion to the size of the pile. This shock may be repeated as long as the

the pasteboard between the metals retains its moisture. Volta conceives that this apparatus in every respect resembles the electric organ of the torpedo and gymnotus electricus.

As soon as this discovery became known in England, a variety of experiments were performed with the new apparatus, and many very interesting and important facts were discovered: these are for the most part detailed in Nicholson's Journal, Vol. IV. & seq.

In the 4th vol. p. 174, is a paper written by Mr. Nicholson himself. He begins with a description of Volta's pile, and then relates the results of some experiments which were performed upon a similar one by himself and Mr. Carlisle. By using the revolving doubler they found that the electricity was minus in the silver end of the pile, and plus in the zinc end. Mr. Nicholson proposed that the influence should be permitted to pass from one end of the pile to the other through a tube of water; for this purpose a division was made in the conducting wire, which was composed of copper, and the two ends of it were terminated in a small tube of water. Immediately the wire connected with the silver end of the apparatus began to produce a gas, which was found to be hydrogen, while at the same time the wire connected with the zinc end became oxidated. Recourse was then had to a wire of platina which is not oxidable, and immediately gas began to be evolved from both ends. Upon examining the gases separately, that from the silver end was hydrogen, that from the zinc end oxygen, and they were generated nearly in the proportion requisite to produce water. This experiment, as well with respect to its immediate effects, as with regard to the consequences which may be deduced from it, may, I think, be justly considered as the most important that has occurred since the discovery of oxygen by Dr. Priestley. The electric spark was distinctly visible in these experiments.

Carlisle and Nicholson determine the electricity of the pile, and discover its chemical action, and the decomposition of water,

into hydrogen and oxygen.

Importance of this experiment.

Electric spark visible.

Mr. Cruickshank relates his experiments, in which he made use of the interrupted circuit after the manner of Mr. Nicholson; he caused the influence to pass first through distilled water tinged with litmus, and afterwards through water tinged with Brazil wood. In the first case there was a redness produced by the zinc, and in the second by the silver wire. By submitting a portion of water for a long time to the action of the pile, a sensible diminution was observed in its bulk. He observed the

Cruickshank. Effect on chemical tests.

the production of hydrogen and the oxidation of the zinc end when he used a communication of copper; and when he employed a wire which was not oxidable, he obtained both hydrogen and oxygen according to the observation of Mr. Nicholson. He permitted the influence to pass through a solution of a metallic salt, and found after some time, that the metal began to be revived at the silver end of the apparatus.

Reduction of metals from their solution.

Mr. W. Henry decomposes sulphuric and nitric acids, but not the muriatic.

Mr. William Henry, of Manchester, submitted concentrated sulphuric acid to the influence of galvanism: he made use of a wire of platina. Not only the water which always enters into the composition of this acid, but also part of the acid itself, appeared to be decomposed, as the oxygen which he procured was in a larger proportion to the hydrogen obtained than is sufficient to compose water. Nitric acid was also decomposed, and the water of muriatic acid. When oximuriatic acid was subjected to its influence, the water was decomposed, and the acid was deoxygenated. Gasses do not appear to be conductors of this influence, so that Mr. Henry was not able to try its effects in decomposing muriatic acid gas. It also appears to have the power of decomposing ammonia.

Oximuriatic was deoxygenated.

Gasses could not be subjected to it.

Ammonia was decomposed.

(To be continued.)

* * * On account of the great Quantity of original Communications, the *Scientific News and Account of Books* is necessarily deferred.

X. Y. Z. is informed that the derivation of chrome or chromium is from $\chi\rho\omega\mu\alpha$, color; and was adopted by Vauquelin, because its compounds with oxygen are coloured. See *Philos. Journal*, quarto, II. 444.



M.^r Boswell's improvements in the hydraulic Engines,

Fig. 3. Pressure Engine.

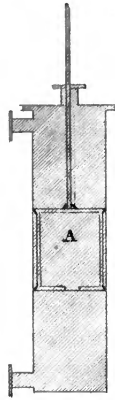


Fig. 1. at Schemnitz.

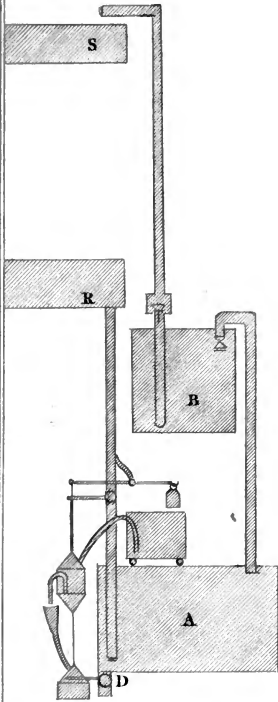
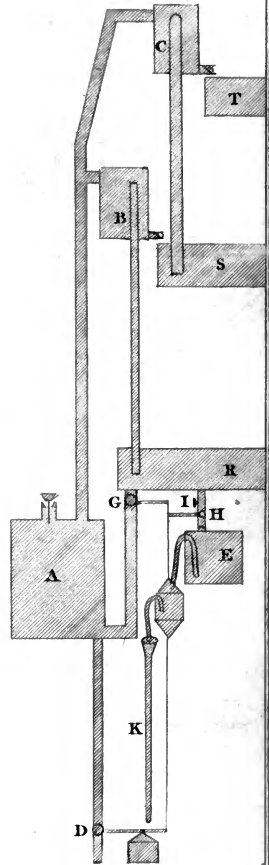
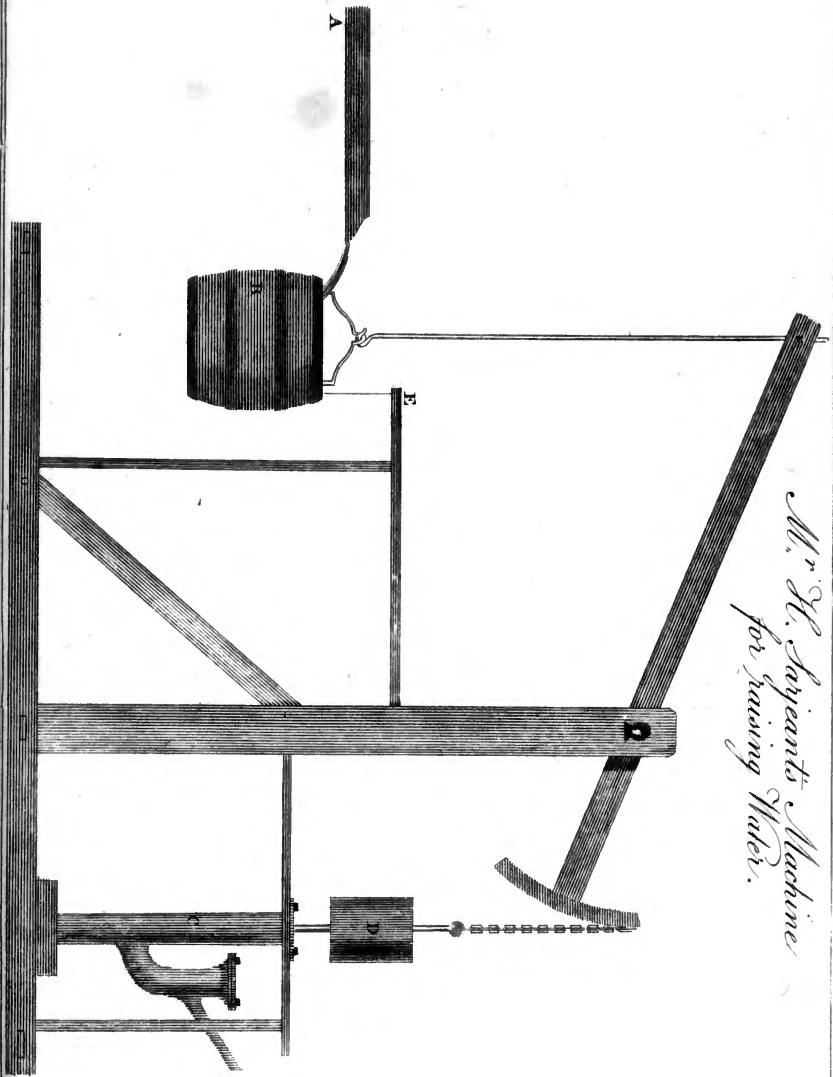


Fig. 2. M.^r Goodwyns.





*Mr. H. Jayeant's Machine
for raising Water.*





Stove of Cit. Guyton.

Fig. 1.

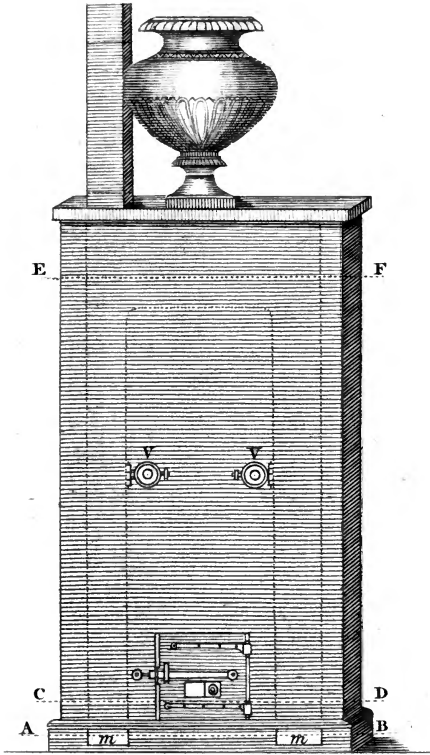
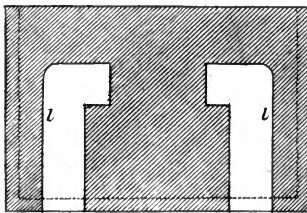


Fig. 2.



Section of the Stove of Guyton.

Fig. 7.

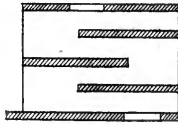


Fig. 4.

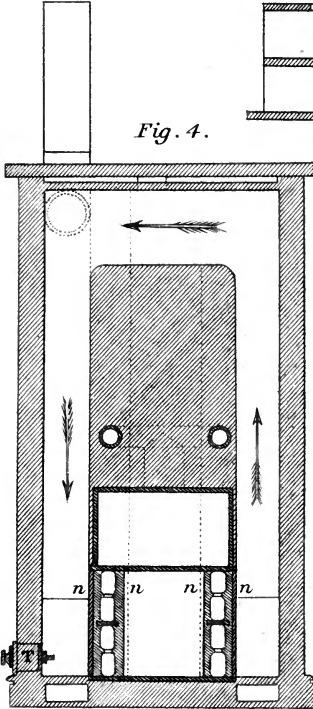


Fig. 6.

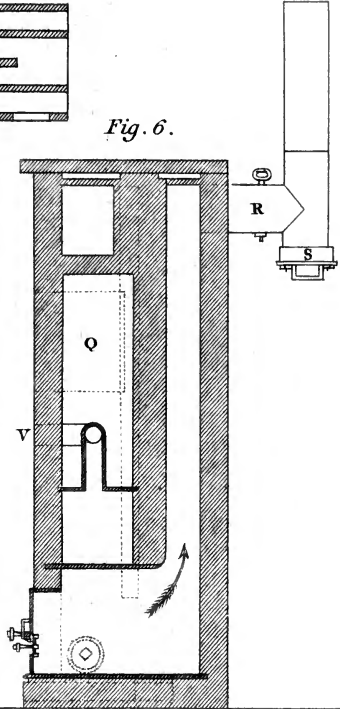


Fig. 3.

H

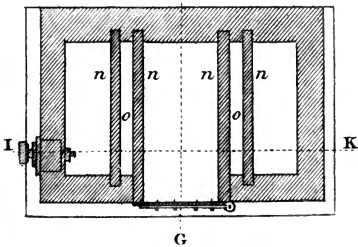
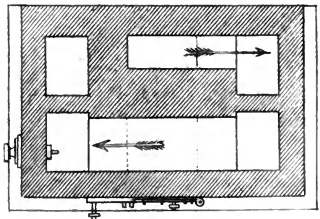


Fig. 5.







*Strong framed Levers for steam Engines by
M^r. J. G. Hornblower & others.*

Iron Cradle .

Fig. 1 .

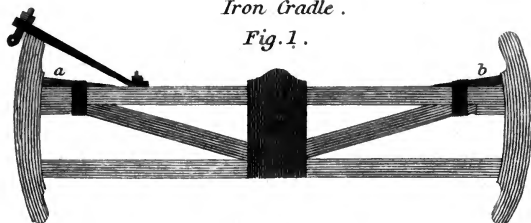


Fig. 2 .

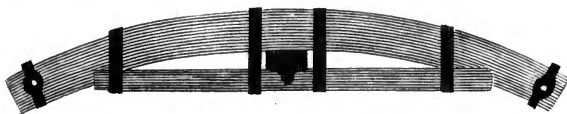


Fig. 3 .

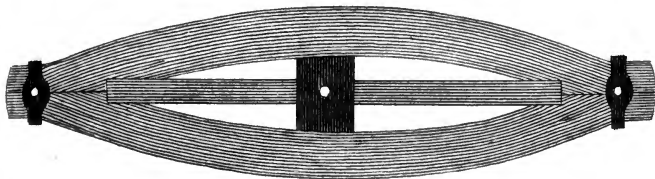
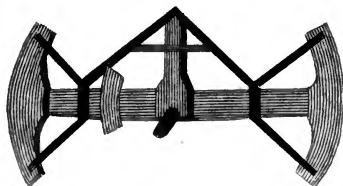


Fig. 4 .



D.^r Young on the theory of Light & colours.

Fig. 2.

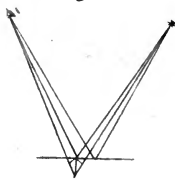


Fig. 3.

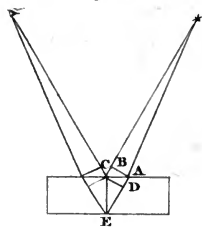


Fig. 1.

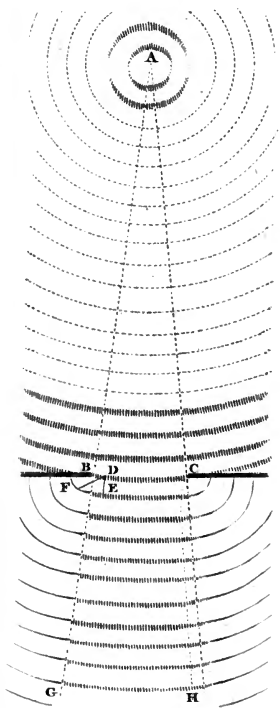
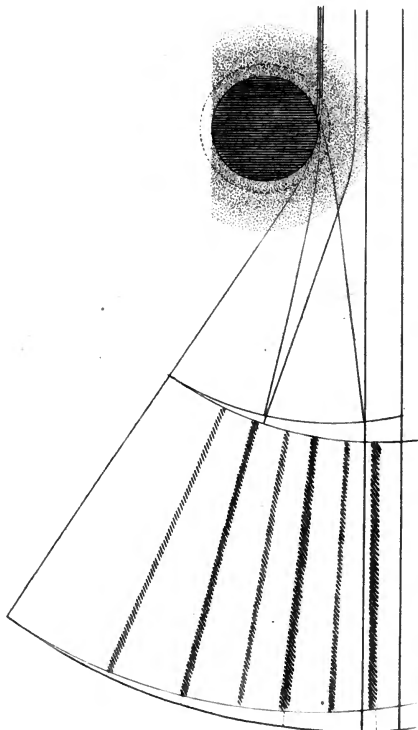


Fig. 4.

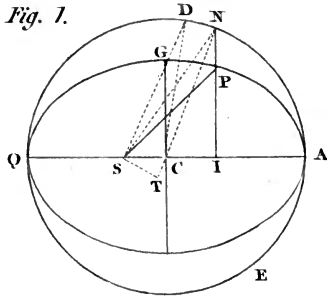






*Mr Gregory's Investigation of
Mr Pearson's Analogy, &c.*

Fig. 1.



*Mr Gough on the
Phenomena of Sound.*

Fig. 2.

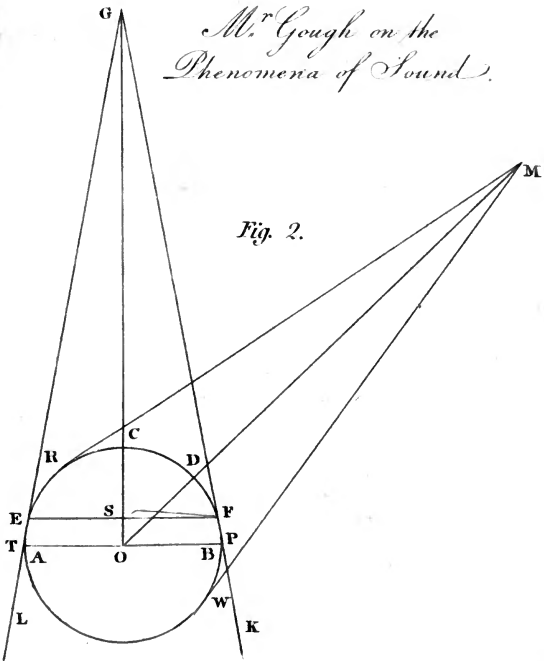


Fig. 5.

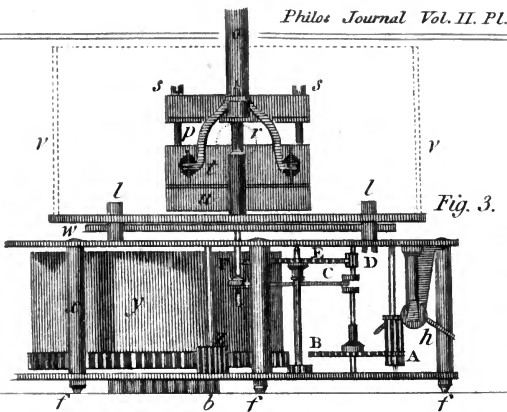


Fig. 3.

Fig. 2.

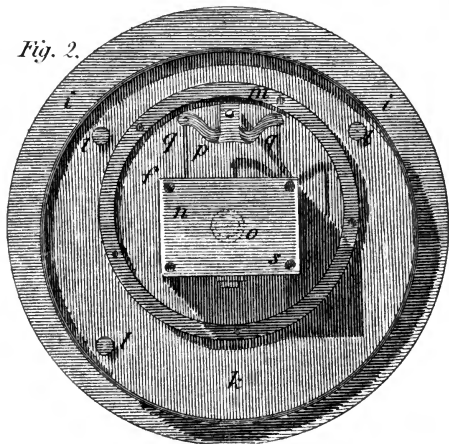


Fig. 4.

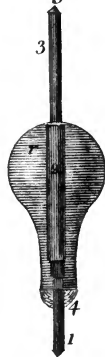
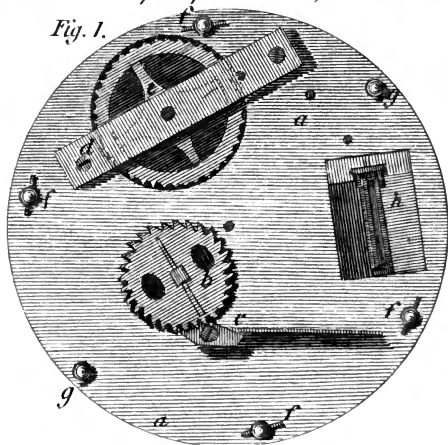


Fig. 6.



Mechanical Lamp of Carcel & Carreau.

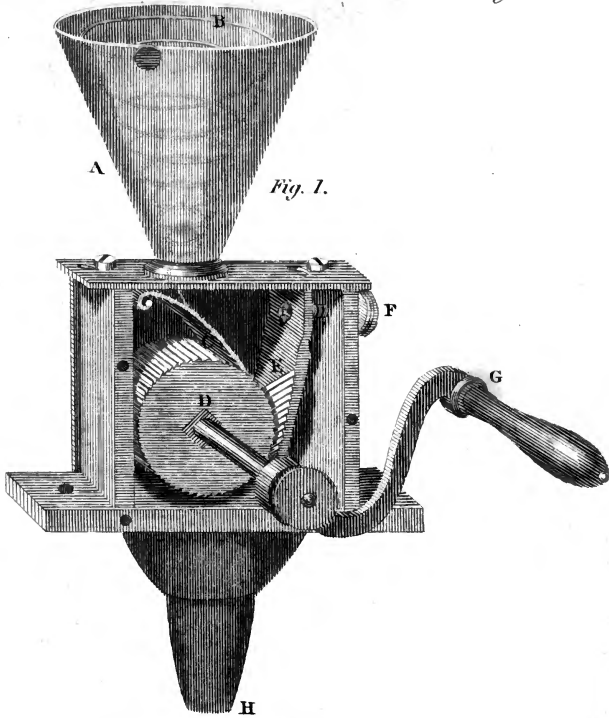
Fig. 1.



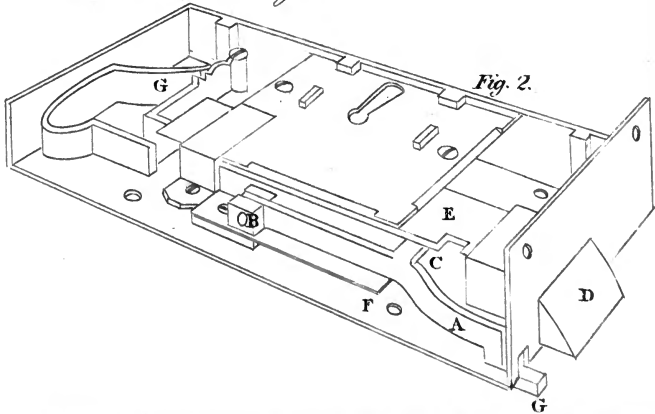




Mill by Mr. Garnet Terry.



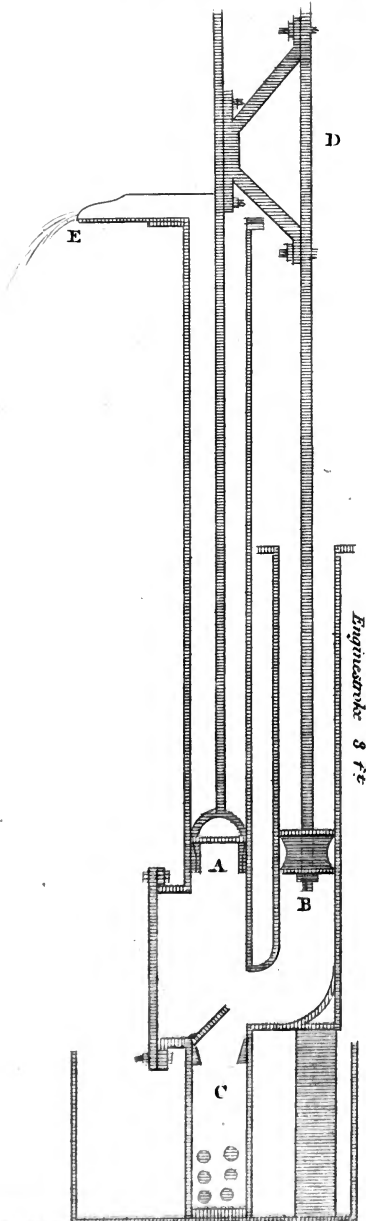
Drawback Lock by Mr. Bullock.



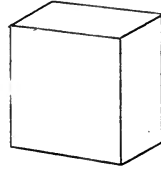




*Method of applying a temporary
forcer, by M.^r Trevithick.*



*Crystals
Fig. 1.*



*described by
C.^{te} de Bournon.*

Fig. 2.

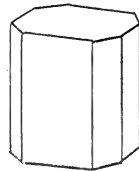
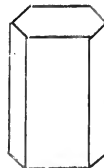


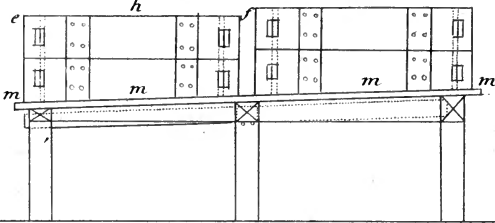
Fig. 3.



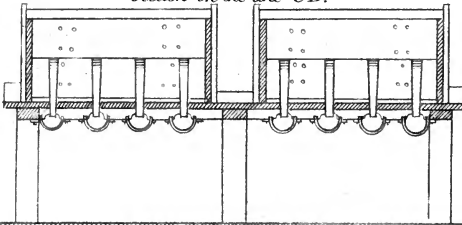
Enginestroke 8 ft

*Cit. Hapel Lachenaies apparatus
for claying Sugars.*

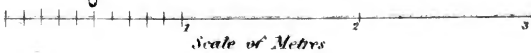
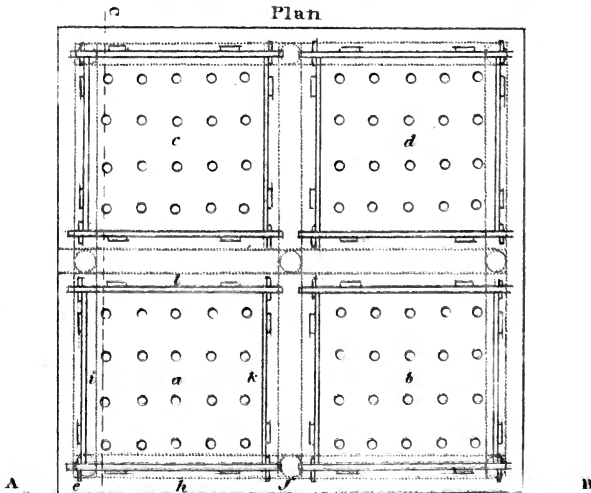
Elevation on the line AB.



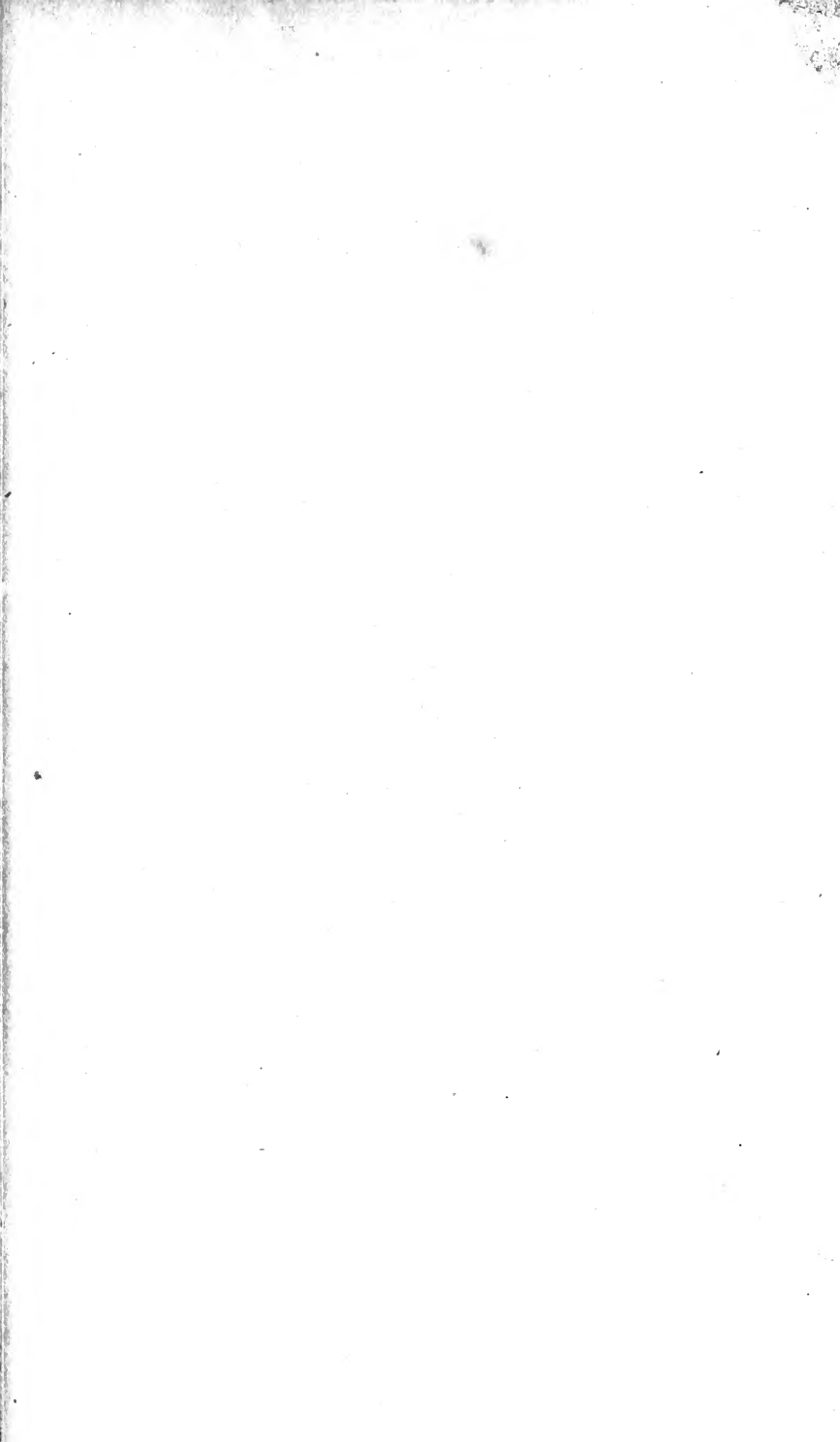
Section on the line CD.



Plan







*M^r Rob^t. Jameson's Observations
on the Formation of Granite.*

Fig. 1.

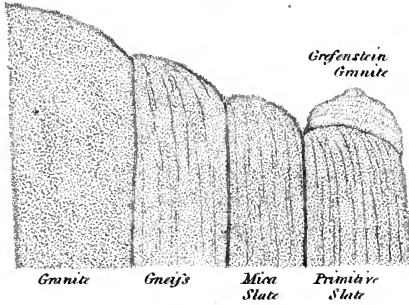
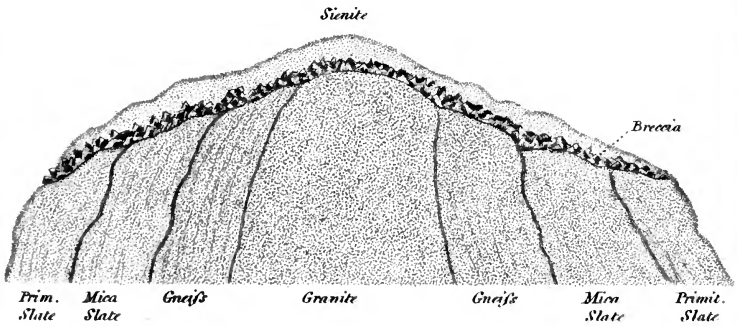
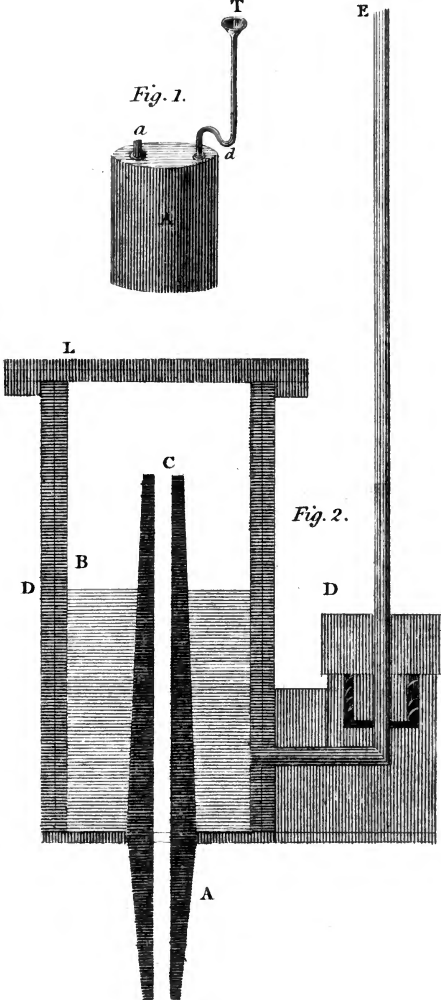
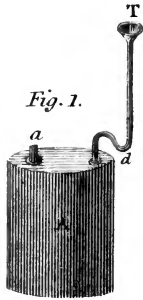


Fig. 2.

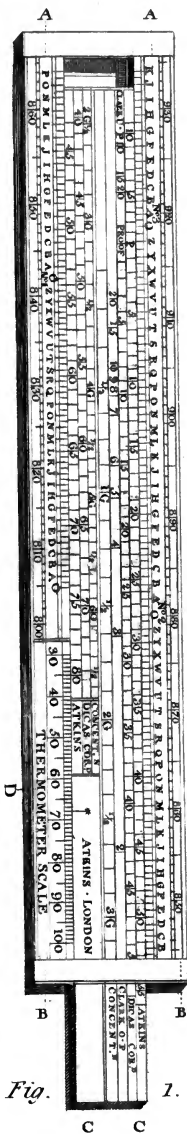


*Mr. Banks's Experiments on the velocity
of effluent Air or Gas*

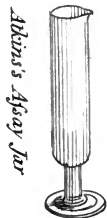
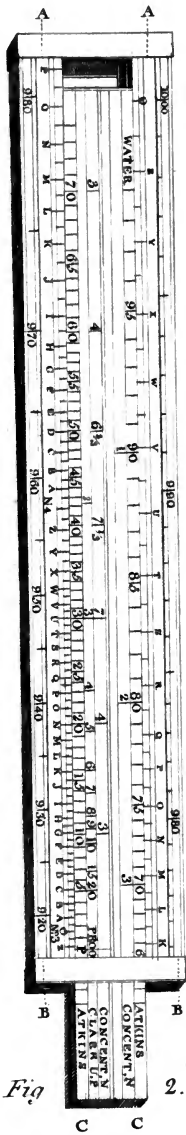
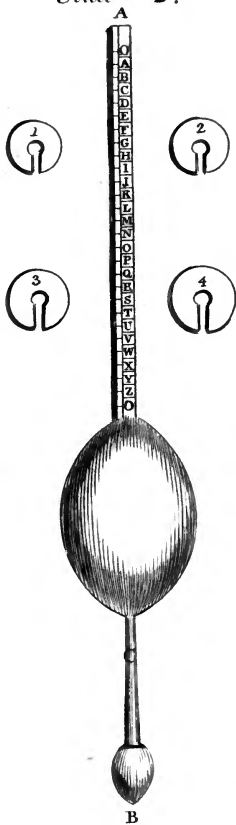




Atkins's Spirit Hydrometer.

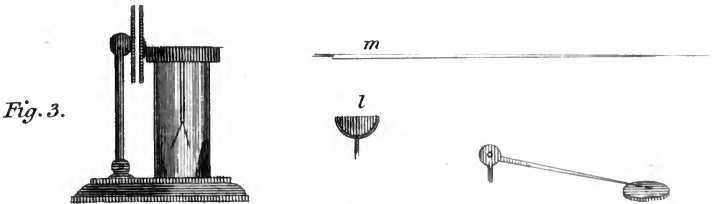
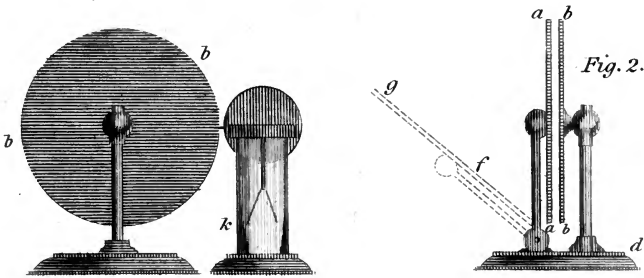
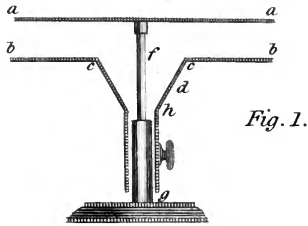


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Compound Condensers of Electricity:
By W. Reed and W. Cuthbertson.





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