A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS:

ILLLUSTRATED WITH ENGRAVINGS.

BY WILLIAM NICHOLSON.

VOL. II.

LONDON:

PRINTED FOR THE AUTHOR, AND SOLD BY G. G. AND J. ROBINSON, PATERNOSTER-ROW.

M.DCC.XCIX.

TABLE OF CONTENTS

TO THIS SECOND VOLUME.

APRIL 1798.

MAY 1798.

JUNE 1798.

V. A

JULY 1798.

AUGUST 1798.

SEPTEMBER 1798.

III. Ob-

OCTOBER 1798.

NOVEMBER. 1798.

Historical

VIII. Ob-

DECEMBER 1798.

V. An

JANUARY

JANUARY 1799.

Tunnel

FEBRUARY 1799.

VII. On

MARCH 1799.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

APRIL 1798.

ARTICLE I.

A Memoir and new Experiments on the Artificial Cinnabar of Mr. Kirchoff. By the Count APOLLOS DE MOUSSIN POUSCHIN, *Chamberlain to his Majesty the Emperor of all the Russias, Vice-President of the College of Mines, Honorary Member of the Imperial Academy of Sciences, at Petersburg*, &c. &c.*

THE annual consumption of cinnabar by a variety of manufacturers and artists, and the certain sale of this beautiful colour, render it undoubtedly an interesting object of political economy for all states which possess the ores of quicksilver, as well as a branch of speculation for the merchant which has hitherto supported the manufactories of vermilion, and has even fixed the attention of the industrious Hollanders, who not being able to derive advantage from the fabrication of this product at first hand, have established, as it is said, mills for cinnabar, in which this colour gains the brilliancy and smoothness necessary for its employ in the arts. This process, which they mysteriously conceal, has hitherto been attended with profits the more considerable, as it is pretended that great part of the cinnabar which is adulterated by different processes is vended through Europe from these manufactories at first hand†. The methods by which cinnabar has hitherto been

* Translated from the French manuscript, communicated by Charles Hatchett, Esq. who received it from the author, with some specimens of the cinnabar, which he finds to be of the best quality. The memoir was addressed to the Societé des Amis Scrutateurs de la Nature.

† Mr Tuckert, who several times assisted, or was present during the preparation of artificial cinnabar at the manufactory of Mr. Brand, without the Utrecht gate at Amsterdam, has given an account of the processes in

been obtained are too well known, and it would be superfluous to mention them here. But as the various inconveniences of sublimation, grinding, and the other preparations of this colour, are frequently prejudicial to the health, and sometimes expose the lives of the workmen to inevitable danger; and as the loss of vessels and the expence of fuel are but too frequently felt by those who undertake this fabrication; it is evident that a method of obtaining cinnabar subject to none of these inconveniences, and requiring little of expence excepting the first cost of the mercury, must become truly valuable to this branch of industry. We are indebted to Mr. Kirchoff, a young chemist of the greatest hopes, who is attached to the pharmaceutic department, for the first discovery of a method no less ingenious than obviously of great probable advantage to commerce. His claim in this respect is beyond controversy. For, though cinnabar has been obtained in the humid way by means of volatile liver of sulphur, there never could have been any hope of applying that method to the purposes of trade, on account of the heavy expence of the re-agent. But the intermedium used by Mr. Kirchoff promises, on the contrary, all the advantages which the most scrupulous calculation can require, and, when brought to perfection, will probably cause the method of sublimation to be abandoned by all who may be desirous of engaging in this branch of commerce, without exposing themselves to unnecessary risque.

I shall proceed to lay before this illustrious Society all the details of the process of Mr. Kirchoff, as well as the inconveniences which have hitherto presented themselves. It is with great pleasure that I avail myself of this opportunity of doing justice to the merits of this chemist, which have been hitherto concealed by his excess of modesty; and I have annexed to this memoir my own proper experiments, which have enabled me to remove some of the principal difficulties which Kirchoff met with in his work. I have no

in Crell's Journal, of which an abstract is inserted in the fourth volume of the Annales de Chimie, page 25. It is concisely as follows: (1.) Ethiops mineral is prepared by combining 150 pounds of sulphur with 1080 pounds of pure mercury, by a moderate heat, in a flat-bottomed polished iron vessel. 2. The ethiops, after slight pulverization, is put into earthen bottles, each capable of containing a quart of water. 3. Three large pots or sublimatory vessels, made of very pure clay and sand and luted, are then taken and placed upon iron circles over furnaces, constructed in such a manner that the flame of the fuel, which is turf, circulates freely round the vessels to two-thirds of their height. 4. When the vessels are red-hot, a bottle of the ethiops is poured into the first, another into the second, and another into the third. In the subsequent progress of the operation, two, three, and perhaps more bottles may be poured in at a time; but this depends on the strength of the inflammation exhibited by the ethiops after its introduction, the flame of which sometimes rises to the height of four and even six feet. When this is a little diminished, the mouth of the vessel is covered with a plate of iron, one foot square and an inch and a half thick, which perfectly closes it. In this manner during thirty-four hours the whole of the prepared matter is introduced into the three pots; that is to say, into each pot 360 pounds of mercury and 50 of sulphur. 5. The fire is then kept up till the sublimation is completed, and afterwards suffered to go out; which requires 36 hours from the time of the complete charge. It is judged to have the proper intensity, when, upon taking off the cover, a brisk flame appears, but does not rise more than three or four inches out of the pot. 6. During the last 36 hours, the mass is stirred every quarter or half hour with an iron rod. 7. When all is cold, the vessels are removed by means of the iron circles, which prevent their breaking. The cinnabar is taken out by breaking the vessel. Each pot constantly affords 400 pounds of cinnabar, the loss of original weight in each being 10 pounds. The cinnabar does not attach itself to the plates of iron, because they are continually taken off excepting towards the end, when the vessels are left untouched. These plates are not in the least corroded. N.

other

other claim than that of having endeavoured to render his discovery as useful as possible to technical chemists.

Mr. Kirchoff, who has operated in the small way only, gives the following proportions for the production of cinnabar in the humid way:

Three hundred grains of mercury are triturated in a vessel or cup of porcelain, with 68 grains of flowers of sulphur, until an ethiops is accurately produced, in which no metallic globules can be perceived by the magnifier. As this degree of union of the mercury and sulphur cannot be obtained when both are perfectly dry, Mr. Kirchoff adds a few drops of a solution of caustic vegetable alkali; and when the union of the mercury and sulphur is complete, he adds to the mixture a solution of 160 grains of the same alkali dissolved in an equal quantity of water. He heats the vessel containing these ingredients over the flame of a candle, and triturates without interruption during the heating with a glass pestle. In proportion as the liquid evaporates, he adds clear water from time to time, so that the ethiops may be constantly covered to the depth of near an inch. For the above quantities, it is requisite to continue the trituration nearly two hours; at the end of which time the mixture begins to change from its original black colour to a brown, which usually happens when a large part of the fluid is evaporated. From this first moment the transition from brown to red is very rapid. No more water is then to be added, but the trituration must be continued without intermission. When the mass has acquired the consistence of a jelly, the red colour becomes more and more bright with an incredible degree of quickness. This is the most important point of time for the success of the operation. For, if the heat be not continued long enough, the colour will not acquire the utmost degree of beauty; and on the contrary, if it be continued only two seconds too long, the fine red of the cinnabar passes as quick as lightning to a dirty brown. Mr. Kirchoff, as well as Mr. Lowitz, who has repeated his experiments, could not succeed in amending this brown colour; which consequently renders the success of the operation very precarious. This inconvenience is productive of another; namely, that it is nearly impossible, at the instant of the production of the colour of the cinnabar, to make any observation of the quality of the tint. For the colour of the liver of sulphur masks that of the cinnabar; so that in some instances the most beautiful red is obtained, and in others a colour inclining to orange, which, though bright and vivid, is not the tinge required in this product. In one of his experiments, Mr. Kirchoff obtained a cinnabar which absolutely possessed the colour of carmine; but he had this good fortune only once, when he had no expectation of so beautiful a product, and did not therefore note the attendant circumstances.

This is nearly the whole detail of the experiments hitherto made by Messrs. Kirchoff and Lowitz. The cinnabar they obtained still exhibited mercury in the metallic state, which it was rather difficult to get rid of, and did not appear until after the filtration of the liver of sulphur, and the edulcoration and drying of the vermilion.

I shall now proceed to relate what I have done respecting this object, principally with a view to facilitate the manufacturing in the large way, the restoration of the cinnabar which had assumed a brown colour instead of red, and the certainty of the tint in masses somewhat considerable. I am very far from being content with the success of my experiments; but I think I have at least discovered great part of the theory of the operation, and that in practice the manufacturer will foon difcover the neceffary proportions and manipulations. I therefore claim and hope for the grant of indulgence in favour of the defire I have, that the valuable difcovery of Kirchoff may fpeedily become as ufeful to fociety as undoubtedly it will fooner or later be.

The firft experiment I made in fmall, according to the exact proportions of Kirchoff, did not fucceed. I could not transform my ethiops into cinnabar. I attribute this want of fuccefs to an evaporation too rapid, and the want of a fufficient quantity of fluid; not having added water in proportion as the mixture became thick, fo that the whole foon became perfectly dry. The mafs became ftrongly attached to the china cup, and conftantly preferved its black colour. I recommenced the operation with more fuccefs, and all my experiments afforded cinnabar more or lefs beautiful, provided the mixture at the commencement was kept at the proper degree of fluidity. Moft frequently however I obtained a colour, which, though very brilliant, inclined rather to yellow than red; a fault which the cinnabar of fublimation poffeffes but too frequently, and is fometimes a proof of its falfification. Among the colours which I obtained, brown prefented itfelf but too frequently, in confequence of having neglected the moment of terminating the operation. I made the following experiments on thefe defective colours:

After well edulcorating a cinnabar of this kind, I poured on three different portions: the nitrous, the acetous, and the muriatic acids. Thefe were expofed for feveral hours to a mild digefting heat. I had fuppofed that the defect of the colour might arife either from a partial reduction of the mercury, or partial fufion of the fulphur, or perhaps from an adulteration of the mercury by lead. In thefe three experiments, therefore, my aim was to diffolve the reduced mercury, to oxide the fulphur, or to carry off the lead by the acetic acid. None of thefe experiments anfwered my expectation; for, though part of the mercury was diffolved and part of the fulphur oxided, the colour continued brown. That with the nitrous acid was even darker. The acetic acid afforded no indications of lead.

As the refult of thefe experiments fhowed that the bad colour of the cinnabar did not arife from a portion of reduced mercury, I wifhed to know whether the metal were not combined with too large a portion of fulphur; for which purpofe, I digefted cauftic alkali on the cinnabar. The alkaline folution became yellowifh, and the colour of the cinnabar fomewhat lighter, but was not a good red. It always inclined to yellow; and though it approached very near the colour of common cinnabar, and was even more vivid, yet it was not red enough.

I had little expectation of reftoring the proper colour and vividnefs to my damaged cinnabar. Nothing more remained but to try the addition of mercury to the brown colour which was the object of my refearch. I therefore moiftened near an ounce and a half of this colour with water, then gradually added near three ounces of running mercury, and triturated the whole on a very hard Siberian porphyry. Contrary to my expectation, the mafs, inftead of becoming black, affumed the colour of an extremely pale cinnabar, without the fmalleft degree of fire or livelinefs. I then mixed two drams of the flowers of fulphur, and collected the whole in a porcelaine cup. Upon this I poured a folution of the cauftic alkali, evaporated as before to a very thick confiftence. The mixture paffed from yellow to an obfcure brown, and at the moment of the thickening a large quantity of running mercury was feen at the bottom of the cup. I added a dram and a half of fulphur,

fulphur, with a few drops of water, and triturated the whole until the mercury had entirely combined with the reft of the mixture. I then added water, and began the evaporation, agitating the mixture continually with the peftle. The brown colour began towards the end of the evaporation to brighten, and at laft changed to the colour of bad fealing-wax, conftantly inclining to brown. Forefeeing that if I continued the evaporation I fhould rifk a fecond obfcuration of the colour, I placed the cup which contained the mixture on a ftove which had been heated. At that time I totally defpaired of correcting the colour of cinnabar fpoiled at the firft evaporation; but having infpected my cinnabar at the end of two hours, I faw with pleafure that the colour had become finer as the mafs became thicker. I triturated it in the cup with the addition of a few drops of water, and repeated this operation five or fix times in the courfe of the day. At the end of 24 hours, my cinnabar was very thick and very good. I added more water, and left the colour on the ftove for three days; at the end of which the cinnabar had acquired the utmoft brilliancy, and was equal to the fineft fpecimens of Mr. Kirchoff, excepting that in which he had obtained a fhade of carmine, as I have already remarked. After repeating this experiment with fuccefs with one pound of mercury, thirty-five drams of flowers of fulphur, and ten ounces of cauftic alkali, I found reafon to make the following general obfervations upon this procefs:

It is neceffary for the production of cinnabar,

1. That the mercury fhould be perfectly mixed with the flowers of fulphur; for which reafon, after having triturated it with the peftle in a glafs mortar, with the addition of a fmall quantity of cauftic alkali to moiften it, it muft be levigated upon porphyry, or fuch other hard ftone as cannot mix its particles with thofe of the cinnabar, becaufe every foreign fubftance is highly prejudicial to the beauty of the colour. If the quantities operated upon exceed one pound, it will be neceffary to procure a mill for grinding colours, which muft alfo be of a filiceous ftone. The mixture upon a levigating ftone is evidently too expenfive for a manufacturing procefs in the large way.

2. The beft veffels for evaporating the mixture are thofe of porcelaine, or the yellow ware of England. The peftles ought to be glafs.

3. The heat of a fand-bath is fufficient to this operation. The heat of ebullition is not neceffary; but at the beginning, particularly when there is much liquid, the heat muft approach as nearly as poffible to that of boiling.

4. The receptacle in which the fand is placed muft be deep enough to admit the whole of the porcelaine or earthen veffel in which the cinnabar is produced, to be plunged therein, and environed by an equal atmofphere of heat. The fand round the veffel need not exceed the thicknefs of a full inch.

5. The mixture muft be perpetually ftirred: otherwife the mafs will fubfide by its weight, and become clotted; a circumftance which it is effential to avoid.

6. A certain degree of thicknefs is requifite during the operation, in order to afford a good colour; but great care muft be taken not to fuffer the mafs to become too thick, and more particularly the agitation muft be ftrongly kept up at thefe periods of condenfation.

7. Though it is poffible to obtain from the fand-bath a very beautiful cinnabar which may be immediately edulcorated, yet it is much more prudent and advantageous not to

fuffer the product to acquire its whole perfection on the evaporating furnace, but to take it in time from the fand-bath, and leave it for three or four days in a degree of heat equal to that of the ftove of an apartment after it has been heated for two or three hours. Care muft be taken to add a few drops of water, and to agitate the mixture from time to time. By this precaution, the difagreeable event of feeing the colour pafs from red to yellow, and often to brown, is avoided, as well as the fubfequent labour and expence of trituration with mercury, and additional liver of fulphur.

8. Before the cinnabar is edulcorated, it may be left to become almoft perfectly dry by the heat of the ftove; by which means the colour will acquire the moft brilliant tint.

After this account of the production of cinnabar in the humid way, I fhall fpeak of the edulcoration and drying of this colour, which have prefented pheunomena of the greateft importance to the fabrication in the large way, and are alone fufficient to fecure real advantages to the fpeculator in this article.

Having obtained ten ounces of cinnabar, I poured twenty pounds of boiling water upon it in a large earthen jar, for the purpofe of difengaging the liver of fulphur. At that moment the liquid was obfcured and became abfolutely black. I flightly agitated the mixture. The cinnabar very fpeedily fell to the bottom, and permitted me to decant this black liquor, of which I fhall hereafter fpeak. After having poured other water upon the cinnabar, which was confiderably beautiful, it acquired a flight yellow colour. After the fettling of the colour, I decanted it, and continued to edulcorate until the decanted water had neither the faline fulphureous tafte, nor hepatic fmell.

When I had poured for the laft time five or fix pounds of boiling water on my colour, I agitated it ftrongly, and poured it out into a veffel of cryftal glafs. A plentiful depofition of colour was made in an inftant. It was not bad, but did not poffefs the brilliancy of the particles which floated in the water above this depofition. I agitated the water very flightly, that it might become more highly charged with colouring matter, but not fo much as entirely to derange the mafs which had fubfided. I rapidly decanted the liquid part into another veffel. The moft denfe portion remained at the bottom, and on the fides of the firft glafs, and was of a very beautiful colour. I repeated this wafhing four times with the fame precautions, taking care always to pour the liquid part haftily into another veffel. The refidue of the laft wafhing was entirely brown, and the tints of my cinnabar were beautiful in the fame order as I had obferved in the decantation. That is to fay, the colour in the firft veffel was more brilliant and lefs deep than in the fecond, and fo of the others. This experiment fhowed not only that wafhing adds to the beauty of the cinnabar obtained by the procefs of Kirchoff, but that it is poffible, more efpecially in works on a large fcale, to obtain a great variety of tints by mixture, without admitting any foreign colour or material.

After having thus obtained different cinnabars by wafhing, I dried them on a furnace flightly heated, and found them more or lefs deep, but generally very good. They did not, however, poffefs the fire of fuch cinnabars as inclined to a yellow colour. I put thefe colours into capfules of paper, and left them for fome days on the fame ftove, which had continued to be regularly heated. At the end of this time, upon opening my papers it appeared to me that the colour was improved; and had in particular become more lively

 than

than on the firft day. In order to afcertain whether this phenomenon was owing to fimple drying, or to heat, I placed a pinch of the lighteft colour on a fmall piece of glafs, which I heated very ftrongly with a candle. I then enjoyed a very beautiful fight, of which the refult gave me the greateft pleafure. The powder of cinnabar became brown in an inftant, and foon afterwards nearly black, or rather of the deepeft violet grey; but when, on the fuppofition that I had fpoiled my cinnabar, I threw it from the glafs upon a paper, I was very agreeably furprifed to obferve it in the twinkling of an eye pafs through all the tranfitions from obfcure violet to a beautiful carmine red, and to obferve more particularly that this colour had acquired the fire and brilliancy which was wanting before the experiment. As an object of comparifon, I had fome cinnabar of commerce, of which the pond, or forty pounds (Ruffian), is fold at Peterfburgh for eighty roubles. I put a pinch of this on the fame flip of glafs, and fubjected it to the fame treatment. The colour fpeedily became black, and, after cooling, acquired a very bad deep brown tinge, refembling umber of a bad quality, without recovering its original red. I repeated this experiment in a larger way with the fame fuccefs, and by that means afcertained a method of giving in general much brilliancy to my cinnabar, and of producing the moft brilliant carmine tint. This heat, which muft be very ftrong, requires, neverthelefs, to be conducted with much prudence. The cinnabar muft not fume, nor exhibit grey globules on its furface; for in thefe cafes it would be effectually fpoiled. This phenomenon feems to prove a decompofition and abforption of air; but it remains to be fhown whether it be hydrogene, oxygene, or azote, which the cinnabar takes up in this experiment.

Thefe conftitute the moft effential facts I have obferved with regard to the production of cinnabar in the humid way. I intend, when time permits, to give a continuation of this memoir. I have already made fome new experiments, which, though they may not yet promife any advantage to the manufacturer in the large way, may be ufeful to the painter and interefting to the chemift.

The black liquor which I at firft decanted from my cinnabar, depofited a fmall portion of ethiops, not furcompofed. I muft add in conclufion, that the manufacturer in the large way muft of courfe fubmit his colour to feveral elutriations, collect the depofitions in feveral veffels, and afterwards mix his colours when dried. It would be likewife neceffary for the finer forts, to provide a furnace, in which either the carmine colour or the requifite fire and brilliancy may be given. A furnace of this kind may very eafily be contrived, and it would be unneceffary to defcribe its conftruction in this place. It is my wifh, that the fociety to whom I have the honour to addrefs the prefent memoir will confider it as a proof of my refpect and gratitude. By admitting me into the number of its members, a new proof is afforded of its indulgence to the weakeft efforts in favour of the fciences and natural hiftory. The excellent difcovery of Mr. Kirchoff has led me to afcertain fome new facts, of which I am defirous that commerce fhould have the advantage; but the honour of the difcovery belongs to him, and deferves to be the more infifted on, as his modefty is no lefs confpicuous than his knowledge.

II. *On*

II.

On the Luminous Appearance said to be exhibited by Phosphorus in Azotic Gas.

By DR. ALEXANDER NICHOLAS SCHERER, *Counsellor of the Mines to his Serene Highness the Duke of Saxen Weimar**.

AT the time when the principles of the new theory were newly discussed, the enquiries of chemists were employed upon some of its chief assertions. Besides examining the experiments that metallic oxyds do emit oxygen gas during their reduction, attention was also paid to another, by which it was observed, that the combustion of bodies in oxygen gas is always accompanied by a total absorption.

Mr. Goettling, who exerted his industry on such experiments, made use of the apparatus he employed in several experiments of the same nature. He accidentally observed, that the phosphorus continues to shine for some time in the azotic gas, which remains after its combustion in atmospheric air. What he principally observes on this head is, that phosphorus shines very strongly in azotic gas, even more so than in atmospherical air; that it is converted into phosphoric acid by this process; and that the azotic gas totally disappears. Hence he was led to infer, that azotic gas must contain oxygen, as well as the oxygen gas itself does.—But if so, what is the difference between these two species of gas? This question was apparently solved by another experiment he made. Phosphorus does not shine so well in oxygen gas at a low temperature, as it does in the azotic; and when phosphorus gives light in atmospherical air, it is always with extrication of heat; whereas the light is not attended with heat in azotic gas. He concludes therefore, that the azotic gas consists of oxygen combined with light, in the same manner as, on the other hand, the oxygen gas consists of that principle combined with caloric; and moreover that caloric has a stronger affinity to oxygen than light has.—On this Mr. Goettling builds an entirely new theory of chemistry, from the consideration that the new mixture of azotic gas, which he pretends to have discovered, does not admit of the hitherto adopted way of explaining its influence.

Mr. Goettling has explained these facts at large in his *Beytrag zur Berichtigung der antiphlogistischen Chemie.* Weimar 1794. This work was universally well received, and some were inclined to give it full credit without further examination. Mr. Goettling himself has publicly explained his new theory, and adapted the whole of his chemical lectures to that system. A few philosophers only ventured to offer their doubts concerning it. Among these are Dr. Eimbke; *Intelligenzblat der allg. litt. Zeit.* 1794, No. 92, and Yelin; *Erlang. gel. Zeit.* 1794, No. 80. The former observed, that when phosphorus had ceased to shine in atmospheric air, it would afford no more light in the remaining azotic gas. The second found, that phosphorus, after undergoing a very long continued heat in atmospherical air, does also leave behind an aëriform residue, in which the phosphorus will by no means shine any longer.

At this time I had commenced a public course of lectures on Chemistry. I therefore thought myself obliged to make new enquiries for the purpose of giving an impartial ac-

* Received from the Author.

count

count of this new discovery; but to avoid being led into error by individual observations, as is frequently the case with experiments, I joined Dr. Jaeger from Stuttgard, who was just then visiting me on a journey. We made a great number of experiments together, which we endeavoured to diversify in every possible manner, in order to discover all the concurrent circumstances. In the *Jena gel. Zeit.* 1794, No. 113, we published a short account of one of the chief occurrences; but the full description of the results from our investigations some time after was given in the following book, *Uber das Leuchten des Phosphors im atmospherischen Stickgas.* Weimar 1795. To this work we have added another treatise on Goettling's theory by Dr. Pfaff.

The chief aim of this enquiry was to shew by decisive experiments, *whether phosphorus does really give light in azotic gas, and whether it be totally decomposed by it?* Neither of these two we could find by any process of operation. We made comparative experiments over water and over quicksilver, and clearly perceived that Mr. Goettling, having closed his vessels merely by water, could not avoid a source of error. We spared no exertions to investigate as exactly as possible the method of preparing azotic gas, and by that means discovered a variety of interesting circumstances. We always found that the fumes of *phosphoreous acid*, rising by a momentary combustion of phosphorus in atmospherical air, did so far envelope the remaining portion of oxygen, that the luminous appearance from the union of this with another part of the phosphorus could not be again produced until the water had absorbed the acid fumes. We always found however that this light was of short duration, accompanied by a very inconsiderable diminution of the elastic fluid, and that it was altogether impossible to cause the phosphorus to emit any more light in the remaining air. On this account we had every reason to consider this as pure azotic gas, and to declare those experiments of Mr. Goettling incorrect, from which the combustion of phosphorus was said to take place in the *purest azotic gas.*

To all this Mr. Goettling made no reply, except that he gave an answer in the *Intelligenzbl. der allgem. Literat. Zeit.* 1794, No. 117, to our first very short notice, tending to place the whole investigation in an unfavourable point of view. Of our treatise, though printed above two years ago, he has not yet taken any notice.

Soon afterwards Mr. Girtanner took some pains to explain the result of Mr. Goettling's experiments, *Ibid.* 1795, No. 23, by supposing the azotic gas employed had contained some water, which was decomposed by the phosphorus. He did not, however, take the trouble to ascertain by experiments, whether phosphorus does really give light in azotic gas thoroughly freed from oxygen. Mr. Goettling replied to this, by alledging, that if the water which exists dissolved in the gases were the cause of this light, the phosphorus ought also to emit light in oxygen gas, which according to his experiments did not take place. *Ibid.* No. 27. He afterwards published a more ample elucidation of the short answer here related in the *Almanach fuer Scheide Kuenstler,* 1795, asserting that the luminous appearance of phosphorus could not be obtained in an azotic gas, which had been procured by a long continued heating of the phosphorus in atmospheric air, because in that case the phosphorus mixes with the azotic gas, and prevents it from maintaining the light. He added, that the gas thus circumstanced is found to recover this property by adding a few drops of an acid. But he was by no means aware that his observation contained a very important objection against his own theory. For if, according to that theory, azotic gas be really a compound of oxygen and light, why should the phosphorus, on being mixed with it, ceafe to act on its oxygen? Ought it not rather to do this more speedily when the cohesion of its parts is destroyed? Besides which, the addition of acids is not fair, because more acid than is necessary to saturate the phosphorus dissolved in it, may in this way become united with the azotic gas. Thus I have found that the phosphorus, which does not shine in pure carbonic acid gas, begins to give light in that medium as soon as a small portion of acid is added. The answer given by Dr. Jaeger was more explicit. He discovered in particular, that the volume of the azotic gas was never diminished when the luminous appearance of the phosphorus was reproduced by means of acids. *Gren's Neues Journal der Physik.* B. II.

It was not long after, that Professor Lampadius published his experiments on this subject. At first he seemed to find every thing exactly as described by Mr. Goettling, *Ibid.* B. I.; but soon afterwards he made the interesting discovery, that by a careful decomposition of atmospherical air, by means of nitrous gas, azotic gas may be procured, so free from oxygen, that phosphorus cannot be made to shine in it. *Sammlung pract. chemisch. Abhandl.* B. I.

Professor Hildebrandt repeated Mr. Goettling's experiments with the greatest care. *Crell's Annal.* 1796, B. I. These experiments are, no doubt, the most exact of any that were made for this purpose. He has perfectly confirmed our experiments. He has proved this beyond all question, by several experiments, of which I shall mention but one.—In the same azotic gas in which phosphorus had ceased to shine, and fresh phosphorus would not afford light, the luminous appearance took place as soon as oxygen gas was added. But this lasted no longer than till the latter was consumed; for there was precisely as much of the whole volume of the mixture absorbed, as made up the quantity of the oxygen gas that was added.

There are several well known papers on this subject, in the *Annales de Chimie*, written by French chemists; but none of their authors have paid the least attention to what has been transacted on this matter: a circumstance which I have thought proper to be mentioned in this place. But they have added the important fact, that phosphorus is soluble in azotic gas. Probably they had no intelligence of what has been written about it in Germany; and as this appears to me to be the case with England, I considered it of some importance, to give the shortest possible view of the chief points of these transactions. I have endeavoured to give a more ample detail of all the particular facts, as far as they were known down to October 1796, in a work of mine, equally unknown in this country: *Nachtraege zu den Grunzuegen der neuern chemischen Theorie*, pag. 326—349, in which the whole extent of these enquiries may be seen with greater ease by the systematical arrangement of the experiments. It is to be hoped that the dispute will end here, and that the facts, (1.) that no shining of the phosphorus can in any respect take place in such azotic gas as is quite free of oxygen, and (2.) that it cannot be made to shine unless some other body be added which is capable of affording oxygen,—will be admitted as incontrovertibly established.

LONDON, *Feb.* 27, 1798.

III. On

III.

On M. Lazowski's new Barometer, or Weather Instrument. By a Correspondent.

To Mr. Nicholson, Editor of the Philosophical Journal.

Sir, *Kendal, Jan. 31, 1798.*

THE following experimental essay is too trifling to require an introductory apology, were it not for the high name of Dr. Hutton, which unavoidably occurs in the course of it, as the subject of the present communication was first suggested by a paragraph in his Mathematical and Philosophical Dictionary, published in 1795.

When any thing new in science is announced, it ought to be made public as generally and as speedily as possible. In this respect Dr. Hutton has only done his duty as a compiler, in bringing the English reader acquainted with the supposed discovery of a new instrument capable of indicating approaching changes of the weather. On the other hand, when any thing novel is communicated to the lovers of science, it is the business and undoubted right of every friend to enquiry, not to receive the proposal on bare authority, but to subject it to the test of argument and experiment, in order to establish it as a fact, or refute it as an error.

Having premised thus much in my own vindication, I will in the next place transcribe the paragraph alluded to above. "To the foregoing may be added a new sort of Barometer, or Weather Instrument, by the sound of a wire. This is mentioned by M. Lazowski, in his Tour through Switzerland: it is as yet but in an imperfect state, and was lately discovered there by accident. It seems that a clergyman, though near-sighted, often amused himself with firing at a mark, and contrived to stretch a wire so as to draw the mark to him to see how he had aimed. He observed that the wire sometimes sounded as if it vibrated like a musical cord; and that after such soundings, a change always ensued in the state of the atmosphere, from whence he came to predict rain, or fine weather, &c.*"

After perusing this singular narrative, I found myself at a loss in attempting to refer the discovery to any known class of phenomena, or to explain it by the affections of the atmosphere, considered as depending on the barometer, electricity, or even the hygrometer, without admitting a supposition, which is not discountenanced by the preceding report, viz. that one end of the wire was fixed to a frame of wood, while the other end was stretched over a nail or metal pin in the same by a weight. Under these circumstances, it appeared not impossible, that the wood-work might contract and expand from successive variations in the air's humidity, thereby putting the pin in motion, which may be supposed to scratch the wire, and give rise to the sound in question. But with a view to ascertain if a vibratory motion can be excited in metallic strings by any other change in the state of the atmosphere, I fixed a number of copper and iron wires, in the beginning of April 1795, in an open place: they were different lengths, from three to six feet, and of different diameters, varying from the thickness of a fine thread to the size of a small cord; they were all stretched by metal pins, resembling those used in a harpsichord, and so disposed as to make various angles with the meridian and the horizon. The apparatus, being thus completed, was

* See Hutton's Dictionary, article *Barometer*.

was carefully attended to through the ſummer, particularly when the aſpect of the ſky and ſtate of the barometer ſeemed to predict an approaching change; but the enquiry was fruitleſs, the promiſed ſound was never heard; nor did any other circumſtance worthy notice occur, except that ſome of the ſmalleſt wires ſnapped occaſionally in the cold nights of the ſpring. The failure of this experiment ſeems to confirm the ſuppoſition that M. Lazowſki's inſtrument is at beſt but an imperfect Hygrometer, and that it does not promiſe any new diſcovery relative to the properties or compoſition of the atmoſphere *. J. G.

IV.

Obſervations on Scylla and Charybdis †. *By the Abbé* LAZZARO SPALLANZANI, *Profeſſor of Natural Hiſtory at Pavia, F.R.S. &c. &c.*

SCYLLA and Charybdis, according to the fables of the poets, are two ſea monſters whoſe dreadful jaws are continually diſtended to ſwallow unhappy mariners; the one ſituated on the right, and the other on the left extremity of the Strait of Meſſina, where Sicily fronts Italy.

Dextrum Scylla latus, lævum implacata Charybdis
Obſidet: atque imo barathri ter gurgite vaſtos

* The paſſage in Dr. Hutton's Dictionary will admit of the ſenſe, that the obſerver (who perhaps never took notice of his wire, except when buſied in the amuſement there mentioned) did not hear any ſpontaneous ſound, but merely perceived a muſical vibration at certain times during the act of drawing the mark along the wire, which muſt have agitated it. This might ariſe from tenſion, by ſome hygrometric change in the ſituation of the points of ſupport. It may be remarked, that a wire for the uſe the clergyman applied it to muſt have been much longer than ſix feet; and that it was probably leſs ſtretched than the wires expoſed by my correſpondent.

If the ſounds were ſpontaneous, the fact will be referable to the Eolian harp of Merſennus, ſince re-invented by Oſwald. The inſtrument conſiſts of a ſounding board about two feet or more in length, upon which ten or twelve (catgut) ſtrings of equal length but different thickneſs are tuned in uniſon, at as low a pitch as the ſmalleſt of the ſtrings can bear. When this is placed in a current of air, ſuch for example as paſſes through a window partly opened, it emits a variety of contemporaneous and ſucceſſive tones, which from their changes in melody, harmony, and ſwell, and a certain wild ſtrangeneſs in the whole effect, rivet the attention, and ſometimes afford exquiſite pleaſure. The Eolian harp has been celebrated by Thomſon and other poets. I have not made experiments to analyſe its mode of operation; but am diſpoſed to think, (1) that it can only give the trumpet notes; (2) that the note to be afforded by any one ſtring depends on the quantity of the impulſe of the wind being greateſt near one end of that ſtring; (3) that the ſame quantity and direction of impulſe will agitate a longer portion of the ſlack ſmall ſtring than of the tenſer thick ſtring, ſo that in the vibratory ſubdiviſions the ſmaller ſtring will give the graver tone; (4) that a powerful tone drawn from one ſtring may diſpoſe the other ſtrings to vibrate uniſons, fifths, thirds, octaves, and other concords, more or leſs remote according to the circumſtances.

If M. Lazowſki's wire acted upon the principle of the Eolian harp, it might be preſumed that the recurrence as well as the nature of the ſound emitted would be governed by the force and angle of direction of the wind; and, if ſo, much would depend on the expoſure the direction and the tenſion of the wire. Whether on this ſuppoſition it could afford any more certain indication of approaching change of weather, than is uſually obtained from a ſimple obſervation by the wind vane, muſt be decided by facts, if ſuch ſhould hereafter appear. N.

† Travels in the Two Sicilies. Engliſh Tranſl. IV. 168.

Sorbet

Sorbet in abruptum fluctus, rurſuſque ſub auras
Erigit alternos, et ſidera verberat undâ.
At Scyllam cæcis cohibet ſpelunca latebris,
Ora exertantem, et naves in ſaxa trahentem.
Prima hominis facies, et pulchro pectore virgo
Pube tenus: poſtrema immani corpore priſtis,
Delphinûm caudas utero commiſſa luporum.

VIRG. Æneid. Lib. III.

Far on the right her dogs foul Scylla hides:
Charybdis roaring on the left preſides,
And in her greedy whirlpool ſucks the tides,
Then ſpouts them from below: with fury driv'n
The waves mount up, and waſh the face of heav'n.
But Scylla from her den with open jaws
The ſinking veſſel in her eddy draws,
Then daſhes on the rocks. A human face
And virgin boſom hide her tail's diſgrace;
Her parts obſcene below the waves deſcend
With dogs encloſ'd, and in a dolphin end. DRYDEN.

I have no difficulty in availing myſelf of the deſcription of a poet in a work dedicated to the inveſtigation of truth, nor ſhall I heſitate to cite ſimilar paſſages from another poet; ſince, however exaggerated theſe may be by the glowing colours of imagination, they contain truth, and afford a ſubject for intereſting enquiries.

I ſhould have thought myſelf to have merited the greateſt cenſure, if, when I was in the ſtrait of Meſſina, I had not viſited two places of which ſo much has been written, and which have been rendered ſo famous by the numerous ſhipwrecks they have occaſioned.

I firſt proceeded in a ſmall boat to Scylla. This is a lofty rock, diſtant twelve miles from Meſſina, which riſes almoſt perpendicularly from the ſea on the ſhore of Calabria, and beyond which is the ſmall city of the ſame name. Though there was ſcarcely any wind, I began to hear, two miles before I came to the rock, a murmur and noiſe like a confuſed barking of dogs, and, on a nearer approach, readily diſcovered the cauſe. This rock, in its lower parts, contains a number of caverns, one of the largeſt of which is called by the people there *Dragara*. The waves, when in the leaſt agitated, ruſhing into theſe caverns, break, daſh, throw up frothy bubbles, and thus occaſion theſe various and multiplied ſounds. I then perceived with how much truth and reſemblance of nature Homer and Virgil, in their perſonifications of Scylla, had pourtrayed this ſcene, by deſcribing the monſter they drew as lurking in the darkneſs of a vaſt cavern, ſurrounded by ravenous barking maſtiffs, together with wolves to increaſe the horror:

Ενθα δ' ενι Σκυλλη ναιει δεινον λελακυια·
Της ήτοι φωνη μεν όση σκυλακος νεογιλης
Γινεται. HOM. Odyſſ. XII.

Here

Here Scylla bellows from her dire abodes,
Tremendous peſt! abhorr'd by man and gods!
Hideous her voice, and with leſs terrors roar
The whelps of lions in the midnight hour! Pope.

The Greek poet, when he pourtrays the rock which is the habitation of Scylla, finiſhes the picture higher than the Latin, by repreſenting it as ſo lofty that its ſummit is continually wrapped in the clouds, and ſo ſteep, ſmooth and ſlippery, that no mortal could aſcend it, though he had twenty hands and twenty feet.

'Οι δε δυω σκοπελοι, ὁ μεν ουρανον ευρυν ικανει
Οξειῃ κορυφῃ, νεφελη δε μιν αμφιβεβηκε
Κυανεη, το μεν ουποτ' ερωει, ουδεποτ' αιθρη
Κεινου εχει κορυφην, ουτ' εν θερει ουτ' εν οπωρῃ.
Ουδε κεν αμβαιη βροτος ανηρ, ου καταβαιη,
Ουδ' ει ὁι χειρες γε εεικοσι και ποδες ηεν.
Πετρη γαρ λις εστι περιξεστῃ ειχυια. Hom. Odyſſ. XII.

High in the air the rock its ſummit ſhrouds
In brooding tempeſts and in rolling clouds;
Loud ſtorms around and miſts eternal riſe,
Beat its bleak brow and intercept the ſkies.
When all the broad expanſion, bright with day,
Glows with th' autumnal or the ſummer ray;
The ſummer and the autumn glow in vain,
The ſky for ever low'rs, for ever clouds remain.
Impervious to the ſtep of man it ſtands,
Though borne by twenty feet, though arm'd with twenty hands.
Smooth as the poliſh of the mirror riſe
The ſlippery ſides, and ſhoot into the ſkies. Pope.

Such, three thouſand years ago, or nearly ſo, appeared the rock of Scylla, according to the obſervation of Homer; and ſuch is nearly its appearance at this day.

The accuracy of this truly " firſt great painter of antiquity," which has likewiſe been obſerved by ſcientific travellers in other deſcriptions which he has given, ſhews that the level of the waters of the ſea was at that time at nearly the ſame height as at preſent, ſince, had it ſunk only a few fathom, it muſt have left the foot of the rock, which, according to my obſervations, is not very deep, entirely dry. And this I conſider as one among ſeveral ſtrong arguments, that the moſt remarkable ſinkings of the ſea are anterior to the time of Homer.

Such is the ſituation and appearance of Scylla: let us now conſider the danger it occaſions to mariners. Though the tide is almoſt imperceptible in the open parts of the Mediterranean, it is very ſtrong in the ſtrait of Meſſina, in conſequence of the narrowneſs of the channel,

channel, and is regulated, as in other places, by the periodical elevations and depreſſion of the water. Where the flow or current is accompanied by a wind blowing the ſame way, veſſels have nothing to fear, ſince they either do not enter the ſtrait, both the wind and the ſtream oppoſing them, but caſt anchor at the entrance; or, if both are favourable, enter on full ſail, and paſs through with ſuch rapidity that they ſeem to fly over the water. But when the current runs from ſouth to north, and the north wind blows hard at the ſame time, the ſhip which expected eaſily to paſs the ſtrait with the wind in its ſtern, on its entering the channel is reſiſted by the oppoſite current, and, impelled by two forces in contrary directions, is at length daſhed on the rock of Scylla, or driven on the neighbouring ſands; unleſs the pilot ſhall apply for the ſuccour neceſſary for his preſervation. For, to give aſſiſtance in caſe of ſuch accidents, four-and-twenty of the ſtrongeſt, boldeſt and moſt experienced ſailors, well acquainted with the place, are ſtationed night and day along the ſhore of Meſſina; who, at the report of guns fired as ſignals of diſtreſs from any veſſel, haſten to its aſſiſtance, and tow it with one of their light boats. The current, where it is ſtrongeſt, does not extend over the whole ſtrait, but winds through it in intricate meanders, with the courſe of which theſe men are perfectly acquainted, and are thus able to guide the ſhip in ſuch a manner as to avoid it. Should the pilot, however, confiding in his own ſkill, contemn or neglect this aſſiſtance, however great his ability or experience, he would run the moſt imminent riſk of being ſhipwrecked. In this agitation and conflict of the waters, forced one way by the current, and driven in a contrary direction by the wind, it is uſeleſs to throw the line to diſcover the depth of the bottom, the violence of the current frequently carrying the lead almoſt on the ſurface of the water. The ſtrongeſt cables, though ſome feet in circumference, break like ſmall cords. Should two or three anchors be thrown out, the bottom is ſo rocky that they either take no hold, or, if they ſhould, are ſoon looſened by the violence of the waves. Every expedient afforded by the art of navigation, though it might ſucceed in ſaving a ſhip in other parts of the Mediterranean, or even the tremendous ocean, is uſeleſs here. The only means of avoiding being daſhed againſt the rocks or driven upon the ſands in the midſt of this furious conteſt of the winds and waves, is to have recourſe to the ſkill and courage of theſe Meſſineſe ſeamen.

In proof of the truth of this aſſertion, I might adduce many inſtances related to me by perſons deſerving of credit. But I was myſelf an eye-witneſs to the ſituation of a trading veſſel from Marſeilles, which had one day entered the Strait by the mouth on the north ſide, at the time that I was on a hill looking towards the ſea. The current, and a north wind which then blew ſtrong, being both in its favour, the veſſel proceeded under full ſail into, and had paſſed one half of the Strait, when, on a ſudden, the ſky became overcaſt with thick clouds, and violent guſts of wind aroſe, which in an inſtant changed the direction of the current, and turned up the ſea from its bottom. The mariners had ſcarcely time to hand the ſails, while the furious waves broke over the ſhip on every ſide. Whether they merely followed the practice uſual with ſhips in diſtreſs, or whether they were acquainted with the laudable cuſtom of the Meſſineſe, I cannot ſay; but they fired two guns; immediately upon which one of the barks employed on this ſervice haſtened to the aſſiſtance of the diſtreſſed veſſel, and, taking it in tow, began to make every exertion to carry it ſafely into the harbour.

If I had ſeen with fear and ſhuddering the danger of the ſailors on board the veſſel, which

which I expected every moment would be swallowed up in the waves; I beheld with wonder and pleasure the address and bravery of the Messinese mariners, who had undertaken to steer safely through so stormy a sea the ship entrusted to their care. They extricated it from the current, which impelled it towards destruction; changed the helm to this side, or to that; reefed or set the sails, as the wind increased, or abated; avoided the impetuous shocks of the waves, by meeting them with the prow, or opposing to them the side, as either method appeared most proper to break their violence; and by these and other manœuvres, which I am unable to describe, these brave mariners, amid this dreadful conflict of the sea and the wind, succeeded in their undertaking, and brought the vessel safe into the harbour.

But enough of Scylla:—we will now proceed to Charybdis. This is situated, within the Strait, in that part of the sea which lies between a projection of land named *Punta Secca*, and another projection on which stands the tower called *Lanterna*, or the light-house, a light being placed at its top to guide vessels which may enter the harbour by night.

On consulting the authors who have written of Charybdis, we find that they all supposed it to be a whirlpool. The first who has asserted this is Homer, who has represented Charybdis as a monster, which three times in a day drinks up the water, and three times vomits it forth.

——— *δια Χαρυβδις αναρροιβδει μελαν ὑδωρ,*
Τρις μεν γαρ τ᾽ανιησιν επ᾽ ἡματι, τρις δ᾽αναρροιβδει
Δεινον. Hom. Odyss. XII.

Beneath Charybdis holds her boisterous reign
'Midst roaring whirlpools, and absorbs the main;
Thrice in her gulphs the boiling seas subside,
Thrice in dire thunders she refunds the tide. Pope.

The description of Virgil above cited, differs from that of Homer only in placing a deep gulph below. Strabo, Isidorus, Tzetzes, Hesychius, Didymus, Eustathius, &c. repeat the same. The Count de Buffon adopts the idea of Homer in full confidence, and places Charybdis among the most celebrated whirlpools of the sea. "Charybdis, in the strait of Messina, absorbs and rejects the water three times in twenty-four hours*." Strabo tells us, that the fragments of ships swallowed up in this whirlpool are carried by the current to the shore of Tauromenium (the present Taormina) thirty miles distant from Charybdis†. In confirmation of this tradition, an amusing though tragical anecdote is related of one Colas, a Messinean diver, who, from being able to remain a long time under water, had acquired the surname of *Pesce* (the fish). It is reported that Frederic, king of Sicily, coming to Messina purposely to see him, made trial of his abilities with a cruel kind of liberality, by throwing a golden cup into Charybdis, which if he brought up was to be the reward of his resolution and dexterity. The hardy diver, after having twice astonished the spectators by remaining under water a prodigious length of time, when he plunged the third time appeared no more; but, some days after, his body was found on the coast, near Taormina.

* Buffon, Hist. Nat. tom. ii. in 12mo.

† Καταποθεντων δε και διαλυθεντων τα ναυαγια παρασυρεται προς ηιονα της Ταυρομενιας. Lib. VI.

From

From the authorities here adduced, it is evident that Charybdis has hitherto been confidered as a real whirlpool, by both ancient and modern travellers who have given any account of it.

As I was therefore fo near to this celebrated place, I determined to endeavour to afcertain, if poffible, what it really is. It is diftant from the fhore of Meffina about feven hundred and fifty feet, and is called by the people of the country *Galofaro*, not from the agitation of the waves as fome have fuppofed, but from καλος and φαρος; that is, *the beautiful tower*, from the light-houfe erected near it for the guidance of veffels. The phenomenon of the Calofaro is obfervable when the current is defcending; for, when the current fets in from the north, the pilots call it the *defcending rema**, or current; and when it runs from the fouth, the *afcending rema*. The current afcends or defcends at the rifing or fetting of the moon, and continues for fix hours. In the interval between each afcent or defcent, there is a calm which lafts at leaft a quarter of an hour, but not longer than an hour. Afterwards, at the rifing or fetting of the moon, the current enters from the north, making various angles of incidence with the fhore, and at length reaches the Calofaro. This delay fometimes continues two hours. Sometimes it immediately falls into the Calofaro, and then experience has taught that it is a certain token of bad weather.

As I was affured by the pilots moft experienced in this practical knowledge, that there was no danger in vifiting the Calofaro, I refolved to avail myfelf of the opportunity. The bark in which I made the excurfion was managed by four expert mariners, who, perceiving me fomewhat intimidated as I approached the place, encouraged me, and affured me they would give me a very near view of the Calofaro, and even carry me into it without the leaft danger.

When I obferved Charybdis from the fhore, it appeared like a group of tumultuous waters; which group, as I approached, became more extenfive and more agitated. I was carried to the edge, where I ftopped fome time to make the requifite obfervations, and was then convinced beyond the fhadow of a doubt, that what I faw was by no means a vortex, or whirlpool.

Hydrologifts teach us, that by a whirlpool in a running water we are to underftand that circular courfe which it takes in certain circumftances; and that this courfe, or revolution, generates in the middle a hollow inverted cone, of a greater or lefs depth, the internal fides of which have a fpiral motion. But I perceived nothing of this kind in the Calofaro. Its revolving motion was circumfcribed to a circle of at moft an hundred feet in diameter; within which limits there was no incurvation of any kind, nor vertiginous motion, but an inceffant undulation of agitated waters which rofe, fell, beat, and dafhed on each other. Yet thefe irregular motions were fo far placid, that nothing was to be feared in paffing over the fpot, which I did; though our little bark rocked very much from the continual agitation, fo that we were obliged conftantly to make ufe of our oars to prevent its being driven out of the Calofaro. I threw fubftances of different kinds into the ftream. Such as were fpecifically heavier than the water funk, and appeared no more; thofe which

* I have obferved that at Meffina, as well as in other parts of Sicily, words of the Greek language, which was once that of this ifland, are ftill retained. Thus the word *rema* derived from ῥευμα, a flowing, or ftream, is ufed to fignify the current of this ftrait.

were lighter remained on the furface, but were foon driven out of the revolving circle by the agitation of the water.

Though from thefe obfervations I was convinced that there was no gulph under the Calofaro; as otherwife there would have been a whirlpool, which would have carried down into it the floating fubftances; I determined to found the bottom with the plummet, and found its greateft depth did not exceed five hundred feet. I was likewife informed, to my no fmall furprife, that beyond the Calofaro, towards the middle of the ftrait, the depth was double.

I could not therefore but conclude from thefe facts, that at that time there was no whirlpool in Charybdis. I fay *at that time*, fince the cafe might be very different when the fea was tempeftuous. I therefore made enquiry relative to this of the pilots, thofe, efpecially, who, from their tried experience, were appointed by the public to give affiftance in ftorms to foreign veffels, and who had frequently feen Charybdis in its greateft fury. The following is the fubftance of the anfwers they gave me:

When the current and the wind are contrary to each other, and both in their greateft violence, efpecially when the fcilocco, or fouth wind, blows; the fwelling and dafhing of the waves within the Calofaro is much ftronger, more impetuous, and more extenfive. It then contains three or four fmall whirlpools, or even more, according to the greatnefs of its extent and violence. If, at this time, fmall veffels are driven into the Calofaro by the current, or the wind, they are feen to whirl round, rock, and plunge; but are never drawn down into the vortex. They only fink when filled with water, by the waves beating over them. When veffels of a larger fize are forced into it, whatever wind they have they cannot extricate themfelves; their fails are ufelefs; and after having been for fome time toffed about by the waves, if they are not affifted by the pilots of the country, who know how to bring them out of the courfe of the current, they are furioufly driven upon the neighbouring fhore of the Lanterna, where they are wrecked, and the greater part of their crews perifh in the waves*.

If we confider maturely thefe facts, we fhall find that a great part of what has been written relative to Charybdis is very erroneous. We have feen how many authors, from Homer to the prefent time, have defcribed it as a real whirlpool, or great gulph revolving in itfelf, within the circumference of which fhould any fhip enter, it is immediately drawn to the centre and fwallowed up. When the current is dying away, or when there is no current, this defcription has no refemblance to truth. Charybdis is then perfectly innocent, as I have been fully convinced by my own obfervations; and even when it is agitated

* The following account of the fhipwreck of a veffel in the Calofaro was fent me, after my return from Sicily, by the Abbate Grano from Meffina:

"About three weeks ago, we were fpectators of the finking of a Neapolitan polacca in the Calofaro, on its paffage from Puglia, laden with corn. A moft violent fouth-cafterly wind blew, and the veffel, with all fails fet, endeavoured to reach the harbour, ftanding off from the Calofaro; but the head of the current from the entrance by the faro took her, and drew her impetuoufly into it; where, without being able to make ufe of her fails, fhe remained for fome time toffed about by the waves, which at length, either breaking over her, or opening her fides by their furious beating, fent her to the bottom. The crew, however, and a part of the cargo were faved by the fpeedy affiftance given by our mariners in two fmall barks, who had the courage to encounter the danger. You will perceive from this, in what manner the waves may fink fhips in Charybdis, without the neceffity of fuppofing a whirlpool."

and dangerous, it ſtill contains no incavation, or gulph of the nature of a vortex; but merely a ſtrong agitation and daſhing of its waves, which produces thoſe ſmall whirlings of its waters, which are only accidental and not to be feared. So far likewiſe is Charybdis from drawing to itſelf and ſwallowing veſſels, that it rather repels them, and throws them to a diſtance.

This error has ariſen like many others with reſpect to the productions of nature. Homer, in relating the voyage of Ulyſſes through the Strait of Meſſina, was the firſt who deſcribed Charybdis as an immenſe vortex, which abſorbs and rejects the water, and the ſhips that approach it; exemplifying his account by the fate of ſome of the companions of his hero, who were carried away by the whirlpool. The writers who came after him, whether poets, orators, hiſtorians, or geographers, have followed him in this deſcription, without any one of them taking the pains to repair to the place and examine it himſelf. Even Fazello the Sicilian, who was ſo induſtrious in aſcertaining facts, and whoſe accounts of his country are ſo accurate, clearly ſhews in his deſcription of Charybdis, that he had never obſerved it himſelf; and concludes his narration with the erroneous ſuppoſition above cited, that the things ſwallowed up by Charybdis are conveyed by ſubmarine currents to the ſhores of Taormina.

Among all who have written on this ſubject, we only find Cluverius who ſeems, at leaſt at firſt view, to have viſited the place. I ſhall tranſcribe his words:

" Ego ſane, cum Charybdis noſcendæ gratia aliquot dies Meſſanæ ſubſiſterem, et ab hominibus ejus loci, maximè vero nautis, non Siculis modo, ſed et Belgis, Britannis et Gallis, qui hoc fretum frequentes navigant, diligentius eam rem ſciſcitarer, nihil omnino certi ipſis perdiſcere potui, adeo ſcilicet totum negotium omnibus obſcurum et incognitum erat. Tandem tamen reperi Charybdim, quæ incolis, patriis vocabulis, dicitur Caloſaro, ſub prædicta ad Meſſanenſem portum pharo eſſe mare rapidè fluens, atque in vortices actum: quod non τρὶς ἐπ' ἤματι ut tradit Homerus, id eſt *ſingulis diebus ter*, abſorbet ingenti gurgite, revomitque aquas, ſed quoties vehementiori fluctu fretum comitatur."

" I remained ſome days at Meſſina, with a view to obtain ſome information relative to Charybdis; but though I made every enquiry of the people of the place, and principally the ſailors, not the Sicilian only, but the Italian, Dutch, Engliſh, and French, who frequently navigate that Strait, I could learn nothing ſatisfactory, ſo little was known by them on the ſubject. At length however I found Charybdis, which the natives call *Caloſaro*, under the lighthouſe before mentioned near the harbour, to be a ſea rapidly flowing, and forming vortices. It does not abſorb the waters in its vaſt gulph, and reject them *thrice in a day*, as Homer tells us; but as often as the ſea runs high in the Strait."

From the expreſſion " I found Charybdis" we might be induced to believe, that he made his obſervations on the ſpot. It is certain however that he does not explicitly tell us ſo: and when treating of a phenomenon, of which he was ſo anxious to obtain an accurate knowledge, which he could not procure even from the Meſſineſe ſailors, it is ſtrongly to be preſumed, that he would not have ſuppreſſed a circumſtance of that importance. As Charybdis may be ſeen from the ſhore, if he only went thither and turned his eyes towards it, he might with truth aſſert that he had diſcovered it. The other adjuncts to his account, that Charybdis is a rapid ſea, and that it abſorbs and rejects the water in

 a ſtorm,

a ftorm, convince me that he had not a juft idea of it, but fatisfied himfelf with the old tradition concerning Charybdis.

It may be obferved, that the fituation of Charybdis, as it has been hitherto defcribed, does not exactly agree with that affigned it by Homer. Let us refer to the poet. The goddefs Circe gives the following directions to Ulyffes, with refpect to the navigation of the Strait of Meffina:

> Ὁι δε δυω σκοπελοι, ὁ μεν ουρανον ευρυν ικανει . . .
> Τον δ' ἑτερον σκοπελον χθαμαλωτερον οψει Οδυσσευ,
> Πλησιον αλληλων, και κεν διοιςευσειας.
> Τῳ δ' εν ερινεος εστι μεγας φυλλοισι τεθηλως,
> Τῳ δ' ὑπο δια Χαρυβδις αναρροιβδει μελαν ὑδωρ. Hom. Odyff. XII.

> High o'er the main two rocks exalt their brow . . .
> Clofe by, a rock of lefs enormous height
> Breaks the wild waves, and forms a dangerous ftreight;
> Full on its crown a fig's green branches rife,
> And fhoot a leafy foreft to the fkies:
> Beneath, Charybdis holds her boifterous reign
> 'Midft roaring whirlpools, and abforbs the main. Pope.

The firft of the rocks here mentioned by Homer is Scylla, which he defcribes at length; and near the other, according to this poet, Charybdis is fituated. The diftance from one of thefe rocks to the other is an arrow's flight, *και κεν διοιςευσειας*, which does not at all accord with the prefent fituation of Scylla. How are we to explain this difagreement? Shall we fay that Homer, availing himfelf of the licence in which poets are indulged, has fpoken hyperbolically? I know not whether the connoiffeurs in poetry will permit fuch a licence. Or fhall we fuppofe that Charybdis was once much nearer to Scylla; but that in a long feries of ages it has changed its place, and removed under Meffina? Such a fuggeftion might, perhaps, be favourably received, if in remote times any confiderable change had taken place in the Strait: but we know not of any; and it is not probable, that a change fo remarkable as the removal of Charybdis from its place would have been paffed over in filence by Sicilian writers. Within the prefent century, it is true, this Strait, of which fo much has been faid, has become narrower; but at the fame time we know, that long before this event Charybdis was fituated where it is at prefent. The ancient and uninterrupted tradition of the Meffinefe refpecting this fact is confirmed by the authority of the moft celebrated Italian, Latin, and Greek writers. Fazello tells us, "Charybdis ex parte Siciliæ paulo fupra Meffanam." "Charybdis is fituated on the fide of Sicily, a little beyond Meffina."—Ovid fays,

> "Hinc ego dum muter, vel me Zanclæa Charybdis
> "Devoret!"

> "Let dire Charybdis in Zanclæan feas,
> "Devour me if I change!"

And it is well known that Zancle was the ancient name of Meffana, now Meffina. Tzetzes in Lycophron fays, Ἡ Χαρυβδις περι Μεσσηνην εστι. "Charybdis is fituated near Meffina."

fina." Strabo likewife, after having mentioned Meffina, proceeds, Δεικνυται και Χαρυβδις, μικρον προ της πολεως, εν τω πορθμω. "Charybdis is feen in the ftrait a little before we reach the city." Several other writers might be cited to the fame purpofe.

From all thefe reafons and hiftorical teftimonies we muft then conclude that Homer was not exact with refpect to the fituation of Charybdis; nor can it be a great offence to fay, that in this paffage of his long poem he has certainly nodded. The accuracy of feveral of his defcriptions of various places in Sicily cannot be denied. It is fuch that we muft either fuppofe that he had himfelf travelled in thofe parts, as is the opinion of many; or at leaft that he had procured very faithful and circumftantial information from others. Of this the rocks of Scylla are an example. But, as to the fuppofed whirlpool of Charybdis, and its fituation, I think we may venture to affirm he never faw it himfelf, and that the accounts he had received of it led him into error.

We will now enquire what foundation there is for the faying, which became proverbial, that "he who endeavours to avoid Charybdis dafhes upon Scylla;" and which was applied by the ancients to thofe who, while they fought to fhun one evil, fell into a worfe.

On this fubject I likewife made enquiries of the Meffinefe pilots abovementioned, and to what better mafters could I apply for the elucidation of fuch a proverb? They told me that this misfortune, though not always, yet frequently happens, unlefs proper meafures are taken in time to prevent it. If a fhip be extricated from the fury of Charybdis, and carried by a ftrong foutherly wind along the ftrait towards the northern entrance, it will pafs out fafely; but fhould it meet with a wind in a nearly oppofite direction, it would become the fport of both thefe winds, and, unable to advance or recede, be driven in a middle courfe between their two directions, that is to fay, full upon the rock of Scylla, if it be not immediately affifted by the pilots. They added, that in thefe hurricanes a land wind frequently rifes, which defcends from a narrow pafs in Calabria, and increafes the force with which the fhip is impelled towards the rock.

Before I began to write on Scylla and Charybdis, I perufed the greater part of the ancient authors who have written on the fubject. I obferve that they almoft all reprefent thefe difaftrous places in the moft gloomy and terrifying colours, as continually the fcene of tempefts and fhipwrecks. Thefe terrors and this deftruction, however, they are far from exhibiting in the prefent times, it rarely happening that any fhips are loft in this channel, either becaufe their pilots poffefs the knowledge requifite for their prefervation, or becaufe they apply for the neceffary affiftance. Whence then arifes this great difference between ancient times and the prefent? Can we fuppofe that Scylla and Charybdis have changed their nature, and become lefs dangerous? With refpect to the former, we have feen that this hypothefis is contradicted by fact; Scylla ftill remaining fuch as it was in the time of Homer: and with regard to the latter, from the Strait of Meffina becoming narrower, Charybdis muft be at prefent more to be feared than formerly, as it is well known that an arm, channel, or ftrait of the fea is the more dangerous in proportion as it is narrow. I am rather of opinion that this difference arifes from the improvement of the art of navigation, which formerly, in its infancy, dared not launch into the open fea, but only creep along the fhore, as if holding it with its hand.

Alter

Alter remus aquas, alter tibi radat arenas,
Tutus eris; medio maxima turba mari.

PROPERT. Lib. III.

To ſhun the dangers of the ocean, ſweep
The ſands with one oar, and with one the deep.

But time, ſtudy, and experience have rendered her more mature, better informed, and more courageous; ſo that ſhe can now paſs the wideſt ſeas, brave the moſt violent tempeſts, and laugh at the fears of her childhood.

To exemplify and ſupport the probability of this opinion, it will not be neceſſary to recur to the early and rude ages; much more modern times will furniſh us with ſufficient proofs. That part of the Adriatic, which ſeparates Venice from Rovigno in Iſtria, is certainly not the moſt propitious ſea to navigators. The danger of being hurried in ſix hours from one ſhore to the other, and there ſtranded; the frequency of violent winds which prevail there; the ſhallows and ſand-banks which break the waves and render them wild and irregular, may certainly cauſe ſome ſerious reflection in thoſe who embark to make the paſſage. So lately as the laſt century, the ſhipwrecks in this ſea were ſo numerous, and had ſo terrified the people of Rovigno, that, when any one was obliged by urgent buſineſs or any other cauſe to go to Venice, he conſidered himſelf as more likely to die than live; and, if he was the father of a family, uſed to make his will before he embarked. The Advocate Conſtantini, a native of that country, and a man of learning and ingenuity, told me when I was there, that he had read more than one of theſe teſtaments, depoſited among the public archives.

But at preſent I will not ſay it is a diverſion or pleaſure to make this paſſage, ſince, as ſtorms are not unfrequent, it is neceſſary to be cautious; but ſerious accidents rarely happen. I have myſelf three times made it without meeting with any cauſe of alarm. To what can this difference be attributed, but to the improvement of the nautical art? Beſides that the mariners of Rovigno were not then ſo expert in the management of their veſſels as at preſent; they made uſe of certain barks of ſo improper a conſtruction, as I was aſſured by the abovementioned Conſtantini, that it was impoſſible they ſhould long reſiſt the violence of the ſea. Thoſe on the contrary that have been built ſince that time, being of a broad and flat figure and very ſolid, are capable of withſtanding the moſt furious ſtorms. They are there called *bracere*, and are in great reputation in all the neighbouring countries. We here find a part of the ſea in which veſſels were formerly ſo frequently wrecked, and which could not be traverſed but at the riſk of life, now deprived of all its terrors, and rendered eaſily paſſable, merely by the improvements made in the art of navigation.

As a farther and ſtill more convincing proof that the dangers of Charybdis and Scylla, though in themſelves the ſame that they anciently were, have been diminiſhed, and the dread they inſpired removed by the rapid advances to perfection which this art has made in modern times; I ſhall adduce an example in another ſea no leſs an object of terror from tempeſts and ſhipwrecks, I mean the Cape of Good Hope, called the Stormy Cape by the firſt diſcoverer, and, by the mariners of thoſe times, the Raging Lion. How dreadful were the dangers of this place, where the two oceans deſcending down the oppo-

ſite

site sides of Africa met and clashed together; where contending winds, whose power was greater in the boundless ocean; where mountainous waves, rocks, and whirlpools threatened inevitable destruction! What preparations, what caution were thought necessary for the ship which was to make this dangerous passage? Able pilots who had frequently made the voyage; masts and yards secured by additional ropes; a large supply of sails and cables, thicker and stronger than usual; and a double rudder, that in case one should be damaged there might be another to act. The mariners were to be fastened to their posts by strong ropes; the passengers shut down below, and the deck left clear for the crew; a number of whom stood with hatchets in their hands, ready to cut away the masts should it be necessary. The guns were stowed in the hold as ballast, and the port-holes, windows, and every kind of aperture, carefully closed. Such were the precautions taken in the last century, on doubling the Cape of Good Hope; but how few of them are now necessary to perform this voyage in perfect safety! Of this I have had the satisfaction to be certified by an English gentleman, Mr. Macpherson, with whom I had the pleasure of conversing in Pavia, in July 1790; and who had twice doubled this Cape in his voyages to India; a gentleman of great respectability for his information, for the various long voyages he has made, and the honourable employments he has held.

The facility with which this passage may now be made, is therefore the consequence of the perfection to which the art of navigation has arrived; and the same we may conclude with respect to Charybdis and Scylla, which at present have nothing terrible but the name, to those who pass them with the requisite precautions.

V.

Instructions for refining Saltpetre by a new Process. By J. A. CHAPTAL, J. P. CHAMPY and BONJOUR.*

THE crude saltpetre is to be beaten small with mallets, in order that the water may more easily attack every part of the mass. The saltpetre is then to be put into tubs, five or six hundred pounds in each tub. Twenty per cent. of water is to be poured into each tub, and the mixture well stirred. It must be left to macerate, or digest, until the specific gravity of the fluid ceases to augment. Six or seven hours are sufficient for this first operation, and the water acquires the density of between 25 and 35 degrees. (Sp. Gr. 1.21, and 1.306. See Philos. Journal I. 39.)

The first water must then be poured off, and a second portion of water must be poured on the same saltpetre amounting to 10 per cent.; after which the mixture must be stirred up, suffered to macerate for one hour, and the fluid drawn or poured off.

Five per cent. of water must then be poured on the saltpetre; and after stirring the whole, the fluid must be immediately drawn off.

When the water is drained from the saltpetre, the salt must be thrown into a boiler containing 50 per cent. of boiling water. When the solution is made, it will mark between 66 and 68 degrees of the hydrometer. (Sp. Gr. 1.848, and 1.898.)

* Translated from the Journal de Physique, bearing date August 1794, but lately published.

The

The folution is to be poured into a proper veffel, where it depofits by cooling about two-thirds of the faltpetre originally taken. The precipitation begins in about half an hour, and terminates in between four and fix hours. But as it is of importance to obtain the faltpetre in fmall needles, becaufe in this form it is more eafily dried, it is neceffary to agitate the fluid during the whole time of the cryftallization. A flight motion is communicated to this liquid mafs by a kind of rake, in confequence of which the cryftals are depofited in very flender needles.

In proportion as the cryftals fall down, they are fcraped to the borders of the veffel, whence they are taken with a fkimmer, and thrown to drain in bafkets placed on treffels, in fuch a manner that the water which paffes through may either fall into the cryftallizing veffel, or be received in bafons placed underneath.

The faltpetre is afterwards put into wooden veffels in the form of a mill-hopper or inverted pyramid with a double bottom. The upper bottom is placed two inches above the lower on wooden ledges, and has many fmall perforations through which water may pafs to the lower bottom, which likewife affords a paffage by one fingle aperture. A refervoir is placed beneath. The cryftallized faltpetre is wafhed in thefe veffels with 5 per cent. of water; which water is afterwards employed in the folution of faltpetre in fubfequent operations.

The faltpetre, after fufficient draining, and being dried by expofure to the air upon tables for feveral hours, may then be employed in the manufacture of gunpowder.

But when it is required to ufe the faltpetre in the fpeedy and immediate manufacture of gunpowder, it muft be dried much more ftrongly. This may be effected in a ftove, or more fimply by heating it in a flat metallic veffel. For this purpofe the faltpetre is to be put into the veffel to the depth of five or fix inches, and heated to 40 or 50 degrees of the thermometer (or about 135 of Fahrenheit). The faltpetre is to be ftirred for two or three hours, and dried fo much that, when ftrongly preffed in the hand, it fhall acquire no confiftence, nor adhere together, but refemble a very fine dry fand. This degree of drynefs is not required when the powder is made by pounding.

From thefe circumftances we find, that two faline liquids remain after the operation, (1) the water from the wafhing; and (2) that from the cryftallizing veffels.

We have already remarked, that the wafhing of the faltpetre is performed in three fucceffive operations, in which, upon the whole, the quantity of fluid made ufe of amounts to 35 per cent. of the weight of the crude faltpetre. Thefe wafhings are eftablifhed on the principle, that cold water diffolves the muriates of foda, and the earthy nitrates and muriates, together with the colouring principle, but fcarcely attacks the nitrate of potafh.

The water of thefe three wafhings therefore contains the muriate of foda, the earthy falts, the colouring principle, and a fmall quantity of nitrate of potafh, the amount of which is in proportion to that of the muriate of foda, which determines its folution.

The water of the cryftallizing veffels contains a portion of the muriates of foda, and of the earthy falts which efcaped the operation of wafhing, and a quantity of nitrate of potafh, which is more confiderable than that of the former folution.

The waters made ufe of at the end of the operation, to whiten and wafh the cryftals depofited in the pyramidal veffel, contain nothing but a fmall quantity of nitrate of potafh.

Thefe

These waters are therefore very different in their nature. The water of the washings is really a mother water. It must be collected in vessels, and treated with potash by the known processes. It must be evaporated to 66 degrees (or 1,848 sp. gr.), taking out the muriate of soda as it falls. This solution is to be saturated with 2 or 3 per cent. of potash, then suffered to settle, decanted and poured into crystallizing vessels, where 20 per cent. of water is to be added to keep the whole of the muriate of soda suspended.

The waters which are thus obtained by treatment of the mother water, may be mixed with the water of the first crystallization. From these the marine salt may be separated by simple evaporation; and the nitrate of potash, which they hold in solution, may be afterwards obtained by cooling.

The small quantity of water made use of to wash and whiten the refined saltpetre, contains nothing but the nitrate of potash: it may therefore be used in the solution of the saltpetre when taken from the tubs.

From this description it follows, that a manufactory for the speedy refining of saltpetre ought to be provided with (1) mallets or rammers for pounding the saltpetre, (2) tubs for washing, (3) a boiler for solution, (4) a crystallizing vessel of copper or lead, in which the saltpetre is to be obtained by cooling, (5) baskets to drain the crystals, (6) a wooden case or hopper for the last washing and draining the saltpetre, (7) scales and weights for weighing, (8) hydrometers and thermometers to ascertain densities and temperatures, (9) rakes to agitate the liquor in the crystallizing vessel, (10) skimmers to take out the crystals and convey them to the baskets, (11) syphons or hand-pumps to empty the boilers.

The number and dimensions of these several articles must vary according to the quantity of saltpetre intended to be refined.

If it be proposed to refine ten thousand weight of crude saltpetre per day, the requisite men and utensils may be determined as follows:

Part of the ground near the magazine may be disposed for conveniently breaking and pounding the saltpetre.

This ground should be paved with large flat stones very uniformly, or with thick pieces of wood. Mallets similar to those used in pulverizing gypsum may be applied to this use. Two men are sufficient to weigh and pound the saltpetre, and stow it in the magazine.

As the three washings require two days, and each tub can hold only five or six hundred pounds of saltpetre, it would require twenty days to refine ten thousand weight (with one tub).

These tubs are two feet and a half in height, and the same in diameter. They must be very well made, in order that the water of the washing may not leak out. They are to be placed solidly on a plane slightly inclined, of such a material as shall not imbibe the water which may be spilt during the operation, but transmit it to a reservoir placed at the extremity of the row of tubs.

Twenty of these tubs must be disposed in two parallel lines; the planes on which they are set may incline towards each other, and form, by their union, the gutter or cavity for transmitting into the common reservoir such waters as may escape. These tubs are perforated at the distance of two inches from the bottom. The aperture is closed by a spigot.

Four men may be appropriated to the washing of the saltpetre. It is a part of their duty to convey the saltpetre from the magazine to the tubs, and from the tubs to the boiler.

It is fcarcely neceffary to obferve that the tubs muft ftand fufficiently apart, and be fo difpofed that the work may be eafy and convenient.

A boiler of a conical form, five feet wide and four deep, will ferve for three operations per day, and confequently to refine fifteen thoufand weight. A fingle man is fufficient to attend the boiler.

The veffel for cryftallization is of lead or copper, and muft be as near the boiler as poffible. Its depth is fifteen inches, its length ten feet, and its width eight. It muft be placed on a very folid fupport, fo that its bottom may every where reft upon it. It is convenient to raife this fupport of mafonry about twelve inches above the ground. By this means the borders of the cryftallizing veffel will be 27 inches above the floor, which will render the operation eafy and convenient.

It has appeared to us of advantage to give to the bottom of the cryftallizing veffel, an inclination of four inches from the fides to the middle, merely in the longitudinal direction. The folutions may be emptied for feveral fucceffive times from the boilers into the cryftallizing veffel, after having taken out the cryftals depofited from each folution.

Four men appear neceffary for the operation of the cryftallizing veffel. Their bufinefs is to agitate the fluid continually with the rakes. They collect without intermiffion on the borders of the veffel the cryftals which fall down, and convey them with a fkimmer to the bafkets prepared for their reception and draining. Thefe fame workmen put the faltpetre into the wooden veffel for the laft wafhing and drainage, and carry the refined faltpetre into the magazine.

For want of a large cooler for cryftallization a fhallow boiler may be ufed, or the fame veffels which ferve for cryftallization in the prefent works for refining this falt.

To prepare the faltpetre for the manufacture of gunpowder, it may be dried, after refining, by two proceffes, (1) by expofing it to the open air, or the fun, for feveral hours, upon tables; or (2) by expofing it in a fhallow metallic veffel for two hours, to the heat of 40 or 50 degrees (about 135 Fahrenheit). In either cafe it muft be agitated and ftirred with fcarcely any interruption, in order to dry it fpeedily and equally.

Confiderable experience has fhewn us, that the procefs here defcribed is the moft fimple and economical. But, to prevent others from trying fuch methods as might feem promifing, though we have thought fit to reject them, it will be proper to offer the following remarks:

1. Trial has been made to diffolve the faltpetre, cryftallize it, and then wafh it to feparate the fea-falt.

This procefs appears moft advantageous at firft fight, becaufe it faves the pounding; but it has great inconveniences. 1. Crude faltpetre diffolved in 50 per cent. of water, and poured into the veffel for cryftallization, does not depofit the fame quantity of faltpetre as it would do if wafhed before the folution. This difference depends on the muriate of foda in the crude faltpetre, which facilitates the folution of the nitrate of potafh, and confequently the water of the cryftallizing veffels muft retain in folution more nitrate of potafh, when the crude faltpetre is diffolved, than when it has been previoufly wafhed with cold

cold water, and deprived of the marine salt which it contains. 2. The washing of saltpetre, when it is performed after the solution and crystallization, requires 40 or 50 per cent. of water instead of 35.

3. Trial has been made to dissolve saltpetre in 20 or 25 per cent. of boiling water; to take out the sea-salt in proportion as it falls by the ebullition of the liquor; to dilute the fluid with 30 per cent. of additional water, and then to convey it into the vessel of crystallization. It was expected that the washings with cold water might be avoided, or considerably diminished by these means; but, not to mention that ebullition maintained for four or five hours to separate the sea-salt supposes very great consumption of time, fuel, and saltpetre, the washings are still indispensable to remove the colouring principle, and to carry off the last portions of muriate of soda.

4. It may be thought that the quantity of water in the washings might probably be diminished; but there is reason to fear that when the saltpetre is loaded with sea-salt, it cannot be perfectly refined by the use of a less quantity of water than we have prescribed.

5. The operator may also be tempted to diminish the proportion of water employed in the solution; but we are convinced by numerous experiments, that this proportion is the most suitable. If it be increased, the saltpetre remains dissolved in the fluid; if it be diminished, it settles or falls down in a solid mass. Observation has proved, that the degree of saturation best adapted to this work, is between the 66th and 68th degrees of the hydrometer (sp. gr. 1.848, and 1.896.)

6. It may likewise be thought more simple and economical to treat the solution of crude saltpetre with potash. But it is to be feared in this case that part of the alkali might be employed in decomposing the muriate of soda, to convert it into muriate of potash; and it must be observed, that this last salt is not at all proper to decompose the earthy nitrates, notwithstanding the assertions of skilful chemists to that effect.

It appears, therefore, more convenient to defer the treatment of the mother waters, and not to use potash till after the sea-salt has been separated by evaporation.

This process, therefore, unites a number of advantages.

1. It consumes much less fuel: for, instead of two long solutions and ebullitions, nothing more is required than to give the water a boiling heat in order to dissolve the saltpetre.

2. It requires less time. Three days are sufficient to purify the saltpetre to the degree suitable for making gunpowder.

3. It disposes the saltpetre to dry more readily. As the crystals are no larger than small needles, a few hours' exposure to the air are sufficient for its complete desiccation. This advantage is inestimable, particularly in a season wherein several months would be required to drain the large loaves of nitre; and in which, consequently, the fabrication of gunpowder would be either retarded or suspended, and the drying-houses encumbered with quantities of humid nitre.

4. It requires less space. A boiler five feet in diameter and four in depth, a vessel for crystallization of a few feet dimensions, and thirty tubs, are perfectly sufficient to refine fifteen thousand weight daily.

5. It occasions less loss. Very accurate experiments have shown that the solutions used in the ancient process occasioned a loss of saltpetre, by mere evaporation, which amounted to 7 per cent. of the original quantity. By this new process, the water which holds the salt-

petre in ſolution is never heated to boiling, the ſalt does not remain in the boiler, and the evaporation is almoſt nothing.

VI.

On the Light emitted by Superſaturated Borate of Soda, or Common Borax. By Mr. F. ACCUM.

To Mr. NICHOLSON.

SIR,

THAT two flints and ſeveral other ſiliceous ſtones, ſtruck againſt each other, appear luminous on the ſide ſtruck upon; and that phoſphate of lime, tremolite, ſugar, gum elemi, black jack, and various reſins become luminous, and emit phoſphoric ſparks in the dark, when ſcratched with a ſharp inſtrument, or ſtruck againſt one another, are well known to every tyro of natural philoſophy; but that *ſuperſaturated borate of ſoda* poſſeſſes this property in the higheſt degree, has not perhaps been hitherto remarked. Two pieces of this ſalt, of conſiderable magnitude, ſtruck againſt another, or a ſwift blow with any ſharp inſtrument upon it, produces ſuch a flaſh of white light, as none of the before-mentioned ſubſtances are capable of giving. It deſerves therefore a place under the claſs of thoſe kinds of phoſphoric ſubſtances which give a perceptible light by attrition or percuſſion, without having been expoſed either to the ſolar or artificial light; for which reaſon I take the liberty of laying this before you, begging you will give theſe lines a place in your much-admired Journal of Natural Philoſophy.

I remain, Sir, your very humble ſervant,

FREDERICK ACCUM.

Hay-Market, No. 17.

VII.

New Conſtruction of the Air Pump. By Sir GEORGE S. MACKENZIE, *Bart.*

FIG. I. plate 1, repreſents a ſection of the barrel. C is a cup for oil to moiſten the collar of leathers L, in which the piſton rod R works. φ is a plug—P P P P is the piſton, which is ſolid, except a ſpace for a collar of leathers λ, through which the wire W attached to the valve V, paſſes into a perforation in the piſton rod. N is a ſmall nut to prevent the valve from riſing too high. This method of lifting the valve was invented many years ago by Dr. Rutherford of Edinburgh. X is a perforation in the ſide of the bottom of the barrel, into which is inſerted a piece of metal, as in fig. 5, with a ſilk valve tied over it, opening downwards into the pipe K. E is the pipe leading to the receiver.

The conſtruction of the bottom of the barrel is ſeen in fig. 2 and 3. Fig. 4 is the valve and wire. Fig. 5 ſhews the conſtruction of the pipe E leading to the receiver, which is better than bent copper tubes, as theſe are apt to crack. Fig. 6 is caſt ſolid and bored.

Fig,

Fig. 7 is the cap fcrew by which the pipes are fixed. Fig. 8 is the pipe ufed for the condenfing apparatus.

That this pump may work well, it is neceffary that the bottom of the pifton be perfectly flufh with the bottom of the barrel.—The method of operation is as follows:

When the pifton is to be raifed, let the plug φ be opened.—The pifton rifing, expels the air above it through φ. When the pifton is at the higheft, fhut the plug. There will now be no preffure above the pifton, which will greatly facilitate the working. As the pifton rifes, the friction of the collar of leathers λ will raife the valve V, and the air in the receiver through the communication E will expand itfelf into the barrel. When the pifton is depreffed, V fhuts, and the air is expelled through K, to which a pipe as fig. 8 may be attached for condenfation. When the pifton reaches the bottom, no air will be left in the barrel, except the very fmall quantity in the very fmall hole of the valve X, which is very little when compared to the capacity of the barrel. By proceeding in this manner, a very perfect vacuum will be formed in the receiver.—By taking off the receiver and applying the pipe fig. 8, and attaching it to any veffel, and opening the plug φ, we have a complete condenfing apparatus. If required, the air may be taken from the receiver and thrown into another veffel. Moiftened leather ought not to be ufed for fixing the receiver, as vapours are conftantly iffuing from it; a drying lute is better.

This air pump may be made of a much cheaper conftruction than that of the plate, which, however, is the more convenient.

In the obliging letter which accompanied this communication, the author affures me, that its fimplicity and convenience have been found confiderable by experience. The reader will perceive, by turning to the firft volume of our Journal, p. 128, that the happy contrivance of the wire for lifting the lower valve is alfo claimed by Cuthbertfon, who in his pamphlet*, page 6, informs us that the hint of fuch an apparatus was firft given to him by M. Paets van Trooftwyk. It is not faid that Dr. Rutherford carried his invention into practice. This merit is due to Mr. Cuthbertfon and Sir G. M. I remember the fame ingenious thought having been alfo ftated by another philofophical gentleman, in 1783, when the air pump of Haas† was much talked of.

The air pump of Sir George Mackenzie differs in effect from that of Mr. Cuthbertfon in the folidity of the pifton, and in not having an oil veffel to the valve through which the air is extruded. The air pump of Sir George will ceafe to exhauft, fuppofing every thing elfe perfect, when the mafs of air in the receiver bears the fame proportion to an equal volume of external air, as the capacity of the bore of the valve-piece X, bears to the interior capacity of the barrel when the pifton is up. Cuthbertfon's pump will have a fimilar limit with relation to the communication pipe as to the upper valve (Philof. Journal, pl. 7, vol. 1, fig. 1.) and the capacity of the barrel above the pifton when down. In Prince's air pump, and in the project mentioned at p. 131 of the fame volume; if the

* Defcription of an improved Air Pump, &c. by John Cuthbertfon, London; fold by Johnfon. No date, but, as I think, publifhed feven or eight years ago.

† Phil. Tranf. M,DCC,LXXIII.

valves

valves be made to open mechanically, the maximum of exhauſtion will in theory be in the duplicate ratio of the ſmaller ſpace to the larger, aſſuming both ſtrokes to be equal in the barrels through which the air ſucceſſively paſſes. And in Sadler's pump with oil (Ibid. plate xix. fig. 1.) the maximum will be indefinitely great, becauſe every ſtroke muſt take out a like part of the reſidue of air from the receiver.

VIII.

On the Action of Nitre upon Gold and Platina. By SMITHSON TENNANT, *Eſq. F. R. S.**

GOLD, which cannot be calcined by expoſure to heat and air, has been alſo conſidered as incapable of being affected by nitre. But in the courſe of ſome experiments on the diamond, an account of which has been communicated to this ſociety, I obſerved that when nitre was heated in a tube of gold, and the diamond was not in ſufficient quantity to ſupply the alkali of the nitre with fixed air, a part of the gold was diſſolved. From this obſervation I was induced to examine more particularly the action of nitre upon gold, as well as to enquire whether it would produce any effect upon ſilver and platina.

With this intention I put ſome thin pieces of gold into the tube, together with nitre, and expoſed them to a ſtrong red heat for two or three hours. After the tube was taken from the fire, the part of the nitre which remained, conſiſting of cauſtic alkali, and of nitre partially decompoſed, weighed 140 grains; and 60 grains of the gold were found to have been diſſolved. Upon the addition of water, about 50 grains of the gold were precipitated in the form of a black powder. The gold which was thus precipitated was principally in its metallic ſtate, the greater portion of it being inſoluble in marine acid. The remaining gold, about ten grains in weight, communicated to the alkaline ſolution in which it was retained, a light yellow colour. By dropping into this ſolution diluted vitriolic or nitrous acid, it became at firſt of a deeper yellow, but, if viewed by the tranſmitted light, it ſoon appeared green, and afterwards blue. The alteration of the colour from yellow to blue, ariſes from the gradual precipitation of the gold in its metallic form, which by the tranſmitted light is of a blue colour: though the gold is precipitated from this ſolution in its metallic form, yet there ſeems to be no doubt that, while it remains diſſolved, it is entirely in the ſtate of calx. Its precipitation in the metallic ſtate is occaſioned by the nitre contained in the ſolution, which, having loſt part of its oxygen by heat, appears to be capable of attracting it from the calx of gold; for I found, that if the calx of gold is diſſolved by being boiled in cauſtic alkali, and a ſufficient quantity of nitre, which has loſt ſome of its air by heat, is mixed with it, the gold is precipitated by an acid in its metallic ſtate †.

Having

* Philoſophical Tranſactions, M,DCC,XCVII.

† As the precipitation of gold in its metallic form by nitre which has loſt ſome of its oxygen, has not, I believe, been noticed, it may not be improper to mention ſome of thoſe facts relating to it which ſeem moſt entitled to attention. Nitre, which has been heated ſome time, precipitates gold in its metallic ſtate from a ſolution in aqua regia, if it is diluted with water. If a ſolution of gold in nitrous acid is dropped into pure water, the calx of gold is ſeparated, which is of a yellow colour; but if the water contains a very ſmall proportion of nitre, which has loſt ſome of its air by heat (as one grain in ſix ounces), the gold is deprived of its oxygen and becomes blue. The alkali of the nitre does not aſſiſt in producing this effect. Nitrous acid alone, which does

Having found that nitre would diffolve gold, I tried whether it would produce any effect upon platina.

It has been formerly obferved, that the grains of platina, in the impure ftate in which it is originally found, might, by being long heated in a crucible with nitre, be reduced to powder. Lewis, from his own experiments and thofe of Margraff, thought that the iron only which is contained in the grains of platina was corroded by the nitre. But by heating nitre with fome thin pieces of pure platina in a cup of the fame metal, I found that the platina was eafily diffolved, the cup being much corroded, and the thin pieces entirely deftroyed. By diffolving the faline matter in water, the greater part of the platina was precipitated in the form of a brown powder. This powder, which was entirely foluble in marine acid, confifted of the calx of platina, combined with a portion of alkali, which could not be feparated by being boiled in water. The platina which was retained by the alkaline folution communicated to it a brown yellow colour. By adding an acid to it a precipitate was formed, which confifted of the calx of platina, of alkali, and of the acid which was employed.

Silver I found to be a little corroded by nitre: but, as its action upon that metal was very inconfiderable, it did not appear to be deferving of a more particular examination.

IX.

An Account of certain Caufes of Alteration injurious to the Quality of Corn, and the Means of preventing this Change. By B. G. SAGE, of the ci-devant Academy of Sciences, Profeffor of Chemiftry and Mineralogy in the School des Mines de la Monnoie.*

IN the Analyfis of Corn, which I publifhed in 1776, I have fhewn that when the corn no longer contains glutinous † or vegeto-animal matter, it affords flour not adapted to produce a good panary fermentation; that the bread is not white, and has a difagreeable tafte and fmell; that it produces an oppreffion at the ftomach, putrid diforders, and the dry gangrene, like fmutted rye. I did not at that time know the caufe of this alteration of corn; but difcovered it laft year, by attending to the farming operations in the corn country of Beaucé, where I have obferved that the method of houfing or ftacking the corn was more fuited to deftroy than to preferve it.

In fact, the fickle has fcarcely cut the corn before it is collected in fheaves to form fhocks, or larger parcels, which are immediately conveyed into the barns, where they are packed

does not contain its full proportion of oxygen, occafions the fame precipitation, unlefs it is very ftrong; and if a mixture of fuch ftrong nitrous acid, and of a folution of gold in nitrous acid, is dropped into water, the gold is deprived of its oxygen, and is precipitated of a blue colour. Two caufes contribute to produce this effect upon the addition of water. The adhefion of the calx of gold to nitrous acid is by that means weakened, and the oxygen is attracted more ftrongly to the imperfect nitrous acid in confequence of their attraction for water when they are united.

* Journal de Phyf. Sep. for 1794.

† Wheat is compofed of the cortical part called bran, ftarch, faccharine matter, and the glutinous fubftance. The flour obtained at the mills near Paris is compofed of 1-16th part of faccharine and extractive matter, 2-3ds of white fecula called ftarch, and 1-4th part of elaftic glutinous matter. The corn of the fouthern countries contain more. S.

as close as possible, without attending whether the grain and the straw be dry, as well as the more aqueous herbs which are cut along with it. The consequence is, that the corn becomes heated a few hours after it is put away, and this heat is stronger and more durable the larger and the damper the mass. The heat is frequently strong enough to bake an egg, according to the account of the cultivators. For my part, I could not hold my hand in this mass, which is as capable of spontaneous combustion as stacks of hay when put together too wet.

Fire does not manifest itself so often in our barns, because the air can scarcely at all penetrate into them, by reason of the very close stowage. When I asked the farmers why they pressed the corn so much? they affirmed, that their view was to prevent its occupying a large space, and to hinder vermin from finding their way into it.

When this heat is excited in the corn newly stowed away, a smell is emitted for three weeks resembling that of fermenting beer: it seemed at first as if aromatic herbs had been boiled in the neighbourhood.

I have observed the duration of this heat for more than four months, in a barn where the quantity stowed away amounted to a cube of about 40 feet. The corn, when taken out, was rough, ruddy, and more or less decomposed; so that in the lower part of the barn the alteration and decomposition of the glutinous matter was complete, and the grain was no longer proper for vegetation *. The bread made with flour of this corn does not rise well, and, after baking, exhibits a yellowish grey colour.

The farmers of Beaucé have a prejudice that it is good for the grain to sweat and heat, which is contrary to sound reasoning: for in this case the heat is produced by fermentation, which cannot take place but by the decomposition and loss of some of the integral parts of the corn. It is accordingly found, that the saccharine and glutinous matter are more or less destroyed in proportion to the time which the corn has remained in the heated state.

If the sheaves were dry when housed, they would not heat, and the grain would be preserved in perfection. It is proper therefore to suffer them to dry in the field, and not pack them together until they have given out all their moisture. It will also be of advantage to lay them lightly together instead of pressing, in order that the circulation of the air may carry off the last portions of humidity.

Besides the preservation of the grain, another advantage would be obtained, namely, that the straw would be neither heated nor mouldy. Straw in this last state contracts a disagreeable smell, which is repulsive to cattle.

It is in one of the most fertile corn provinces of France, in which the ground is best cultivated, that a method so prejudicial to the grain is employed. The interests of humanity being the same as that of the cultivator, it is to be presumed that, when once instructed, he will change his practice. In fact his gain will be double: for he will constantly have wholesome corn, and will sell it at a higher price than such as has been heated.

As the state and quality of corn is to be judged from the nature and quantity of the glutinous matter, it is proper to describe the process for extracting it.

Take four ounces of wheat flour separated from the bran; mix it with water to form a paste. Let this be kneaded for a quarter of an hour, and afterwards washed by working it

* I think it would be proper to suffer the corn to dry in the sheaf for the purpose of affording wholesome grain; for the fermentation alters, weakens, and often destroys it.

with the hands under water, which is to be changed from time to time. This washing is to be continued until the last quantity of water made use of is no longer discoloured. The substance remaining in the hands is the glutinous matter of a whitish grey colour. If the corn be good, this is elastic; (that is to say, it may be drawn into long strings, which have a disposition to shrink or contract.) If the corn has begun to heat, it is short or brittle. If it has fermented, it will afford none of this glutinous matter.

X.

Description of an Apparatus for disengaging Oxygen Gas, and applying it to the best Advantage. Constructed by JAMES SADLER, *Esq. Chemist to the Admiralty.—To which are added, Observations upon the Blow-Pipe. By W. N.*

FIG. I. plate 2, represents the vertical section of a furnace. The shaded parts denote brick work. A is the ash-hole, B the grate upon which the fuel is placed, C the opening for the reception of the fuel. It is covered with a piece of fire stone, or an earthen cover, at all times except when a supply of fuel is wanted. D is the aperture leading to the chimney F; and between D and F is a chamber for the reception of a mattrass or other vessel E, to be exposed to the action of the flame. The vessel E is put in its place, or taken out, by the opening at the upper part of the chamber; and when at work, that opening is closed by two pieces of fire stone, each of which covers half the aperture, and meets the other by a perfect ground edge, having a notch that leaves a space for the neck of the vessel. Fig. II. is a ground plan of the same furnace, with the apparatus for receiving and applying the oxygen. The letters B, E, F, denote the same parts as in fig. 1. Fig. III. exhibits a vertical section of the mattrass, and other apparatus, denoted by the same letters as in fig. 2. E is the mattrass containing black oxyde of manganese. H is a refrigeratory through which the tube of communication passes. I is a receiver for condensable vapour. The elastic fluid passes through the tube at K into the receiver L, inverted in another vessel of water M; the receiver being suspended by a string passing over a pulley, which therefore admits of a variation at pleasure of the reaction for extruding the air through the blow-pipe N P. The part N of the blow-pipe is of porcelain; and a lamp O is placed beneath for the purpose of heating the air before it issues from the orifice P. The letters G, K, and Q, denote cocks to be occasionally closed when the receiving apparatus is required to be separated or removed.

The advantages of this apparatus are, first, the simplicity of the furnace, which is applicable to a variety of uses, as well as that particularly stated in this account. As the current of atmospheric air from A, through the grate to B, D, and F, does not pass above the roof of the passage D; the upper part of the fire-place towards C, where the combustion cannot reach, may be considered as a repository for fuel, upon the principle of the athanor, and might, if required, be made equally capacious. This fuel, before it arrives at the place of combustion, serves also as a cover when the actual cover is taken off to supply the consumption; besides which, there is not the least possibility of deranging or disturbing the vessels on such occasions, as is too often the case in the common air furnace. The aperture at E affords

affords a degree of convenience, equally obvious, with regard to the putting in, placing, and taking out the veſſels. Of the refrigeratory, the receiver I, and the apparatus M L, little more need be ſaid than that they are conſtructed in the forms which experience has ſhewn to be the moſt ſimple and efficacious. The parts N O P exhibit an improvement which is found to be of great importance. Mr. Sadler obſerved, in the courſe of his experiments, that the effect of the oxygen, when recently produced, was much greater than ſome hours afterwards. There was no reaſon to conclude that this difference aroſe from any change in the purity of the fluid: he therefore concluded that it muſt be cauſed by a difference of temperature. When the cold oxygen is brought into contact with a combuſtible body at a very elevated temperature, it muſt be concluded, that part of the caloric diſengaged at the inſtant of combination muſt be employed in raiſing the temperature of the maſs of oxygen, and conſequently that the intenſity of the combuſtion will be leſs. He therefore determined to ſupply this portion of caloric from another proceſs of combuſtion, carried on near the external ſurface of the tube through which the oxygen is tranſmitted. O is the lamp for that purpoſe, affording a flame, which heats the tube N, and gives an elevated temperature to the oxygen before it paſſes out of the ſmall aperture P. The heat excited in a piece of charcoal urged by this ſtream of oxygen is ſo great, as to fuſe the pureſt ſpecimens of native rock cryſtal, and alſo thoſe of lime. The other effects are likewiſe proportionally greater.

Obſervations on the Common Blow-Pipe.

CHEMISTS and mineralogiſts are too well acquainted with the uſe of this inſtrument, to require any long enumeration of its advantages. To behold with eaſe ſuch proceſſes as demand much labour and time in furnaces, and cannot in this laſt ſituation be eaſily and comfortably inſpected;—to ſee theſe performed in the open air in a few ſeconds, with all the changes of colour, ebullition, ſcoriation, and the like;—to remark the nature of the vapours which fly off, and to note the precipitation of metallic ſubſtances from their fluxes, or the effects they produce on the ſeveral kinds of glaſs:—theſe are a few of the advantages which have brought this inſtrument into eſtimation.

It is well known that the common practice of blowing with the mouth, though very ready, and requiring an inſtrument of inconſiderable coſt, is not ſo advantageous as the extruſion of air by means of bellows, or other mechanical contrivances. The air exhaled from the lungs has already been deprived of part of its oxygen, and is loaded with humidity. The proceſs of blowing, even to the moſt ſkilful, is attended with ſome fatigue, and requires a degree of confinement of the head and one of the hands, which conſiderably diminiſhes the power, as well as the eaſe of the operator. Bellows, at the price of two and three guineas the ſet with a few additional implements, have been contrived for this purpoſe. It ſeemed probable to me, that theſe inſtruments are larger and more coſtly than is requiſite. To aſcertain the value of this ſuſpicion, I made the following experiments:

A blow-pipe nearly of the figure deſcribed by Bergmann, whoſe internal diameter was about one fifth of an inch at the ſmalleſt part, was inſerted through the cork of a bottle, of which the contents were $17\frac{1}{4}$ cubic inches. The cork was notched in ſuch a manner that

that when the pipe was ſtuck in the neck of the bottle previouſly filled with water, the whole could be inverted without any portion running out, though, from the ſize of the notch, a ſlight agitation was ſufficient to produce that event. In this ſituation the mouth was applied to the blow-pipe, and the air ſtrongly blown into the bottle. The water immediately flowed out, that is to ſay, in eighteen ſeconds. The experiment was twice repeated, and the aperture of the nozle of the blow-pipe was $\frac{1}{20}$th of an inch. Hence it follows, that the quantity of air emitted from the blow-pipe was not quite one cubic inch in a ſecond. For it was $17\frac{1}{4}$ cubic inches in eighteen ſeconds. It will appear alſo from an eaſy calculation, that the velocity was not quite four feet in a ſecond.

The nozle uſed in the foregoing experiment was adapted to the flame of a lamp: but it was too large for the flame of the candle called a ſhort eight, which is of tallow, nine inches long, three qrs. inch in diameter, having ſixteen yarns of cotton in its wick, and weighing $\frac{1}{8}$th part of a pound avoirdupois. The experiments were repeated with a nozle well ſuited to the flame of this candle. The aperture of this laſt was rather more than $\frac{1}{30}$th of an inch in diameter. The bottle was emptied once in 20 ſeconds, and twice with uncomfortable exertion in 18 ſeconds each time. If the velocity of emiſſion had been the ſame in both experiments, this laſt would have required about 36 ſeconds. It may therefore be inferred, that in the experiment with the largeſt aperture, the eſcape of air was ſo ſpeedy as conſiderably to diminiſh the preſſure by which it was driven out.

In order to aſcertain the condenſation of the air in the blow-pipe, a ſmall quantity of water was put into the bottle, and the blowing continued for a few ſeconds beneath the thumb, which was applied to the orifice of the neck. Air was ſuffered to eſcape at the ſame time through the lips, ſo as nearly to produce the ſame effect with regard to the muſcles of the mouth, as if the ſame had been emitted through a blow-pipe. When the action was at its utmoſt, the thumb was ſuddenly cloſed on the aperture, and the bottle inverted. In this ſituation the line of the upper ſurface of the water was carefully marked, after which the thumb was gently withdrawn, and a portion of the water flowed out in conſequence of the ſpring of the included air. By the depreſſion of the water, it was found that the air had been condenſed by about $\frac{1}{28}$th part of the whole, and conſequently would have ſuſtained a little more than an inch of mercury in a ſimple gage. It ſeemed probable, however, that the exertion of blowing into a bottle by a ſhort temporary effort, might be very different from the ſteady action of blowing through a pipe. To prove this more clearly, I bended a glaſs tube nearly of the ſame internal diameter as the blow-pipe into a ſyphon, the legs of which formed an angle of about 45 degrees of each other. Into this, mercurywas poured to occupy ſeveral inches in length, and the tube was fixed ſo that one of its legs continued vertical, while the other was acceſſible to the mouth. Upon blowing into the latter orifice, it was found that, by an eaſy or moderate action, the mercury was ſuſtained to the height of about $\frac{4}{10}$ths of an inch above its level; that when the preſſure was ſtrong, the height was about half an inch; and that it was poſſible, by very ſtrong exertion, to keep the mercury at one inch: but the lips ſoon became tired. The mercury might indeed be urged to near two inches, but not in a way that could have been maintained for even an extremely ſhort time in actual work.

From theſe facts, if we take half an inch for the medium ſtation of the gage, the preſſure

 for

for extruding the air will anſwer to about a quarter of a pound avoirdupois upon each ſquare inch of ſurface, which is not more than an eighth part of the preſſure in the regulating belly of the blowing machines at our great foundries. I think the quantity of one cubic inch per ſecond is quite as much and probably more than iſſues out of the blow-pipe in any courſe of experiment. A pair of bellows capable of extruding ſomewhat more than two cubic inches at a ſtroke, would conſequently ſupply as much air as the pipe would deliver, provided the ſtrokes ſucceeded each other about once in two ſeconds, which appears to be a convenient rate of working, and by no means too quick. In very ſmall bellows the internal contents may be eſtimated at one third of the contents of a parallelopipedon, or ſquare box capable of circumſcribing the bellows when open. As a full allowance, let us ſuppoſe the contents of this imaginary box to be eight cubic inches, and its depth two inches. Its upper ſurface muſt then be equal to four ſuperficial inches. That is to ſay, a pair of bellows fully ſufficient to ſupply the blow-pipe will not require larger dimenſions than three inches in length, one inch and a half in width, with a lift of one inch and a half for each ſtroke. The ſame effect may be produced by a ſyringe one inch in diameter, and two inches and a half long.

The reſervoir for the air may either be another pair of bellows rather larger, and diſpoſed to ſhut by a weight or a ſpring; or it may be ſimply a veſſel of ſufficient capacity to receive the air from the bellows, and emit it in a conſtant ſtream by virtue of its ſpring under condenſation, in the ſame manner as water is emitted from the air veſſel of a fire-engine. The emiſſion of air from ſuch a veſſel may, in a looſe way, be taken to be at half the velocity of its introduction; and, conſequently, at the end of every ſtroke the veſſel will contain half as much more air, than the medium ſtate of condenſation, as amounts to the whole contents of the bellows; and at the beginning of every ſtroke, the veſſel will contain half as much leſs than that quantity. From theſe conſiderations, it appears that the ſteady ſtream from a fire-engine could not be produced but by two pumps alternately acting, and that the air anſwers ſcarcely any other purpoſe in the ſmall air-veſſels of thoſe engines, than that of rendering the intruſion of the non-elaſtic water leſs ſudden and violent. It will alſo appear from calculation, (as it does from practice, in the regulating bellies of blowing engines, on which the re-action is afforded by a variable force ſomewhat reſembling the ſpring of the air) that a very large veſſel is required to give a practical uniformity to the emitted ſtream, when the action of intruſion is not conſtant. Hence it appears, that an air veſſel cannot be uſed to advantage, unleſs the bellows to our ſmall apparatus be made double by two fixed outer boards, with valves opening inwards, and a moveable diaphragm alternating between them, and forcing the air through valves opening into one common nozle—or unleſs the ſyringe, if uſed, be made to act both ways, namely, by the returning as well as by the direct ſtroke. A very neat and compact apparatus for the blow-pipe might be made in this way; but it is probable that the conſiderations and their advantages may be more particularly applicable to the large engines at the ſmelting-works.

XI. *A ſhort*

XI.

A short Account of the Life of PELLETIER. *Read at the Public Sitting of the National Institute of France, the 15th Vendemiaire, in the Year VI. By* CITIZEN LASSUS, *Secretary to the Class of Natural Philosophy and Mathematics.*

IN the course of the last trimestre we have had the misfortune to lose one of our colleagues, Bertrand Pelletier, born at Bayonne in 1761. His life was confined to the short space of 36 years; but his actions have left an impression on the minds of men which time shall not efface.

It frequently happens that young men, sincerely desirous of instruction, have no means or place where they can be assisted in the development of their natural talents, no master who may point out the direct road to science, and that order and method without which the efforts of the individual too often lead him from the object of his pursuit, instead of bringing him nearer to it. This was not the case with young Pelletier. He found every advantage in his father's house, where he received the first elements of the art of which he was afterwards the ornament; and his subsequent progress was made under our colleague Darcet, who having remarked in him that sagacity which may be called the instinct of science, admitted him among the pupils attached to the chemical laboratory of the college of France. Five years of constant application and study under such a master, who was himself formed by nature, perfected by experience, and affectionately disposed towards his pupil, afforded this young man a stock of knowledge very unusual at his age. He soon gave a convincing proof of this, by publishing, at the age of 21, a set of very excellent observations on the arsenical acid. Macquer, by mixing nitre with the oxyde of arsenic, had discovered in the residue of this operation a salt soluble in water, susceptible of crystallization in tetrahedral prisms, which he denominated the neutral arsenical salt. It is the arseniate of potash. He was of opinion that no acid could decompose it; but Pelletier showed that the sulphuric acid distilled from it does disengage the acid of arsenic. He showed the true cause why the neutral arsenical salt is not decomposable in closed vessels, and particularly the order of affinity by which the salt itself is formed in the distillation of the nitrate of potash, and the white oxyde of arsenic. He explains in what respects this salt differs from what Macquer called the liver of arsenic. Pelletier had been anticipated in this work by Scheele, by Bergmann, by the academicians of Dijon, and by our colleague Berthollet; but he possessed at least the merit, in the first essay of his powers, of having clearly developed all the phenomena of this operation, by retaining and even determining the quantity of gas it was capable of affording. After the same principles it was that he decomposed the arsenico-ammoniacal salt, by showing how, in the decomposition of this last, the pure arsenical acid is obtained in the form of a deliquescent glass. In this work we may observe the sagacity with which he was enabled to develope all the phenomena of these compositions and decompositions, by tracing those delicate threads of scientific connection which connect the series of facts, and are imperceptible to ordinary minds.

Encouraged by the success of these first works, which he presented with the sensibility of grateful attachment to his instructor, he communicated his observations on the crystallization of sulphur, cinnabar, and the deliquescent salts; the examination of zeolites, particularly the false zeolite of Fribourg in Brisgaw, which he found to be merely an ore of zinc;

obfervations on the dephlogifticated or oxygenated muriatic acid, relative to the abforption of oxygene; on the formation of ethers, particularly the muriatic and the acetous; and feveral memoirs on the operation of phofphorus made in the large way, its converfion into phofphoric acid, and its combination with fulphur and moft metallic fubftances.

It was by his operations on that moft aftonifhing production of chemiftry, phofphorus, that he burned himfelf fo dangeroufly as nearly to have loft his life. After the cure of his wound, which confined him to his bed for fix months, he immediately began the analyfis of the various plumbagos of France, England, Germany, Spain, and America, and found means to give novelty and intereft to his work even after the publication of Scheele on the fame object. The analyfis of the carbonate of barytes led him to make experiments on animals, which prove that this earth is a true poifon, whether it be adminiftered in the form of the native carbonate of barytes, or whether it be taken from the decompofition of the fulphate, even though again combined with another acid.

Chemifts have given the name of Strontian to a newly-difcovered earth, from the name of the place where it was firft found. Pelletier analyfed it, and difcovered it in the fulphate of barytes. He likewife analyfed the verditer of England, of which painters and paper-hangers make fo much ufe. He difcovered a procefs for preparing it in the large way, by treating with lime the precipitate obtained from the decompofition of nitrate of copper by lime. By his procefs, verditer is afforded equal in beauty to that which comes from England. He was likewife one of the firft chemifts who fhowed the poffibility of refining bell metal, and feparating the tin. His firft experiments were made at Paris; after which he repaired to the foundry at Romilly to verify them in the large way. The following year he was received a member of the Academy of Sciences at Paris, and fhortly afterwards went to La Fere, with our colleague Borda and General Daboville, to affift in experiments upon a new gunpowder. Being obliged, in order to render his experiments more decifive, to pafs great part of the day in the open air during a cold and humid feafon, his health, which was naturally delicate, became confiderably impaired. He began to recover his health, when he again became the victim of his zeal for the fcience he fo fuccefsfully cultivated. He had nearly perifhed by refpiring the oxygenated muriatic acid gas. A violent attack of convulfive afthma, which returned during feveral days, was the firft confequence of this unhappy accident. The diforder then feemed to abate, but it was incurable. The affiftance of art was infufficient to fave him, and he died of a pulmonary confumption in the flower of his age.

Such was the man whofe premature lofs we now lament. His attachment for the fcience to which he had devoted himfelf, remained during the whole of his exiftence, and in the laft moments of his life it formed an interefting object of his converfation. He poffeffed that activity of mind fo neceffary to the refearch of truths which are inacceffible to men of cold and languid fentiments. As a man of fcience, his reputation is bright and unblemifhed. As a citizen, his private virtues, his probity and good conduct will long continue objects of regret.

XII. *Extracts*

XII.

Extracts from the Syſtême du Monde of M. La Place *.

THIS work of La Place explains the leading points of the ſyſtem of the world. It is as it were an abſtract of a large work, in which this profound geometer propoſes to treat the ſame objects by the principles of the moſt ſublime geometry, and which he promiſes ſoon to publiſh. I ſhall copy ſome of the reſults of this author.

"The degree meaſured at the Cape of Good Hope in 37° ſouth latitude †, is found to be 307999,8 feet, which is very nearly the ſame as the degree of France, under the parallel of 50°, and greater than that which was meaſured in Pennſylvania, at the latitude of 43° 56, of which the length is no more than 307195,2 feet. The degree of the Cape is alſo greater than the degree meaſured in Italy in the latitude of 47° 80, which was found to be 307680,6 feet. Neverthelefs it ought to be ſmaller than every one of theſe degrees, if the earth were a regular ſolid formed by the revolution of a meridian perfectly alike on each ſide of the equator. Every fact leads us to conclude that this is not the caſe." Vol. I. page 105.

He concludes, that the terreſtrial meridian is a line of double curvature.

"Terreſtrial bodies ſituated under the equator deſcribe, by virtue of the rotation in each ſecond of time, an arc of 40″ 1395 of the circumference of the terreſtrial equator. The radii of this equator being 19634778 feet very nearly, the verſed ſine of this arc is 0,0389704 feet. Gravitation cauſes bodies to fall at the equator through a ſpace of 11,23585 feet in one ſecond. The central force neceſſary to retain bodies at the ſurface of the earth, and conſequently the centrifugal force ariſing from its rotatory motion, is to gravity at the equator in the proportion of 1 to 288,3. The centrifugal force diminiſhes the weight, and bodies do not fall at the equator but by virtue of the difference of the true force. If we therefore uſe the word *gravity* to denote the total weight which would take place, excluſively of the diminution it undergoes, the centrifugal force at the equator is extremely near $\frac{1}{289}$th part of gravity. If the rotation of the earth were 17 times more rapid, the arc deſcribed in a ſecond at the equator would be 17 times greater, and its verſed ſine would be 289 times more conſiderable. The centrifugal force would then be equal to gravity, and bodies would ceaſe to preſs or weigh towards the earth at the equator." Page 263.

"To explain the double motion of rotation and progreſſion in the earth, it is ſufficient that the ſuppoſition be admitted of the primitive impulſe having been given at a ſmall diſtance from its centre of gravity; which diſtance, ſuppoſing the planet to be homogeneous, muſt have been nearly the $\frac{1}{160}$th part of its radius." Page 299.

"The probability is infinitely ſmall, that the original projection of the planets, ſatellites, and comets, ſhould have paſſed through their centres of gravity. All theſe bodies muſt therefore have a rotative motion. From a ſimilar reaſon, the ſun, which turns on its axis, muſt have received an impulſe, which not having paſſed through its centre of gravity, car-

* Expoſition du Syſtême du Monde, par Pierre Simon La Place, de l'Inſtitut National de France et du Bureau des Longitudes, 2 vol. in 8vo. A Paris de l'Imprimerie du Cercle Social, Rue du Théâtre François, No. 4.—I have not the work, but tranſlate from Dr. Lametherie, in the Journal de Phyſique, Auguſt 1794.

† The author divides the circle into 400 parts.

ries

ries it through ſpace with the planetary ſyſtem, unleſs this motion be ſuppoſed to have been deſtroyed by an impulſe in the oppoſite direction; a circumſtance by no means probable.

"The impulſe given to an homogeneous ſphere, in a direction which does not paſs through its centre, will cauſe it to revolve conſtantly round the diameter, which is perpendicular to a plane paſſing through its centre, and the line of direction of the impreſſed force. New forces acting on all its parts, and of which the reſult paſſes through its centre, will not change the parallelism of its axis of rotation. Thus it is that the axis of the earth remains always nearly parallel to itſelf in its revolution round the ſun, without its being neceſſary to ſuppoſe, with Copernicus, an annual motion of the poles of the earth round thoſe of the ecliptic.

"If the body poſſeſs a certain figure, its axis of rotation may change every inſtant. The determination of theſe changes, whatever may be the forces acting on the bodies, is one of the moſt intereſting problems of mechanics reſpecting hard bodies, on account of its connection with the preceſſion of the equinoxes, and the libration of the moon. The ſolution of this problem has led to a curious and very uſeful reſult; namely, that in all bodies there exiſt three axes perpendicular to each other, round which the body may turn uniformly when not ſolicited by external forces. On this account theſe axes have been called principal axes of rotation.

"A body or ſyſtem of bodies, poſſeſſing weight, and of any figure whatever, oſcillating round a fixed and horizontal axis, forms a compound pendulum. No other pendulum exiſts in nature. The ſimple pendulums ſo frequently treated of are pure geometrical conceptions, proper to ſimplify the objects of diſcuſſion. It is eaſy to refer to theſe ſuch compound pendulums as have their parts immoveably fixed together. If the length of the ſimple pendulum, whoſe oſcillations are iſochronous with thoſe of the compound pendulum, be multiplied by its total maſs, and by the diſtance of its centre of gravity from the axis of oſcillation, the product will be equal to the ſum of rhe products of each particle of the compound pendulum, multiplied by the ſquare of its diſtance from the axis. It is by means of this rule, diſcovered by Huyghens, that experiments with compound pendulums have been applied to ſhew the length of the ſimple pendulum, which beats ſeconds."

The author enters into a conſiderable detail reſpecting the atmoſpheres of the planets. "In all the changes to which the atmoſphere is ſubject (ſays he, vol. ii. p. 128.) the ſum of the products of the particles of the revolving body and its atmoſphere, multiplied reſpectively by the areas they deſcribe round the common centre of gravity, the radii being projected on the plane of the equator, remain the ſame in equal times. Suppoſing, therefore, that, by any cauſe whatever, the atmoſphere ſhould become contracted, or that part thereof ſhould become condenſed on the ſurface of the body, the rotatory motion of the body and its atmoſphere would he accelerated: for, the radii vectores of the areas deſcribed by the particles of the original atmoſphere becoming ſmaller, the ſum of the products of all the particles, by their correſponding areas, cannot remain the ſame unleſs the velocity be augmented.

"The atmoſphere is flattened towards the poles, and ſwelled out at the equator. But this oblateneſs has its limits; and in the caſe where it is greateſt, the ratio of the polar and equatorial diameter is as two to three.

"The

"The atmosphere cannot extend itself at the equator to a greater distance than to the place where the centrifugal force is exactly equal to the force of gravity. With regard to the sun, this point is remote from its centre to a distance measuring the radius of the orbit of a planet which would make its revolution in the same period as that luminary employs in its rotation. *The solar atmosphere cannot therefore extend to the orbit of Mercury*; and consequently it cannot produce the zodiacal light, which appears to extend even beyond the orbit of the earth.

"The point where the centrifugal force balances that of gravitation is nearer the body the more rapid its rotation. If we conceive the atmosphere to extend as far as this limit, and afterwards to contract and condense by cooling at the surface of the body, the motion of rotation will become more and more rapid, and the extreme limit will continually approach towards the centre. The atmosphere will therefore successively in the place of the equator abandon zones of fluid, which will continue to circulate round the body, because their centrifugal force is equal to their gravity. But as this equality does not obtain with regard to the parts of the atmosphere distant from the equator, they will not cease to appertain to the planet. It is probable that the rings of Saturn are similar zones abandoned by its atmosphere." (Vol. ii. p. 125.)

"As the motions of the planets and their satellites are performed nearly in the same plane, we must suppose one cause to have acted on all these bodies; and from the prodigious distances between them, it must have been a fluid of immense extent. To have given them in the same direction a motion, nearly circular, about the sun, it is necessary that the fluid must have surrounded that star as an atmosphere. The consideration of the planetary movements lead us therefore to think, that by virtue of an excessive heat the atmosphere of the sun was originally extended beyond the orbits of all the planets, and that it gradually contracted in process of time to its present limits. These effects may have taken place by causes similar to that which occasioned the strong light for several months in the famous star which in the year 1572 appeared all at once in the constellation of Cassiopeia.

"The great eccentricity of the orbits of the comets leads to the same result. It evidently indicates the disposition of a great number of less eccentric orbits; a circumstance which supposes an atmosphere round the sun extending beyond the perihelia of the observable comets, which, by destroying the motions of those which happened to pass within it during the time of its greatest extent, united them to the body of the sun. Hence it follows, that no other comets can at present be in existence, but such as were at that time beyond that interval. And as we cannot observe any comets but those which come near the sun in their perihelium, their orbits must be very eccentric. It also follows, that their inclinations must offer the same irregularities as if these bodies had been projected casually; because the solar atmosphere has not influenced their motions. The long time employed by the comets in their revolutions, the great eccentricity of their orbits, and the variety of their inclinations, are therefore very naturally explained by means of this atmosphere.

"But in what manner have the movements of revolution and rotation of the planets been effected? If these bodies had penetrated as such into the atmosphere of the sun, its resistance must have caused them to fall to its surface. We may therefore conjecture that they were formed at the successive limits of that atmosphere, by the condensation of the zones which it must have abandoned in the plane of its equator during its cooling and condensation at the

the ſurface of that ſtar. We may alſo conjecture, that the ſatellites have been formed in a like manner by the atmoſpheres of the planets. The five phenomena abovementioned naturally flow from theſe hypotheſes, to which the rings of Saturn afford additional probability." (Vol. ii. p. 301.)

The five phenomenaenumerated by the author are: (1) The motions of the planets in the ſame direction and nearly in the ſame plane. (2) The motions of the ſatellites in the ſame direction as the rotations of their planets. (3) The rotations of theſe different bodies and of the ſun in the ſame direction as their projectile motion, and in planes very little differing from each other; and laſtly, (4) The eccentricity of the cometary orbits.

This great eccentricity of the comets, and their motions in all directions, appear to the author a ſufficient reaſon to conclude, that their origin is different from that of the planets. He afterwards enquires into the probability that a comet may ſtrike the globe of the earth, and deſtroy its preſent regularity of appearance, &c.

Theſe are his words, (vol. ii. p, 60.) " The fears which the appearance of comets at that time inſpired, were ſucceeded by an apprehenſion of another nature; leſt, among the great number which traverſe the planetary ſyſtem in every direction, one of them ſhould deſtroy the earth. *They paſs ſo rapidly near us, that the effect of their attraction is not to be feared.* It is only by actually ſtriking the earth that they could produce the dreadful effect: but the ſhock, though poſſible, is ſo very improbable in the courſe of an age; it would require ſo extraordinary a chance for the concurrence of two bodies ſo ſmall with reſpect to the immenſity of the ſpace in which they move, that no reaſonable ground of fear can be maintained in this behalf. Nevertheleſs, the ſmall probability of ſuch an event, if it be conſidered with reſpect to a long ſeries of ages, may become very great. It is eaſy to imagine the effects of ſuch a ſhock upon the earth. The axis and rotatory motion being changed, the ſeas abandon their former poſition, and ruſh to the new equator; great part of the men and animals drowned in this univerſal deluge, or deſtroyed by the violent ſtroke impreſſed on the terreſtrial globe; entire ſpecies annihilated; all the monuments of human induſtry ſwept away:—ſuch are the diſaſters which might enſue from the ſhock of a comet. We ſee therefore why the ocean has formerly covered the high mountains, on which it has left indubitable marks of its preſence; how the plants and animals of the ſouth may have exiſted in the climates of the north, where their remains and impreſſions appear; and laſtly, by an event of this kind, we may explain the novelty of the moral world; the regular proceſſes of which can ſcarcely be traced beyond three thouſand years. The human ſpecies reduced to a very ſmall number of individuals, and to the moſt deplorable ſtate, entirely occupied for a long ſeries of time in the care of its own preſervation, muſt have totally loſt the remembrance of the ſciences and the arts; and when the progreſs of civilization gave efficacy to wants of leſs immediate preſſure, it became neceſſary to repeat again the various gradations of invention, as if men had then for the firſt time been placed on the earth. But however adequate the cauſe may be to theſe phenomena, for which it is aſſigned by ſome philoſophers, I repeat, that we may be perfectly at our eaſe with regard to ſo terrible an event during the ſhort interval of life individual."

XIII. *On*

XIII.

On the Preparation of concrete Acid of Lemons. By DIZÉ, *Apothecary in Chief to the French Army, charged with the Inspection of the General Magazine of Medicines* *.

THOUGH nature presents us with the citric acid nearly in a disengaged state, it is nevertheless confounded in lemon juice with an extractive mucilaginous matter, which opposes the union of its crystallizable particles, and which cannot be separated by the simple process of evaporation and exposure to crystallize.

Scheele was the first who obtained this acid in the solid form. In the year 1774, Georgius in Sweden, and afterwards Du Buisson in France, published observations on the method of concentrating and preserving lemon juice. Although their labours, as well as the experiment of Stahl and Guyton, could not determine the crystallization of this acid, it is nevertheless certain that their researches were very useful to assist Scheele in his discovery of the process he has left us.

I availed myself of an opportunity to repeat the process of Scheele in the large way, and to insist on an essential observation, which that chemist has not sufficiently developed; for it is one of the necessary conditions for success in the crystallization of this vegetable acid. Scheele, after several unfavourable attempts, advises the separation of the extractive and mucilaginous matter of lemon juice, by uniting the citric acid to the basis of calcareous carbonate, with which it forms a citrate of lime, that precipitates on account of its sparing solubility, while the extractive and mucous matter remains dissolved in the fluid. This citrate of lime is afterwards decomposed by a sufficient quantity of sulphuric acid diluted with water, with the precaution to add an excess of this last acid. The sulphuric acid seizes the lime from the citric acid, and forms a sulphate, which falls to the bottom because nearly insoluble; while the citric acid, being set at liberty, is dissolved in the water with which the sulphuric acid was diluted.

Filtration and washing with cold water separate the citric acid entirely from the sulphate of lime, and it may be afterwards obtained in the concrete state by evaporation in stone-ware vessels, at the temperature of boiling water. I have observed that it is very useful to suspend the evaporation every two days, in order to permit the sulphate of lime, which is suspended by the assistance of the citric acid, to fall down.

The citric acid which I have prepared is the product of several chests of lemons. The mass of calcareous citrate which was decomposed was somewhat considerable, and the operation was performed in large stone-ware vessels. The sulphate of lime obtained by the decomposition of the calcareous citrate was well washed in tubs of white wood. The different liquors were united together for evaporation in stone-ware vessels, at the temperature of boiling water. They were clear, light-yellow, and contained an excess of sulphuric acid.

As soon as the liquors were sufficiently concentrated by evaporation, the sulphuric acid exerted its action, and the yellow colour became brown, and even blackish at the end of

* Read to the National Institute of France, and copied in the Journal de Physique, published to supply the defect of the number for September 1794.

the evaporation. The mafs of cryftals which was taken out, after cooling and three days repofe, was confiderable and black.

I fuffered this faline mafs to drain in ofier bafkets, while the evaporation and cryftallization of the remaining fluid were performed. When this firft operation was ended, the whole of the faline mafs was re-diffolved in a fufficient quantity of cold water, and filtered through ftrainers of linen, covered with filtering paper.

The folution which paffed the filter was clear, but of a dirty brown, and the greateft quantity of the matter which had blackened the cryftals remained behind. This was again evaporated, left to cryftallize, and in this manner exhaufted of the citric acid. The cryftals were now yellow, and more regularly figured. By a third folution, filtration, and evaporation, the cryftals were again obtained white, regular, and of the greateft purity. The black matter depofited on the filter was fo trifling that it formed a very flight covering.

Scheele, who was content with having proved the poffibility of obtaining this vegetable acid in a concrete ftate, could not determine its natural form, becaufe he operated on fuch fmall quantities.

My refults afforded cryftals as large as are ufually obtained from the ordinary proceffes of faline folution, and as eafily to be defcribed. They prefent, on fimple infpection, rhomboidal prifms, the fides of which are inclined to each other in angles of about 120 and 60 degrees, terminated at each end by four trapezoidal faces which include the folid angles.

I have before remarked, that Scheele had obferved, as one of the conditions effential to the ready cryftallization of the citric acid, to add a fmall quantity of fulphuric acid in excefs beyond the exact quantity neceffary to decompofe the calcareous citrate. The remark of this chemift not being followed by any explanation, my trials in the large way have proved the great difcernment of the chemift of Gottingen; and that, if he had operated on a quantity of lemon juice equal to that which I purified, he would have afcertained the reafons why this excefs of acid was found to be neceffary in his more confined experiments.

Having afcertained that the black matter remaining on the filters in this procefs is charcoal, it follows that it could not have been afforded but at the expence of a confiderable portion of the mucilaginous matter of the lemon juice, which the citric acid had carried with it in its combination with the bafe of the calcareous carbonate; and that afterwards when the citrate is decompofed, the excefs of fulphuric acid is required to decompofe this mucilage, and precipitate the carbone, as foon as the fluid begins to be condenfed by evaporation. The ingenious experiments of Fourcroy and Vauquelin prove the nature of the action of fulphuric acid on vegetable matter, and ftrongly confirm my inductions *.

Lemon

* For thefe important experiments fee Philof. Journal I. 385.—In order to fhew that mucilage enters into the citrate of lime, and that it is decompofed by fulphuric acid, it feems neceffary to make the experiment with only a very minute excefs of the latter. Scheele thought the acid (Effays, p. 362.) in the citrate to be pure, and he required an excefs of fulphuric acid to be added to infure the faturation of the whole of the lime. I would propofe to the confideration of the learned author of this paper, to afcertain whether it be not a portion of the acid itfelf which is altered and made to depofit carbone. If fo, the lefs the furplus of fulphuric acid the better: but,

Lemon juice feparated from all the mucous matter which falls down by expofure to the contact of the air for a few hours, that is to fay, fuch as it ought to be for ufe in medicine or the arts, marks five degrees of denfity by the areometer for falts of Baumé (fpecific gravity 1.034). One hundred pounds of this juice require for faturation fix pounds four ounces of calcareous carbonate. The citrate of lime, after being well wafhed and dried, weighs twenty pounds.

One pound of pure cryftallized acid of lemons diffolved in a fufficient quantity of water, demands one pound of carbonate of lime for its faturation. In this experiment the weight of calcareous citrate has diminifhed the mafs rather more than one-fourth part, inftead of increafing it upwards of two-thirds, as in the foregoing combination. Whence it may be concluded, that one hundred pounds of lemon juice faithfully prepared, and of the ftrength of five degrees by Baumé's areometer for falts, contain fix pounds four ounces of pure concrete acid. This ferves to explain two phenomena; that is to fay, the increafe of 13lbs. 2oz. in the calcareous citrate from lemon juice, and the prefence of mucilaginous matter diffolved in that liquid, which enters into the combination of the calcareous citrate, and is afterwards decompofed by the excefs of fulphuric acid neceffary to be added when that citrate is decompofed.

One ounce of diftilled water diffolves an ounce and two drams of citric acid, and produces 13 degrees (R.) of cold by the folution. A like quantity of diftilled water diffolves twice its weight of this acid, when it is heated to 80 degrees, or the boiling temperature. One hundred parts of citric acid diffolved in a fufficient quantity of diftilled water, boiling hot, diffolve 50 parts of calcareous citrate.

A lemonade of the moft agreeable tafte and appearance may be had by diffolving 40 grains of citric acid in a pint of water, with the addition of a fufficient quantity of pure fugar. It may be rendered fragrant by diffolving a fmall quantity of oleo-faccharum, prepared by rubbing a lemon on a lump of fugar. The fugar imbibes the volatile oil of the lemon, and renders it foluble. It is eafy by this means to preferve the whole of the volatile oil of a number of lemons. The oleo-faccharum thus obtained may be mixed in a mortar with a fufficient quantity of fugar. The mixture is then to be dried by a gentle heat, and preferved in well clofed glafs veffels. This method of procuring the flavour of lemons at all times is preferable to employing the volatile oil obtained by diftillation. The action of fire communicates to this laft an acrid flavour, eafily diftinguifhed by a delicate tafte.

I fhall finifh my obfervations by enumerating fome of the characters of this acid, when mixed with different earthy and metallic folutions.

Solutions of the acetites of magnefia, lime, alumine, of the muriates of barytes, lime, alumine, and magnefia, and of the nitrates and fulphates of thefe fame fubftances, do not undergo any change by the prefence of the citric acid.

The muriates and nitrates of zinc, the fulphate, muriate, nitrate, and acetite of copper,

but, on the other hand, if his inference be correct, the furplus muft be a definite quantity, namely, fufficient to deftroy the mucilage. The difference of weight in the citrates formed by the crude and the purified acids with equal dofes of lime, as mentioned in a fubfequent paragraph, may arife either from the prefence of mucilage in the former acid, or a change of affinity for lime produced by the re-action of the excefs of fulphuric acid on the latter: but experiment muft determine which. N.

and

and the nitrate of lead, are not decomposed, but the acetite of lead is immediately decomposed and precipitated in a white powder. The nitrate and acetite of mercury are also decomposed, and the mercurial citrate which falls down is a flaky salt, of a brick-dust colour, more or less red. The citric acid gives a green tinge to the solution of the acetite and sulphate of iron.

The quantity of citric acid which I obtained being considerable, I intend hereafter to give an account of the phenomena it exhibits in its different combinations.

XIV.

Useful Notices respecting various Objects.—Governor for regulating the Motions of Steam-Engines.—Amelioration of Oil.

1. *Governor for regulating the Motions of Steam-Engines.*

THE apparatus mentioned page 424 of the present work by the name of a Governor, is there supposed to be the invention of Mr. Watt. My supposition was grounded upon no other fact than that of having seen it in his engines. It was invented by —— Bunce, Esq. of the Admiralty, who applied it to a crane several years ago, the construction of which was communicated to the Society of Arts, soon after which period it was adopted in steam-engines. I cannot now refer to the volume, as I do not possess the set. But I had the information from the inventor himself.

2. *Amelioration of Oil.*

EVERY one who has occasion to use lamps must be sensible that the colour of the light, as well as the quantity and kind of disagreeable vapour emitted from the flame, depends greatly on the quality of the oil. When oil is kept in an open vessel, it gradually becomes more and more oxygenated, and at the same time less fluid. For both these reasons it is less fit for use. It is less combustible, and less adapted to pass between the fibres of the wick. These observations point out the expediency of keeping oil in well-closed vessels. The fluidity of whale-oil, and the facility of its combustion, may be considerably augmented by an addition of cold-drawn linseed oil.

It is well known that oil may be rendered purer by agitation with water, more particularly with the addition of an acid. The effect of this process is stated to be, that it carries off a portion of mucilage, which is not adapted to answer the purposes to which oil is applied. It may easily be imagined, however, that oil thus treated will retain a portion of aqueous or saline matter, which may render it unfit to be applied to the moving parts of instruments for the purpose of diminishing friction. Some clock and watch makers expose olive-oil to the atmosphere in frosty weather; and select that portion which they find to continue fluid after a considerable part is frozen. This proceeding is grounded on the supposition, that the oil may consist of two different fluids, one of which is supposed to congeal in a less heat than the other; and that this congelation is the principal evil which happens to the oil in time-pieces. It does not seem probable that either of these suppositions are well founded.

For

For the whole of the oil will freeze, if time be allowed; and the thickening of this fluid appears to be produced by chemical change, and not by mere cooling. Mathematical inſtrument-makers, directed, as I imagine, by experience only, find that oil is greatly improved by expoſure to light, which it is aſſerted cauſes it to depoſit mucilage. A very exquiſite regulator having the dead-beat ſcapement of Graham, which requires oil on the pallets, was found to go much more ſteadily when this oil was uſed, inſtead of the oil commonly applied to ſuch inſtruments.

Moſt of the facts here ſtated reſpecting oil were communicated to me by an intelligent cultivator of the ſciences, whoſe name I forbear to add becauſe I neglected to aſk permiſſion for that purpoſe.

NEW PUBLICATION.

Reports of the late Mr. John Smeaton, F. R. S. made on various Occaſions in the Courſe of his Employment as an Engineer. Printed for a Select Committee of Civil Engineers. Sold by Faden in London. Quarto. 412 pages, rather cloſely printed, with a Portrait of the Author, and 2 plates.

THAT Smeaton was a man of ſtrong natural powers and great induſtry, that his experience and obſervations were extenſive, and his ſucceſs highly to the credit of himſelf and his country, are too well known to be inſiſted upon. This firſt volume of his Reports is publiſhed at the expence of Sir J. Banks, Capt. J. Huddart, Wm. Jeſſop, Robert Milne, and John Rennie, Eſqrs. conditionally that the profits ſhould be given to Mr. Smeaton's repreſentatives. If this liberal effort, by which the nation is benefited by ſo valuable a maſs of practical information, ſhould be attended with ſucceſs, another volume will appear, containing the remainder of the Profeſſional Reports of this great engineer. I ſhall take the earlieſt opportunity of communicating the general principles uſed and adopted by Smeaton, in a paper I intend to draw up reſpecting the relative value and effect of firſt movers, and other elementary objects of daily uſe to engineers; and in the mean time ſhall avail myſelf of the Preface, nearly verbatim, to theſe Reports, to give ſome account of the Society of Civil Engineers.

The origin of the Society of Civil Engineers took its riſe from the following circumſtances:—Before or about the year 1760, a new æra in all the arts and ſciences, learned and polite, commenced in this country. Every thing which contributes to the comfort, the beauty, and the proſperity of a country, moved forward in improvement ſo rapidly and ſo obviouſly as to mark that period with particular diſtinction.

The learned Societies extended their views, their labours, and their objects of reſearch. The profeſſors of the polite arts aſſociated together for the firſt time; and they now enjoy a protection favourable to improvement, and not leſs honourable to real merit than to the Public and the Throne, which have with one accord promoted their proſperity.

Nor have theſe exertions failed of producing the adequate effects, comparing the preſent with the paſt ſtate of things.

Military and naval eſtabliſhments were made or enlarged, to promote and extend the true knowledge on which theſe ſciences depend.

The navy of England fails now uncontrouled in every part of the habitable world, and her fhips of war defy the combined power of all other maritime nations.

It was about the fame period that manufactures were extended on a new plan, by the enterprife, the capital, and, above all, by the fcience of men of deep knowledge and perfevering induftry engaged in them.

It was perceived that it would be better for eftablifhments to fet down on new fituations, beft fuited for raw materials and the labour of patient and retired induftry, than to be plagued with the miferable little politics of corporate towns, and the wages of their extravagant workmen.

This produced a new demand, not thought of, till then, in this country—internal navigation. To make communications from factory to factory, and from warehoufes to harbours, as well as to carry raw materials to and from fuch eftablifhments, became abfolutely neceffary. Hence arofe thofe wonderful works, not of pompous and ufelefs magnificence, but of real utility, which are at this time carrying on to a degree of extent and magnitude to which as yet there is no appearance of limitation.

The ancient harbours of this ifland, it may be faid, have ever been neglected, confidering the increafe of its naval power, and a foreign commerce of which there has never been an example in the hiftory of mankind. The fea-ports were (I had almoft faid are) fuch as Nature formed, and Providence has beftowed upon us; and they were but little better previous to that period, notwithftanding fome jettées and piers of defence ill-placed had been made and repeatedly altered, without knowledge and judgment, at *municipal*, not *government's* expence.

This general fituation of things gave rife to a new profeffion and order of men, called Civil Engineers.

In all the polifhed nations of Europe, this was and is a profeffion of itfelf. Academies, or fome parts of fuch inftitutions, were appropriated to the ftudy of it, and of all the preparatory fciences and accomplifhments neceffary to form an able artift, whofe profeffion comprehends the variety of objects on which he is employed, and of which the prefent work is an example, and a proof.

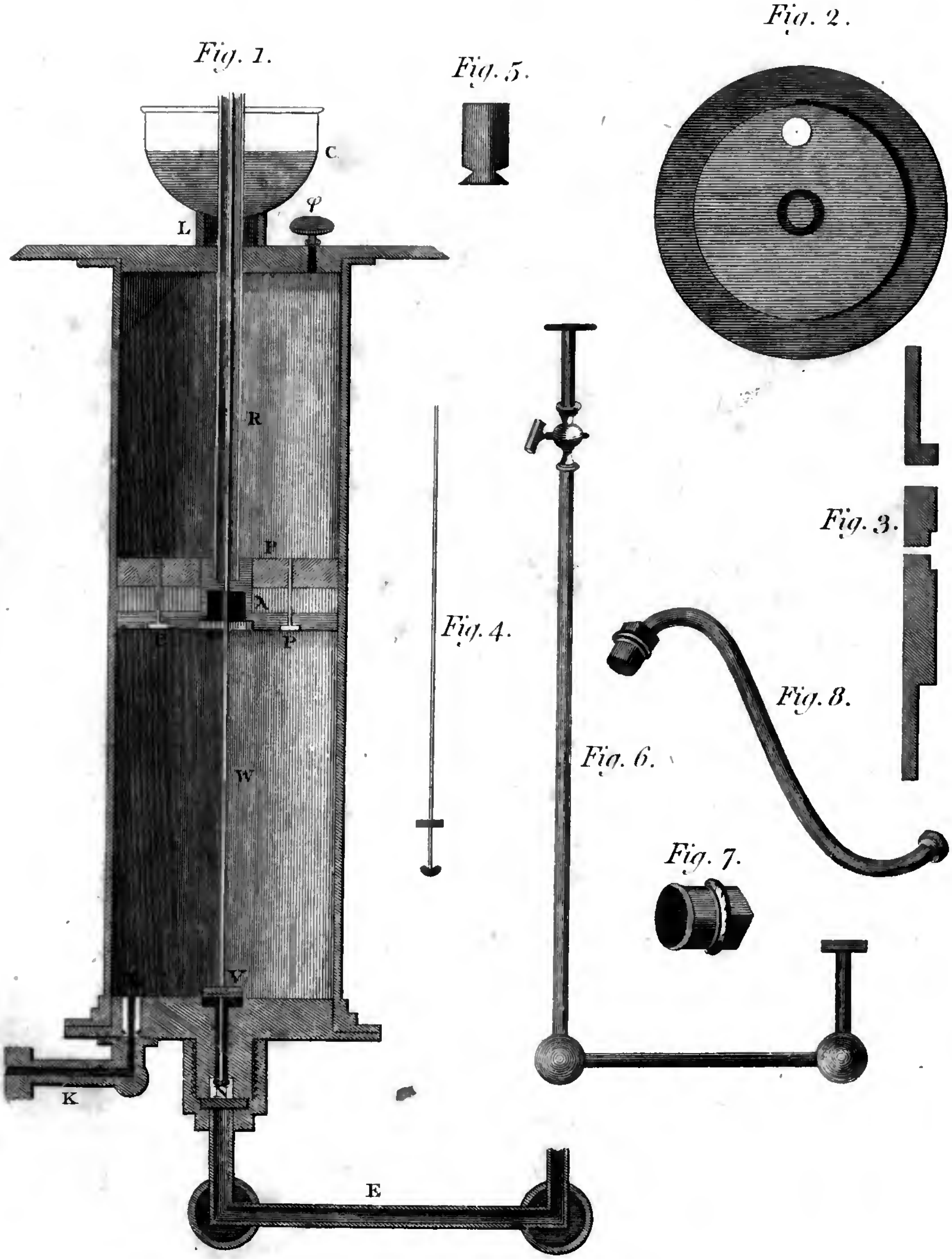
In this country, however, the formation of fuch artifts has been left to chance; and perfons leaned towards the public call of employments in this way, as their natural turn of mind took a bias. There were no public eftablifhments, except common fchools for the rudimental knowledge neceffary to all arts, naval, military, mechanical, and others.

Civil Engineers are a felf-created fet of men, whofe profeffion owes its origin not to power or influence, but to the beft of all protection, the encouragement of a great and powerful nation; a nation become fo from the induftry and fteadinefs of its manufacturing workmen, and their fuperior knowledge in practical chemiftry, mechanics, natural philofophy, and other ufeful accomplifhments.

When any one who has read the varied particulars of this publication, fhuts and lays it down for contemplation, he will reflect on the natural talents and fagacity requifite in that mind which applies to fuch a profeffion; on the patient application neceffary to acquire all the fubfervient learning previous to the commencement of it; and on the wonderful and varied powers which this work exhibits.

[*To be concluded in our next.*]

Air pump by Sir G. S. Mackenzie Bart.



Barlow sculp.

Apparatus for Experiments with Oxygen.

Philos. Journal Vol. II. Pl. I. facing p. 48.

Fig. 1.

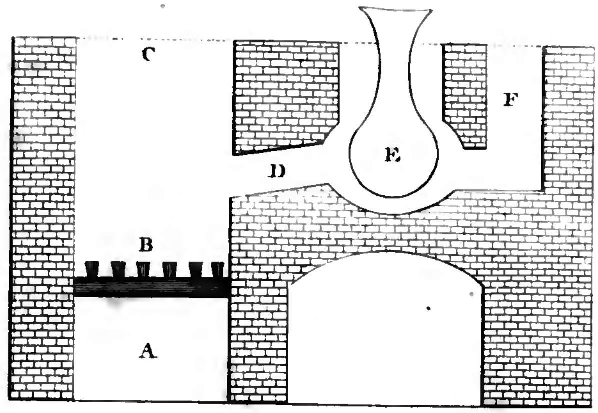


Fig. 2.

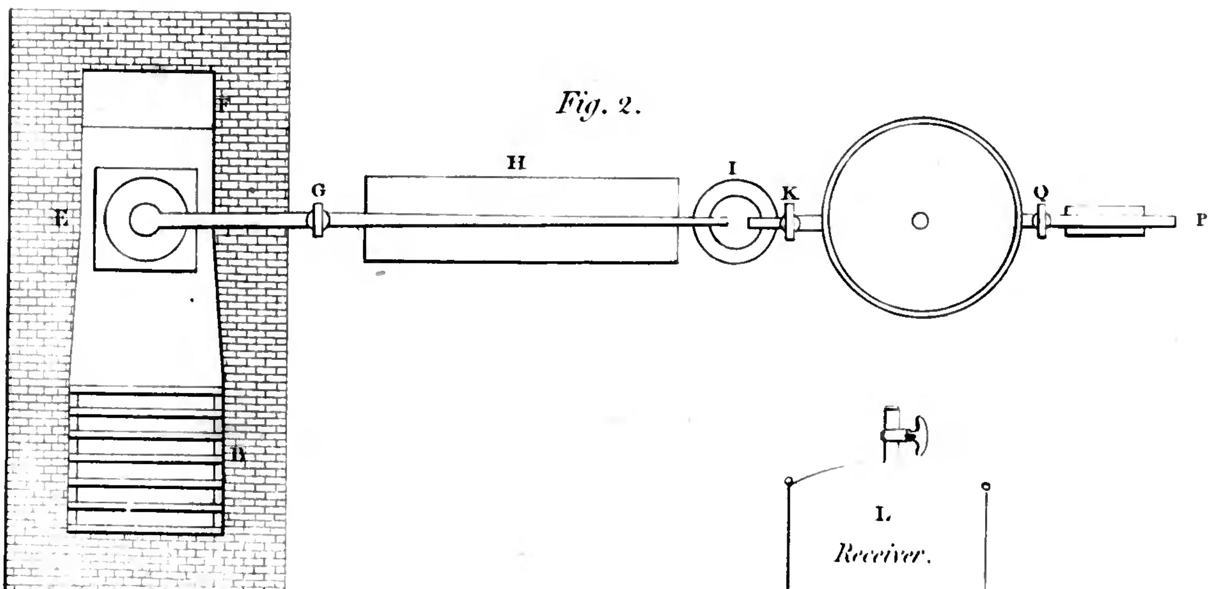
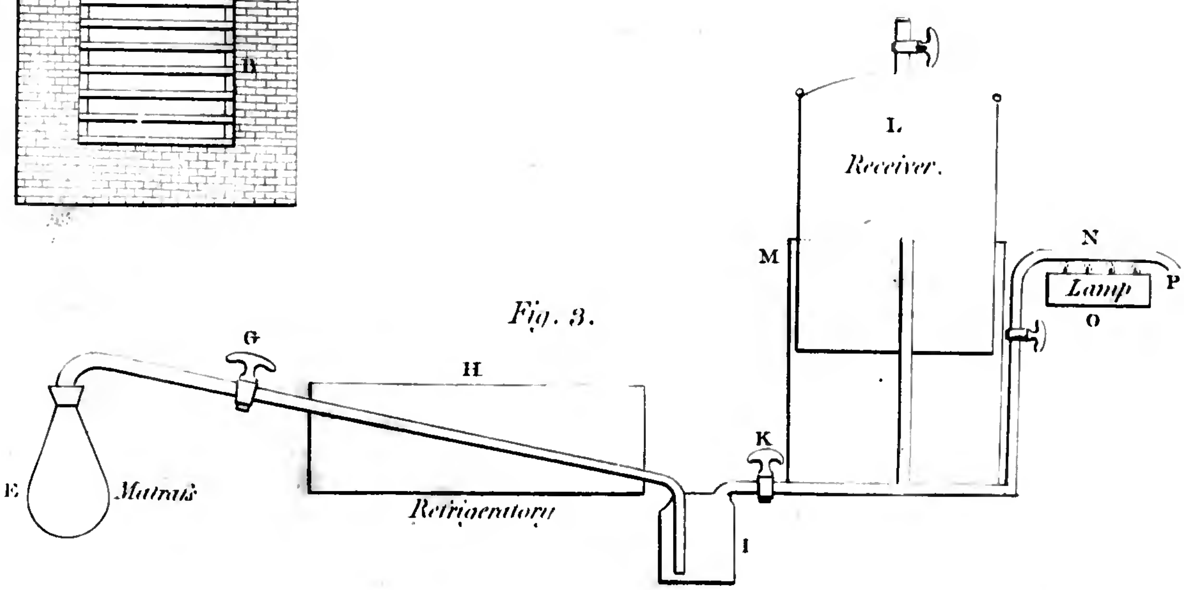


Fig. 3.



Barlow sculp.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MAY 1798.

ARTICLE I.

On the maintaining Power in Clocks and Watches.

[Concluded from Page 429, Vol. I.]

IF we suppose the pendulum of a clock to preserve its length invariable * during all the changes of temperature to which it is exposed, its vibrations will measure equal portions of time so long as the arc of vibration continues the same. The resistance of the air and of friction are prevented, by the maintaining power, from destroying the vibration. This power may be applied in different parts of the arc; and may, according to circumstances, render the time of vibration either shorter or longer, or it may simply maintain it without altering the time. Whether it alter the whole time or not will be of no consequence to the performance of the regulated machine, provided the effect of the maintaining power is itself subject to no variation. For the thing required is, that all the vibrations, when once brought to an accurate adjustment, should continue invariable. It is therefore essential to a good time-piece, either, 1. that the maintaining power itself shall not vary; or, 2. that it should be applied to the pendulum or balance in such a manner, as that in all variations of the power itself the time of vibration may continue the same; or, 3. that it shall act during a very small part only of the time of vibration, in order that the effect of its variations may be less.

* Philosophical Journal, I. page 58.

When the train of wheels in a clock is driven by a weight, the more immediate cauſe of variation conſiſts in the periodical irregularities of the wheels, the pinions, and the pivots, which tranſmit the force diminiſhed by friction and oblique actions, which are by no means uniform: the remoter cauſe of variation conſiſts in the oil becoming leſs fluid the longer it is expoſed to the air. But when a train is driven by a main ſpring, the cauſes of irregularity are greatly increaſed by the diminution of force in the ſpring as it runs down, and its friction in the box; neither of which are ſo regular as to admit of a good correction from the ſpiral of the fuſee round which the chain, that acts upon the movement, is drawn. Perfect equality of the maintaining power is not therefore to be expected. The only good expedient, independent of the eſcapement, appears to be that of cauſing the tr in to go by a weight or ſpring, ſerving only to wind up the laſt wheel once during every revolution, this wheel being urged by an appropriate weight or ſpring. By ſuch an expedient the clock in effect becomes a time-piece with one wheel only, in which the irregularities are minute, and their recurrence too frequent to produce any conſiderable effect on the vibrating part.

When the irregularity of the firſt mover is propoſed to be remedied by its mode of application, it is clear that it muſt give an increaſe of velocity to ſuch vibrations as are performed through longer arcs, in order that the times may continue the ſame. Attempts have been made to accompliſh this object, by giving a proper figure to the face of the pallets of the eſcapement; but it does not appear that ſuch attempts have been ſucceſsful.

The third method of equalizing the effects of the maintaining power with regard to time is founded on the conſideration, that, if this impulſe could be given in an inſtant, the whole of the vibration would be free, and reſemble that of a diſengaged pendulum or balance; and if the forces, by which the vibrating body returns to its point of quieſcence, were as the ſpaces to be moved through, all the vibrations would be performed in equal times, whether long or ſhort, or whether the maintaining power were greater or leſs. This diſpoſition is peculiarly adapted to the balance. For it is poſſible to form the pendulum ſpring and adapt the maintaining power to ſo ſmall a part of the vibration, that the times ſhall not perceptibly vary, even when the power itſelf is greatly altered.

Under the latter points of view the eſcapement becomes an object of great importance, ſince on this the manner of application of the maintaining power muſt depend. It ſcarcely ſeems neceſſary to ſubdivide or arrange the principles on which the moſt eminent artiſts have endeavoured to give perfection to their eſcapements. The principles will perhaps beſt ſhew themſelves in the form of remarks upon the ſeveral conſtructions.

One of the moſt ancient eſcapements is that which is at preſent applied in common pocket watches. It is repreſented Fig. 1. Pl. III. and is beſt ſuited to the long vibrations of the balance, which was invented earlier than the pendulum *. A B denotes the rim of a con-

* Doctor Derham on Clockwork, p. 86. fourth edition, affirms, that vibrations are no where mentioned or alluded to in the clocks deſcribed by the ancients: yet we learn from Vitruvius that they poſſeſſed time-pieces with wheel-work. Falconet, *Academie des Inſcriptions*, vol. xx. thinks that all the mechanical time-pieces made before the 16th century were clepſydræ with wheel-work for reckoning the ſmaller portions of deſcent.

a contrate wheel called the crown wheel, having its teeth pointed and sloped on one side only, so that the points advance before any other part of the teeth during the motion. C D are two pallets, or flaps, proceeding downwards from the verge E F. The pallets are nearly at right angles to each other; and when the balance F G fixed to the verge is at rest, the pallets remain inclined to the plane of the wheel in an angle of about 45 degrees; but when it is made to vibrate, one of the pallets is brought nearer to the perpendicular position, while the other becomes more nearly parallel. The wheel must be supposed to have one of its teeth resting against a pallet by virtue of the maintaining power. This tooth will slip off or escape as the pallet rises towards the horizontal position, at which instant a tooth on the opposite side of the wheel will strike against the other pallet which is down. The returning vibration, by raising this last pallet, will suffer that tooth to escape, and another tooth will apply itself to the first-mentioned pallet. By this alternation the crown wheel will advance the quantity of half a tooth each vibration, and the balance or pendulum will be prevented from coming to rest, because the impulse of the teeth against the pallets will be equal to the resistances from friction and the re-action of the air.

On this escapement it may be remarked, that the pendulum or balance is constantly connected with, and influenced by, the maintaining power, except during the exceedingly small time of the drop of the wheel from one pallet to the other; on which account the measure of time will greatly vary, when the force of vibration is merely equal, or not much greater than the maintaining impulse. This is shewn in a striking manner by urging the movement of a common watch by means of the key. If the key be pressed in the usual direction of winding up, the beats of the vibration will become very slow, or even stop; and if the pressure be made in the opposite direction, the vibrations will become very loud and quick.

descent. Professor Venturi, in his *Essai sur les Ouvrages Physico-Mathématiques de Léonard de Vinci*, page 28, has a curious note on this subject, from which I here make some extracts. The common escapement described in the text was well known to de Vinci, who describes an instrument acting by an escapement of this kind, similar, as he says, to the verge of the balance in watches, which he does not seem to mention as a new thing. He died about 1513. The isochronism of the pendulum was known to Galileo in 1600, who before his death, namely about 1633, proposed to apply it to clocks. The actual application by Huyghens was made before 1658, when he published his *Horologium oscillatorium*. He applied it by means of the common escapement already in use with the balance, and still retained in our table clocks. Sanctorius had made the same application near forty years before that time, as appears by his *Commentarii in Avicennam, (quest. 56)* printed in 1625, in which several instruments are described as having been publicly exhibited and explained to his auditors at his lectures in Padua for thirteen years previous to that time.

There is a manuscript (No. 7295) in the National Library of France, written about the middle of the 15th century by H. Arnault, Physician to the Dukes of Burgundy, who died in 1465. This author describes a planisphere which Jean de Fondeur *(Fusoris)* his master had constructed for the Duke, and which Arnault himself had afterwards repaired. Whence it appears that the instrument was constructed about the commencement of the 15th century. At folio 59 a design is seen of the watch which gives motion to the planisphere. It is nearly the modern watch. The balance is called *circulus affixus virge paletorum, qui cum ea de vi movetur*. He calls the crown wheel, which forms the common escapement on the pallets of the verge of the balance, by the name of *folietus*. At folio 60 the striking part is described, where we have the terms *voletus*, the *brochie levantes malleum*; *crux media inter movimentum et sonneriam*; *cavalc* and *chivola* for *cheville*. M. Venturi thinks that the watches of Wallingfort and Dondi, in the 14th century, were constructed on the principle of the balance. Robert Hooke added the spring to the balance in the year 1658.

This

This escapement not being adapted to such vibrations as are performed through arcs of a few degrees only, another construction has been made which has been in constant use for about a century in clocks, with a long pendulum beating seconds. Fig. 2. A B represents a vertical wheel called the swing wheel, having thirty teeth. C D represents a pair of pallets connected together, and moveable in conjunction with the pendulum on the centre or axis F. One tooth of the wheel in the present position rests on the inclined surface of the inner part of the pallet C, upon which its disposition to slide tends to throw the point of the pallet farther from the centre of the wheel, and consequently assists the vibration in that direction. While the pallet C moves outwards and the wheel advances, the point of the pallet D of course approaches towards the centre in the opening between the two nearest teeth; and when the acting tooth of the wheel slips off, or escapes from the pallet C, another tooth on the opposite side immediately falls on the exterior inclined face of D, and by a similar operation tends to push that pallet from the centre. The returning vibration is thus assisted by the wheel, while the pallet C moves towards the centre, and receives the succeeding tooth of the wheel after the escape from the point of D. In this manner the alternation may be conceived to go on without limit.

The same observation which was made upon the escapement fig. 1. is applicable also to this; namely, that the vibrating part is constantly under the influence of the maintaining power, except during the interval of the drop, or actual escape of the wheel from one pallet to the other. The indirect action of an inclined plane, with the influence of oil which it necessarily requires, may also be mentioned to shew that this escapement is not equal to what might have been produced by a skilful variation of the other. One principal recommendation of this escapement seems to have been the facility with which it affords an index for seconds in the face of the clock. Though the pendulum, according to this construction, is constantly connected with the maintaining power in a clock, yet the variations of that power have not the same mischievous effect as in a watch, because the momentum of the pendulum, compared with the impulse of the maintaining power, is prodigiously greater in the former of these instruments. A very considerable change in the maintaining power of a clock with a long pendulum, will not cause a greater variation than a few seconds in the daily rate.

The celebrated George Graham improved this escapement very much by taking off part of the slope farthest from the points of the pallets; instead of which part he formed a circular or cylindrical face, having its axis in the centre of motion. Pallets of this kind are seen on the opposite side of the wheel at E and G, having H for their centre or axis. A tooth of the wheel is seen resting upon the circular inner surface of the pallet G, which is not therefore affected by the wheel, excepting so far as its motion arising from any other cause may be affected by the friction of the tooth. If the vibration of the pendulum be supposed to carry G outwards, the slope surface will be brought to the point of the tooth, which will slide along it and urge the pallet outwards during this sliding action. When the tooth has fallen from the point of this pallet, an opposite tooth will be received on the circular surface of E, and will not affect the vibration, excepting when the slope surface of E is carried out so as to suffer the tooth to slide along it. In the two former escapements there is always a certain portion of vibration takes place after the drop which drives the pallets back, and causes the index also to recede through a small arc. This has been distinguished diftinguifhed by the name of a recoil. Other confiderable objections, befides that of the continued action of the maintaining power, have been made againft efcapements with a recoil; but it would lead us too far into the minute departments of this fubject to difcufs them. The efcapement of Graham, and all fuch as have no recoil, have been called dead beat efcapements, becaufe the index for feconds falls directly through its arc, and remains motionlefs on the line of divifion till the next vibration. It may be obferved, that the maintaining power in Graham's efcapement may be applied during a fmall portion only of the vibration; and that an increafe of the maintaining power tends to enlarge the arc of vibration, but fcarcely interferes with its velocity.

The effect of the efcapement which has been called horizontal *, becaufe the laft wheel in watches of this conftruction has its plane parallel to the reft of the fyftem, is fimilar to that of the dead beat efcapement of Graham. In fig. 3, the horizontal wheel is feen with twelve teeth, upon each of which is fixed a fmall wedge fupported above the plane of the wheel, as may be feen at the letters A and B. On the verge of the balance there is fixed part of a hollow cylinder of fteel or other hard material, the imaginary axis of which paffes through the pivots of the verge. C reprefents this cylindrical piece, into which the wedge D may be fuppofed to have fallen. While the vibration caufes the cylindrical piece to revolve in the direction which carries its anterior edge towards the axis of the wheel, the point of the wedge will merely rub the internal furface, and no otherwife affect the vibration of the balance than by retarding its motion. But when the return of the vibration clears the cylinder of the point of the wedge D, the wheel will advance, and the flope furface of the wedge acting againft the edge of the cylinder will affift the vibration of the balance. When the edge of the cylinder arrives at the outer point of the wedge D, its pofterior edge muft arrive at the pofition denoted by the dotted lines of continuation; immediately after which the wedge or tooth E will arrive at the pofition e, and reft on the outer furface of the cylinder, where it will produce no other effect than that of retardation from friction, as was remarked with regard to the wedge D, until the courfe of the vibration fhall bring the pofterior edge of the cylinder clear of the point of the wedge. In this laft fituation, the wedge will act on the edge of the cylinder, and affift the vibration, as in the former cafe, until that edge fhall arrive at the outer or pofterior point of the wedge; immediately after which the leading point will fall on the inner furface of the cylinder in the firft pofition, as was fhewn in the wedge D.

* In the feventh volume of *Machines et Inventions approuvées par l'Academie Royale des Sciences*, I find, p. 137, that the Sieur *Baufré*, a French clock-maker, fettled in London, contrived the horizontal fcapement in 1704. He applied an hollow portion of a cylinder to the verge of the balance, the edges of which were thick enough to afford the inclined furfaces of Graham's pallets, and thefe were acted upon by two common wheels perfectly fimilar and fixed on the fame axis, fo that the teeth of one correfponded with the hollows of the other. In the fame work, p. 141, the Regle artificielle du temps of Sully, p. 248—251, is quoted to fhew that Sir Ifaac Newton poffeffed one of thefe watches, marked with the name of Baufré. Peter le Roy improved this fcapement by fubftituting a portion of a cone inftead of the cylinder (Machines et Inventions, 1742), with a double wheel, the axis of which was at right angles to the verge. And M. Gourdain, in the fame year (p. 141 of the work laft quoted), made another conftruction, in which a flat circular piece of metal, out of which a portion of about 90° was cut, was fubftituted in the place of the cylinder, and received the action of a wheel with wedge-teeth, acting alternately in oppofite directions in the notch, and refting on the flat furface of the metal during the time of repofe. The fcapement defcribed in the text appears to have been a combination of the inventions of Baufré and Gourdain.

Horizonta

Horizontal watches were greatly esteemed during the last thirty years, until lately, when they gave place to those constructions which are known by the name of detached or free escapements. In the common escapement, fig. 1, an increase of the maintaining power increases the recoil, and accelerates the vibrations; but with the horizontal escapement there is no recoil; and an increase of the maintaining power, though it may enlarge the arc of vibration, will not necessarily diminish or alter the time. It is accordingly found, that the experiment of altering the maintaining power by the application of the key, does not alter the rate in the same perceptible manner as in common watches. Exceedingly perfect time-pieces on the horizontal principle, with a compensation for temperature applied to the spring, were made about fifteen years ago by Emery, of Charing-cross, and other artists; but I am informed that their performance became sensibly less accurate after a few years wear. Whether the difficulty of execution, or the badness of workmanship in low-priced watches of this kind, or whether the subsequent great improvements in this useful engine, have caused the horizontal principle to be laid aside, I know not; but I believe few if any watches of this construction are at present made.

The dead beat escapement of Graham, and the horizontal escapement, both appear to be improvements of the common anchor escapement, fig. 1. which by these expedients is deprived of its recoil, but still retains the wedge action, which has much friction, and necessarily requires oil. The quantity of direct push in the escapement fig. 1, which requires no oil on the face of the pallets, appears to be the cause why it acts so freely, and is so little liable to injury or disorder. I wish it were less difficult to ascertain the inventors and improvers of useful contrivances. But in proportion to the utility and universal adoption of any instrument, it always becomes more easy to mention it by a single term. Definitions, descriptions, and inventors' names naturally become discarded; and from this cause it seems as if a moderate or a speculative invention might carry the name of its inventor to remote ages, while the great benefactors of society are forgotten. I do not know who first undertook to improve escapements by pallets resembling those of fig. 1. in the direct action they receive; but the earliest contrivance of this kind I have met with is that of M. Le Roy fils aîné, of which the account bears date Sept. 6, 1748. As it is in effect the same escapement as is now in the highest esteem in the time-pieces of our best artists, I shall here describe the latter, and mention the particulars of that of M. Le Roy in a note.

Fig. 3. Pl. III. represents the free escapement of our best portable time-pieces. Fig. 4. exhibits the escapement on a larger scale. On the verge of the balance is fixed a circular piece of sapphire, or hard steel, EL, out of which a sectoral piece is cut. HG is a straight spring fixed near its extremity H, and having at the other extremity a pin G, against which one of the teeth of the wheel D rests when the train is at rest. This spring has a slight tendency towards the centre of the wheel, but is prevented by the stop K from throwing the pin farther inwards than just to receive the point of the tooth. I is a very slender spring fixed at the end I, and pressing very slightly against the pin G, in a direction tending to throw it from the wheel D, but which on account of the greater power of HG it cannot effect. It may be observed that the spring I proceeds a little beyond the pin G.—F is a lever proceeding from the verge of the balance directly opposite the end of the spring I, and long enough to strike it in its vibration. The action is as follows:—From

 the

the preſſure of the main ſpring the wheel (fig. 4) is urged from D towards F, but is prevented from moving by the pin G. Let the balance be made to vibrate, and the lever F will move through the arc Ff, ſtrike the inner extremity of the ſpring I, and diſplace the pin G. At this inſtant the face E, which may be called the pallet, will have arrived at the poſition e, againſt which the tooth of the wheel will fall, and communicate its impulſe through about 15° or 16° of the vibration. But F quits the ſpring I ſooner than the wheel quits the pallet E, and conſequently the pin G will have returned to its firſt ſtation before the wheel can have advanced a whole tooth, and the ſpring or detent HG will receive the wheel as before, immediately after its eſcape from the pallet. The returning vibration of the balance will be made with the piece EL perfectly at liberty between two teeth of the wheel, as in the ſketch, and the back ſtroke of the lever F againſt the tender ſpring I, will have no effect whatever on the pin G; this ſpring being like the back ſpring of the jacks of the harpſichord, active in one direction only. The third vibration of the balance will unlock the detent as before; the impulſe will again be given, and the whole proceſs will be repeated; and in this manner, the balance, though it may vibrate through the greateſt part of the entire circle, will be entirely free of the works, except during the very ſmall time of the drop of the wheel*.

It is hardly neceſſary to make any remark on this eſcapement. It requires little or no oil; and when all the parts, particularly the pendulum ſpring, are duly adjuſted, it is found that a very great variation in the firſt mover will remarkably alter the arc of vibration without affecting the rate. The piece EL might have conſiſted of a ſingle pallet or arm, inſtead of a portion of a circle or cylinder; but ſuch a piece would have been rather leſs convenient to make in ſapphire, or ruby, as in the beſt time-pieces, and would alſo have been leſs uſeful. For if by any accident or ſhock the pin G ſhould be diſplaced for an inſtant, the wheel D will not run down, becauſe it will be caught upon the circular ſurface of EL.

* The echappement à détente of Le Roy (Recueil des Machines, &c. VII. 385) was different from the above deſcribed in ſeveral particulars not eſſential to the general principle. 1. The wheel was contrate, and in the drawing has 44 thin teeth. 2. The face E was afforded by an actual pallet concave towards the tooth. 3. The detent was forked, and moved on pivots at the angular point. One leg preſented a ſtop to one of the teeth of the wheel, while the other leg reſted againſt the periphery of a ſemi-cylinder upon the verge of the balance. The detent was kept to this poſition by a tender back ſpring. 4. In the actual performance the vibration of the balance carried the ſemi-cylinder clear of the leg of the detent, which conſequently would have quitted the wheel, if the back ſpring had been ſtrong enough. Immediately after the arrival at this poſition, the pallet ſtruck a tooth of the wheel, made it recoil, and delivered the detent, which flew inwards againſt the radius of the ſemi-cylinder. The wheel being thus ſet at liberty, was driven back through an angle of near 20°, as I eſtimate from the drawing, and then in its return followed the balance through about 60° or 70°, during which period the ſemi-cylinder recovered the poſition requiſite to throw the detent in the way of the ſucceeding tooth, and retain the wheel, while the balance continued its vibration, diſengaged from every action but that of the rub of the ſemi-cylinder againſt the tail of the detent. The judicious mechanic will perceive that the diſpoſition of the parts in the eſcapement fig. 4. are much for the better. For (a) the flexures of ſprings afford a leſs variable deduction from the moving force, than the frictions of ſurfaces in the pivots of Le Roy's detent, and his ſemi-cylinder. (b) The force loſt in the recoil is greater and more variable than that employed againſt the ſpring I in fig. 4. (c) The balance is perfectly diſengaged for a much larger portion of both vibrations in the modern, than in the original eſcapement, and (d) the ſmall number of teeth in the wheel D affords an advantage in regulating the figure and contacts of the parts.

It

It is indeed very eafy to obferve, that the piece EL would operate without the detent, though with much friction during the time of repofe. The tooth of the wheel would in that cafe reft upon its circular face.

This efcapement is better calculated for a long than a fhort vibration. I have feen it adapted to an half feconds pendulum clock, in the form delineated in fig. 6, Pl. III. It kept up a vibration of 8° with a very heavy pendulum, and no great maintaining weight, but I was not informed of the quantities of thefe weights. The part ABDC is fixed to the pendulum rod. BC is a lever moveable on the joint B, but not capable of falling below its horizontal pofition. It terminates in a claw at C. At D is a pallet of agate or hard ftone, having a flat polifhed face. F G H I is a detent moveable on the centre H, but kept in its pofition againft the pin or ftop K, by means of the counterpoife G. As it is now reprefented, a tooth I of the wheel bears againft the end of the lower arm or branch. Suppofe the pendulum to be made to vibrate, and it will pafs clear of the teeth I L ; but when the claw C ftrikes againft the upper end F of the detent, the flope furface of thofe extremities will caufe the lever B C to rife, and the claw in its return will draw F towards the wheel, and confequently difengage I. The tooth E will therefore fall on the pallet D, and impell the pendulum in its vibration ; but before it quits the pallet the claw C will be drawn away from F, and the counterweight G will reftore the original pofition of the wheel, which will receive and hold the tooth M during the whole of the vibration made in the direction B C. The next vibration in the oppofite direction will difplace the detent as before, and by thefe alternations the motions will continue fo long as the maintaining power continues to act.

In the two laft efcapements we have feen the variable effects of the maintaining power almoft entirely removed, as far as can be practically difcerned. Fig. 7.* exhibits the efcapement of Mudge, in which the balance is perfectly detached from the train of wheels, except during the extremely fhort interval of ftriking out the parts which ferve the purpofe of detents. O N E B Q is the circumference of the balance, vibrating by the action of a fpiral fpring as ufual on its axis C A D H paffing through the centre C: the axis is bended into a crank, A X Y D, to make room for the other work. L M, Z W, are two rods fixed to the crank at the points L and Z, parallel to X Y. c d e f r s are fixed parts of the machine. T R is an axis concentric with that of the balance, and carrying an arm G o nearly at right angles to it, and a fmall auxiliary fpring u, which is wound up whenever the arm G o is moved in the direction o h. p is a curved pallet fixed to the axis T R, which receives the tooth of the balance wheel near the axis. The tooth, proceeding along the curved furface, by the force of the main fpring turns the axis and its arm G o, and winds up the fpring u. A fmall projection at the extremity of the curved furface of the pallet p prevents the farther progrefs of the tooth, when the arm o G has been turned through an arc o h, of about 27°; and, confequently, the fpring u has been wound up through the fame angle or arc, o G h = 27°.——F S is another axis exactly fimilar to TR. It carries its arm I o, and fpring v, and the tooth of the balance wheel l m winds up the

* From Atwood's Inveftigations for determining the Times of Vibration of Watch Balances. Philof. Transf. 1794.

fpring

fpring v, by acting on the pallet q, and is detained by a projection, after having carried it through an angle of 27°, exactly as in the former cafe. The arcs paffed through by the arms G o and I o, and marked in the figure, are alfo denoted by the fame letters on the rim of the balance.

The effect of this efcapement may be thus explained: Let the balance be in the quiefcent ftate, the main fpring being unwound, and the branch or crank in the pofition reprefented in the figure. If the quiefcent points of the auxiliary fprings coincide with that of the balance fpring, the arm G o will juft touch the rod L M, and in like manner the arm I o will juft touch the rod W Z; the two arms G o and I o in this pofition are parallel to the line C O. This pofition of the balance and auxiliary fprings remains as long as the main fpring of the machine continues unwound; but whenever the action of the main fpring fets the balance wheel in motion, a tooth thereof meeting with one or other of the pallets p or q, will wind up one of the auxiliary fprings; fuppofe it fhould be the fpring u. The arm G o being carried into the pofition G h, by the force of the balance wheel acting on the pallet p, remains in that pofition as long as the tooth of the balance wheel continues locked by the projection at the extremity of the pallet p; and the balance itfelf not being at all affected by the motion of the arm G o, nor by the winding up of the fpring u, remains in its quiefcent pofition: confequently no vibration can take place, except by the affiftance of fome external force to fet the balance in motion. Suppofe an impulfe to be given fufficient to carry it through the femi-arc O B, which is about 135° in Mr. Mudge's conftruction.

The balance, during this motion, carries with it the crank A X Y D, and the affixed rods L M, Z W. When the balance has defcribed an angle of about 27° = the angle o C h, or o G h, the rod L M meets with the arm G H, and by turning the axis T R, and the pallet p in the direction of the arc o h, releafes the tooth of the balance wheel from the projection at the extremity of the pallet p: the balance wheel immediately revolves, and the lower tooth meeting with the pallet q, winds up the auxiliary fpring v, and carries the arm I o with a circular motion through the angle o I k, about 27°, in which pofition the arm I o remains as long as the tooth of the balance wheel is locked by the pallet q. While the fpring v is winding up through the arc o k, the balance defcribes the remaining part of the femi-arc h B, and during this motion the rod L M carries round the arm G h, caufing it to defcribe an angle h C B, or h G B, which is meafured by the arc h B = 108°. When the balance has arrived at the extremity of the femi-arc O B = 135°, the auxiliary fpring u will have been wound up through the fame angle of 135°, that is to fay, 27°, by the force of the main fpring acting on the pallet p, and 108° by the balance itfelf, carrying along with it the arm G o, or G h, while it defcribes the arc h B. The balance therefore returns through the arc B O, by the joint action of the balance fpring and the auxiliary fpring u; the acceleration of both fprings ceafing the inftant the balance arrives at the quiefcent point o. When the balance has proceeded in its vibration about 27° beyond the point O, to the pofition C k, the rod Z W meets with the arm I k, and by carrying it forward releafes the tooth of the balance wheel from the pallet q. The balance wheel accordingly revolves, and the upper tooth meeting with the pallet p winds up the auxiliary fpring u, as before. The balance with the crank proceeding to defcribe

 the

the remaining semi-arc k E, winds up the spring v through the further angle k C E =108°, and returns through the semi-arc Eo, by the joint action of the balance spring and the auxiliary spring v, both of which cease to accelerate the balance the instant it has arrived at O.

It may be remarked, in this curious escapement, that the motion of the balance in its semi-vibration from the point of quiescence is opposed through an arc of no more than 108, but is accelerated in its return through the whole arc of 135°, and that the difference is what maintains the vibrations; and moreover, that the force from the wheel being exerted to wind up each auxiliary spring during the time it is totally disengaged from the balance, this last organ cannot be affected by its irregularities, except so far as they may render it more difficult to disengage the rim of the pallet from the tooth. The balance describes an arc of about 8° during this disengagement.

From a passage in a pamphlet of Mr. Mudge, printed in 1763, which I have seen, and is quoted by Count Bruhl *, it is clearly shewn that that artist had the notion of the principle of this escapement in his mind at least thirty-five years ago. From the same author's description it appears that Mudge had organized his notions in the present form before August 1771, and completed his first time-keeper two years afterwards, viz. in 1773. The performance of such time-keepers as have been made on this construction has proved excellent, but not superior, as I am informed, to such as have been made with the escapement No. 4. There are, however, many circumstances in the execution of the workmanship of engines of this delicate nature, which must affect their performance whatever may be their principle. The theory of Mudge's escapement appears to be more perfect than that of the other. But whether its six pivots in the time-measurer, which require oil and adjustments for position when intended for the pocket, and the care required in settling the points of quiescence in the three distinct springs, may render it less easy to be carried into practical effect, will be in a great measure an object of opinion, into which I cannot now enter.

If Mr. Mudge was the first who imagined an escapement absolutely detached, I think it is to Mr. Alexander Cumming that we are indebted for the first execution of a like contrivance of his own. This well-known artist, in the year 1763, made a clock for the king, in which there was no friction during the repose of the movement, and the maintaining power was applied to the inclined faces of a pair of pallets†; and some time previous to 1766, he improved the same so as to render it as perfectly detached as, in the nature of things, an escapement can be. As this has been so many years in possession of the public, and is, as I think, simplified in the sketch, fig. 8,—I shall only say that it consists of an anchor like that of Graham, but having a detent or claw at each end instead of pallets, and two separate pallets pressed (by a lever and weight to each) towards the centre of a wheel like AB, fig. 2; that these pallets are severally raised during the absence of the pendulum, which displaces the detent, by virtue of which, when the pendulum becomes joined with either, the maintaining force is constant and greater in the descent than in the ascent. It seems reasonable to infer, that Mr. Mudge must have paid great attention to the escape-

* On the investigation of astronomical circles; (page 3, of register) a pamphlet of 32 pages octavo, printed in London 1794.

† Cumming's Elements of Clock and Watch Work, p. 78.

ment

ment of Cumming during his labours for organizing his own, which he did about five years afterwards.

Fig. 8. is the ſketch of an adaptation of Mudge's eſcapement to a clock. LM is part of the periphery of the wheel. GA, GB, are two arms ſeparately moveable on the ſame axis, and terminating in the pallets A, B. Theſe pallets have inclined faces, with a claw or detent at the lower part of each. GO, IO are tails proceeding from each pallet-piece reſpectively, and the dark ſpot at N repreſents a pin proceeding from the pendulum rod, and capable of moving either of the tails according to the courſe of the vibration. The dotted circles u and v repreſent weights which are ſtuck upon two pins, and may be changed for others, greater or ſmaller, until the moſt ſuitable quantity is found. Suppoſe the wheel to be urged from L towards M, and the pendulum made to vibrate by external impulſe. The pin N proceeding towards L will ſtrike the tail G O, raiſe the pallet A, and ſet the wheel at liberty; which ſliding along the inner ſurface of the pallet B, will raiſe it, and ſtop againſt the claw at its lower end. I O will conſequently be carried into the poſition IP; and the pallet A in its return will be oppoſite a vacancy, which will permit the tail GO to follow the pin N as far as the perpendicular ſituation. The pendulum will therefore be aſſiſted by the weight u through a longer arc in its deſcent, than it was impeded by it in its aſcent. In the oppoſite ſemi-vibration towards M, the pendulum will proceed unoppoſed by v, while it paſſes through the angle OIP, when it will raiſe B, and permit the wheel to elevate the pallet A. In the motion on this ſide of the perpendicular, it is alſo clear that the deſcent will be more aſſiſted than the aſcent was impeded. Whence it follows that the clock will continue to go: and no variation of the force of the wheel LM, which raiſes the pallets in the abſence of the pendulum, will affect the vibration, except ſo far as it may afford a variable reſiſtance at the detent or claw.

From obſerving that the detached eſcapements require a very ſtrong firſt mover*, and that pendulum clocks meaſure time with great accuracy by means of Graham's dead beat eſcapement, fig. 2, notwithſtanding the influence of oil and friction during the repoſe of the movement, I was induced to contrive an eſcapement which ſhould be as free as the crown wheel eſcapement, but have no recoil or action during the time of reſt, nor any detent, nor require oil on its pallets. I made a drawing at the beginning of the year 1784, which I ſhowed to various mechanics at that time. It has ſince been adapted to a clock in my poſſeſſion, and has been going very freely for a year paſt. Fig. 9 is a ſketch in which GH repreſents a ſteel wheel; D and E are pallets of agate, with flat poliſhed faces. The pallet D is fixed to the lever DC, which is confined to its preſent ſituation by the loaded branch or arm CB reſting on the ſtop k. The lever EC is alſo kept in its ſituation

* A pocket time-piece by Brockbank, making 5 vibrations, or $2\frac{1}{2}$ beats per ſecond, the radius of whoſe balance was 0.35 inch, and arc deſcribed by the external ſurface of the expanſion pieces 0.45 inch radius, was maintained by a force which, on trial at the axis of the fuſee, proved equal to $9\frac{1}{4}$ oz. troy, paſſing through 12 inches in 24 hours. Another, of the ſame dimenſions and conſtruction, was driven by $11\frac{3}{4}$ oz. with the ſame daily fall. And a large box time-keeper of Arnold, beating half-ſeconds, with a balance of two radii, each one inch long, was maintained by a like force of 113 ounces. This is nearly as much as is required to afford a ſemi-vibration of $3\frac{1}{2}^{\circ}$ in a clock, with a ſeconds pendulum of 8lb. by the common eight-day train with Graham's eſcapement (fig. 2.), but three times as much as is required for a well made regulator. A common watch with a balance 0.8 inch diameter, was found to have a maintaining power of $5\frac{1}{4}$ oz. falling daily through 12 inches.

by

by the loaded arm CA refting upon I. A pin N proceeds from the pendulum rod to its fituation between the levers. The centre of motion of the pendulum is in the continuation of the axis of the pallets. Suppofe the vibration to be begun, the pin N in its progrefs will lift the pallet D towards L, while part of the weight B will be fupported by the tooth of the wheel which will follow, and at laft efcape, and fuffer the oppofite tooth F to fall on the pallet E. But in its return or defcent the pendulum will be acted on by the whole weight B, and confequently its vibration will be kept up. When the pin N raifes the pallet E, a fimilar effect will take place on the fide of the perpendicular towards M, and thus the procefs will go on as long as the wheel GH retains any force.

The ancient efcapement with the crown wheel, verge, and pallets, has continued in ufe for three centuries, becaufe of the advantage of an almoft direct pufh againft a retiring furface, which enables it to go without oil. In the efcapement laft defcribed, the impulfe being more direct than on thofe common pallets, it goes more freely without oil; and inftead of the re-action at the ends of the vibrations, the pendulum is perfectly difengaged during the whole time of repofe.

From this general view of the principal efcapements, together with that of the compenfations for temperature before explained *, the philofophical reader may form fome eftimate of the cautions requifite to form the valuable inftrument by which a meafure of time is afforded. I hope it fcarcely need be added, that I have endeavoured to do juftice in the hiftorical narration, and fhall be happy to rectify any miftakes, if better information fhould detect them.

II.

Copy of a Letter from Profeffor WILSON, *of Glafgow, on the Art of multiplying Copies of engraved Plates and Stamps in relief* †.

SIR, *Glafgow College, 23d March,* 1798.

I DOUBT not but you will have the goodnefs readily to excufe the liberty which I take upon the prefent occafion of addreffing you by a printed letter. I have been induced to do fo, the more conveniently to communicate to you fome account of what poffibly may be thought entitled to the appellation of a NEW ART, and which, for particular reafons, it has been thought adviseable to make more or lefs generally known, even at this very early ftage of it.

Some years ago, upon the difcovery of the fingular property of the fluor acid in corroding glafs, when it was fo common to hand about bits of glafs-plate frofted over by this chemical agent, it happened to ftrike me that we were indulging too long in a barren admiration of mere novelty, and overlooking a matter of real importance to which it evidently pointed. The general effect of what then fo much amufed us, when the plates

* Philof. Journal, I. p. 56.

† I firft faw this letter at Sir Jofeph Banks's; but having fome doubt as to the propriety of requefting it of him for publication, I fent to Mr. Taffie, who favoured me with another copy for that exprefs purpofe. N.

were

were viewed by transmitted light, suggested the possibility of formally etching with delicacy and perfection upon glass. This thought no sooner occurred than it challenged some attention in consequence of perceiving that glass, from its extreme hardness and lubricity, would preserve the execution bestowed upon it vastly longer than copper-plate, were it possible to introduce its services at the rolling press. But whilst harbouring these surmises, the extreme fragility of glass-plate, contrasted with the violent pressure it must endure between the rollers, had well nigh made me relinquish them altogether. It often happens, however, that things, which at the first glance seem very unattainable, yield to a little reflexion, when they are fairly set before the mind in the light of valuable desiderata.

In the present case it soon became very evident, that the thinnest and frailest piece of glass-plate, whether straight or uneven, could, in regard to the strongest pressure, either general or topical, be rendered as robust and as rigid as iron or steel. The expedient which occurred to me for that purpose, consisted in nothing but joining the glass-plate to flat pieces of these metals, of considerable thicknefs, by an intervening lamina of proper cement, taking care that it affords a very complete contact.

Agreeable to this it is well known, that so far back as the year 1791, by fortifying glass-plates of a moderate size in this way, and aiding them further by a simple apparatus on the press board, I succeeded completely in making them pass safely between the rollers, and in obtaining many impressions, such as the fluor acid by the process of etching could afford. Upon finding, however, that the best of these etchings were very paltry, and still regarding the safe passage of the glass between the rollers as an interesting experiment, I was insensibly led to consider, whether by the lapidary's wheel, or by any other possible means whatever, we could so operate upon glass-plate as to render it a productive subject at the rolling press. This step appeared of some consequence, as being all that was now wanted for establishing an improvement not destitute of importance. Not long after, certain views and methods occurred to me, which recently have been prosecuted experimentally, and which raise considerable expectations of our obtaining the mastery over an art, whereby the tame scratchings of the fluor acid would be left far behind, and the execution and spirit of the graver itself transferred from copper-plate to glass-plate, in inscriptions of all kinds, and in designs or embellishments of any sort, especially where hatching constitutes the manner; and all this with the further advantage, that any number of such glass-plates may be obtained quite alike, as being derived from the same original pattern.

The means of effecting a matter of such apparent difficulty, like most other advances in the Arts, or even in the Sciences, as soon as ever propounded, will, doubtless, be thought extremely simple and obvious. But whatever small portion of merit may be allowed in the present instance, should the means prove fully adequate to the end, it will partly consist, as will be seen immediately, in my having given an entire new direction to another ingenious art which has been long cultivated, and by none with so much genius and ability as by the justly celebrated Mr. Tassie of London.

In November last, certain circumstances recalling my attention to the present object, I was led to explain, by letter to Mr. Tassie, the method of printing at the rolling press with glass-plates, and to specify to him, at some length, the steps by which I wished him immediately to proceed, in attempting for me the execution of caſts or copies in glaſs from certain copper-plates which I ſent him, and which I had got engraved here for the purpoſe. It afforded me much ſatisfaction to find that this eminent artiſt received my application with great candour, and that he purſued the object pointed out with promptneſs, about which I had applied to him in the line of his profeſſion. Though, indeed, at the beginning ſome difficulties ſtood in the way, yet by a repeated correſpondence, and tranſmiſſion of different eſſays from his furnace, anſwering to patterns ſent up at different times by the mail coach, Mr. Taſſie ſoon furniſhed me with ſeveral caſts in glaſs and enamel, from engraved copper-plates, which appear to juſtify my firſt conceptions, and to go ſome length, even thus early, to enſure the ſucceſs of the plan and method, which I had brought forward.

The other day I made ready for printing, three of theſe plates; two of them bearing inſcriptions, and the other a head in profile from Le Brun. I might have mentioned that the plates, when fully finiſhed, have an appearance of great elegance; thoſe eſpecially which are made of white glaſs, called enamel; and it is remarkable how eaſily they are cleaned from the ink, in the courſe of the workmen's operations, though they never are heated farther than by handling them. From the three plates above mentioned, I now take the liberty of ſending you proofs *, or impreſſions, incloſed. So far as I know, they are the firſt of the kind ever produced. As the very firſt offerings, therefore, of a new art, they may poſſibly appear to you in a light more or leſs intereſting, and as an earneſt of ſomething conſiderably more perfect ſoon to follow, when we are aſſiſted by engravings of ſome elegance, and executed with more addreſs in ſeveral reſpects, with a view to a deſtination ſo peculiar.

It will not eſcape you, that by means of a ſeries of glaſs-plates, inconſiderable in point of number, deriving the engraving from the ſame original pattern, to ſucceed one another at the preſs, juſt before ſigns of wearing might appear, a vaſt many impreſſions, all *perfectly ſimilar*, might be obtained, even for ages. This at once would be beſtowing a new character upon the rolling preſs, which hitherto has been much circumſcribed in this reſpect, by the periſhable nature of engraved copper-plates, and from their having no relation to any common archetype. Much could be ſaid of the advantages of thus commanding an ever enduring identity amongſt the impreſſions afforded by engraved plates, even of a ſmall ſize, eſpecially in the inſtance of circulating Bank Paper. I confeſs, indeed, it was the frequent alarming forgeries upon ſuch currency, eſpecially that of the Bank of England, which moved me in November laſt to bring to experiment the preſent ſcheme, which has been for ſeveral years in my mind. Should the glaſs-plates be carried to a ſufficient pitch of delicacy, I perceive, on ſeveral accounts, that they would afford a refuge of ſingular importance againſt all attempts of forgery; provided we were to found upon archetypes of copper-plate highly elaborated, and, beſides the mere inſcription, exhibiting by collateral embelliſhment the peculiar manner of ſome eminent maſter in the art of engraving.

* It would have been evidently uſeleſs to have copied theſe plates by way of exhibition to the reader. They are not diſtinguiſhable from copper-plate prints; unleſs, perhaps, by ſome minute circumſtances ariſing from the ſubſequent poliſhing of the glaſs. But this laſt obſervation muſt be uncertain, unleſs proofs from the original copper were alſo had for compariſon. N.

Before

Before concluding, I may just mention, that should the methods followed by Mr. Tassie, of moulding with plaister of Paris and Tripoli, not afford sufficient delicacy to the plates, I am already not without other resources likely to carry us farther in arresting an art, which seems recommended by views of considerable utility. I allude here partly to a method which occurred to me more recently, of having the original engraving done upon a flat and polished surface of steel, which, by the fly press, may be communicated to a flat and polished piece of pure gold. This to serve in the furnace as a *permanent mould* for giving the impression to the glass. From some experiments I have actually made in a small way, it should seem as if this process promises well; particularly, by giving the fire surface still more delicacy, and which, consequently, would require a smaller degree of finishing, in the process for bringing it ultimately to a sufficient polish; upon which circumstance alone, now depends the certainty of our being able virtually to engrave upon glass-plate, with a very considerable degree of perfection. Should it be necessary in the end to resort to the gold, Mr. Tassie's consummate skill and address at the furnace, in the application of the glass, would doubtless contribute greatly to the success.

One recommendation amongst others of this method would be, that, from the indestructible nature of pure gold, if properly managed in applying the heat, all the plates would actually be derived from one and the same mould; whereas, by the other method, the moulds themselves, which are necessary for every cast, are derivative, though indeed from the same original pattern.

I have the honour to be, Sir,

Your most obedient servant,

PAT. WILSON.

P. S. I have purposely reserved, to follow in a postscript, a very short mention of another subject bearing a close analogy to the foregoing.

You may perhaps know that of late years the art of cutting designs upon box-wood has arrived at an uncommon degree of perfection, and that the celebrated Messrs. Bewicks, especially, have carried their execution in this respect to a pitch of elegance rivalling copper-plate, and which was believed to be utterly unattainable before their time.

Having often regretted that such rare specimens of art, as they have produced, were so perishable, from the frailness of the materials upon which so much genius and labour were expended, I was induced also to send to Mr. Tassie, amongst other models, some designs in box-wood, executed by Mr. Bewick, with directions to mould from them, in the view of obtaining casts or copies in glass. The returns which I received to all those patterns completely answered my expectations, as being at once as perfect as the originals.

From the success of this experiment, which also I have had long in contemplation, and from what has been established in the way of making glass safely resist any pressure, it will readily occur that an improvement of considerable magnitude has now been shown evidently to depend upon a proper co-operation of the two arts of engraving upon box-wood, or upon brass, and of moulding, with a view of obtaining such cuts or engravings in so durable a substance as glass.

P. W.

III.

Inſtructions concerning the Manufacture of Steel, and its Uſes. By VANDERMONDE, MONGE, *and* BERTHOLLET. *Publiſhed by Order of the Committee of Public Safety*.*

Preliminary Obſervations.

IRON is a combuſtible body. It loſes its metallic properties by being burned. When iron filings are expoſed to ſtrong heat in a crucible, and frequently ſtirred to bring the parts ſucceſſively in contact with the air, the metallic aſpect diſappears. It aſſumes a brick-duſt colour, and is found to have increaſed in weight. Part of the air of the atmoſphere has combined with it, and produced this change. This portion of the atmoſphere is called oxygene.

Iron is found in this ſtate in its ores. The proceſs of extracting it from theſe ores principally conſiſts in depriving it of oxygene.

Charcoal has the property of effecting this change. During combuſtion it abſorbs and unites with oxygene, and it will attract the oxygene from iron when theſe ſubſtances are in contact at an elevated temperature. Theſe effects of air and charcoal are very perceptible when tin is kept in fuſion. A grey pellicle is ſoon formed on the ſurface, which has no metallic ſplendour. If this pellicle be taken off, a ſecond is formed, and in this manner the whole of the tin may be converted into a ſubſtance reſembling earth, and known by the name of droſs. By expoſure of the droſs to heat, together with a ſmall quantity of charcoal powder, tallow, or reſin, the oxygene is abſorbed by theſe combuſtible ſubſtances, and the tin becomes reduced to the metallic ſtate.

Charcoal not only poſſeſſes the property of depriving the iron of the air which was united with it, but it is alſo capable of being diſſolved in the iron in a ſtrong heat, and by this ſolution it communicates new properties to it. It changes it into ſteel †.

Crude or caſt iron may be conſidered as a metal not completely reduced, which conſequently retains a portion of the baſis of air, or oxygene, to which it was united in the ore; and as this reduction may be carried farther according to circumſtances, the conſequent variations are among the leading cauſes of the different properties we obſerve in caſt iron. The white caſt iron accordingly holds in ſolution much oxygene and little charcoal; the grey caſt iron on the contrary contains more of this laſt ſubſtance, but is much more perfectly deprived of oxygene. A greater proportion of coal muſt be uſed in the furnace to obtain this laſt. The properties of theſe two kinds of iron depend only on this difference: the former is more brittle and fuſible, but it is eaſy in the refining furnace to deprive it of oxygene by the action of ignited charcoal, which combines with this principle. The ſecond has retained leſs oxygene, but contains much more charcoal. It is ſofter, and preferable for ſuch uſes as require this ſoftneſs; but it is more difficult to convert it into malleable iron, becauſe a larger portion of coal is required to be deſtroyed, and in this ſtate it conſiderably reſiſts combuſtion.

* I have omitted a few political reflections of a temporary nature contained in this memoir.

† Other ingredients enter into ſteel, and are perhaps eſſential to it, particularly phoſphorus. See the analyſis of Vauquelin. Philoſ. Journal, I. 252.

Forged

Forged iron perfectly refined would confift of the metal completely reduced, and containing no foreign fubftance, not even charcoal. Such iron is not to be met with in the market. The beft Swedifh iron always contains a portion of oxygene which has efcaped the operations of the furnace and the refinery, and it is always contaminated by a dofe of charcoal, very fmall indeed, but which perhaps it is impoffible totally to eradicate.

Other circumftances likewife influence the qualities of iron, particularly with refpect to the fabrication of fteel. This metal, according to the nature of the ores which afford it, may have the defects of being brittle when cold, or brittle when hot. Thefe are refpectively called cold-fhort, or red-fhort iron. We fhall not here difcufs the caufes which produce thefe bad qualities*; but fhall only remark that fuch iron, or its ores, affords bad fteel, and muft therefore be carefully avoided.

Steel is diftinguifhed into three kinds:—natural fteel; fteel of cementation; and caft fteel.

Concerning Natural Steel.

THE fteel obtained immediately from the ore by fimple fufion, is called natural fteel. It is likewife diftinguifhed by the name of German fteel, becaufe it comes principally from Germany.

Whether the crude iron fhall affume the nature of bar iron, or of fteel, depends on circumftances; but thefe are not difficult to be explained, from the doctrine already laid down.

Grey crude iron is alone proper to afford fteel; for which purpofe it is requifite that the oxygene it contains fhould be feparated, and the coal from which its grey colour arifes fhould be intimately combined with it. The converfion of the crude iron into fteel depends on thefe proceffes †.

Hence it follows as a leading rule, that no attempts muft be made to convert the white crude iron into fteel in this way, notwithftanding it may be capable of affording excellent bar iron. The firft operation muft be conducted in fuch a way as to afford grey crude iron, by adding a greater proportion of coal in the charge of the furnace.

The appearance of crude iron is often deceitful with refpect to its nature. The grey crude iron in plates or fmall portions, fuddenly cooled, has the colour of the white crude iron. But the nature of thefe irons may be afcertained by the fimple procefs defcribed at the end of this memoir.

When a proper crude iron is in readinefs, it is neceffary, in order to convert it into bar iron, that it fhould be much more expofed to the action of the air by frequent ftirring, and by removing the fcoriæ; but when fteel is defired, it is lefs expofed, and fuffered to remain covered by thofe fcoriæ. In the former procefs the charcoal is burned by the contact of the air, and the iron is left confiderably more pure; but in the fecond the charcoal is preferved; part of which combines with the oxygene which ftill remained in the iron, and

* Cold-fhort iron is afforded by ores which contain a fmall portion of phofphoric acid, which combines with the metal in the ftate of phofphorus. Red-fhort iron, which is much lefs common than the other, contains arfenic; but it is probable that the other femi-metals may produce the fame effect. *Note of the authors.*—Probably the volatile metals may moft eminently conduce to this effect. N.

† See Philof. Journal, I. 328.

 ferves

ſerves to ſeparate it, while the other portion combines with the iron itſelf, and gives it the qualities of ſteel.

The diſpoſition of the hearth or fire-place, and the poſition of the tuyer or nozle of the bellows, are two objects which require much attention. In order to obtain iron, the fire-place muſt be larger than for ſteel, and the tuyer muſt be inclined ſo as to direct the blaſt to the ſurface of the iron. The hearth is to be filled with charcoal, and the crude iron placed thereon to the level of the upper part of the tuyer. The heat is applied moderately, and by degrees, in order that the iron may not flow, but be kept in the ſtate of a paſte. It is to be ſtirred occaſionally with the rake, frequently brought into the direction of the blaſt, and the ſcoriæ muſt from time to time be taken out.

To produce ſteel, a bed of ſmall or powdered charcoal is to be laid round the fire-place, which muſt be moiſtened and rammed down. Light fuſible ſcoriæ are added; the tuyer is uſually more inclined, and the fuſion more haſtily urged, in order that the metal may flow, and immediately ſink beneath the ſcoriæ, which are not removed till the end of the operation.

The ſame proceſſes are not every where followed: but a little attention ſerves to ſhow that they are all founded on the ſame principles; namely, that in producing ſteel the coaly principle of the iron is not burned, whereas in the production of bar iron the operation is ſo conducted as to burn that principle. We ſhall proceed to mention ſome inſtances.

In Styria, where good ſteel is made, the caſt iron is reduced into thin plates, which are fuſed in the refinery. The ordinary maſſes or loupes, which have been ſuffered to cool in the furnace, inſtead of having been drawn off, are alſo fuſed for ſteel, of which they have begun to aſſume the character during their maceration in the pot, which, with this intention, was lined with charcoal, and wherein they were covered with the ſcoriæ. The plates as well as theſe maſſes, which are previouſly divided into ſmaller pieces, are refined with the precautions neceſſary to determine the formation of ſteel.

A circumſtance which contributes much to the goodneſs of the ſteel is, that after it has been drawn out by the hammer, the bars are thrown red-hot into water, and afterwards broken in pieces for the purpoſe of ſeparating the perfect ſteel from that which is of the nature of iron. The hard ſteel is alſo ſeparated from the ſoft. Packets or truſſes of theſe are made up, conſiſting of twelve or fifteen pieces each, obſerving that the two outer pieces are ſoft ſteel. Theſe truſſes are welded together, and drawn out again to a ſmall ſize; by which means the quality of the ſteel is rendered conſiderably more uniform. The greateſt quantity of German ſteel, as well as that which is moſt eſteemed, is made in Carinthia. Their proceſſes deſerve to be particularly attended to. We ſhall here give a ſhort account of the particulars from Haſſenfratz, who made his obſervations on the ſpot, and has communicated them to us.

The crude iron is reduced into thin plates, or leaves, when it is drawn from the ſmelting furnace. For this purpoſe a mould, or hemiſpherical cavity, is prepared before the furnace. It is formed of the ſcoriæ reduced into very fine powder, and wetted to make them adhere together.

The work is then opened with an iron bar, in order that the ſcoriæ may flow into the mould, and diſſipate its moiſture. Theſe are in the next place taken out, and the metal itſelf is ſuffered

to

to flow at firſt in a ſmall ſtream, and afterwards more ſpeedily. The aperture is enlarged in proportion as it flows out, and at laſt the ſcoriæ fall on the iron, and cover it in the mould. The furnace is then again cloſed, and the blaſt renewed. Water being thrown on the ſcoriæ which occupy the upper portion of the mould, they become fixed, and in this ſtate are removed. A ſecond portion of water is then thrown on the naked ſurface of the metal, which congeals to a ſmall depth. The thin congealed plate is taken off, and a ſecond aſperſion of water is made, which affords a third plate. In this manner the proceſs is continued, until as much of the metal is converted into plates as can be effected during the fluidity of the maſs.

At ſome works the iron is melted in a particular ſurnace from the pig, for this purpoſe; but this ſecond operation is evidently waſteful both of time and fuel.

The plates are intended to be made into either iron or ſteel.

In the proceſs for making bar iron, the firſt operation conſiſts in roaſting the plates on a hearth, upon which they are arranged; a paſſage being formed with bricks, in order that the wind of the bellows may be directed from one extremity to the other. They are then covered with charcoal, and urged ſtrongly with the bellows. The plates by this roaſting, which deſtroys the charcoal of the caſt iron, begin to aſſume the qualities of bar iron, after which they are carried to the finery furnace. The body of this furnace is more capacious than that which is intended for ſteel. The iron is covered with charcoal and ſcoriæ, and the tuyer is inclined ſo that the blaſt ſtrikes on the plates of metal. When the fuſion is complete, the ſcoriæ are let out, the maſs is frequently turned to expoſe it to the blaſt, and, laſtly, the proceſs being completed, the iron is conveyed to the hammer.

If the object be to form ſteel, the furnace made uſe of is more contracted and deep. It is lined with pulverized charcoal, moiſtened and rendered ſolid by beating. The plates are diſpoſed therein, and covered with ſcoriæ and charcoal. The poſition of the tuyer is nearly horizontal, in order that the ſtream of air may ſtrike the fuel, and not the metal. When the metal begins to aſſume the ſolid ſtate, the coal is taken off, the ſcoriæ are ſuffered to flow out, and ſcales and fragments of ſteel are driven by hammering into the ſoft maſs.

The piece is afterwards melted a ſecond time with the ſame precautions as before; and when the metal is thought to be ſufficiently refined, the ſcoriæ are drawn off, and the maſs is conveyed to the hammer to divide it into ſeveral pieces, which are to be ſeparately forged out.

We ſee that all theſe operations are directed to the means of deſtroying the charcoal of the crude iron, when bar iron is wanted; but when ſteel is required to be made, the metal is not only preſerved from the contact of the air, but the veſſel is lined with charcoal, in order that, by its contact with the fuſed matter, it may ſupply any portion of that principle which may be wanting.

In the foregoing proceſs there are two fuſions of the metal. In the latter it is not only completed by the ſecond fuſion, but it is rendered more homogeneous. This method is excellent, and is perhaps the only means by which an exceedingly good ſteel can be had.

The other part of the proceſs is worthy of much attention, namely, the reduction of the crude iron into plates. When bar iron is wanted, theſe plates roaſt with more facility

 on

on account of the great ſurface they preſent to the air. And when ſteel is wanted, they are more readily fuſed, and ſink beneath the ſcoriæ, which prevents the charcoal of the iron from being conſumed by the action of the air. On the contrary, they abſorb what might have been wanting from the lining of the veſſel, which is prepared in ſuch a manner as to ſupport itſelf without being conſumed through the whole of the operation.

When the ſteel has congealed in the furnace, it is taken out and divided into ſeveral portions more or leſs conſiderable, which are carried to the hammer. Here a ſeparation is made of ſuch portions as are not reduced into ſteel, but iron, and which occupy the ſurface of the pieces. Each piece is drawn out into bars, which are reduced into other ſmaller bars of different dimenſions, by ſeparating the ſofteſt parts from thoſe which are more hard.

For ſteel of a ſuperior quality, ſeveral bars of the ſoft and hard kinds are united by welding and forging. The hardeſt are placed in the middle.

We have ſhown that, in order to obtain ſteel from crude iron, it is neceſſary to have an iron abounding with coal; but there is an exceſs which is hurtful. The black crude iron, which contains too much coal, affords a ſteel ſo brittle as to be of no uſe. This kind of ſteel becomes fixed with more difficulty than good ſteel. When the workman perceives this ſymptom, he may prevent the bad effect by adding a certain quantity of old iron fragments, which deprives the too ſteely metal of its exceſs of coal, and, by incorporating with it, produces an uniform maſs of good ſteel. When the crude iron is of ſuch a nature as to afford brittle ſteel, it is uſual to mix in the refining furnace a quantity of another kind of crude iron, which may modify its quality.

Though iron and ſteel are diſtinguiſhable by very ſtriking qualities, there is, nevertheleſs, a point of contact at which they are confounded: the ſofteſt ſteel may be conſidered as a very hard iron, and, in fact, the ſeveral kinds of iron differ in hardneſs by the ſame principle which conſtitutes ſteel. They all retain a ſmall portion of charcoal, which eſcapes the operation of refining. Thoſe which contain the leaſt are under like circumſtances more flexible, ſoft, ductile, and ſuſceptible of acquiring by the action of the hammer that fibrous form which conſtitutes what is called the grain of iron. Hence it is that different kinds of bar iron are ſometimes obtained from the ſame crude iron, though the operation is apparently the ſame. It is ſufficient for this effect that the inclination of the tuyer be changed.

Concerning Steel obtained by Cementation.

THE ſteel of cementation is formed by means of a cement, with which bars of forged iron are ſurrounded in a caſe diſpoſed in the middle of a furnace, where they are ſubjected to a ſtrong heat.

We muſt repeat that the good quality of the iron is a condition indiſpenſable for obtaining good ſteel. It is of importance to chooſe the beſt kind; and the Engliſh, who almoſt excluſively prepare the ſteel of cementation, retain for that object all the iron of Roſlagia, which is the beſt iron of Sweden, and for which they pay a high price.

It is not enough that the iron ſhould contain no noxious principle. It is alſo neceſſary that it ſhould be forged with care, and its parts well united. For if it contain flaws or clefts

clefts within the bars, they become much more perceptible when the iron has acquired the nature of ſteel; and it would not be practicable to unite them perfectly, becauſe the parts of ſteel are much leſs diſpoſed to weld and adhere together than thoſe of iron. We have had convincing proofs in our own experiments, that the irons of France of good quality, ſuch as thoſe of the ci-devant Berry, afford only a bad ſteel when cemented in the ſtate they uſually are delivered in from the forge; but the ſame iron, after having been carefully forged and hammered, formed ſteel equally good with that which had been made at the ſame time from an excellent iron of Sweden. In another experiment the ſteel prepared with iron of the ci-devant comté de Foix, which had been well forged, produced ſteel of a quality equal to that which had been obtained in the ſame operation with Swediſh iron.

Hence it reſults, 1. That the beſt Swediſh iron owes its property of forming good ſteel, leſs to any particular quality of the ore than to the care with which it is forged and ſubmitted to the action of the hammers: 2. That we poſſeſs in France irons capable of affording good ſteel, provided care be taken that they be well forged; but the mere neglect in this operation may prove fatal to a plan in other reſpects well conducted.

The firſt attention which ought therefore to be paid in the attempt to manufacture ſteel is to procure good iron, to examine whether it be well forged; and in caſe this operation has not been ſufficiently attended to, it muſt be forged and made ſound before it is ſubjected to cementation.

It has long been ſuppoſed that the cement proper for the ſteel-making proceſs ought to contain ſaline, inflammable, fat or ſulphureous parts, which were ſuppoſed to penetrate the iron and change it into ſteel. Hence have ariſen many pretended ſecrets, which have diverted the attention of thoſe who have engaged in undertakings of this kind under the guidance of ignorant pretenders. There are no ſecrets in the compoſition of the cement. The Engliſh uſe no material but the charcoal of wood reduced to powder; and in fact the only eſſential condition is, that the iron ſhould become impregnated with the very ſubſtance of the charcoal uniformly to its centre.

When the bars or plates of iron which are to be converted into ſteel are ready for the furnace, they are cut to the length of the caſe or crucible in which the cementation is to be made. A bed of charcoal powder, ſifted through a coarſe ſieve, and ſlightly wetted, is laid in the bottom of the caſe. Upon this is placed a row of iron bars at a little diſtance from each other. This firſt layer of iron is then covered with a bed of charcoal powder, which fills the ſpaces within the bars, and riſes to the height of half an inch above them. A new range of bars is then laid and covered with charcoal as before, and in this ſucceſſive way the operator proceeds till the caſe is full. The laſt row is covered with charcoal powder, over which is laid a bed of ſand to cover its ſurface entirely, and prevent its being deſtroyed by combuſtion. The ſand muſt be moiſtened and well preſſed together in the form of a roof, riſing higher than the ſides of the caſe, ſo as to be ſeveral inches thick in the middle.

When the preparation or charging of the caſe is finiſhed, the furnace is diſpoſed for making the fire, which is gradually increaſed, and muſt be kept up for a longer or ſhorter time according to the quantity of ſteel, and conſequently according to the ſize of the caſe. At Newcaſtle, where between twenty-five and thirty thouſand weight of ſteel is cemented in two caſes contained in a furnace, the operation laſts five days and five nights. At one

of

of the extremities of the furnace as well as of the case, it is usual to form a hole, by which a bar may be taken out when it is supposed that the cementation may be sufficiently advanced. The workman judges from the colour, and the blisters on its surface, whether the steel be perfect. If he cannot depend on his judgment in this respect, trial is made of this steel by hardening and breaking it. If the cementation have not penetrated as far as the centre, it is easy to distinguish, by the fibrous fracture, that part which still retains the nature of iron.

When the steel is taken out of the cementing furnace, its surface is covered with inequalities and blisters, whence it is called blister steel. In this state its fracture presents very large facets, and resembles brittle iron of a bad quality. Before it is brought to market it is usually forged into flat bars seven or eight lines broad; after which it is suffered to cool in the air without plunging it in water. By this treatment its grain becomes much closer.

As the extremities of the bars thus converted into steel are usually flawed and less perfect, they are cut off and forged together in faggots. This steel is used for instruments of agriculture. If the fire have not been sufficiently active, or kept up for the proper time, the bars are not cemented to the centre; whence they become of unequal hardness, especially if they be not very carefully forged. When the fire has been too intense, the steel becomes too brittle, and difficult to be managed on account of its having dissolved too large a quantity of charcoal. Yet it is impossible to establish any rule for the management of the fire, because it must vary according to the form and size of the furnace, the number and thickness of the bars, and the nature of the fuel.

The form and magnitude of the furnaces vary considerably in the different works where iron is cemented. The objects to be aimed at are, to give the furnace a degree of solidity which shall enable it to resist a great number of operations, to cause the flame and heat to circulate equally on all sides of the case, and to produce the greatest quantity of heat with the smallest expence of fuel.

One very important observation respecting the extent of dimensions which may be given to these furnaces is, that no advantage, or at least very little, in the consumption of fuel is obtained by enlarging these dimensions, because it is necessary that the whole of the heat should be suffered to dissipate at the end of each operation. The case is very different in other manufactories where the accumulated heat may serve for successive processes; for the whole of such fuel as is employed in raising the temperature to the necessary degree in cases of interruption would be entirely lost.

Prudence demands, that he who is desirous of improving or extending the arts should not blindly be led away by the seduction of projects. It is proper to begin the operations on a small scale, in order to render the practice familiar, before furnaces of a certain magnitude are constructed *.

(To be concluded, with Annotations, in our next.)

* Reference is here made in the original to drawings of furnaces; one contrived by Jars for cementing three or four hundred weight of steel; another of the furnace at Newcastle; another heated by wood, together with some designs relative to the manipulations at Carinthia. None of these, however, are annexed to the memoir. N.

IV.

Observations Chemical and Economical on various Subjects *.

1. M. TROMSDORFF, Professor at Erfurt, has observed that sulphurated hydrogenous gas takes fire and burns with a strong flame by means of the nitrous acid.

2. M. Linck, Professor at Rostoc, finds that three parts of nitrous gas, and two of hydrogene gas, obtained by sulphuric acid and iron, are scarcely or not at all diminished when exposed to day-light over water. Common air is not more diminished by this admixture kept a long time: but the mixture itself of these two gases is diminished by the addition of new portions of nitrous gas. M. Linck concludes from this observation, that part of the oxygene of the nitrous gas combined with the hydrogene and formed water, and that the remaining oxygene and azote formed a mixture similar to the air of the atmosphere.

Citizen Vauquelin made a similar remark ten years ago on nitrous gas placed over a solution of the hydro-sulphuret of lime. The diminution of the gas was considerable.

3. The urine of animals which feed on vegetables does not contain phosphoric acid, but an acid of a vegetable nature. On this passage C. Vauquelin remarks, that Rouelle announced this fact twenty years ago, and that C. Fourcroy and himself have lately proved it by a great number of experiments, which have moreover shewn that the acid which supplies the place of the phosphoric in these animals is the benzoic.

On this occasion it may be remarked, that the vegetable kingdom affords phosphorus; that the bones which are produced and grow in animals feeding only on vegetable food contain this substance; whence it appears probable that their urine is not at all times divested of phosphoric acid. M. Giobert in his memoir on phosphorus (Annales de Chimie, xii. 23.) affirms that the urine of horses is nearly as proper for his process as that of men; that is to say, that it affords nearly as much phosphorus. But he does not positively relate any experiment to this effect.

4. A German correspondent of the work before us affirms, that the blowing machines or bellows for the iron works in the Hartz, are cubical boxes of wood, which are more easily made than the iron cylinders made use of in England, and that they are very advantageous in the use. From various accounts there is reason to think, that the density and velocity of the stream of air from our iron cylinder worked by a steam engine are greater than has ever been afforded by the wooden bellows or other blowing engines formerly in use. The height to which this air will support a column of water is between six and seven feet, or at a medium near six inches of mercury.

5. Attempts have been made in Saxony, and particularly at Freyberg, to increase the quantity of water made use of to move the machines in that country. It has been found convenient to establish pumps worked by the wind, to raise a portion of the water which passes through the galleries, or is collected in the works. It is not intended to make use of this inconstant first mover to clear the mines of water, but simply to fill the reservoirs which work the hydraulic machines, at the same time that the secondary advantage of assisting to keep the mines clear is obtained. By this means the power of the wind, changeable as it is, may be rendered steadily useful. The celebrated Leibnitz first thought of this con-

* Journal des Mines, No. 29.

trivance, and endeavoured, though at that time without ſucceſs, to carry it into effect. I apprehend that his reſervoir may not have been ſufficiently capacious. Attempts have been made in England, to render the action of ſmall ſtreams more ſteady and conſtant by the intervention of a reſervoir, which ſhould retain a large portion of the water when wet weather rendered it plentiful, and ſupply the defect at ſuch times as the water failed for want of ſufficient rain.

6. An experiment has been made in the large way at the ſalt-work of Artern, ſituated in the circle of Thuringia, dependent on the electorate of Saxony, on the poſſibility of obtaining ſea ſalt merely by the heat of the ſun, after having brought the ſalt water to as high a degree of concentration as the proceſs of graduation is capable of affording. This ſalt work was the firſt eſtabliſhment of the kind in Saxony, by Mr. Borlach, to whom the undertakings of this nature are ſo much indebted ; and it will probably have the honour of being the firſt in which this new proceſs ſhall ſucceed. Experiments on a ſmaller ſcale have already afforded the higheſt hopes of ſucceſs. Thoſe which have been attempted in the large way, though at the end of the warm weather, have afforded encouraging reſults.

For this purpoſe a number of veſſels of wood have been placed in a field upon poſts, at the height of five or ſix feet from the ground. They can be covered or uncovered in an inſtant by a moveable roof made of thin boards, accordingly as the weather is clear or rainy. Though the ſummer was nearly over when the experiments began, ſalt was obtained in this manner by the mere heat of the ſun ; and this ſalt was much purer, and of a more lively and agreeable taſte, than that which is obtained by evaporation in boilers. There is every reaſon to expect that the whole of the ſalt which can be obtained at theſe works, will be ſeparated in this manner without the uſe of any combuſtible. A great number of theſe caſes are accordingly prepared, to give all the neceſſary activity to this method of operating next year.

Citizen Charles Coquebert, in a note on this ſubject, remarks that the celebrated Haller publiſhed, in the Memoirs of the French Academy for the year 1764, a ſet of experiments on the evaporation of ſalt-waters made at the works in the canton of Berne, of which he was the director; and adds, that the experiments are intereſting, but that the economical calculations are grounded on ſuch erroneous foundations, that they would ſerve only to miſlead thoſe who from the reputation of the author might uſe his calculations relative to any undertaking in the large way.

V.

*An Analyſis of the Earthy Subſtance from New South Wales, called Sydneia, or Terra Auſtralis. By CHARLES HATCHETT, Eſq. F.R.S.**

IN 1796, the Right Hon. Sir Joſeph Banks, P.R.S. favoured me with a ſpecimen of the Sydneia, which had been lately brought to England. A portion of this I ſoon after examined,

* From the Philoſ. Tranſ. 1798. The introductory part of Mr. Hatchett's paper conciſely ſtates the reſults of Wedgwood's paper (from which an extract is copied in Philoſ. Journ. I. 405);—that Profeſſor Blumenbach, in

ed, in a curfory manner, by muriatic acid, but did not obtain any precipitate when water was added to the filtrated folution.

Upon mentioning this circumftance, and expreffing a defire to examine this fubftance with more accuracy, Sir Jofeph Banks, with his ufual readinefs to promote every fcientific enquiry, not only permitted me to take fpecimens from different parts of the box which contained the earth already mentioned, but (that every doubt might be obviated) gave me about 300 grains which remained of the identical fubftance examined by Mr. Wedgwood.

Upon thefe the following experiments were made; and, to diftinguifh them, I fhall call the firft, No. 1, and that examined by Mr. Wedgwood, No. 2.

SECT. II.

Analyfis of the Sydneia, *No.* 1.

THE Sydneia, No. 1, is in maffes and lumps, of a pale greyifh white, intermixed with a few particles of white mica, and alfo occafionally with fome which are of a dark grey, refembling graphite or plumbago.

It eafily crumbles between the fingers, to a powder nearly impalpable, which has rather an unctuous feel.

Small fragments of vegetable matter are alfo commonly found intermixed with it; and the general afpect is that of an earthy fubftance which has been depofited by water.

Experiment 1.—400 grains were put into a glafs matrafs, and one quart of diftilled water being added, the whole was boiled to one-fourth.

The liquor was then filtrated, and a portion being examined by the re-agents commonly ufed, afforded no trace of matter in folution. The remainder was then evaporated, without leaving any refiduum.

Experiment 2. About 200 grains of the earth, rubbed to a fine powder, were put into a glafs retort, into which I poured three ounces of concentrated pure muriatic acid. The retort was placed in fand, and the acid was diftilled, till the matter in the retort remained dry. Two ounces of muriatic acid were again poured on it, and diftilled as before, till only one fourth remained. The whole was then put into a matrafs, which was placed in an inclined pofition, fo that, when the earth had fubfided, the liquor might be decanted without difturbing the fediment.

When it had remained thus for 12 hours, the acid was carefully poured into a glafs veffel: but, as I obferved that it was not fo perfectly tranfparent as before it had been thus employed, I fuffered it to remain 24 hours, but did not perceive any fediment. Half of this liquor was diluted with about twelve parts of diftilled water, and, after a few hours, a very fmall quantity of a white earth fubfided.

in his Handbuch der Naturgefchichte, p. 567, 568, mentions that he had examined a portion of this earthy fubftance, by means of muriatic acid after the manner of Wedgwood, and obtained a flight precipitate;—that M. Klaproth had alfo examined it, whofe refults he likewife gives (for which fee the page of our Journal laft mentioned);—and that the identity of the fubjects examined by Wedgwood and Klaproth had been much queftioned by me. The author then proceeds to relate the hiftory and particulars of his own analyfis, of which the words of the text are an exact copy. N.

This

This however did not appear to me to be a precipitate caused by a change in the chemical affinities, but rather an earthy matter which had been suspended in the concentrated acid, and afterwards deposited, when the liquor was rendered less dense by the addition of water. To ascertain this, I poured the remaining portion of the concentrated liquor on a filter of four folds: it passed perfectly transparent, and, although diluted with twenty-four parts of water, it remained unchanged, and as pellucid as before. I now filtrated the former portion, and added it to that already mentioned.

It was then evaporated to dryness, and left a pale brownish mass, which was dissolved again, by digestion, in the smallest possible quantity of muriatic acid.

Water was added, in a very large proportion, to this solution, without producing any effect; I then, by prussiate of potash, precipitated a quantity of iron, which was separated by a filter.

The clear solution was then saturated with lixivium of carbonate of potash, and a white precipitate was produced, which was collected and edulcorated. This, when digested with diluted sulphuric acid, was dissolved; and the superfluous acid being driven off by heat, boiling water was poured on the residuum, and completely dissolved it.

To this solution some drops of lixivium of potash were added, and, by repeated evaporations, the whole formed crystals of alum.

From the above experiment it appeared, that the muriatic acid had only dissolved some alumine and iron; but, in order to satisfy myself more completely in respect to the component parts of this substance, I made the following analysis.

Analysis. A. 400 grains were put into a glass retort, which was then made red-hot during half an hour. Some water came over, and the earth afterwards weighed 380.80 grains, so that the loss amounted to 19.20 grains. The greater part of this loss was occasioned by the dissipation of the water imbibed by the earth; to which must be added, the loss of weight caused by the combustion of a small portion of vegetable matter.

B. The 380.80 grains were rubbed to a fine powder, and being put into a glass retort, 1470 grains of pure concentrated sulphuric acid were added. The retort was then placed in a small reverberatory, and the fire was gradually increased, till the acid was distilled over: it was then poured back on the matter in the retort, and distilled as before, till a mass nearly dry remained.

On this, boiling distilled water was repeatedly poured, until it no longer changed the colour of litmus paper, and was devoid of taste. The undissolved portion was then dried, and made red-hot; after which it weighed 281 grains.

C. I now mixed the 281 grains with 300 grains of dry carbonate of potash, and exposed the mixture to a strong red heat, in a silver crucible, during four hours. The mass was loose, and of a greyish white: it was softened with water, and, being put into a retort, sulphuric acid was added to a considerable excess. The whole was then distilled to dryness; and when a sufficient quantity of boiling water had been added, it was poured on a filter, and the residuum was well washed: it was then made red-hot, and afterwards weighed 274.75 grains.

D. The solutions of B and C were added together, and were much reduced by evaporation. Pure ammoniac was then employed to saturate the acid, and a copious loose precipitate

pitate of a pale yellowish colour was produced; which, collected, edulcorated, and made red-hot, weighed 103.70 grains.

E. The filtrated liquor of D was again evaporated; and carbonate of potash being added, a slight precipitation of earthy matter took place; which, by the test of sulphuric acid, proved to be some alumine which had not been precipitated in the former experiment: this weighed 1.20 grain.

F. The 103.70 grains of D were completely dissolved when digested with nitric acid, excepting a small residuum of siliceous earth, which weighed 0.90 grain.

G. The nitric solution was evaporated to dryness, and a second portion of the same acid was added, and in like manner evaporated. The residuum was then made red-hot, and digested with diluted nitric acid, which left a considerable portion of red oxyde of iron. The solution was again evaporated, and the residuum, being treated as before, again deposited some oxyde of iron, much less in quantity than the former.

The whole of the oxyde was then heated with wax in a porcelain crucible, was taken up by a magnet, and weighed 26.50 grains.

H. The nitric solution of G was saturated with ammoniac, and a loose white precipitate was formed; which, edulcorated and made red-hot, weighed 76 grains.

I. These 76 grains were dissolved when digested with diluted sulphuric aid; and, when the excess of acid had been expelled by heat, the saline mass was dissolved in boiling water. To this solution I added some lixivium of potash, and, by gradual and repeated evaporations, obtained the whole in regular octoedral crystals of alum.

K. The 274.75 grains of C now alone remained to be examined. They appeared to consist of siliceous earth, mixed with the dark grey shining particles already mentioned; but, as I shall describe, in the following experiments, the process by which these were separated, I shall now only say that they amounted to 7.50 grains.

L. The earth with which the abovementioned particles were mixed weighed 267.25 grains. This earth was white, and arid to the touch: when melted with two parts of soda, it formed a colourless glass; and with four parts of the same it dissolved in water, and formed a *liquor silicum*: it was therefore pure siliceous earth or silica.

The substance here examined was composed therefore of the following ingredients:

		Grains.
Pure siliceous earth or silica -	F.	0.90
	L.	267.25
Alumine - - - -	E.	1.20
	H.	76
Oxide of iron - - - - -	G.	26.50
Dark grey particles - -	K.	7.50
Water and vegetable matter -	A.	19.20
		398.55

The foregoing analysis was repeated several times, and always with similar results; excepting that, as I had taken the specimens from different parts of a large quantity, I found that the proportions of the ingredients were not constantly the same: that of the siliceous

 earth,

earth, for example, was ſometimes greater, and the alumine and iron proportionably leſs. Some ſpecimens were alſo nearly or totally deſtitute of the dark grey ſhining particles: in ſhort, every circumſtance was ſuch as might be expected from a mixed ſubſtance, which, from the nature of its formation, cannot have the ingredients in any fixed proportion *.

As this ſubſtance agreed in its general characters, for the greater part, with that deſcribed by Mr. Wedgwood, and as it was indiſputably brought from the ſame place, there appeared every reaſon to believe that the nature of both was the ſame; but, to obviate as much as poſſible any doubt or objection, I determined to repeat the experiments, and the analyſis, on that portion which remained of the identical ſubſtance examined by Mr. Wedgwood, and which from that period had been reſerved by Sir Joſeph Banks, who kindly favoured me with it for this purpoſe.

SECT. III.

Analyſis of the Sydneia, *No.* 2.

THIS ſubſtance, as has already been mentioned, conſiſts of a white tranſparent quartzoſe ſand, a ſoft opaque white earth, ſome particles of white mica, and a quantity of dark lead-grey particles, which have a metallic luſtre.

The Sydneia, No. 2, appears chiefly to differ from No. 1, by being more arenaceous, and by a larger proportion of the dark grey particles. Many experiments, ſimilar to thoſe made on No. 1, already deſcribed, were made on this ſubſtance, with pure concentrated muriatic acid; but, as none of theſe afforded any appearance of a precipitate by the means of water, I do not think it neceſſary to enter into a circumſtantial account of them, and ſhall proceed therefore to the analyſis.

A. 100 grains were expoſed to a red heat, in a glaſs retort, and, after half an hour, were found to have loſt in weight 2.20 grains.

B. The 97.80 grains which remained were mixed with 300 grains of dry carbonate of potaſh, and the mixture was expoſed to a ſtrong red heat, in a crucible of ſilver, during three hours.

When cold, the maſs was ſoftened with water, and was put into a glaſs matraſs. I then added three ounces of pure concentrated muriatic acid, and digeſted it for two hours in a ſtrong ſand heat. Boiling water was then added; and the whole being poured on a filter, the reſiduum was edulcorated, dried, and made red-hot: it then weighed 85.50 grains.

C. The filtrated ſolution was evaporated to one fourth; and pure ammoniac being added, a precipitate was formed, which, after a red heat, weighed 10.70 grains.

D. One ounce of muriatic acid was poured on the 10.70 grains, in a matraſs, which was then heated. The whole of the 10.70 grains was diſſolved, excepting a ſmall portion of ſiliceous earth, which weighed 0.30 grain.

* The deſcription given by Mr. Klaproth convinces me that his experiments were made on a portion of this ſubſtance. Moreover, when my late friend Mr. Haidinger was in London, I gave him ſome of this earth for his collection; ſo that, whether Mr. Klaproth made his experiments on that which had been received by Mr. Haidinger from Sir Joſeph Banks, or from myſelf, it is not leſs certain that he operated on that which might be regarded as the genuine *Sydneia.*

E. The

E. The muriatic solution was then reduced, by evaporation, to about one fourth; to which I added a large quantity of distilled water, which did not however produce any change. I then gradually added a solution of pure crystallized prussiate of potash, and heated the liquor till the whole of the iron was precipitated; after which, ammoniac precipitated a loose white earth, which, edulcorated and made red-hot, weighed 7.20 grains. The iron precipitated by the prussiate may therefore be estimated at 3.20 grains.

F. The 7.20 grains of the white earth were digested with sulphuric acid, and, after the excess of acid had been expelled by heat, boiling water was poured on the saline residuum. The solution was then gradually evaporated, with the addition of a small portion of lixivium of potash, and afforded crystals of alum, without a trace of any other substance.

G. I now proceeded to examine the 85.50 grains of B. These appeared to consist of siliceous earth, or fine particles of quartz, mingled with a considerable quantity of the dark grey shining particles.

Mr. Wedgwood was of opinion that these were a peculiar species of plumbago or graphite. Professor Blumenbach, on the contrary, regards them as molybdæna: and Mr. Klaproth believes them to be *eisenglimmer* or micaceous iron ore.

When rubbed between the fingers, they leave a dark grey stain, and the feel is unctuous, like that of plumbago, or molybdæna: the traces which they make on paper also resemble those of the abovementioned substances, but the lustre of the particles approaches nearer to that of molybdæna.

In order therefore to determine whether or not they consisted totally or partially of molybdæna, I put the 85.50 grains into a small glass retort, and added two ounces of concentrated nitric acid. The retort was then placed in a sand heat, and the distillation was continued till the matter remained dry. The acid was then poured back into the retort, and distilled as before; but I did not observe that the grey particles had suffered any change, nor were nitrous fumes produced, as when molybdæna is thus treated.

To be more certain, however, I digested pure ammoniac on the residuum; and, having decanted it into a matrass, I evaporated it to dryness, without perceiving any vestige of oxyde of molybdæna, or indeed of any other substance.

It was evident, therefore, that molybdæna was not present; and, as the general external characters and properties corresponded with those of plumbago, I was inclined to believe that these were particles of that substance, and not micaceous iron, as Mr. Klaproth imagined. To determine this, the following experiment was made:

H. 200 grains of pure nitre in powder were mixed with the 85.50 grains, and the mixture was gradually projected into a crucible, made strongly red-hot. A feeble detonation took place at each projection; and, after a quarter of an hour had elapsed, the crucible was removed.

When cold, the mass was porous and white, without any appearance of the dark grey particles. Boiling water was poured on it; and the whole being put into a matrass, one ounce of muriatic acid was added, and digested with it in a sand heat. By evaporation it became gelatinous: it was then emptied on a filter, and, being well washed, dried, and made red-hot, weighed 75.25 grains.

The appearance of this was that of a white earth, arid to the touch. When melted

 with

with two parts of ſoda, a colourleſs glaſs was formed; and, with four parts of the ſame, it was ſoluble in water, and produced *liquor ſilicum:* it was therefore pure ſiliceous earth.

I. The filtrated liquor was ſaturated with ammoniac; and, upon being heated, a few browniſh flocculi were precipitated, which, when collected and dried, weighed 0.40 grain. This precipitate was diſſolved in muriatic acid, and was again precipitated by pruſſiate of potaſh, in the ſtate of Pruſſian blue.

The liquor from which the flocculi of iron had been ſeparated was then examined, by adding carbonate of potaſh, and, laſtly, by being evaporated to dryneſs; but it no longer afforded any earthy or metallic ſubſtance: ſo that, by the proceſs of detonation with nitre, the 85.50 grains afforded 75.25 grains of pure ſiliceous earth, with 0.40 grain of iron; and, as the dark grey ſubſtance was deſtroyed, excepting the 0.40 grain of iron above mentioned, and as 9.85 grains of the original weight of 85.50 grains were diſſipated, there can be no doubt but that this ſubſtance, amounting to 10.25 grains, was carburet of iron or plumbago; eſpecially as ſome experiments which I purpoſely made, on that from Keſwick in Cumberland, were attended with ſimilar reſults.

It is alſo evident, that theſe particles could not be *eiſenglimmer,* or micaceous iron, as nitre has little or no effect on that ſubſtance, when projected into a heated crucible.

In a ſubſequent experiment on the ſame, the crucible was removed immediately after the laſt projection; and I then obſerved that an efferveſcence, with a diſengagement of carbonic acid, took place, upon the addition of the muriatic acid, as is uſual when pure plumbago is decompoſed by nitre, and that leſs of the gelatinous matter was formed by evaporation.

The cauſe of this difference was evidently the duration of the red heat; for, in the firſt inſtance, the alkali developed by the decompoſition of the nitre had time to unite with the ſiliceous earth, ſo as, when diſſolved, to form *liquor ſilicum*; but, in the ſecond experiment, a portion of alkali remained combined with the carbonic acid, produced by the carbon of the decompoſed plumbago.

The produce of 100 grains by this analyſis was,

		Grains.
Silica - - -	D.	0.30
	H.	75.25
Alumine - - -	F.	7.20
Oxide of iron - -	E.	3.20
Graphite or plumbago -	I.	10.25
Water - -	A.	2.20
		98.40

Mr. Wedgwood ſays, that ſulphuric acid cannot diſſolve the precipitated earth, and has but little effect on the mixed ſubſtance, even when diſtilled to dryneſs; but, from the preceding experiments, I had reaſon to believe that the aluminous earth and iron would be ſeparated by reiterated diſtillation; I therefore repeated the analyſis in the following manner.

Second Analyſis of the Sydneia, *No.* 2.

A. 100 grains of the earth were put into a glaſs retort, upon which 400 grains of pure concentrated ſulphuric acid were poured. The retort was placed in a ſmall reverberatory, and

and the fire was continued till a dry mafs remained. 400 grains of the acid were again poured in, and diftilled as before. Upon the dry mafs boiling water was poured, and the whole was then emptied on a filter, and edulcorated. The refiduum, after a red heat, weighed 87.75 grains, and confifted of filiceous earth, mixed with fome mica, and with particles of plumbago.

B. The filtrated folution, by ammoniac, afforded a precipitate, which weighed 9.50 grains; and, being examined, as in the former experiment, yielded 6.50 grains of alumine, and 3 grains of oxide of iron.

The plumbago was feparated from the filiceous matter, in the manner already defcribed, and amounted to about 10 grains.

By this analyfis I obtained,

	Grains.
Silica and mica - -	77.75
Alumine - - -	6.50
Oxide of iron - - -	3
Plumbago - - -	10
	97.25

It appears therefore that the Sydneian earth, when treated with fulphuric acid, is capable of being for the greater part decompofed; and Mr. Wedgwood probably did not fucceed, becaufe his procefs was in fome refpect different, or that the diftillation was not fufficiently repeated.

I have not thought it neceffary to be more circumftantial in the account of this fecond analyfis, as the operations were fimilar to thofe of the former.

SECT. IV.

THESE experiments prove, that the earthy fubftance called *Sydneia*, or *terra auftralis*, confifts of filiceous earth, alumine, oxide of iron, and black lead or graphite.

The prefence of the latter appears to be accidental, and it probably was mixed with the other fubftances at the time when they were tranfported, and depofited, by means of water; for this appears evidently to have been the cafe, from the general characters of this mixed earthy fubftance.

The quartz and mica, which are fo vifible, indicate a granitic origin; and the foft white earth has probably been formed by a decompofition of feldt fpar, fuch as is to be feen in many places, and particularly at St. Stephen's in Cornwall. The granitic fand which covers the borders of the *Mer de Glace*, at Chamouni, in Savoy, alfo much refembles the *terra auftralis*, excepting that the feldt fpar is not in a ftate of decompofition: in fhort, the general afpect, and the analyfis, concur to prove, that the *Sydneia* has been formed by the difintegration and decompofition of granite, or *gneifs*.

Mr. Wedgwood's experiments are fo circumftantial, that had I only examined the earth laft brought to England, I fhould have fuppofed, with Mr. Nicholfon, that I had operated on a different fubftance; but, as I had an opportunity to examine, by analyfis, a portion of the fame earth on which Mr. Wedgwood made his experiments, and as I received it from

Sir

Sir Joseph Banks, the same gentleman who had furnished Mr. Wedgwood with it, no suspicion can be entertained about its identity.

Some of the experiments which I have related, and which prove that some of the finer earthy particles remained suspended in the concentrated muriatic acid, and were precipitated when the acid was diluted with water, appear in some measure to account for the mistake which has been made, in supposing that a primitive earth, before unknown, was present; but this alone will not account for many of the other properties mentioned by Mr. Wedgwood: such as,

1st. The repeated and exclusive solubility in the muriatic acid, and subsequent precipitation by water.

2dly. The butyraceous mass which was formed by evaporation. And

3dly. The degree of fusibility of the precipitated earth.

These, indeed, I can by no means explain, but by supposing that the acids used by Mr. Wedgwood were impure. This supposition appears to be corroborated by a passage in Mr. Wedgwood's paper, where he says, "Here the Prussian lixivium, in whatever quantity it was added, occasioned no precipitation at all, (only the usual blueishness arising from the iron always found in *the common acids*.")* Now if (as it seems from this expression) Mr. Wedgwood employed the common acids of the shops, without having previously examined and purified them, all certainty of analysis must fall, as the impurity of such acids is well known to every practical chemist †: but whether this was the cause, or not, of the effects described by Mr. Wedgwood, I do not hesitate to assert, that the mineral which has been examined does not contain any primitive earth, or substance possessing the properties ascribed to it, and consequently, that the Sydneian genus, in future, must be omitted in the mineral system.

VI.

The Method of making strong Artificial Magnets. By M. Coulomb‡.

I SHALL here present the methods which I have found successful in constructing artificial magnets of very great force at a moderate expence. * * * * When a steel rod or plate is required to be rendered magnetic, and two bars are used for this purpose, it is obvious that it must be of advantage to cause the poles of these bars to act in conjunction with each other. This has given rise to the method of the double touch. Fig. 1, Plate IV. shows the former practice of this method. If the bar n s be required to be impregnated, the two bars SN, S'N', were placed vertically at the distance of seven or eight lines from

* Philosophical Transactions, vol. lxxx. part ii. p. 313.

† It appears from Wedgwood's paper, that nearly one fifth part of the mineral was taken up by muriatic acid, and that his solution was reckoned to have about six grains of the soluble matter to three ounces of the acid. From the experimental process of boiling, it seems probable that the actual quantity of acid made use of, bore an higher proportion to the matter taken up. If we admit the supposition of impurity in Wedgwood's acid, the quantity of matter precipitable by water was probably less than two grains in the ounce: and after rejecting the Sydneian earth, as we undoubtedly must, it may perhaps be an object worthy of enquiry to determine what that substance was in which Wedgwood observed the peculiar properties related in his paper. N.

‡ Journal de Physique, xliii.

each

each other, more or lefs according to their force; the points S and S′ reprefenting the fouth, and the points N, N′ the north poles. In this fituation the two bars were moved from one end to the other of the bar n s.

M. Æpinus has remarked, that in this method the centre of action of the two magnets NS, N′S′ being neceffarily placed at fome diftance from their extremities at the point u for example, the action on the points of the bar n s comprehended between the two bars is made very obliquely, and confequently does not communicate as much magnetifm as the fubject is capable of receiving. Whence, inftead of placing the two bars upright in this operation, M. Æpinus advifes that they fhould be inclined, as in fig. 2; and in this pofition moved from one end of the bar to the other.

I have, in fact, found by the magnetic balance defcribed at the beginning of this memoir*, that the method of M. Æpinus is preferable to the other; but I have alfo obferved, that it does not give the needles the perfect faturation of magnetifm, and that frequently when the needle is of confiderable length, feveral poles are formed on the intermediate parts, the action of which is indeed fmall, but neverthelefs perceptible. I attribute thefe to the particular action of each magnet, which tends to produce on the points paffed over by the magnets an effect contrary to that which is defired. In our figure 2, the pole S for example being placed upon the needle, tends at the fame time to give to the point q, which is placed under the bar, the fame kind of magnetifm as at the point u; that is to fay, on the hypothefis of two magnetical fluids capable of moving towards the extremities of the needle, if the point u be drawn towards the point n, the point q which is near it will be drawn towards the point s, after this point q fhall have been paffed by both magnets. In my hypothefis, in which the magnetic fluid is not capable of moving, except in the integrant parts, the molecules u and q, which are near each other, tend to become magnetized in oppofite directions; and muft produce a diminution of magnetifm towards the extremities of the needle, where the magnetic fluid muft be moft condenfed; a circumftance which may produce feveral poles in very long needles, as is proved by experience. This obfervation, which neceffarily refults from the accurate meafures afforded by my experiments, obliged me to depart from the method of M. Æpinus. The following is that which, after various trials, has proved by the magnetic balance to be the moft advantageous.

In my operation I ufe four very ftrong magnets impregnated by a firft procefs, which I fhall prefently defcribe. I place my two ftrongeft magnets (fig. 3.) NS, NS, on an horizontal plane in one right line, at fuch a diftance that they may be a few lines nearer to each other, than the length of the needle n s intended to be magnetized. I afterwards take the two magnets N′S′, and inclining them as in the method of Æpinus, I place them firft on the middle of the needle, or with their poles nearly in contact. I then draw each magnet, without changing its inclination, to the extremity of the needle, and repeat this operation five or fix times on each face of the needle. It is clear that in this operation the poles of the needle n s remain fixed and invariable at the extremities of the needle, by means of the two ftrong magnets NS on which it refts. The effect produced by thefe can only be

* The excellent and well known method of Coulomb of meafuring the forces of electricity and magnetifm, is by fufpending an horizontal lever or bar by a fine wire at the point of fufpenfion; which bar being turned caufes the wire by its torfion to exert a determinable force againft the action intended to be meafured. N.

 augmented

augmented by the action of the two superior magnets, which concur in magnetizing all the particles of the needle in the same direction.

As the needle n s, placed between the two large magnets in the preceding operation, acquires by the joint action of the four bars a degree of polarity which is more than it can preserve when separated from them, it follows that at the moment of this separation the needle loses part of the magnetism it derived from those forces, and that its magnetism diminishes until the magnetic action of the whole needle on each of its parts is in equilibrio with the coercive force. Hence, upon separating the needle from the magnets, it is found to be saturated with magnetism.

I have found likewise, that in this method of magnetizing, there is a greater certainty of giving to both surfaces of needles, intended to determine the magnetic meridian, an equal degree of magnetism; a circumstance deserving of the greatest attention in the construction of compasses, if the needle be suspended with its broadest surface parallel to the horizon.

The Construction of Artificial Magnets.

I TAKE fig. 4. thirty bars of steel hardened and tempered to the temper of a spring * 5 or 6 lines broad, 2 or 3 lines thick, and 36 inches long. The blades of fencing foils, such as are found in the shops, make pretty good magnets. English sheet steel † (la tôle d'acier d'Angleterre) cut into pieces one inch wide, hardened and lowered to spring temper, is preferable. When each compound magnet is to contain no more than 15 or 20 pounds of steel, it is sufficient to make the bars 30 or 36 inches long.

I magnetize each bar singly, according to the method already described. I then take two rectangular parallelopipedons of very soft iron, well polished, 6 inches in length, between 20 and 24 lines broad, and 10 or 12 lines thick‡: with these two parallelopipedons represented fig. 4. at N and S, I form the armor of my magnet by enveloping one extremity of each parallelopipedon with a stratum of my magnetic bars, so that the extremities of the parallelopipedons may project beyond the extremities of the bars 20 or 24 lines, and the other part may be enveloped by the ends of the set of bars. On this first layer of steel bars of 3 or 4 lines thick, I place a second which is three inches shorter than the first, so that the first projects beyond the second 18 lines on each side. The whole is secured at the ends by two binding pieces of copper, which press the bars close together, and prevent the armor from escaping.

Fig. 4. represents two artificial magnets composed according to the method just described. N and S are the extremities of the two iron parallelopipedons. The two other extremities are inclosed by the bars. Each magnet thus compounded is solidly connected

* By other experiments, the author has ascertained that the magnetism acquired by steel is least when it is either absolutely hard or annealed by a white heat, and greatest when it has been hardened and annealed by a very obscure red heat; from which middle term it diminishes either way, whether the bar be harder or softer. N.

† We have two sorts at least in the London market. The common sort is sold retail at 6d. or 7d. per lb. the finer, under the name of cast-steel (which I suppose it to be) is sold at one shilling per lb. and deserves its price. I have not been able to procure sheet steel thicker than 1-8th of an inch. N.

‡ All the weights and measures in this paper are French; but as extreme precision is no where implied, it was unnecessary to reduce them. N.

 together

together by the copper pieces marked a, b, a′, b′. The pieces of contact A, R, join the opposite poles of the magnets.

Experience has shewn me, that with an apparatus of this form, each part weighing 15 or 20lb. a force of 80 or 100 pounds will be required to separate the pieces of contact; and that when an ordinary needle of the compass is placed on the two extremities of our two compound bars, fig. 41 they become magnetized to saturation without its being necessary to rub them with the upper pair of magnets. It is scarcely necessary to observe, that when magnets of greater force are desired, it is necessary, in proportion as the number of bars is increased, to augment their length also, and the dimensions of the parallelopipedons of iron which serve for the armor. It would be easy to ascertain the different dimensions which the magnets ought to have, in a manner sufficiently accurate for practice, from the laws of magnetism, and the position of the centre of action of the bars of steel, of different lengths and thickness, which we have explained in the course of this memoir.

VII.

On the Separation of Argillaceous Earth from Magnesia. By Mr. F. ACCUM.

To Mr. NICHOLSON.

SIR,

THE separation of argillaceous earth from magnesia, when both are combined in one substance, as is very often the case in mineral bodies, has been hitherto considered as one of the most difficult operations in the whole analysis of artificial and natural compounds. The experimental chemist, who is practically employed in the investigation of mineral substances, must be sensible of the difficulties which unavoidably accompany the present method of operation; and must likewise allow, that notwithstanding the utmost care, accuracy and skill, it is scarcely possible to avoid uncertainty in his conclusions and results. For these reasons, I beg leave to observe, that the above object may be accomplished to the utmost precision in the following manner: When both these earths are in a solution of muriatic acid in a perfectly saturated state, the argillaceous earth may be *totally* separated by means of carbonate of ammoniac; as this will only disengage that earth, and the magnesia will be retained in solution; which may then be separated by adding solution of pure potash or soda to the remaining fluid.

I am, Sir, your humble servant,

Haymarket, No. 17. FREDERICK ACCUM.

VIII.

Extracts from the Manuscripts of Leonard de Vinci. With Remarks, by J. B. VENTURI, Professor of Natural Philosophy at Modena, Member of the Institute of Bologna.*

I. ON the descent of heavy bodies combined with the rotation of the earth, De Vinci shows by a figure, that a body let fall from an eminence will continue perpendicularly over the same spot, notwithstanding the rotation of the earth, and consequently infers that it will describe a spiral line. (It is an ellipsis.)

It was at the commencement of the 16th century that the works of Nicolas de Cusa were printed, in which that author endeavoured to renew the ancient doctrine of the motion of the earth, though in a confused and metaphysical method. The writing of de Vinci is nearly of the year 1510, and shows that this notion was in a state of discussion in the minds of discerning men before the time of Copernicus. It has been asserted, that Regiomontanus supported this doctrine; but he opposes it directly in a writing preserved by Schoner, and in his commentaries on the Almagest †. The doctrine of the motion of the earth was publicly maintained for the first time at Rome, in 1533, by Widmanstat ‡, who affirmed that he had learned it from Copernicus. The work of this last did not appear till 1543. Vinci was the only man at that time who was sufficiently acquainted with mechanics to apply the theory of combined motion to the fall of heavy bodies, an application of which the honour was assumed by Gassendi in the last century §. In this state the opinion of philosophers remained, until D'Alembert demonstrated, that heavy bodies projected towards the zenith ought not to fall exactly at the place whence they set out. A similar idea has been taken up in my country. The tower Asinelli in Bologna is about three hundred feet high. A ball exactly round being let fall from this height to the earth, ought to deviate nearly six lines from the perpendicular. J. B. Gullielmini made the experiment in 1792 with great care. He could not avoid some aberrations, of which the mean result, however, confirmed the truth of a fact which had before been demonstrated by astronomy and mechanics ‖.

II. Concerning the earth divided into fragments.—L. de Vinci affirms, that if the earth were cut into fragments, and dispersed through the surrounding space, a single fragment being let fall would be carried to the common centre, which it would pass to a nearly equal distance on the opposite side, and return again nearly to the place whence it set out; and in this way the vibrations would continue:—that if all the fragments were suffered to fall at different times, they would meet, strike, and break each other, and a tumultuous

* From the "Essai sur les Ouvrages Physico-Mathématiques de Léonard de Vinci," of which notice is taken in Philos. Journal, I. 599. I have abridged the passages in many instances. All the remarks in the text are by Professor Venturi.

† Schoneri Opera, pars secunda, cart. 127. Regiomontan. in Almagestum, l. p. conclus. 5.

‡ Marini Anchiatri Pontificii, tom. 2, pag. 351.

§ De motu impresso a motore translato. Paris 1642.

‖ De diurno terræ motu experimentis confirmato. Bonon. 1792.—An experiment of this nature was proposed to the Royal Society, and a discourse read thereon by Robert Hooke, who inferred that the ball would in our latitude fall to the S. E. This, on trial about December 1679, proved to be the case. See Ward's Lives of the Gresham Professors, p. 184. N.

commotion

commotion would be produced in the atmofphere, which would continue for years, until all the parts were united about the common centre *.

III. Concerning the earth and the moon.—That the earth is a ftar; that the fcintillation of the ftars is an affection of the eye †; that the earth performs the fame office of illumination to the moon, as the moon to the earth, but with contrary phafes; that the earth in lunar eclipfes does not receive light by reflection from the moon, *nor the moon from the earth* in folar eclipfes; that the obfcure illumination of the dark part of the moon at the beginning of the firft and end of the laft quarters is produced by reflection of folar light from the earth.

IV. On the action of the fun upon the ocean.—Our author affirms that the heat of the fun caufes the waters of the fea to rife in an eminence beneath the equator, from which they flow on all fides, as may be obferved in water heated over a fire; and that the aqueous eminence following the fun in the diurnal motion is carried through about a thoufand miles per hour.

De Vinci has here applied the fame principle to the waters of the ocean, as Halley has fince ufed to explain the trade winds in the atmofphere ‡.

V. The ancient ftate of the earth.—When the water of rivers depofited its mud upon the marine animals living near the fhore, this mud impreffed itfelf upon the animals themfelves. When the fea afterwards retired, this mud became petrified all round, and within the fhells of teftacea which it had penetrated. Thefe are found in various places; and moft of the fhell-fifh petrified in the mountains have their fhells entire, particularly thofe of the greateft age and hardnefs. In anfwer to a fuppofed objection, that the influence of the ftars may have formed thefe fhells in the mountains, the author requires to be fhewn any place where this operation of the ftars is in actual progrefs to form fhells of different ages and fpecies in the fame place. And how upon that fyftem it can be explained why the gravel has become indurated in ftrata at different heights in the mountains. This gravel, continues he, has been tranfported thither from various places by the currents of rivers. It was formed of fragments of ftone, of which the corners have been worn down by the frictions, blows, and falls they have undergone in the water which rolled them to their prefent fituation. And how can this fyftem explain the great number of different kinds of leaves bedded in ftone near the tops of mountains? and the Alga, a marine plant, intermixed with fhells and fand, and petrified in a mafs with fea crabs broken and confounded with the fame fhells.

* In this fection it is evident, as Profeffor Venturi remarks, that our author has a clear comprehenfion of the inertia of matter. Another ftep in the procefs of reafoning would have given a revolution, to his falling fragment, in an appropriate orbit. The vibration of a body in a cavity through the centre of the earth to the antipodes, and the retention of the moon in its orbit by the combination of the projectile and gravitating forces are ftated in a paffage of fome length by Plutarch, in his treatife *De placitis philofophorum*, as I well remember, but have not the work at hand to quote. N.

† On this fubject it may be remarked in favour of fcintillation being a confequence of the irregular denfity of the air, and not an affection of the eye, 1. That it is lefs on lofty mountains, and countries where the air is almoft conftantly ferene. 2. That in the fame ftar it is lefs, the greater the altitude. And 3. That, contrary to a general obfervation, it may be obferved through a telefcope, provided the difc of the ftar be enlarged by deranging the focal adjuftment. N.

‡ And very lately again applied to the fea by Count Rumford. Philof. Journal, I. 573. N.

The

The ſea changes the equilibrium of the earth. The oyſters and other ſhells which are formed in the mud of the ſea, atteſt the change which the earth has undergone about the centre of the elements. Large rivers always wear away the earth which they detach by friction from their beds. This corroſion diſcovers to us many banks of ſhells heaped together in different layers, and the ſhell-fiſh have lived in the ſame place when the waters of the ſea covered them. Theſe banks in the courſe of time have been covered by other ſtrata of mud of different thickneſſes; ſo that the ſhells have been bedded in the mud heaped above them in ſuch a manner as to riſe above the ſurface of the water. At the preſent time theſe beds are at the height of hills and mountains, and the rivers, by wearing them away, diſcover the ſtrata of ſhells at their ſummits. Here then is a portion of the earth become lighter, which continually riſes while the oppoſite parts approach nearer and nearer to the centre of the earth; and that which was formerly the bottom of the ſea is now become the ſummits of the mountains.

When a river forms banks of mud or ſand, and afterwards quits them, the water that runs from theſe maſſes ſhews the manner in which the mountains and valleys may by degrees be formed in a ſoil riſing from the bottom of the ſea, though this ground might at firſt have been nearly plain and uniform. The water which flows from this land elevated from the bottom of the ocean, begins to form currents at the lower parts, and excavates the beds of rivulets which receive the fluid from the neighbouring parts. The rivulets, afterwards fed by the rain waters, become broader and deeper every day, and are converted into torrents paſſing through ravines: they unite into rivers, and by continually wearing away their banks they convert the land between them into mountains. The rains have inceſſantly ſwept and degraded theſe mountains. The elevated rock remains ſurrounded by the air; the earth of the ſummit and its ſides has deſcended to its baſe, and, by raiſing the bottom of the ſea which ſurrounded that baſe itſelf, has forced it to retire to a diſtance.

Vinci is here the firſt among modern philoſophers who maintained that the greateſt part of the continents have formerly exiſted at the bottom of the ſea. We cannot reject this doctrine, which is proved by every geological obſervation; but we do not yet diſcern the means of reconciling the ſucceſſive tranſportation of the ſea, on the ſurface of the globe, with the laws of gravitation. L. de Vinci offers an explanation which may well deſerve ſome examination. It is different from the opinion of Bernier, who has aſcribed a motion to the centre of gravity, without changing the arrangement of the ſolid parts of the earth. But the profound geometer, who has lately traced the ſyſtem of the world, agrees that the obſervations of Bouguer and Maſkelyne on the attraction of mountains do not entirely determine the denſity of the interior part of the earth *. We may therefore ſtill ſuppoſe at preſent, as Vinci does in his writings, that the fluid maſs in our globe is equal, or may be even greater than the ſolid portion: or, if this hypotheſis be not admitted, it will be ſufficient if we ſuppoſe that there are ſeveral detached ſolid maſſes in our globe, ſo that each continent may be conſidered as a ſmall part of the total maſs of the earth. Whence the maſs of matter tranſported by the rain during the lapſe of centuries from the ſummits of mountains to the bottom of the ſea, may be ſufficiently great, in proportion to the maſs of each continent, to cauſe thoſe continents to emerge more and more above the ſurface, or

* Expoſition du Syſtême du Monde, par P. S. La Place.

to

to cause them to rise on one side, while they become immersed on the other, until at length some great shock may reverse them entirely, or cause some new masses to rise from the bottom of the sea. This will be, it may be said, an hypothetical notion; but it is a notion which shews that de Vinci contemplated the cause of the phenomenon in a way sufficiently agreeable to the mechanism of gravitation. No philosopher since his time has yet thought of a more satisfactory explanation *.

VI. Concerning flame and air.—Where flame is produced, a current of air takes place around, which is necessary to preserve and augment the flame. The stronger the motion of the air, the more brilliant the flame and the greater the heat. Fire incessantly destroys the air which nourishes it; and it would produce a vacuum, if other air did not rush in and fill it. When the air is not in a state proper to receive the flame, neither flame nor any terrestrial or aërial animal can live. No animal can live in a place where flame cannot live.

Smoke is produced in the centre of the flame of a candle, because the air which enters into the composition of the flame cannot penetrate to its interior part. It is arrested at the surface of the flame, which it condenses; by becoming the aliment of the flame it is transformed into it, and leaves a void space, which is successively filled by other air.

* I must confess that the difficulties of this theory do not appear to be removed either by the observations of the author or his learned commentator. I apprehend that the hypothesis requires or asserts, that some natural process should take place to raise the land above the surface of the water, which was originally beneath that surface; and the assumed principle is, that it shall rise because lighter than an equal mass of water. The specific gravities of mineral substances are totally repugnant to this notion of floating continents, unless we suppose them to be hollow, which is by no means rendered probable to the required degree. Neither do we know of any facts which shew that the longitudes and latitudes of places on the several continents are liable to change, as must be the case if each continent were a separate mass capable of librating on a comparatively narrow face of support, as seems to be implied in one part of Professor Venturi's annotation. It appears however to be well ascertained, that the sea has formerly surmounted the level of high, and perhaps the highest mountains; and from the regularity of disposition which in many instances is observable (Philos. Journal, I. 221.) it may be doubted whether the shock of a comet (Philos. Journal, II. 42.) and the supposed change of the earth's axis of rotation are sufficient to account for its subsequent depression. I think it is scarcely disputable, that the whole of the solid mass of the earth is connected together, and that the fluid portion occupies the cavities to which it can have access in this solid. If we suppose the globe of the earth to have been formerly enveloped by a sea, whose surface was between two and three miles higher than the surface of the present ocean, or even much less elevated, the simple enquiry will be, what has become of it? It cannot have assumed the elastic state; for we know the medium weight of the atmosphere, which corresponds with little more than a shell of water covering the earth to the depth of about thirty-three feet. We are therefore led to the following objects of investigation: 1. Is the earth solid throughout, or does it afford natural cavities into which the water may, in the course of ages, have found its way? 2. As a very large part of the present surface of the earth has been thrown up by the chemical process of volcanic combustion, and these processes must have left cavities beneath the surface, it may become a matter of computation to determine the solid contents of volcanic product above the present level of the sea, in order to ascertain the quantity of depression the sea would have suffered on the supposition of its having sooner or later flowed into those cavities. 3. If, upon careful enquiry into these facts, it should be ascertained, or rendered highly probable, that the primitive sea has in a great measure disappeared by flowing into cavities in the solid mass of the earth, would not the globe of the moon afford an instance in confirmation of such a process? For the obscure parts of that globe which were at first thought to be seas are observed to be cavities, probably the beds of ancient seas. It seems likely that the component parts of the moon (whatever may be those of the other planets) are nearly the same as of the earth. Yet its atmosphere as well as its surface indicates a great want, if not the total absence of water. Has it required less time for the lunar sea to be absorbed?—Much more might be said: but on an object where facts are wanting, and conjectures plentiful, too much has perhaps been said already. N.

Muschen-

Muſchenbroek and moſt of the philoſophers of the preſent century have attributed ſcarcely any other function to the air than that of compreſſing the caloric or fuel in a ſtate of combuſtion, and blowing away the aſhes. Chemiſtry has lately proved what Vinci had before diſcerned, and Mayow and Hooke had ſuſpected towards the end of the laſt century.

VII. and VIII. On Statics.—The author explains the theory of the oblique lever, the inclined plane, and the general principle of virtual velocities in machines.

Vinci and Galileo both obſerved, that the deſcent of heavy bodies is made more ſpeedily through an arc of a circle than by an inclined plane; but their proofs are imperfect in certain reſpects. It has ſince been demonſtrated that the cycloid is the curve of quickeſt deſcent. Nevertheleſs, I find that there is a minimum of time of deſcent in a circular arc, which may be determined in a ſynthetical method of conſiderable ſimplicity, by means of the following theorem.

An arc of a circle which does not exceed 60° is a curve of ſpeedier deſcent than any other curve which can be drawn within the ſame arc:—and the arc of 90° is a curve of ſpeedier deſcent than any other curve which can be drawn without the ſame arc*.

IX. Concerning water drawn from a canal.—The quantity of water which iſſues from a canal through a given aperture, may vary from many cauſes. 1. From the greater or leſs height of the water of the canal above the aperture. 2. From the greater or leſs ſwiftneſs of the water along the ſide or bank, in which the opening is made. 3. From the greater or leſs convergence of the ſides of the aperture. 4. The greater or leſs thickneſs of the ſide of the canal. 5. Whether the aperture be circular, ſquare, triangular, or oblong. 6. Becauſe the aperture is more or leſs obliquely ſituated with regard to the bearing or direction of the ſide. 7. Or more or leſs inclined to the horizon. 8. Whether the opening be in a convex or concave part of the bank. 9. Whether there be cavities or prominences in the bed of the canal oppoſite the aperture. 10. Whether the air do or do not inſinuate itſelf into the current of water which iſſues forth. 11. Whether the water at its emiſſion fall freely in the air, or is conducted away by an open trough, or by a pipe cloſed all round. 12. Whether this conducting pipe have a greater or leſs diameter with regard to the quantity of water which deſcends in the tube itſelf. 13. Whether the ſame conducting pipe have a greater or leſs length in its deſcent. And 14. Whether the bore of this pipe be equal or rough, ſtraight or curved.

It muſt be admitted here, as well as elſewhere, that De Vinci, though poſſeſſed of a mind ſo powerful as to diſcern the true cauſes of natural phenomena, had not the advantage of modern analyſis to aſcertain and demonſtrate the quantum of action of theſe cauſes. But, on the other hand, it muſt be allowed that hydraulics even in our time has not yet perfectly and accurately determined all the laws according to which the fourteen circumſtances pointed out by Leonardo affect the quantity of water emitted through an aperture of a given magnitude.

X. Concerning circular eddies, or whirls of water.—The author marks the effect of the inertia of bodies which produces a centrifugal force in curve-lined motions. He remarks that the cavity produced in water by a whirling motion is leaſt at the bottom, becauſe there the

* For the demonſtration I muſt refer to the Eſſay, p. 19. N.

preſſure is greateſt; and that, contrary to the obſervable motion in a wheel, the parts neareſt the axis in the preſent caſe move the ſwifteſt *.

XI. On viſion.—The inverſion of images formed in the darkened chamber by rays admitted through a ſmall hole in a plate of metal are explained by a diagram, and applied to the phenomena of viſion. It is alſo remarked, that the apparent magnitude of remote objects may be enlarged by rendering the pencils of light more convergent. This is ſhewn by a figure, but the means are not perſpicuouſly explained.

XII. On military architecture.—Under this title the author gives directions for conſtructing military works; and conſiders their relative properties with regard to attack and defence. He exhibits, as his commentator remarks, a ſketch nearly complete of the art of fortification, and the attack and defence of places, at the commencement of the ſixteenth century. We obſerve platforms, buttreſſes or counterforts, ravelins, the glacis ſurrounding the ditch, trenches, advantages of the ricochet, mines and countermines, and in ſome deſigns of de Vinci there are even counterguards. It muſt however be remarked, that Leonardo was much ſuperior to the engineers of his time; in proof of which his obſervations in this ſection may be compared with what Machiavel delivers on the ſame ſubject in his Art of War, and what Albert Durer has written upon fortification †.

It is pretended that the baſtions of Verona ‡ were the firſt which were conſtructed with right-lined faces, flanked in the modern faſhion; and the merit of this invention has been aſcribed to San Michel. The baſtions of Verona were however conſtructed after the death of Leonardo, who had explained the neceſſity of that diſpoſition, and has marked embraſures in each flank of the baſtion in one of his figures.

Geuſs, in his *Théorie de l'Art du Mineur* ‖, reproaches Vallière for having robbed Peter Navarro of the glory of the invention of mines, to attribute it without foundation to Francis Georgi §. Vallière may be defended by the teſtimony of Biringuccio, who was the countryman and contemporary of Georgi. He expreſsly ſays ¶, "that it was this engineer who, being conſulted by Navarro reſpecting the means of taking the caſtle of Naples, propoſed and executed the mines, and that the glory remained with the commander General Navarro, becauſe renown diſpoſes itſelf on the ſide of great men." But, in fact, the invention of mines with gun-powder is more ancient than either of theſe perſonages. The Genoeſe executed them in 1487 **. The manuſcript N° 7239 of the French National Library was written by an Italian who lived at the commencement of the 15th century. The author explains very clearly (fol. 32) the manner of taking a caſtle ſituated on a mountain, by the operation of mines ††.

XIII. Concerning certain inſtruments.—1. The proportional compaſſes. Its centre is moveable; and, as the author remarks, it may be uſed for the proportions of incommenſurable quantities, and alſo to form an oval which ſhall have a given ratio to a given circle.

* Profeſſor Venturi has treated this ſubject in his Recherches Expérimentales, &c. p. 58, for an account of which work ſee our Journal, I. 525.

† De urbibus arcibus caſtellis, fol. Paris, 1535.

‡ Maffei Iſtoria di Verona, part. 3, cap. 4.

‖ In 8vo. Maſtricht 1778. § 21.

§ Folard, tom. iii.

¶ Pyrotechnia, liv. 10. cap. 4.

** Guicciardini Iſtoria, lib. vi.

†† Mr. Venturi gives a more particular account of this manuſcript and its contents in the notices at the end of the preſent treatiſe.

circle. M. Venturi informs us, that from a drawing in the manufcript it does not appear to have been different from the inftrument now in ufe. 2. An inftrument to fhew the conftitution and denfity of the air, and when the weather inclines to rain. From the figure it appears to have been a balance loaded at each end, and provided with a graduated circle to fhew its changes of inclination. M. Venturi thinks that one of the weights in equilibrio was a body proper to become charged with the humidity of the atmofphere. This does not very well agree with the confideration of denfity, though it muft be confeffed that the equal magnitudes of the two weights in the drawing do not give much countenance to the notion of its having been a ftatical barofcope. 3. A leathern cafe or clothing for a diver in the pearl fifhery. 4. A piece *(bâton)* which locks alternately in the teeth of a crown wheel to operate like the verge of the balance in clocks; but for what purpofe is not faid *.

XIV. Two chemical proceffes.—1. A flaming ball compofed in the following manner:—Take the charcoal of willow, nitre, brandy, refin, fulphur, pitch, and camphor. Mix the whole together over the fire. Plunge a woollen cord in the mixture, and form it into balls, which may afterwards be provided with fpikes. Thefe balls being fet on fire, are thrown into the enemy's veffels. It is called the Greek fire, and is a fingular compofition, for it burns even upon the water. Callinicus the architect taught this compofition to the Romans (of Conftantinople), who derived great advantage from it, particularly under the emperor Leo, when the Orientals attacked Conftantinople. A great number of their veffels were burned by means of this compofition.

2. Oil fit for painting. Choofe the moft perfect nuts, take off the fhell, foak them in a veffel of water to feparate the fkin; then leave the kernels in clear water, which is to be changed as often as it becomes turbid, which may be fix or eight times. After a certain time, the nuts on being ftirred will be found to mix with the water, and form a folution refembling milk. Expofe this in plates to the open air, and the oil will rife to the furface. In order to obtain this oil very clean and pure, provide cotton wicks, one end of which is to be dipped in the oil, while the other part paffes over the border of the veffel, and defcends into the neck of a glafs phial two inches below the furface of the liquor in the plate. The oil will by degrees filtrate along the wick, and pafs very fine into the phial, all the impurities remaining in the plate. All the oils of grain and of nuts are equally clear. It is the manner of extracting them which renders them foul.

The compofition of the Greek fire here given by Vinci, is found nearly in the fame words in Baptifta Porta†, whence it appears that both authors derived their information from the fame fource. The modern difcoveries in chemiftry refpecting combuftion have difclofed the whole fecret of compofitions which burn without accefs to the atmofphere, but by means of oxygene afforded from nitre. The balls defcribed by Frezier ‡, which fhine on the furface of water, are nothing elfe but the Greek fire.

XV. Concerning method.—It is always of advantage to the underftanding to acquire

* The interefting commentary or note on this laft paffage has already been given, for the moft part, at page 51 of the prefent number.

† Mag. Natur. lib. xii. cap. 2.

‡ Des feux d'artifices, edit. de 1747, p. 355. See alfo p. 332.

knowledge

knowledge whatever may be its nature; we may afterwards choose the good, and reject the useless. (Qu.?)

Theory is the General; Practice is the Army.

Experiment is the interpreter of the contrivances of nature. This interpreter never deceives us. Our judgment sometimes misleads itself in expectation of consequences which experiment refuses to verify.—We must recur to experiment, and vary the circumstances until we have deduced general rules, for it is experience which affords the true rules.—But you may ask, What is the advantage of these rules? I reply, that they direct our researches into nature, and the operations of art. They prevent us from deceiving ourselves or others by the promise of results which cannot be obtained.

There is no certainty in sciences to which some part of the mathematics cannot be applied, or which do not depend in some manner upon mathematical knowledge.

In the study of sciences which depend on the mathematics, those who do not consult nature, but authors, are not the children of nature. I would call them her grand-children. Nature, in fact, is the sole leader of true genius. But such is the folly of men. He who chooses rather to learn from the original source, instead of applying to authors, becomes an object of derision for his singularity *.

IX.

Observations and Experiments on the Formation of Sulphate of Soda, or Glauber's Salt, in Salt Waters, at a Temperature beneath the Freezing Point of Water, and upon an easy Method of disengaging all the deliquescent Salts. By M. Gren †.

THE influence of temperature upon the established laws of mutual decomposition of the neutral and middle salts, is a circumstance very essential to be observed. Many salts are not decomposed above the temperature of freezing water, though very well beneath that temperature. Such, for example, are the sulphate of alumine and the muriate of soda; the sulphate of magnesia and the same muriate.

The formation of Glauber's salt during congelation in salt water, wherein previous analysis did not discover it, had long since engaged the attention of the author. He concluded that the sulphate of soda owed its origin to the mutual decomposition of sulphate of lime and muriate of soda, at a temperature below zero. A more accurate analysis of these waters after the severe cold of 1794 and 1795, proved that he was mistaken, and that the sulphate of soda had been formed by the double decomposition of the muriate of soda and the sulphate of magnesia. The excessive cold of 1785 had already exhibited this decomposition to Scheele, and he had shown that sulphate of soda was produced from a mixture of two parts of the solution of sulphate of magnesia, and one part of the muriate of soda, exposed to the temperature of ice.

* Professor Venturi intends to publish as speedily as circumstances will permit, in three complete treatises, the whole works of De Vinci on mechanics, hydraulics, and optics. N.

† The memoir of M. Gren was addressed by himself in manuscript to the Council of Mines in France, who published it in the xxviith Number of the Journal des Mines. Citizen Descotils abridged the same in the xxivth volume of the Annales de Chimie, page 121. The above paper is a translation from his abridgment. N.

M. Gren

M. Gren was convinced by experiment, that the sulphate of magnesia in salt waters forms at a sufficient degree of cold all the sulphate of soda which can then be afforded, and of which they did not before contain the smallest particle. He endeavoured, but constantly without success, to reproduce the muriate of soda, and the sulphate of magnesia, by an elevation of temperature. So that salt waters which contain sulphate of magnesia change their nature; and their composition is no longer the same when they have been subjected to some degrees of cold below the freezing point; and a subsequent increase of heat will not restore their former state.

The quantity of muriate of magnesia in salt waters which contain sulphate of magnesia, is increased by a temperature beneath zero. It is therefore productive of a real loss to suffer the salt water to remain in the basons during the winter, because they will afterwards afford a muriate of soda of a bad quality, and the sulphate of soda which is obtained will not compensate for this disadvantage. It appears likewise that the hope of concentrating sea water by frost is chimerical.

It was an important question to be resolved in the art of making salt, what might be the means of separating the deliquescent salts even before the evaporation. In this respect M. Gren has succeeded. His processes are founded on the decomposition, well known to chemists, of muriate of lime by sulphate of soda, and muriate of magnesia by caustic lime.

If the water contain only muriate of lime with the muriate of soda, the sulphate of soda is sufficient: if there remain muriate of magnesia and sulphate of soda, lime will suffice. In this case muriate of lime is formed, which afterwards decomposes the sulphate of soda by double affinity.

If the water contain the muriate and sulphate of magnesia without sulphate of soda, it will be necessary to employ lime and sulphate of soda at the same time.

The same substances must also be employed, if the water contain the muriates of lime and magnesia together; which then entirely exclude the sulphates of soda and of magnesia.

Lastly, if the sulphate and the muriate of magnesia be present with the sulphate of soda, lime only need be employed, unless the quantity of the sulphate of soda be found insufficient. In this case a portion of the salt must be added.

These processes introduce no foreign salt into the waters, because they are already saturated with sulphate of lime, which does not render the muriate of soda more impure, because it separates in the state of schlot. They have the advantage to clear it of the sulphates of soda and magnesia, which alter the purity of the muriate of soda, and render it bitter. Besides which, in the practice, no mother water remains. The whole of the fluid may be evaporated to the last drop, and all the salt extracted, which will constantly be pure marine salt. The directors of salt works will easily perceive how much profit will thus be obtained with regard to time and fuel consumed in the evaporation and drying of the salts. But this is merely a secondary profit, greatly inferior to the principal advantage of obtaining a pure salt not deliquescent, but capable of resisting the impression of the air, and of being preserved and transported without loss.

The strongest objection against the use of these processes may arise from the necessity of purchasing sulphate of soda; and this objection would be very well founded, if the salt waters were not for the most part of such a nature as to afford it themselves at a small expence; that is to say, by their maceration at a temperature below zero. It would be sufficient

cient to leave a certain quantity in the bafons expofed to the cold during the winter. It would not indeed be proper to extract the common falt from thefe waters, after having feparated the fulphate of foda, becaufe they would then contain too great a quantity of muriate of magnefia. It would be more advantageous to add fulphate of iron to prepare the fulphates of foda and of magnefia.

With regard to fuch waters as do not contain any fulphate of magnefia, which is very rare, it would ftill be of advantage to purify them by thefe means; becaufe the water muft be very bad, and highly charged with deliquefcent falts, to require more than fifty or feventy-five pounds of fulphate of foda for thirty quintals of falt obtained by evaporation.

In order to make ufe of this method, it is proper to be well acquainted with the principles which enter into the compofition of the water intended to be purified. It then will be known whether lime alone, or fulphate of foda, or both fubftances together, ought to be ufed. Immediate experiment muft afterwards determine what quantity ought to be ufed to infure the moft perfect fuccefs.

X.

A Report made to the Council of Trade and Mines in Spain, on a new Kind of Wood for Dyeing, named Paraguatan. By D. DOMINIQUE GARCIA FERNANDEZ, *Infpector of Coinage* *.

IN compliance with the order of the Supreme Council of Commerce and Mines I have undertaken a chemical examination of the wood known at Guiana by the name of Paraguatan; which I have carried as far as appeared neceffary to afcertain the nature of the wood, and the advantages to be derived from it. In the firft place I obferved that the bark, the wood properly fo called, and the leaves of paraguatan afford different colours. The leaves do not deferve notice, becaufe they produce only a falfe and difagreeable colour. My refearches were chiefly directed to the bark, which is the part moft deferving of attention. But my remarks upon the bark may alfo be extended to the wood; for, though this affords a different colour, it prefents nearly the fame phenomena as the bark itfelf.

When the bark is boiled in water it affords a coloured extract, which, when expofed to the action of the fulphuric, muriatic or nitric acid, refifts their agency for a longer time than brazil or logwood does. The colour may be revived by means of alkalis, after it has been deftroyed by combination with acids.

Vinegar, lemon juice and tartar render this colour more brilliant. They give it a fine rofe colour; whereas thefe acids entirely deftroy the colours of brazil and logwood.

The fecula of the bark of paraguatan fixes and attaches itfelf to wool, cotton, and filk. The colour is brighter on filk than on wool, and brighter on wool than on cotton.

The fame fecula dried is afterwards foluble in alcohol. It communicates to it a tinge fimilar to that afforded by cochenille.

By mixing alum with a very concentrated decoction of the fame fubftance, a kind of

* Tranflated from the Spanifh by Cit. Venturi, Annales de Chimie, XXIII. 320.

lac

lac is obtained, which is neither fo bright nor fo beautiful as is obtained from cochenille by the fame procefs.

The fame decoction mixed with nut galls afforded me a precipitate of a faint rofe colour. The infufions of brazil wood and logwood mixed with the infufion of galls affume a deeper and browner colour. Ours, on the contrary, becomes brighter, and affumes a pale tinge of the rofe, or fome other fimilar and delicate colour.

It muft be confeffed that the colour obtained from paraguatan has not the force of that of cochenille. But it is fuperior to thofe of madder, brazil wood, and logwood. For it refifts vinegar, lemon juice, and tartar. Soap itfelf does not deftroy it fo quickly as it does thofe of brazil and logwood.

Our bark likewife affords the advantage, that, by employing it in certain dofes, and giving a due preparation to filk, we may obtain various fhades of rofe and poppy colour, which cannot be had by means of carthamus but by difficult proceffes, long wafhings, alkaline mixtures, and other embarraffing manipulations.

By examination of the external appearance of a piece of the wood of paraguatan, it appears to me to be the fame tree which Francis Correal * fays he obferved in the province of Popayan, not far from Guiana. The fame author relates, that this tree is different from that of brazil; that the trunk is of the thicknefs of a man's thigh; that its bark is filled with longitudinal indentations; that the wood and bark are of a fine red; and that the Indians ufe this wood mixed with a red earth to dye their cotton garments.

The colour obtained from paraguatan does not refift the action of light. No colour can withftand this teft. It is enough that our colour withftands it longer than thofe of brazil and logwood. I muft not conceal, that thefe two trees afford the colouring matter in greater abundance than the paraguatan.

From thefe facts I confider the paraguatan as one of the moft valuable productions which America furnifhes to Spain. It may be advantageoufly employed in the art of dyeing throughout Europe. It is to be wifhed that enquiry might be made at Popayan, in order that the earth and the wood mentioned by Correal might be procured. It is alfo defirable that the governor of Guiana fhould collect all the information which can be procured refpecting the paraguatan, and tranfmit the fame to us with other famples of the wood, the leaves and the flowers of this tree, to enable us to determine its fpecies.

The knowledge of this wood begins to be extended. For I have lately received a portion of its bark, and a red matter which an Englifhman, named *Milnes*, has obtained from Guiana. It is to be prefumed that this matter is the fame as is mentioned by Francis Correal in his travels.

SCIENTIFIC NEWS.

THE adminiftration of finances and contributions of the French Republic in Italy, having at its difpofal a great number of the moft valuable pieces of fculpture, painting, and marbles†, arifing from confifcations made of (the property of) the enemies of the French Republic,

* Voyage aux Indes Occidentales, p. 420, of the French tranflation printed in 1722.

† *Marbres*, which I underftand to denote tablets, ftatues, bas-reliefs, and every other work formed of marble. N.

Republic, being defirous of fpeedily difpofing of all thefe mafter-pieces of art, has charged citizen Hubert (who is) attached to the adminiftration of finances in quality of artift, to offer them for fale to individuals, or focieties. The lovers of the arts, academies of painting, fculpture, and the fciences, will have a matchlefs opportunity of acquiring the treafures of art, of which catalogues are in the hands of the whole world, and defcriptions in all the publications of travels through Italy.

It will therefore fuffice to offer for fale the marbles of the famous Villa Albani, the galleries of the Princes Albani, Brafchi, and other monuments, of which a catalogue will be given, with eftimates of price made by artifts, upon which a confiderable abatement will be allowed. Bills (payable) in gold or filver on neutral countries, fuch as Florence, Venice, Vienna, and even Switzerland, will be accepted. Purchafers belonging to nations at war with France will have paffports for tranfporting whatever they may purchafe, whether by land or by fea, and for embarking the fame at the ports of Rome and Civita Vecchia. The agents belonging to the fame inimical powers may, on their approach to Italy, procure all the neceffary paffports to come with all fecurity to treat and choofe themfelves the objects which may fuit their wifhes, by addreffing Citizen Hubert, rue de la Croce, at Rome. Speed and ready money are the beft means to be ufed in this bufinefs. Feb. 28th, 1798.

The foregoing advertifement was fent by the therein mentioned Hubert to Mr. Trevor, the Britifh Minifter at Turin, and by him tranfmitted to Sir Richard Worfley in London, where it arrived on the 27th of March. I obtained it of the Right Hon. Sir Jofeph Banks, Bart. It is unneceffary for me to make any comment, either political or otherwife, on its contents.

Account of the Society of Civil Engineers. [Concluded from p. 48.]

THE fame period gave rife alfo to an affociation of fome gentlemen employed as abovementioned. They often met accidentally, prior to that union, in the houfes of parliament and in courts of juftice, each maintaining the propriety of his own defigns, without knowing much of each other. It was however propofed by one gentleman to Mr. Smeaton, that fuch a ftate of the profeffion, then crude and in its infancy, was improper ; and that it would be well if fome fort of occafional meeting in a friendly way was to be held, where they might fhake hands together and be perfonally known to one another. That thus the fharp edges of their minds might be rubbed off, as it were, by a clofer communication of ideas, no ways naturally hoftile; and might promote the true end of the public bufinefs upon which they fhould happen to meet in the courfe of their employment, without joftling one another with rudenefs too common in the unworthy part of the advocates of the law, whofe intereft it might be to pufh them on perhaps too far in difcuffing points in conteft.

Mr. Smeaton immediately perceived the utility of the idea, and at once embraced it. In March 1771, a fmall meeting was firft eftablifhed on Friday evenings, after the labours of the day were over, at the Queen's Head tavern, Holborn. And from a few members at firft, it foon increafed, fo that in the fpace of twenty years they amounted to fixty-five and upwards. But of thefe there were only about fifteen who were real engineers employed in public works, or private undertakings of great magnitude.

Among thefe we find the names of Yeoman, Smeaton, Grundy, Milne, Nickalls, Jeffop, Golborne, Whitworth, Edwards, Jofeph Prieftley, Major Watfon, Boulton, Whitehurft, Rennie, Watt, and fome others. The other members were either amateurs, or ingenious workmen and artificers connected with and employed in works of engineering.

This

This aſſociation declared itſelf a ſociety, and a regiſter was kept of the names and numbers of its members. Converſation, argument, and a ſocial communication of ideas and knowledge in the particular walks of each member, were at the ſame time the amuſement and the buſineſs of the meetings.

In this manner, ſometimes well attended, and at other times not ſo, as the members were diſperſed all over England, the ſociety proceeded until May 1792, when it ceaſed to exiſt by mutual conſent of the principal members.

Some untoward circumſtances in the behaviour of one gentleman towards Mr. Smeaton, gave riſe to the diſunion. No one was ever more obliged than that gentleman (who is now deceaſed) to Mr. Smeaton for promoting him in buſineſs, and many eſſential offices in life. The offence given was done away by an apology at the deſire of the company, and by the good-nature of Mr. Smeaton; but the remembrance of it had an effect on all preſent.

Afterwards it was conceived and intended to renew this ſociety in a better and more reſpectable form. Steps were taken for that purpoſe, and Mr. Smeaton agreed to be a member. But before the firſt meeting could be held, he was no more. He died the 28th of October 1792, and their firſt meeting was in April 1793.

It was conceived it would be a better plan that the members ſhould dine together at a late hour after attendance in parliament, and paſs the evening in that ſpecies of converſation which provokes the communication of knowledge more readily and rapidly than it can be obtained from private ſtudy or books alone.

The firſt meeting of this new inſtitution, *The Society of Civil Engineers*, was held on the 15th of April 1793, by Mr. Jeſſop, Mr. Mylne, Mr. Rennie, and Mr. Whitworth.

The conſtitution was agreed on, and afterwards acceded to by all. That there ſhould be three claſſes in the ſociety. The firſt claſs as ordinary members, to conſiſt of real engineers, actually employed as ſuch in public or in private ſervice. The ſecond claſs as honorary members, to conſiſt of men of ſcience, and gentlemen of rank and fortune, who had applied their minds to ſubjects of civil engineering, and who might, for talents and knowledge, have been real engineers, if it had been *their good fortune* to have it in their power to employ others in this profeſſion: and alſo of thoſe who are employed in other public ſervice, where ſuch and ſimilar kinds of knowledge are neceſſary. And the third claſs as honorary members alſo, to conſiſt of various artiſts, whoſe profeſſions and employments are neceſſary and uſeful to, as well as connected with, civil engineering.

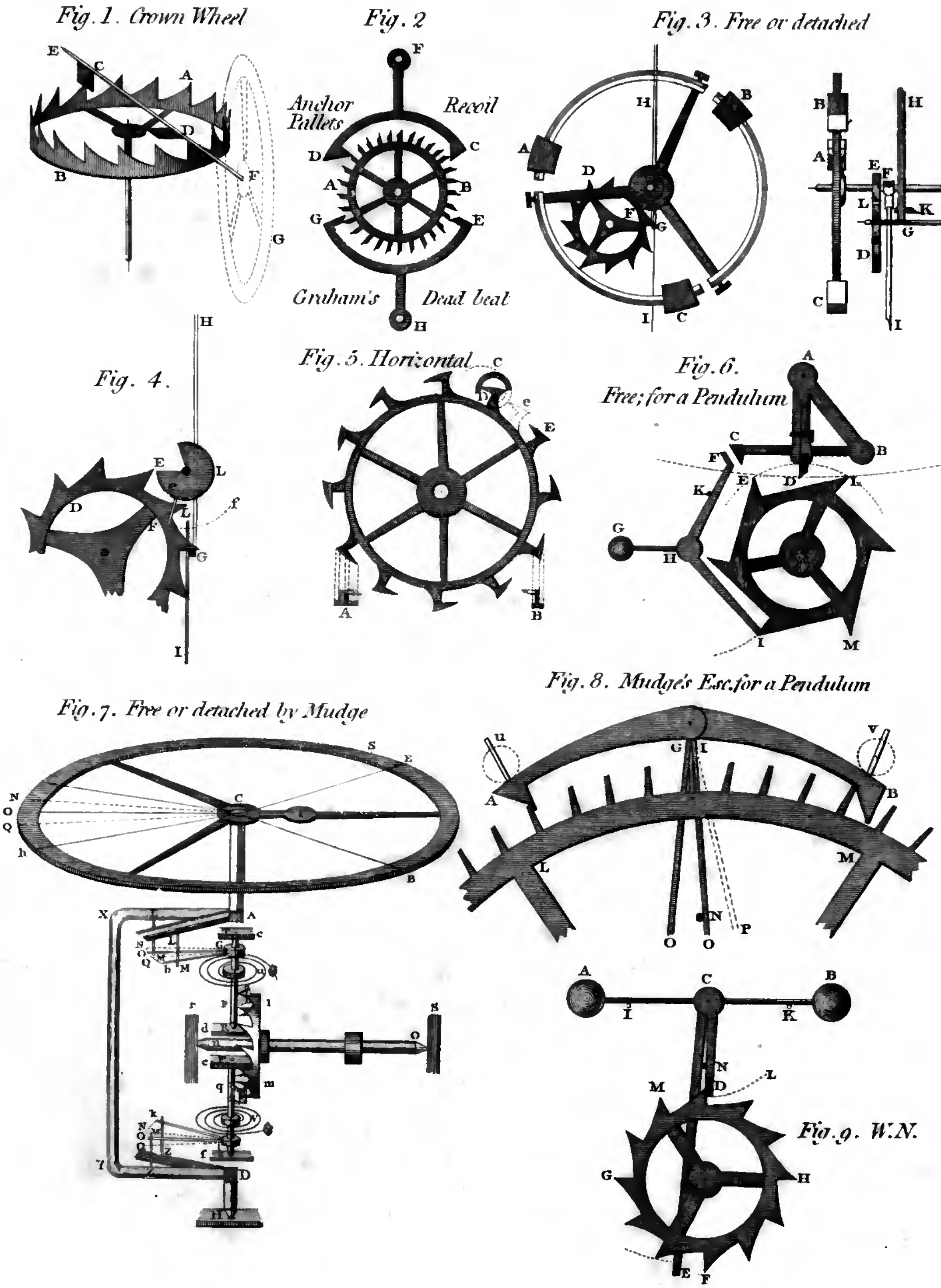
The meetings are held at the Crown and Anchor in the Strand, every other Friday during the ſeſſion of parliament. And the liſt of members are: Of the

Firſt claſs. Ordinary members. William Jeſſop, Robert Whitworth, John Rennie, F.R.S. Ed. Robert Mylne, F.R.S. James Watt, F.R.S.—L. and Ed. James Golborne, Sir Thomas H. Page, Knt. F.R.S. John Duncombe, Captain Joſeph Huddart, F.R.S. Henry Eaſtburne, William Chapman, M.R.I.A. James Cockſhutt.

Second claſs. The Right Hon. Sir Joſeph Banks, Bart. P.R.S. Knight of the Order of the Bath, &c. Sir George A. Shuckburgh Evelyn, Bart. F.R.S. General Bentham, Joſeph Prieſtley, Eſq. Doctor Charles Hutton, F.R.S. Henry Oxendon, Eſq. The Right Hon. the Earl of Morton, F.R.S. John Lloydd, Eſq. F.R.S. Rt. Hon. Ch. Greville, Eſq. F.R.S.

Third claſs. William Faden, geographer; Jeſſe Ramſden, F.R.S. inſtrument-maker, &c. John Troughton, inſtrument-maker, &c. John Foulds, mill-wright, &c. Samuel Phillips, engine-maker; Samuel Brooke, printer; John Watté, land-ſurveyor, &c.

Escapements.



Barlow sculp.

M. Coulomb's method of exciting Magnetism.

Fig. 1.

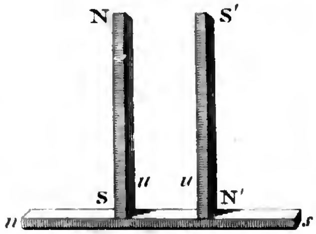


Fig. 2.

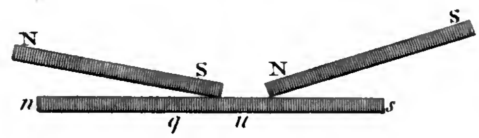
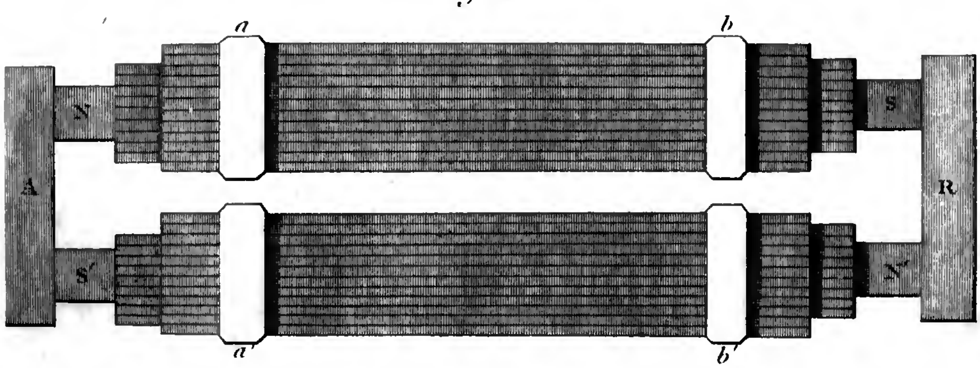


Fig. 3.

Fig. 4.



Barlow sculp.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JUNE 1798.

ARTICLE I.

Abstract of a Memoir on Camphor and the Camphoric Acid, read to the First Class of the National Institute of France. By BOUILLON LA GRANGE *.

CAMPHOR, of which the relation, the co-existence, and almost the identity with the volatile oils, have long since fixed the attention of chemists, has appeared to me to deserve a particular examination. I was desirous of confirming the discovery of Mr. Kosegarten on the property of camphor to become converted into a peculiar acid by the action of nitrous acid. The enquiries to which this undertaking has led me, have not only confirmed his discovery, but have likewise presented various facts of immediate interest to the vegetable analysis.

Camphor has long been an object of chemical attention. Several chemists have supposed that it exists in all the aromatic plants, particularly those which contain abundance of volatile oil; but as the camphor obtained from most of these plants constantly retains the odour of the plant which afforded it, there is reason to think, according to the remark of Citizen Fourcroy, that it is combined with the volatile oils of these vegetables. I hope to clear up this fact as soon as I shall procure a sufficient quantity of recent volatile oils; for my experiments have shewn that these afford the greatest quantity.

Proust, who has made experiments relative to this object, has merely indicated a process

* Annales de Chimie xxiii. 153.

for obtaining the camphor of feveral volatile oils; but he has neither defcribed its character, nor fhewn in what refpect it might differ from ordinary camphor.

The experiments we are about to defcribe may perhaps hereafter afford fome light on the nature of this fingular fubftance.

We know that the earths, the faline earthy fubftances, and the alkalis, have no action upon camphor; but as chemifts had hitherto employed the alkalis in combination with carbonic acid, it was effentially neceffary to try the action of the pure alkalis.

I fhall not here enlarge upon the various experiments I have made. It will be fufficient to remark, that I have obtained no fatisfactory refult. The camphor is always volatilized. It feems as if a fmall quantity was diffolved; but it is fenfible only to the fmell, for I have not been able to afcertain its prefence by any other means.

The acids diffolve camphor. The nitric acid is moft commonly employed for this purpofe, to form a medical preparation known by the improper name of oil of camphor.

I fhall not attempt to explain the inconveniencies and injudicious method of preparing this oil. It is known that it is impoffible to ufe it in any vehicle whatever without regenerating the camphor; confequently there remained to medical practitioners no other means but of employing this oil alone, or the camphor united to alcohol. But if we examine the action of this oil on the animal economy, it will be immediately feen that it cannot be exhibited alone.

Thefe confiderations engaged me in the fearch for the means of obtaining camphor in the fluid ftate, without the addition of an acid. Though the refult of my experiments has not been fuccefsful, it will neverthelefs be ufeful to fcience in fixing our notions refpecting the nature of this fubftance.

SECTION II.

Proceffes for obtaining the Decompofition of Camphor.

TAKE one part of camphor and fix of clay. Let the clay be pulverized in the dry ftate, and fifted through a fine fieve; on the other hand, the camphor is to be pulverized by means of alcohol, and the whole is then to be mixed together in a mortar. When the mixture is well made, add a fmall quantity of water, not too much, but enough to admit of forming the matter into a pafte, which is to be made into balls of the fize of an olive. Place thefe on a hair fieve to dry in a ftove.

When the balls are perfectly dry, let them be introduced into a retort, which is to be placed on a fand bath, with a receiver containing fome diftilled water, and well luted at the neck. In this difpofition of the apparatus the retort muft be gradually heated, and kept for feveral hours in this ftate, after which the heat is to be raifed.

If the heat be not well managed, it fometimes happens that part of the camphor fublimes to the neck of the retort. This however is no reafon for difcontinuing the procefs, which muft be carried on fo long as any oil is feen to come over. The fire is then to be put out; and when the apparatus is cold, the receiver may be unluted, and the fublimed camphor, as well as the refidue in the retort, muft be taken out. The whole is then to be pounded in a mortar, and the operation recommenced as before, until, even by a confiderable degree of heat, neither fublimation nor oil appears.

A fecond

A ſecond Proceſs.

The foregoing operation may be conducted in another manner, namely by reducing the two ſubſtances into a very fine powder, and introducing them into the retort.

This method, though apparently more ſpeedy, is in fact much leſs ſo than the foregoing; for the camphor riſes with much more facility, and the diſtillation muſt be oftener repeated, which renders the operation much more tedious.

Third Proceſs.

Alumine is likewiſe a medium which may be uſed with advantage.

The alumine is prepared by means of ammoniac; and it is afterwards waſhed with diſtilled water, in order to obtain it as pure as poſſible. It is then ſlightly dried.

Take two parts of alumine, or three, when it is wetted, to one part of camphor. The camphor is firſt to be pulverized, and the alumine afterwards added. The whole may be formed into a paſte with water, or otherwiſe the pulverulent mixture is to be introduced into the retort; but I prefer the firſt method, as I have already remarked. A receiver containing ſome diſtilled water is then to be adjuſted. The pneumatic apparatus may alſo be adapted. Diſtillation is then to be made by a gentle heat.

By either of theſe proceſſes a volatile oil of a golden yellow colour comes over into the receiver, and floats at the ſurface of the water. We ſhall proceed to deſcribe its characters.

The diſtilled water placed in the receiver is not only neceſſary to prevent the loſs of oil, which would attach itſelf to the interior ſurface, but likewiſe to abſorb a ſmall portion of camphoric acid which is formed during the operation. In fact this water is very aromatic and penetrating: it reddens the tincture of turnſol, and effervesces with the alkaline carbonates. The acid itſelf, at the end of a certain time, cryſtallizes on the ſides of the veſſel.

After the operation is completed, the matter remaining in the retort is of a very deep black colour.

Four ounces of camphor, or 122,284 grammes, may produce

Volatile oil 12 gros, or 45,856 grammes;

Carbone 8 gros, or 30,571 grammes;

Carbonated hydrogen gas; carbonic acid; camphoric acid, each in proportions which I have not been able to determine, on account of the volatility of the camphor.

SECTION III.

An Examination of the Oil obtained from Camphor in the foregoing Experiments.

THE oils obtained by the different proceſſes have no difference in their qualities.

This oil has an acrid burning taſte, and leaves a perception of coolneſs on the tongue. Its ſmell is aromatic, reſembling that of thyme or roſemary. Its colour golden yellow. When expoſed to the open air it partly evaporates, and there remains a brown thick yellow matter of a very ſharp taſte, but ſomewhat more bitter, which at length entirely evaporates.

With the cauftic alkalis it affords an homogenous mixture, foluble in water, and which has all the characters of foaps made with volatile oils.

Alcohol diffolves it entirely; and if diftilled water be added to the folution, the mixture becomes milky, but affords no precipitate.

When brought into contact with the oxigenated muriatic acid, this oil becomes very white, but no precipitate appears.

It therefore differs from that obtained by the nitric acid; becaufe, when mixed with water, with alkalis, or with the oxigenated muriatic acid, camphor is regenerated.

If the muriatic acid gas be paffed through the oil of camphor prepared by the nitric acid, its colour immediately becomes of a rofe red, and the inftant afterwards yellow. This laft colour does not vary until towards the end of the operation.

The oil itfelf does not acquire new properties.

SECTION IV.

Examination of the Matter found in the Retort.

THE matter which remains in the retort after the operation, is, as we have remarked, of a beautiful black, and, when examined as to quantity, it is found to weigh more than the alumine alone.

It confifts of carbone very accurately mixed with the alumine, and accordingly the powder is extremely fine.

In order to feparate the carbone from the alumine, and leave it in a ftate of purity, the alumine muft be faturated with an acid. For this purpofe the fulphuric acid is to be applied in the diluted ftate. This acid diffolves the alumine, and leaves the carbone difengaged, which may be feparated and dried upon the filtre. The fulphat of alumine may be obtained by concentrating the fluid in cryftals, if a fmall portion of alkali be added.

The intimate combination of the carbone with alumine led me to fufpect that this method might probably be advantageous to obtain the carburet of alumine, a preparation hitherto attempted without fuccefs, and confequently unknown to chemifts.

I therefore thought it neceffary to repeat the experiment.

I took an indeterminate quantity of this kind of carburet of alumine, which I placed under a glafs veffel filled with diftilled water, which veffel was inverted over another veffel filled with water.

At the end of a certain time bubbles of air were difengaged, which rofe to the upper part of the veffel, and gradually difappeared. The water in the veffel became depreffed, and was replaced by two gafes, one of which had all the characters of the carbonic acid gas, and the other was inflammable, and proved to be hydrogen.

This experiment confequently proves, that a fmall portion of the water was decompofed, its oxigen having feized the carbone, and formed carbonic acid, while the hydrogen became difengaged.

Notwithftanding the accuracy of this experiment, it is infufficient to prove that an intimate combination had taken place; or, in a word, that a true carburet of alumine had been formed. Neverthelefs, until experiments fhall have eftablifhed the characters which ought

ought to diftinguifh an earthy carburet, particularly that of alumine, I fhall apply this name to diftinguifh the refult of the new product obtained by the decompofition of camphor.

We cannot have any further doubt refpecting the exiftence of carbone in camphor; but we fhould render this object much more interefting, if we could prove our affertion by fynthefis. Vegetables do not, however, in all cafes, permit us to imitate Nature in her proceffes. It frequently happens, that the beft products of Art are but imperfect approximations to natural compounds.

I do not, however, defpair of our forming camphor hereafter by means of a volatile oil. Various effays on the contrary allow me to affert that it is poffible to effect this.

A few experiments which follow will render this affertion probable.

SECTION V.

Proceffes for carbonizing a Volatile Oil.

ONE ounce, or 30,571 grammes of carburet of pot-afh, or pot-afh fufed with charcoal, and 4 ounces, or 122,284 grammes of volatile oil of turpentine, or other volatile oil, were taken. Thefe two fubftances were put into a matrafs, and the whole was macerated by a gentle heat. The oil acquired a very deep black colour; its fmell was changed and became more aromatic. If in thefe experiments the oil thus changed be poured into diftilled water, the fluid becomes white, and a precipitate is formed. If the liquor be left in contact with the atmofpheric air, a few drops of oil rife and float at the furface, but the liquor continues white. From a perfuafion that this combination had formed a faponule, I faturated the pot-afh with an acid, and no true oil was feparated, but it remained fufpended in the water, which was become opaque and white.

As the addition of pot-afh rendered my experiment uncertain, I repeated it in another manner.

By the advice of Cit. Guyton, I paffed hydrogen gas (obtained by the action of fulphuric acid upon zinc) through oil of turpentine, into which I had put the powder of charcoal. I was careful to keep the oil conftantly hot, in order to promote the folution of the coal by the hydrogen gas.

In proportion as the gas was difengaged, the fluid became deeper coloured; and, inftead of continuing limpid, it became yellow, and at laft brown. If the pneumato-chemical apparatus be adapted to the veffel containing the oil, hydrogen gas is collected together with a fmall portion of carbonic acid.

The oil by this treatment has therefore taken up a fmall quantity of carbone and hydrogen, as appears by the difference between its properties and thofe of pure oil; viz. 1. It is deeper coloured: 2. Its fmell is more agreeable, *(fuave)*: 3. And, when mixed with water, this fluid becomes turbid, and the oil does not feparate but by expofure to the air, which leaves the water conftantly opaque.

It muft be admitted that thefe trials afford no fatisfactory refult. But I have no doubt but that a continued fet of experiments on this object would afford decided conclufions, or at leaft fome elucidations of utility to the progrefs of fcience.

[The remainder in our next.]

II. *In-*

II.

Instructions concerning the Manufacture of Steel, and its Uses. By VANDERMONDE, MONGE, *and* BERTHOLLET. *Published by Order of the Committee of Public Safety.*

[Concluded from Vol. ii. p. 70.]

Of Cast Steel.

CAST Steel is produced by fusion of natural steel, particularly that of cementation. The fluid state assumed by the metal in this operation causes the flaws and veins to disappear, and renders the whole mass more uniform.

According to the description which Jars has given us of the manner in which this operation is performed at Sheffield, all kinds of fragments of broken steel are used. The furnace is of the same kind as that of the brass-founder, but much smaller, and supplied with air by a subterraneous communication. At the mouth of the furnace, which is square, and level with the earth, there is an opening against a wall where a chimney is carried up. These furnaces contain only one large crucible nine or ten inches high, and six or seven in diameter. The steel is put into the crucible with a flux, the composition of which is kept secret: and the crucible itself is placed on a round brick standing on the grate. Coak is placed round the crucible, and the upper part of the furnace is filled with it. It is then set on fire; and the upper opening of the furnace is entirely closed by a covering formed of bricks, bound together with iron.

The crucible remains five hours in the furnace before the steel is perfectly fused. Several operations are afterwards made. Moulds formed of two pieces of cast iron, which fit together, and form an octagonal or square cavity, are prepared for casting the steel, which is afterwards hammered out in the same manner as blister-steel, but with less heat and more care, because of the danger of breaking it.

Chalut, officer of artillery, has made experiments on the flux which is best adapted for making cast steel. He is convinced that every kind of glass may be used as a flux, except that which contains lead or arsenic.

The steel being broken into small pieces, is to be covered with the glass. The cover of the crucible must then be put on, and the heat urged to the greatest degree of the brass-founders furnace.

It appears that an extraordinary hardness is sometimes required to be given to cast steel, and that this effect is produced by mixing coaly matter with the flux, to saturate the steel, and give it the highest degree of hardness. It is probable that certain instruments are manufactured by some process of this kind; such as cylinders and laminating rollers, of which the hardness is very great, and the grain perfectly uniform through the whole mass:—but on this subject we have nothing to offer but conjectures.

One of the greatest difficulties we find in this country (France) in the fusion of steel, is to procure good crucibles. The art of pottery, which is truly important in every one of its parts, is that which, of all others, the most strongly solicits our industry.

Concerning the peculiar Properties of the different Kinds of Steel.

CAST ſteel may be conſidered as the moſt perfect for all ſuch inſtruments as require a beautiful poliſh and uniform hardneſs. It is exempt from the flaws, dull ſpots, and veins, which are more or leſs abundant in other ſteel. This ſteel ought to be choſen for ſuch tools as require to be hard and well poliſhed; ſuch as burniſhers, watchmakers' tools, lancets, razors, and ornamental ſteel-work; but it has the inconvenience of not being welded* with iron, and is brittle. It is more difficult to treat in the fire, and is neceſſarily much dearer than common ſteel, becauſe it is obtained by fuſion of that very article.

This kind of ſteel is highly valuable to the arts which promote luxury; but we muſt direct our attention to the ſteel which forms the hatchet, the file, the ſabre, and the hammers of gun-locks.

Steel of cementation ſometimes approaches to caſt ſteel in purity, when excellent iron has been uſed, and particularly when it has been well forged; but in general it preſents ſome threads and veins, is leſs uniform in its texture, and does not become equally hard. This ſteel may be uſed for moſt articles of cutlery, edge-tools, laminating rollers, hammers, ſmall ſprings, files, &c. It is not eaſily welded to iron.

This ſteel is not only uſed for a great variety of objects, but it may be uſed in different proportions in the ſtuffs (Etoffes†) which are uſed when a material is required which ſhall be leſs ſubject to break, as in the fabrication of large ſprings, ſcythes, ſabres, &c.

The material called ſtuff (Etoffe) is a compound of iron and ſteel, ſeveral plates of which are forged and welded together, to afford a ſubſtance partaking of the properties of both. The iron ſeems to give flexibility to the ſteel, while this communicates its hardneſs and elaſticity to the iron. It appears that the art of uniting plates of ſteel and iron, and of contorting them together, is the principal requiſite in the perfection of Damaſcus ſteel.

Natural ſteel is much leſs uniform than the ſteel of cementation. When it is poliſhed, its ſurface is uſually rendered unequal by ſcabrous places, flaws or veins. It is eaſy to diſcover veins of iron with the point of the graver, ſo that it may be conſidered as a natural compoſition; whence it is found, that cutting tools made of this ſteel are leſs ſubject to fail in the edge, and that it bears heating a ſecond time very well; having, as the workmen ſay, more body, and being more eaſy to forge.

In general, according to our eminent cutler Perret, it is adviſeable to uſe the Engliſh cemented ſteel for fine and delicate work, and ſometimes even caſt ſteel; but for ſtrong and heavy work, the preference is to be given to German ſteel, which is the natural ſteel, becauſe it has more body and tenacity.

Upon the whole, it appears to us that the ſteel of cementation is moſt worthy of being attended to; becauſe it is eaſy, at a ſmall charge, to eſtabliſh manufactories on a confined ſcale, and with the deſired expedition; and becauſe this ſteel may, without difficulty, be uſed in any kind of work. Fellow citizens, let us convey to our forges thoſe expenſive baluſtrades and railing, which have nothing to defend; and if we find them to poſſeſs the qualities of good iron, let us convert them into ſteel.

* For the method of welding caſt-ſteel to iron, ſee Philoſ. Jour. I. 575. N.

† I am not aquainted with the appropriate Engliſh term. N.

The

The Method of trying Steel.

THE different properties belonging to each kind of ſteel render the proofs of their quality difficult even ro ſkilful workmen. Caſt ſteel will be thought of a very bad quality by him who is not in the habit of working it; as will likewiſe the ſteel of cementation by the workman who is commonly employed upon German ſteel. The grain of the fracture is a deceitful indication, becauſe its fineneſs varies with its hardneſs; nevertheleſs good ſteel ought always to exhibit an even grain. Caſt ſteel ought to take a good poliſh, and not be too brittle: ſteel of cementation ought to afford gravers capable of reſiſting percuſſion without notching or turning in the edge; natural ſteel ought to weld with eaſe to iron, and make good edge-tools.

There are circumſtances in which it is of advantage to aſcertain whether a manufactured article be formed of ſteel or iron without injuring it. The means of doing this is of the more conſequence to us, becauſe certain unprincipled contractors have delivered ſabres with blades of pure iron, to which a ſlight degree of rigidity had been given in the fabrication; a circumſtance which has induced the Committee of Public Safety to publiſh the directions we ſhall here tranſcribe, and to oblige its agents, entruſted with the reception of every kind of arms of ſteel, to ſubmit them to this proof.

"If a drop of nitrous acid be placed upon the ſurface of poliſhed iron, and, after having left it two minutes, water be thrown thereon, this laſt fluid will carry off the acid with all its contents, and there will remain merely a white ſpot, or the colour of iron newly cleaned.

"If the ſame operation be made on a plate of poliſhed ſteel, the acid likewiſe ſeizes the iron, but does not act on the coaly part. This laſt falls down during the ſolution, and forms a black ſpot, which the projection of the water does not remove, and which even remains for a conſiderable time, on account of its adheſion.

"In order to ſucceed in this operation, a diluted acid muſt be uſed, becauſe the coaly principle does not adhere to the ſurface, but in proportion to the ſlowneſs of the ſolution, and the weakneſs of the effervеſcence.

"If pure or rectified nitrous acid be not at hand, the aquafortis of commerce may be uſed, taking care to dilute it in a certain degree.

"The drop of acid muſt be conveyed with a ſtick of glaſs, or other material, which is not itſelf attacked, and cannot afford any thing capable of changing the reſult.

"The ſmalleſt drop ſuffices. It ought rather to be ſpread out, than kept together, that it may mark a larger ſurface. The ſtopper of a very ſmall bottle, in which the acid is kept, anſwers this purpoſe very well.

"After having made this proof two or three times on iron and ſteel compared with each other, the operator will be enabled to ſpeak with certainty reſpecting the difference of the metals."

Artiſts have long ſince availed themſelves of a method of this kind to diſtinguiſh Damaſcus blades. Theſe blades, as we have obſerved, are compoſed of the ſteel and iron intimately mixed. By this proof, ſays Perret, they preſent ſerpentine veins, ſome of a whitiſh grey, others of a deep grey, and others blackiſh, which are known by the name of the Flowers of Damaſcus.

We

We have remarked that caſt iron, ſufficiently abounding with charcoal, aſſumes the appearance of the white crude iron when it is caſt in plates or too ſuddenly cooled. To aſcertain its nature, nothing more is neceſſary than to clean its ſurface; and by the more or leſs grey or dark colour of the ſpot produced by nitrous acid, a judgment may be formed of its quality.

VANDERMONDE, MONGE, BERTHOLLET.

Annotations upon the Report concerning Steel.

1. NOTWITHSTANDING the great light which has been afforded by the labours of Rinman, Bergman, Vauquelin, and other chemiſts, together with the learned authors of the foregoing paper, we have ſtill much to learn reſpecting the cauſes of the ſeveral qualities of ſteel to be found in the market. Much of this information might probably be obtained by diligent enquiry, and the uſual methods of examination. It is yet to be ſhewn by experiment, whether pure iron united with carbon conſtitutes the beſt ſteel; or whether any additional ingredient, ſuch as phoſphorus, mangaueſe, or other metallic matter, may not be of advantage. If the former poſition be true, the beſt ſteel-iron will be that which is the pureſt; and the beſt cement will be the ſimpleſt coaly matter. On the contrary, if the latter ſuppoſitions ſhould be well founded, the goodneſs of our ſteel will depend, not only upon the component parts of the bar iron, but likewiſe on the adaptation of the cement proper for its converſion. Mr. Duhamel, in the Encyclopédie, article *Acier*, p. 462, found that the charcoal which had been uſed in making ſteel was not fit to be uſed again, by reaſon of the ſlowneſs of its operation. In this caſe it ſeems probable that, though enough of carbone remained, yet ſome more volatile ingredient had been expended in the firſt proceſs. Our authors have well explained the advantage of the iron being well forged and ſound throughout, previous to its expoſure to the cementing proceſs.

2. The grey crude iron may be conſidered as iron ſaturated with as much plumbago or carburet as it can hold in ſolution at a very elevated temperature. When it is ſuffered to cool ſlowly, as in ſoft caſting, the plumbago appears to ſeparate by haſty cryſtallization through the whole maſs, as may frequently be obſerved in its fracture, and as has been ſhewn by plunging a cold bar of iron into the fuſed metal, and withdrawing it covered with the carburet which precipitates upon it. When this iron is poured into a cold mould of metal, or ſuffered to run to a great diſtance from the aperture of reception, in ſand, or otherwiſe if a bar of this metal be heated to whiteneſs, and plunged in water, it becomes very hard; more ſo than ſteel tools of any deſcription are uſually made, and is very white, and cloſer grained in its fracture. The hardneſs muſt depend upon circumſtances in its aggregation, to explain which we poſſeſs no data; but I apprehend the whiteneſs to be an evident conſequence of the union between the iron and the plumbago continuing to be nearly as intimate as it was at the white heat. It is very probable that, in all caſes of the hardening of other ſteels containing leſs carbone, a ſimilar effect may take place.

3. The laminating rollers, concerning which our authors offer nothing but conjecture, are made of this iron caſt in ſand, or metallic moulds of conſiderable thickneſs. I am informed by founders who make them, that the external hard part does not penetrate to a greater depth than about three quarters of an inch; and that the hardneſs is not greater than that of a good graver. The proceſs of turning them in the lathe, in fact, ſhews this.

 For

For they are turned by an extremely flow motion, with a tool felected from the general ftock in the work-fhop, becaufe the greater number even of good gravers will not cut it.

4. Thofe who forge fteel know that it is very eafily degraded in the fire. If a fmall piece, for example, half a cubic inch, of grey crude iron be put into a common fire, and kept red-hot for about half an hour, and at the expiration of that time the heat be fuddenly raifed to whitenefs by the bellows; the internal or fteely part will break its way through the external cruft, which is converted into common iron, and rendered much lefs fufible. This cruft, or hollow fhell, may then be taken out of the fire. All fteel becomes degraded in the fame manner, even by very careful heating. Caft-fteel, drawn into fmall bars, exhibits cloudy lines and veins on its furface when tried by an acid, which no doubt have been produced during the heating and forging.

5. Caft-fteel being made out of broken tools of every kind, cannot of itfelf poffefs a larger dofe of plumbago than the average quantity contained in thofe fteels. But the Englifh caft-fteel is more fufible and more tender under the hammer than German fteel, or the fteel of cementation; which circumftances appear to indicate that it contains more plumbago: and the truth of this induction is confirmed by its exhibiting a much darker fpot than other fteels, when tried by an acid. Chalut did not therefore make this kind of fteel when he ufed glafs only for his flux. It cannot be doubted but that the flux of our manufacturers muft contain charcoal, at leaft. If it be animal coal, which is moft probable, it will alfo contain phofphorus; an ingredient to which the fuperiority of this coal, in cafe-hardening, is probably owing.

6. Tenacity and hardnefs are very frequently confidered as if they were one and the fame quality with regard to implements and rools, though they are certainly very diftinct properties. Tenacity is the oppofite to friability or brittlenefs; hardnefs is the oppofite to foftnefs. It is probable that iron is more tenacious than fteel, and it is certain that foft fteel is more tenacious than hard. Where tenacity and no confiderable degree of hardnefs are wanting, as in fprings, the inferior fteels, or the compounds of fteel and iron, will afford a more fafe, and confequently in many inftances more ufeful article; but where hardnefs and tenacity are both required, the leading quality of the fteel muft be its uniformity. Caft fteel is preferred in England to every other kind, not only for polifhed fteel-work and the beft cutting tools, but likewife for cold chifels, and the hard gravers for turners in metal; in both which laft I have obferved, by confiderable experience, that the common opinion is well founded.

III.

An Enquiry concerning the Source of the Heat which is excited by Friction. By* BENJAMIN, COUNT *of* RUMFORD, *F.R.S. M.R.I.A.*

IT frequently happens that, in the ordinary affairs and occupations of life, opportunities prefent themfelves of contemplating fome of the moft curious operations of nature; and

* From the Philofophical Tranfactions, 1798.—Read January 25, 1798.

very

very interesting philosophical experiments might often be made, almost without trouble or expence, by means of machinery contrived for the mere mechanical purposes of the arts and manufactures.

I have frequently had occasion to make this observation; and am persuaded, that a habit of keeping the eyes open to every thing that is going on in the ordinary course of the business of life, has oftener led, as it were by accident, or, in the playful excursions of the imagination put into action by contemplating the most common appearances, to useful doubts and serviceable schemes for investigation and improvement, than all the more intense meditations of philosophers in the hours expresly set apart for study.

It was by accident that I was led to make the experiments of which I am about to give an account; and though they are not, perhaps, of sufficient importance to merit so formal an introduction, I cannot help flattering myself that they will be thought curious in several respects, and worthy of the honour of being made known to the Royal Society.

Being engaged, lately, in superintending the boring of cannon in the workshops of the military arsenal at Munich, I was struck with the very considerable degree of heat which a brass gun acquires in a short time in being bored, and with the more intense heat (much greater than that of boiling water, as I found by experiment) of the metallic chips separated from it by the borer.

The more I meditated on these phenomena, the more they appeared to me to be curious and interesting. A thorough investigation of them seemed even to bid fair to give a farther insight into the hidden nature of heat; and to enable us to form some reasonable conjectures respecting the existence or non-existence of an *igneous fluid*: a subject on which the opinions of philosophers have in all ages been much divided.

In order that the Society may have clear and distinct ideas of the speculations and reasonings to which these appearances gave rise in my mind, and also of the specific objects of philosophical investigation they suggested to me, I must beg leave to state them at some length, and in such manner as I shall think best suited to answer this purpose.

From *whence comes* the heat actually produced in the mechanical operation above mentioned?

Is it furnished by the metallic chips, which are separated by the borer from the solid mass of metal?

If this were the case, then, according to the modern doctrines of latent heat and of caloric, the *capacity for heat* of the parts of the metal so reduced to chips, ought not only to be changed, but the change undergone by them should be sufficiently great to account for *all* the heat produced.

But no such change had taken place; for I found, upon taking equal quantities by weight of these chips, and of thin slips of the same block of metal, separated by means of a fine saw, and putting them at the same temperature (that of boiling water) into equal quantities of cold water (that is to say, at the temperature of $59\frac{1}{2}$° F.), the portion of water into which the chips were put was not, to all appearance, heated either less or more than the other portion into which the slips of metal were put.

This experiment being repeated several times, the results were always so nearly the same, that I could not determine whether any, or what change had been produced in the metal, *in regard to its capacity for heat*, by being reduced to chips by the borer*.

From hence it is evident, that the heat produced could not possibly have been furnished at the expence of the latent heat of the metallic chips. But, not being willing to rest satisfied with these trials, however conclusive they appeared to me to be, I had recourse to the following still more decisive experiment.

Taking a cannon (a brass six pounder) cast solid, and rough as it came from the foundry, (see fig. 1, plate V) and fixing it (horizontally) in the machine used for boring, and at the same time finishing the outside of the cannon by turning, (see fig. 2.) I caused its extremity to be cut off, and, by turning down the metal in that part, a solid cylinder was formed, $7\frac{3}{4}$ inches in diameter, and $9\frac{8}{10}$ inches long; which, when finished, remained joined to the rest of the metal (that which, properly speaking, constituted the cannon,) by a small cylindrical neck, only $2\frac{1}{5}$ inches in diameter, and $3\frac{8}{10}$ inches long.

This short cylinder, which was supported in its horizontal position, and turned round its axis, by means of the neck by which it remained united to the cannon, was now bored with the horizontal borer used in boring cannon; but its bore, which was 3.7 inches in diameter, instead of being continued through its whole length, (9.8 inches) was only 7.2 inches in length; so that a solid bottom was left to this hollow cylinder, which bottom was 2.6 inches in thickness.

This cavity is represented by dotted lines in fig. 2; as also in fig. 3, where the cylinder is represented on an enlarged scale.

This cylinder being designed for the express purpose of generating heat by friction, by having a blunt borer forced against its solid bottom at the same time that it should be turned round its axis by the force of horses, in order that the heat accumulated in the cylinder might from time to time be measured, a small round hole, (see d, e, fig. 3.) 0.37 of an inch only in diameter, and 4.2 inches in depth, for the purpose of introducing a small cylindrical mercurial thermometer, was made in it, on one side, in a direction perpendicular to the axis of the cylinder, and ending in the middle of the solid part of the metal which formed the bottom of its bore.

* As these experiments are important, it may, perhaps, be agreeable to the Society to be made acquainted with them in their details. One of them was as follows:

To 4590 grains of water, at the temperature of $59\frac{1}{2}$° F. (an allowance or compensation, reckoned in water, for the capacity for heat of the containing cylindrical tin vessel, being included) were added $1016\frac{1}{8}$ grains of gun-metal in thin slips, separated from the gun by means of a fine saw, being at the temperature of 210° F. When they had remained together 1 minute, and had been well stirred about by means of a small rod of light wood, the heat of the mixture was found to be = 63°.

From this experiment, the specific heat of the metal, calculated according to the rule given by Dr. Crawford, turns out to be = 0.1100, that of water being = 1.000.

An experiment was afterwards made with the metallic chips, as follows:

To the same quantity of water as was used in the experiment above mentioned, at the same temperature, (viz. $59\frac{1}{2}$°,) and in the same cylindrical tin vessel, were now put $1016\frac{1}{8}$ grains of metallic chips of gun-metal, bored out of the same gun from which the slips used in the foregoing experiment were taken, and at the same temperature (210°.) The heat of the mixture, at the end of 1 minute, was just 63°, as before; consequently the specific heat of these metallic chips was = 0.1100. Each of the above experiments was repeated three times, and always with nearly the same results.

The

The solid contents of this hollow cylinder, exclusive of the cylindrical neck by which it remained united to the cannon, were $385\frac{3}{4}$ cubic inches, English measure, and it weighed 113.13 lb. avoirdupois, as I found on weighing it at the end of the course of experiments made with it, and after it had been separated from the cannon with which, during the experiments, it remained connected *.

Experiment No. 1.

THIS experiment was made in order to ascertain how much heat was actually generated by friction, when a blunt steel borer being so forcibly shoved (by means of a strong screw) against the bottom of the bore of the cylinder, that the pressure against it was equal to the weight of about 10,000 lb. avoirdupois, the cylinder was turned round on its axis (by the force of horses) at the rate of about 32 times in a minute.

This machinery, as it was put together for the experiment, is represented by fig. 2. W is a strong horizontal iron bar connected with proper machinery carried round by horses, by means of which the cannon was made to turn round its axis.

To prevent, as far as possible, the loss of any part of the heat that was generated in the experiment, the cylinder was well covered up with a fit coating of thick and warm flannel, which was carefully wrapped round it, and defended it on every side from the cold air of the atmosphere. This covering is not represented in the drawing of the apparatus, fig. 2.

I ought to mention, that the borer was a flat piece of hardened steel, 0.63 of an inch thick, 4 inches long, and nearly as wide as the cavity of the bore of the cylinder, namely, $3\frac{1}{2}$ inches. Its corners were rounded off at its end, so as to make it fit the hollow bottom of the bore; and it was firmly fastened to the iron bar (m), which kept it in its place. The area of the surface, by which its end was in contact with the bottom of the bore of the cylinder, was nearly $2\frac{1}{3}$ inches. This borer, which is distinguished by the letter n, is represented in most of the figures.

At the beginning of the experiment, the temperature of the air in the shade, as also that of the cylinder, was just 60° F.

At the end of 30 minutes, when the cylinder had made 960 revolutions about its axis, the horses being stopped, a cylindrical mercurial thermometer, whose bulb was $\frac{32}{100}$ of an inch in diameter, and $3\frac{1}{4}$ inches in length, was introduced into the hole made to receive it, in the side of the cylinder; when the mercury rose instantly to 130°.

Though the heat could not be supposed to be quite equally distributed in every part of the cylinder, yet, as the length of the bulb of the thermometer was such that it extended

* For fear I should be suspected of prodigality in the prosecution of my philosophical researches, I think it necessary to inform the Society, that the cannon I made use of in this experiment was not sacrificed to it. The short hollow cylinder which was formed at the end of it, was turned out of a cylindrical mass of metal about two feet in length, projecting beyond the muzzle of the gun, called in the German language the verlorner kopf, (the head of the cannon to be thrown away,) and which is represented in fig. 1.

This additional projection, which is cut off before the gun is bored, is always cast with it, in order that, by means of the pressure of its weight on the metal in the lower part of the mould, during the time it is cooling, the gun may be the more compact in the neighbourhood of the muzzle, where, without this precaution, the metal would be apt to be porous or full of honeycombs.

from

from the axis of the cylinder to near its surface, the heat indicated by it could not be very different from that of the mean temperature of the cylinder; and it was on this account that a thermometer of that particular form was chosen for this experiment.

To see how fast the heat escaped out of the cylinder, (in order to be able to make a probable conjecture respecting the quantity given off by it during the time the heat generated by the friction was accumulating,) the machinery standing still, I suffered the thermometer to remain in its place near three quarters of an hour, observing and noting down, at small intervals of time, the height of the temperature indicated by it. Thus,

At the end of 4 minutes, the heat, as shown by the thermometer, was	126°
After 5 minutes, always reckoning from the first observation,	125
At the end of 7 minutes, — — —	123
12 — — — —	120
14 — — — —	119
16 — — — —	118
20 — — — —	116
24 — — — —	115
28 — — — —	114
31 — — — —	113
34 — — — —	112
37½ — — — —	111
And when 41 minutes had elapsed — —	110

Having taken away the borer, I now removed the metallic dust, or rather scaly matter, which had been detached from the bottom of the cylinder by the blunt steel borer, in this experiment, and, having carefully weighed it, I found its weight to be 837 grains Troy.

Is it possible that the very considerable quantity of heat that was produced in this experiment (a quantity which actually raised the temperature of above 113 lb. of gun-metal at least 70 degrees of Fahrenheit's thermometer, and which, of course, would have been capable of melting 6½ lb. of ice, or of causing near 5 lb. of ice-cold water to boil,) could have been furnished by so inconsiderable a quantity of metallic dust? and this merely in consequence of a change of its capacity for heat?

As the weight of this dust (837 grains Troy) amounted to no more than $\frac{1}{948}$th part of that of the cylinder, it must have given off 948 degrees to raise the temperature of the cylinder 1 degree; and consequently it must have given off 66360 degrees of heat to have produced that of the experiment!

But, without insisting on the improbability of this supposition, we have only to recollect, that from the results of actual and decisive experiments, made for the express purpose of ascertaining that fact, the capacity for heat, of the metal of which great guns are cast, is not sensibly changed by being reduced to the form of metallic chips, in the operation of boring cannon; and there does not seem to be any reason to think that it can be much changed, if it be changed at all, in being reduced to much smaller pieces by means of a borer that is less sharp.

 If

If the heat, or any confiderable part of it, were produced in confequence of a change in the capacity for heat of a part of the metal of the cylinder, as fuch change could only be fuperficial, the cylinder would by degrees be exhaufted, or the quantities of heat produced, in any given fhort fpace of time, would be found to diminifh gradually in fucceffive experiments. To find out if this really happened or not, I repeated the laft-mentioned experiment feveral times with the utmoft care ; but I did not difcover the fmalleft fign of exhauftion in the metal, notwithftanding the large quantities of heat actually given off.

Finding fo much reafon to conclude, that the heat generated in thefe experiments, or excited, as I would rather choofe to exprefs it, was not furnifhed at the expence of the latent heat or combined caloric of the metal, I pufhed my enquiries a ftep farther, and endeavoured to find out whether the air did or did not contribute any thing in the generation of it.

Experiment No. 2.

AS the bore of the cylinder was cylindrical, and as the iron bar (m), to the end of which the blunt fteel borer was fixed, was fquare, the air had free accefs to the infide of the bore, and even to the bottom of it where the friction took place by which the heat was excited.

As neither the metallic chips produced in the ordinary courfe of the operation of boring brafs cannon, nor the finer fcaly particles produced in the laft-mentioned experiments by the friction of the blunt borer, fhowed any figns of calcination, I did not fee how the air could poffibly have been the caufe of the heat that was produced; but, in an inveftigation of this kind, I thought that no pains fhould be fpared to clear away the rubbifh, and leave the fubject as naked and open to infpection as poffible.

In order by one decifive experiment to determine whether the air of the atmofphere had any part or not in the generation of the heat, I contrived to repeat the experiment under circumftances in which *it was evidently impoffible for it to produce any effect whatever.* By means of a pifton exactly fitted to the mouth of the bore of the cylinder, through the middle of which pifton the fquare iron bar, to the end of which the blunt fteel borer was fixed, paffed in a fquare hole made perfectly air-tight, the accefs of the external air to the infide of the bore of the cylinder was effectually prevented. In fig. 3. this pifton (*p*) is feen in its place : it is likewife fhown in fig. 7. and 8.

I did not find, however, by this experiment, that the exclufion of the air diminifhed in the fmalleft degree the quantity of heat excited by the friction.

There ftill remained one doubt, which, though it appeared to me to be fo flight as hardly to deferve any attention, I was however defirous to remove. The pifton which clofed the mouth of the bore of the cylinder, in order that it might be air-tight, was fitted into it with fo much nicety by means of collars of leather, and preffed againft it with fo much force, that, notwithftanding its being oiled, it occafioned a confiderable degree of friction when the hollow cylinder was turned round its axis. Was not the heat produced, or at leaft fome part of it occafioned, by the friction of the pifton ? And as the external air had free accefs to the extremity of the bore where it came in contact with the pifton, is it not poffible that this air might have had fome fhare in the generation of the heat produced ?

Experiment No. 3.

A QUADRANGULAR oblong deal-box, (fee fig. 4.) water-tight, $11\frac{1}{2}$ Englifh inches long, $9\frac{4}{10}$ inches wide, and $9\frac{6}{10}$ inches deep, (meafured in the clear,) being provided, with holes or flits in the middle of each of its ends juft large enough to receive the one, the fquare iron rod to the end of which the blunt fteel borer was faftened, the other, the fmall cylindrical neck which joined the hollow cylinder to the cannon; when this box (which was occafionally clofed above by a wooden cover or lid moving on hinges) was put into its place; that is to fay, when by means of the two vertical openings or flits in its two ends, (the upper parts of which openings were occafionally clofed by means of narrow pieces of wood fliding in vertical grooves,) the box *(g, h, i, k,* fig. 3.) was fixed to the machinery in fuch a manner that its bottom *(i, k,)* being in the plane of the horizon, its axis coincided with the axis of the hollow metallic cylinder; it is evident from the defcription, that the hollow metallic cylinder would occupy the middle of the box without touching it on either fide, (as it is reprefented in fig. 3.) and that on pouring water into the box and filling it to the brim, the cylinder would be completely covered and furrounded on every fide by that fluid. And farther, as the box was held faft by the ftrong fquare iron rod *(m)*, which paffed in a fquare hole in the centre of one of its ends, (*a*, fig. 4.) while the round or cylindrical neck, which joined the hollow cylinder to the end of the cannon, could turn round freely on its axis in the round hole in the centre of the other end of it, it is evident that the machinery could be put in motion without the leaft danger of forcing the box out of its place, throwing the water out of it, or deranging any part of the apparatus.

Every thing being ready, I proceeded to make the experiment I had projected in the following manner:

The hollow cylinder having been previoufly cleaned out, and the infide of its bore wiped with a clean towel till it was quite dry, the fquare iron bar, with the blunt fteel-borer fixed to the end of it, was put into its place; the mouth of the bore of the cylinder being clofed at the fame time by means of the circular pifton, through the centre of which the iron bar paffed.

This being done, the box was put in its place; and the joinings of the iron rod and of the neck of the cylinder, with the two ends of the box, having been made water-tight by means of collars of oiled leather, the box was filled with cold water, (viz. at the temperature of 60°) and the machine was put in motion.

The refult of this beautiful experiment was very ftriking, and the pleafure it afforded me amply repaid me for all the trouble I had had in contriving and managing the complicated machinery ufed in making it.

The cylinder, revolving at the rate of about 32 times in a minute, had been in motion but a fhort time, when I perceived, by putting my hand into the water and touching the outfide of the cylinder, that heat had been generated; and it was not long before the water which furrounded the cylinder began to be fenfibly warm.

At the end of one hour I found, by plunging a thermometer into the water in the box, (the quantity of which fluid amounted to 18.77 lb. avoirdupois, or $2\frac{1}{4}$ wine gallons,) that its temperature had been raifed no lefs than 47 degrees, being now 107° of Fahrenheit's fcale.

When

When 30 minutes more had elapſed, or 1 hour and 30 minutes after the machinery had been put in motion, the heat of the water in the box was found to be raiſed to 178°.

At 2 hours 20 minutes, it was at 200°; and at 2 hours 30 minutes it ACTUALLY BOILED.

It would be difficult to deſcribe the ſurpriſe and aſtoniſhment expreſſed in the countenances of the byſtanders, on ſeeing ſo large a quantity of cold water heated, and actually made to boil, without any fire.

Though there was, in fact, nothing that could juſtly be conſidered as ſurpriſing in this event, yet I acknowledge, fairly, that it afforded me a degree of childiſh pleaſure, which, were I ambitious of the reputation of a grave *philoſopher*, I ought moſt certainly rather to hide than diſcover.

The quantity of heat excited and accumulated in this experiment was very conſiderable; for, not only the water in the box, but alſo the box itſelf, (which weighed $15\frac{1}{4}$lb.) and the hollow metallic cylinder, and that part of the iron bar which, being ſituated within the cavity of the box, was immerſed in the water, were heated through 150 degrees of Fahrenheit's ſcale; viz. from 60 degrees (which was the temperature of the water and of the machinery at the beginning of the experiment) to 210 degrees, the heat of boiling water at Munich.

The total quantity of heat generated may be eſtimated with ſome conſiderable degree of preciſion as follows:

	Quantity of ice-cold water which, with the given quantity of heat, might have been heated to 180°, or made to boil. In Avoirdupois weight. lb.
Of the heat excited there appears to have been actually accumulated,	
In the water contained in the wooden box, $18\frac{3}{4}$lb. avoirdupois, heated 150 degrees; namely, from 60° to 210° F. - -	15.2
In 113.13lb. of gun metal (the hollow cylinder) heated 150 degrees; and, as the capacity for heat of this metal is to that of water as 0.1100 to 1.0000, this quantity of heat would have heated $12\frac{1}{2}$lb. of water the ſame number of degrees. - -	10.37
In 36.75 cubic inches of iron, (being that part of the iron bar to which the borer was fixed which entered the box,) heated 150 degrees; which may be reckoned equal in capacity for heat to 1.21lb. of water. - - - - -	1.01
N. B. No eſtimate is here made of the heat accumulated in the wooden box, nor of that diſperſed during the experiment.	
Total quantity of ice-cold water which, with the heat actually generated by friction, and accumulated in 2 hours 30 minutes, might have been heated 180 degrees, or made to boil. - -	26.58

From the knowledge of the *quantity* of heat actually produced in the foregoing experiment, and of the *time* in which it was generated, we are enabled to aſcertain *the velocity of its production*, and to determine how large a fire muſt have been made, or how much fuel muſt have been conſumed, in order that, in burning equably, it ſhould have produced by combuſtion the ſame quantity of heat in the ſame time.

In one of Dr. Crawford's experiments, (ſee his Treatiſe on Heat, p. 321) 37lb. 7oz. troy = 181920 grains of water were heated $2\frac{1}{10}$ degrees of Fahrenheit's thermometer, with the heat generated in the combuſtion of 26 grains of wax. This gives 382032 grains of water heated 1 degree with 26 grains of wax; or $14693\frac{14}{26}$ grains of water heated 1 degree, or $\frac{14693}{180}$ = 81.631 grains, heated 180 degrees with the heat generated in the combuſtion of 1 grain of wax.

The quantity of ice-cold water, which might have been heated 180 degrees with the heat generated by friction in the before-mentioned experiment, was found to be 26.58lb. = 188060 grains; and, as 81.631 grains of ice-cold water require the heat generated in the combuſtion of 1 grain of wax to heat it 180 degrees, the former quantity of ice-cold water, namely, 188060 grains, would require the combuſtion of no leſs than 2303.8 grains (= $4\frac{8}{10}$oz. troy) of wax to heat it 180 degrees.

As the experiment (No. 3.) in which the given quantity of heat was generated by friction laſted 2 hours 30 minutes = 150 minutes; it is neceſſary, for the purpoſe of aſcertaining how many wax candles of any given ſize muſt burn together, in order that, in the combuſtion of them, the given quantity of heat may be generated in the given time, and conſequently *with the ſame celerity* as that with which the heat was generated by friction in the experiment, that the ſize of the candles ſhould be determined, and the quantity of wax conſumed in a given time by each candle in burning equably ſhould be known.

Now, I found by an experiment made on purpoſe to finiſh theſe computations, that when a good wax candle of a moderate ſize, $\frac{3}{4}$ of an inch in diameter, burns with a clear flame, juſt 49 grains of wax are conſumed in 6 minutes. Hence it appears, that 245 grains of wax would be conſumed in 30 minutes; and that to burn the quantity of wax (= 2303.8 grains) neceſſary to produce the quantity of heat actually obtained by friction, in the experiment in queſtion, and in the given time (150 minutes), nine candles burning at once would not be ſufficient; for 9 multiplied into 245 (the number of grains conſumed by each candle in 150 minutes) amounts to no more than 2205 grains; whereas the quantity of wax neceſſary to be burned, in order to procure the given quantity of heat, was found to be 2303.8 grains.

From the reſult of theſe computations it appears, that the quantity of heat produced equably, or in a continual ſtream (if I may uſe that expreſſion), by the friction of the blunt ſteel borer againſt the bottom of the hollow metallic cylinder, in the experiment under conſideration, was greater than that produced equably in the combuſtion of 9 wax candles, each $\frac{3}{4}$ of an inch in diameter, all burning together, or at the ſame time, with clear bright flames.

As the machinery uſed in this experiment could eaſily be carried round by the force of one horſe (though, to render the work lighter, two horſes were actually employed in doing it), theſe computations ſhow farther how large a quantity of heat might be produced by proper mechanical contrivance, merely by the ſtrength of a horſe, without either fire, light, combuſtion, or chemical decompoſition; and, in a caſe of neceſſity, the heat thus produced might be uſed in cooking victuals.

But

But no circumſtances can be imagined in which this method of procuring heat would not be diſadvantageous; for more heat might be obtained by uſing the fodder neceſſary for the ſupport of a horſe as fuel.

As ſoon as the laſt-mentioned experiment (No. 3) was finiſhed, the water in the wooden box was let off, and the box removed; and the borer being taken out of the cylinder, the ſcaly metallic powder, which had been produced by the friction of the borer againſt the bottom of the cylinder, was collected, and, being carefully weighed, was found to weigh 4145 grains, or about $8\frac{2}{3}$ oz. troy.

As this quantity was produced in $2\frac{1}{2}$ hours, this gives 830 grains for the quantity produced *in half an hour.*

In the firſt experiment, which laſted only half an hour, the quantity produced was 837 grains.

In the experiment No. 1, the quantity of heat generated in *half an hour* was found to be equal to that which would be required to heat 5lb. avoirdupois of ice-cold water 180 degrees, or cauſe it to boil.

According to the reſult of the experiment No. 3, the heat generated *in half an hour* would have cauſed 5.31lb. of ice-cold water to boil. But in this laſt-mentioned experiment, the heat generated being more effectually confined, leſs of it was loſt; which accounts for the difference of the reſults of the two experiments.

It remains for me to give an account of one more experiment which was made with this apparatus. I found, by the experiment No. 1, how much heat was generated when the air had free acceſs to the metallic ſurfaces which were rubbed together. By the experiment No. 2, I found that the quantities of heat generated were not ſenſibly diminiſhed when the free acceſs of the air was prevented; and, by the reſult of No. 3, it appeared that the generation of heat was not prevented or retarded by keeping the apparatus immerſed in water. But as, in this laſt-mentioned experiment, the water, though it ſurrounded the hollow metallic cylinder on every ſide, externally, was not ſuffered to enter the cavity of the bore (being prevented by the piſton), and conſequently did not come into contact with the metallic ſurfaces where the heat was generated; to ſee what effects would be produced by giving the water free acceſs to theſe ſurfaces, I now made the

Experiment No. 4.

THE piſton which cloſed the end of the bore of the cylinder being removed, the blunt borer and the cylinder were once more put together; and the box being fixed in its place, and filled with water, the machinery was again put in motion.

There was nothing in the reſult of this experiment that renders it neceſſary for me to be very particular in my account of it. Heat was generated as in the former experiments, and to all appearance quite as rapidly; and I have no doubt but the water in the box would have been brought to boil, had the experiment been continued as long as the laſt. The only circumſtance that ſurpriſed me was, to find how little difference was occaſioned in the noiſe made by the borer in rubbing againſt the bottom of the bore of the cylinder, by filling the bore with water. This noiſe, which was very grating to the ear, and ſometimes almoſt inſupportable, was, as nearly as I could judge of it, quite as loud and as diſagreeable when the furfaces rubbed together were wet with water, as when they were in contact with air.

By meditating on the refults of all thefe experiments, we are naturally brought to that great queftion which has fo often been the fubject of fpeculation among philofophers, namely, What is heat?—Is there any fuch thing as an *igneous fluid?*—Is there any thing that can with propriety be called caloric?

We have feen that a very confiderable quantity of heat may be excited in the friction of two metallic furfaces, and given off in a conftant ftream or flux *in all directions*, without interruption or intermiffion, and without any figns of diminution or exhauftion.

From whence came the heat which was continually given off in this manner in the foregoing experiments? Was it furnifhed by the fmall particles of metal detached from the larger folid maffes on their being rubbed together? This, as we have already feen, could not poffibly have been the cafe.

Was it furnifhed by the air? This could not have been the cafe; for in three of thefe experiments, the machinery being kept immerfed in water, the accefs of the air of the atmofphere was completely prevented.

Was it furnifhed by the water which furrounded the machinery? That this could not have been the cafe is evident; *firft*, becaufe this water was continually *receiving heat* from the machinery, and could not at the fame time be *giving to* and *receiving heat from* the fame body; and, *fecondly*, becaufe there was no chemical decompofition of any part of this water. Had any fuch decompofition taken place (which indeed could not reafonably have been expected), one of its compound elaftic fluids (moft probably inflammable air) muft at the fame time have been fet at liberty, and, in making its efcape into the atmofphere, would have been detected; but though I frequently examined the water to fee if any air bubbles rofe up through it, and had even made preparations for catching them in order to examine them if any fhould appear, I could perceive none; nor was there any fign of decompofition of any kind whatever, or other chemical procefs going on in the water.

Is it poffible the heat could have been fupplied by means of the iron bar to the end of which the blunt fteel borer was fixed? or by the fmall neck of gun-metal by which the hollow cylinder was united to the cannon? Thefe fuppofitions appear more improbable even than either of thofe before mentioned; for heat was continually going off or *out of the machinery*, by both thefe laft paffages, during the whole time the experiment lafted.

And, in reafoning on this fubject, we muft not forget to confider that moft remarkable circumftance, that the fource of the heat generated by friction in thefe experiments appeared evidently to be inexhauftible.

It is hardly neceffary to add, that any thing which any *infulated* body or fyftem of bodies can continue to furnifh *without limitation*, cannot poffibly be a *material fubftance*; and it appears to me to be extremely difficult, if not quite impoffible, to form any diftinct idea of any thing capable of being excited and communicated in the manner the heat was excited and communicated in thefe experiments, except it be motion.

I am very far from pretending to know how, or by what means or mechanical contrivance, that particular kind of motion in bodies which has been fuppofed to conftitute heat is excited, continued, and propagated; and I ſhall not preſume to trouble the Society with mere conjectures, particularly on a ſubject which, during ſo many thouſand years, the moſt enlightened philoſophers have endeavoured but in vain to comprehend.

But although the mechaniſm of heat ſhould in fact be one of thoſe myſteries of nature which are beyond the reach of human intelligence, this ought by no means to diſcourage us, or even leſſen our ardour, in our attempts to inveſtigate the laws of its operations. How far can we advance in any of the paths which ſcience has opened to us, before we find ourſelves enveloped in thoſe thick miſts which on every ſide bound the horizon of the human intellect? But how ample and how intereſting is the field that is given us to explore!

Nobody, ſurely, in his ſober ſenſes has ever pretended to underſtand the mechaniſm of gravitation; and yet what ſublime diſcoveries was our immortal NEWTON enabled to make, merely by the inveſtigation of the laws of its action!

The effects produced in the world by the agency of heat are probably *juſt as extenſive*, and quite as important, as thoſe which are owing to the tendency of the particles of matter towards each other; and there is no doubt but its operations are in all caſes determined by laws equally immutable.

Before I finiſh this paper I would beg leave to obſerve, that although, in treating the ſubject I have endeavoured to inveſtigate, I have made no mention of the names of thoſe who have gone over the ſame ground before me, nor of the ſucceſs of their labours; this omiſſion has not been owing to any want of reſpect for my predeceſſors, but was merely to avoid prolixity, and to be more at liberty to purſue without interruption the natural train of my own ideas.

Deſcription of the Figures. Plate V.

FIG. 1. ſhows the cannon uſed in the foregoing experiments, in the ſtate it was when it came from the foundry.

Fig. 2. ſhows the machinery uſed in the experiments No. 1. and No. 2. The cannon is ſeen fixed in the machine uſed for boring cannon. W is a ſtrong iron bar (which, to ſave room in the drawing, is repreſented as broken off); which bar being united with machinery (not expreſſed in the figure) that is carried round by horſes, cauſes the cannon to turn round its axis.

m is a ſtrong iron bar, to the end of which the blunt borer is fixed, which, by being forced againſt the bottom of the bore of the ſhort hollow cylinder that remains connected by a ſmall cylindrical neck to the end of the cannon, is uſed in generating heat by friction.

Fig. 3. ſhows on an enlarged ſcale the ſame hollow cylinder that is repreſented on a ſmaller ſcale in the foregoing figure. It is here ſeen connected with the wooden box (*g*, *h*, *i*, *k*) uſed in the experiments No. 3. and No. 4. when this hollow cylinder was immerſed in water.

p, which is marked by dotted lines, is the piſton which cloſed the end of the bore of the cylinder.

n is the blunt borer ſeen ſideways.

d, *e*, is the ſmall hole by which the thermometer was introduced, that was uſed for aſcertaining the heat of the cylinder. To ſave room in the drawing, the cannon is repreſented broken off near its muzzle; and the iron bar, to which the blunt borer is fixed, is repreſented broken off at *m*.

Fig.

Fig. 4. is a perspective view of the wooden box, a section of which is seen in the foregoing figure. (See *g*, *h*, *i*, *k*, fig. 3.)

Fig. 5. and 6. represent the blunt borer n joined to the iron bar *m* to which it was fastened.

Fig. 7. and 8. represent the same borer with its iron bar, together with the piston, which, in the experiments No. 2. and No. 3. was used to close the mouth of the hollow cylinder.

IV.

An Attempt to discover the Genuineness and Purity of Drugs and Medical Preparations. By FREDERICK ACCUM.

To Mr. NICHOLSON.

SIR,

OF all the possible frauds hitherto practised by mercenary traders, there is none more intimately connected with the welfare of mankind than the art of making or counterfeiting drugs and adulterating genuine medicines.

This unprincipled art is brought to such perfection, that spurious articles are every where to be found in the market, made up with such dexterity, that not only the mere merchant and drug broker, but even the man of skill, is sometimes deceived.

The influence of this practice has indeed been observed by medical men of science, and the rulers of this art have thought it their duty to appoint a court of examiners to investigate the goodness of drugs and medicines in the different chemists' and apothecaries' shops in this metropolis.

How far this great work is accomplished, or to what extent the whole undertaking does either honour and credit to the sagacity of these learned, liberal, and upright examiners, or affords justice to the public, I shall leave to the determination of every individual who has once only been honoured with their visit.

But as moral duty should stimulate every individual to offer all the assistance in his power to suppress or check such detestable practices, I have ventured to recommend that method which, if judiciously executed, will contribute in no small degree to the public good in this respect. If these sketches should tend in any measure to put the unwary on their guard, I shall think myself amply rewarded, and shall not fail to extend them from time to time, and lay them before the public with some confidence.

No. 17, *Hay-market*,
May 1st, 1798.

FRED. ACCUM.

EXAMINATION OF SALINE SUBSTANCES.

Sulphuric Acid.

THE vitriolic acid kept in our shops under the name of oil of vitriol, usually contains not only lead in solution, but likewise iron, copper, and sulphate of potash.

In order to discover these admixtures, a little of the acid must be diluted with distilled water and saturated with pure vegetable alkali, as by this means the copper, iron, and lead become precipitated in the form of a more or lefs dark-coloured powder, which is to be feparated by the filter, and wafhed with diftilled water.

To analyfe this precipitate, it is firft treated with pure ammoniac, which foon acquires a blue colour if copper be prefent.——The blue fupernatant fluid is to be feparated, the remainder wafhed in diftilled water, and then diffolved in pure muriatic acid.

In order to afcertain the prefence of lead, the foregoing folution is mixed with an equal quantity of water impregnated with fulphurated hydrogen gas; and if a dark brown or black precipitate enfues, it is a fure fign that lead is prefent.

Iron that may happen to be in folution is exhibited by the admixture of a few drops of pure pruffiate of potafh or tincture of gall-nuts, which in the firft cafe will produce a blue, and in the fecond a black precipitate.

Sulphate of potafh, or the vegetable alkali, united to fulphuric acid is more difficult to difcover, as for this purpofe a certain quantity of the acid is to be perfectly faturated with carbonate of potafh, or carbonate of ammoniac; to this folution a little tartareous acid is to be added, which by its fuperior affinity will decompofe the vitriolated tartar, and indicate the vegetable alkali under the form of tartarite of potafh feparated from the fluid. If the quantity in the acid be not very fmall, as is feldom the cafe, it may alfo be difcovered by the admixture of ftrong ardent fpirit; for by this means a quantity of fulphate of potafh will be feparated in the form of a white powdery precipitate.

The fpecific gravity of fulphuric acid, according to our pharmacopœia, fhould be to that of diftilled water as 185 to 100.

It is void of fmell—perfectly colourlefs—and boils at 546° of Fahrenheit's thermometer.

Equal parts of vitriolic acid and water, haftily mixed together, fhould produce a heat nearly equal to that of boiling water.

*Nitrous Acid**.

THE nitrous acid of commerce (or double aqua fortis as it is commonly called) always contains either fulphuric or muriatic acid, and often both.

The vitriolic acid may be difcovered by the addition of a few drops of a folution of nitrate of barytes, or acetite of lead, which form infoluble compounds and fall down. The muriatic acid is manifefted by the addition of a folution of nitrate of filver. In this cafe muriate of filver is formed and precipitated.

Its fpecific gravity fhould be to that of diftilled water, as 155 to 100.

If it is of an orange yellow colour, and emits a vaft quantity of elaftic fluid, known by the name of nitrous gas, which forms yellow fumes in our atmofphere. Mixed with water it develops a confiderable degree of heat †.

* The inftruction for preparing nitrous acid according to our pharmacopœia is erroneous, as the proportion of fulphuric acid prefcribed is much too fmall: it confequently leaves a confiderable quantity of nitrate of potafh behind undecompofed. A.

† Pure nitrous acid is deprived of a portion of nitrous gas by the application of heat. It is then colourlefs, and conftitutes nitric acid. Expofure of nitric acid to light expels oxygen, and leaves the acid with that redundancy of azote which conftitutes the nitrous or yellow acid. Nitric acid muft therefore be kept in the dark, or elfe in bottles quite full. N.

Muriatic

Muriatic Acid *.

COMMONLY called ſpirit of ſalt, always contains iron, and frequently ſulphuric acid and copper.

The ſulphuric acid is detected either by the addition of barytes, or acetite of lead, and the reſults are the ſame as in the foregoing experiments.

Copper is inveſtigated by ſuperſaturating this acid with pure ammoniac, which in this caſe will immediately be tinged of a perceptible blue.

Iron is manifeſted by previouſly uniting this acid with carbonate of potaſh and treating it then with tincture of gall-nuts, or pruſſiate of potaſh. The firſt will produce a black, and the latter a blue precipitate.

The ſpecific gravity of muriatic acid ſhould be, 1,70 to 1,000.

Pure muriati c acid is totally deſtitute of colour.

Acetous Acid.

THE concentrated acetous acid commonly met with contains ſulphuric acid, with which it is either fraudulently adulterated, or with which it is impregnated by a careleſs and ſlovenly management during the proceſs for obtaining it.

We often find this acid contaminated by tartareous acid, and not unfrequently by copper or lead. The preſence of ſulphuric acid is here alſo diſcovered by the addition of barytes, or acetite of lead, in the manner already mentioned.

Tartareous acid is detected by ſaturating the acetous acid with vegetable alkali, in conſequence of which a tartarite of potaſh becomes ſeparated in the form of a white powder.

Copper is diſcovered by ſaturating the acid with carbonate of ammoniac, and lead becomes manifeſted by mixing it with water impregnated with ſulphurated hydrogenous gas. In the firſt caſe a blue, and in the ſecond a black precipitate will be formed.

The ſpecific gravity of the moſt concentrated acetous acid ſhould be to that of water as 1,050 to 1,000. It is colourleſs, and of a penetrating ſmell.

Diſtilled Vinegar.

VINEGAR, if not diſtilled in glaſs veſſels, but in a ſtill with a pewter head, always contains lead in ſolution.

To diſcover this, equal quantities of vinegar, and water impregnated with ſulphurated hydrogenous gas, are mixed together, which mixture, if the vinegar is free from lead, will remain unaltered; whereas, on the contrary, the ſmalleſt quantity of this metal will produce a black precipitate.

We frequently meet with diſtilled vinegar adulterated with ſulphureous acid, the preſence of which is detected by means of nitrate of barytes, or acetite of lead, as mentioned before.

* The proportion of ſulphuric acid preſcribed by our royal college of phyſicians for obtaining this acid is much too great, as 18 or 20 ounces are quite ſufficient for a total decompoſition of the quantity of muriate of potaſh ordered.

The

The beſt diſtilled vinegar has a pleaſant taſte and fragrant ſmell, is perfectly colourleſs, and twelve parts of it require one of dry vegetable alkali to neutraliſe it.

Boracic Acid.

ON account of the high price which this acid bears, we often meet with it intentionally adulterated with Venetian talc, aſbeſt, &c.

Genuine boracic acid is ſoluble in five times its quantity of boiling ardent ſpirit, and the ſolution when ſet on fire burns with a green flame. In water it diſſolves tardily; when ſubmitted to the blow-pipe it fluxes to a perfect tranſparent glaſs if genuine; it vitrifies earth and ſtones. If it be added in ſmall quantities to tartarite of potaſh, it tranſforms it into a very ſoluble ſalt. The beſt boracic acid is exhibited in ſmall hexangular ſcaly cryſtals, of a ſhining ſilvery white colour. It diſcovers only a ſlight acid taſte, and does not deliqueſce in the air.

Its ſpecific gravity is 1,480.

Tartareous Acid.

TARTAREOUS acid may very eaſily be contaminated with ſulphuric acid, either fraudulently or in conſequence of a faulty preparation.

The admixture of ſulphuric acid is ſoon diſcovered, if a ſmall quantity of the tartareous acid be diſſolved in diſtilled water, and a few drops of a ſolution of acetite of lead are added to it: by this means a white precipitate is afforded, which, by the addition of a few drops of pure nitric acid, will be entirely rediſſolved if no ſulphuric acid is preſent. The preſence of this acid is ſtill more readily manifeſted by adding to the tartareous acid diſſolved in diſtilled water a few drops of a ſolution of nitrate of barytes, in conſequence of which ſulphate of barytes will immediately be generated.

Pure cryſtallized tartareous acid does not change by expoſure to air; it is very ſoluble in water, and poſſeſſes a grateful acid taſte.

Acid of Amber.

WE find this acid wonderfully adulterated ſometimes with ſulphuric acid and all its combinations, ſometimes with tartareous acid, and now and then with muriate of ammoniac.

The ſulphuric acid is diſcovered by means of nitrate of barytes, or acetite of lead.

The tartareous acid is diſcovered by means of vegetable alkali; for, if this acid be preſent, a quantity of tartarite of potaſh will be formed.

Muriate of ammoniac is diſcovered with reſpect to one of its component parts (the muriatic acid), by the ſolution of nitrate of ſilver; and with reſpect to the other (the ammoniac) by adding to the aqueous ſolution of this ſalt a little vegetable alkali, and, after heating the mixture, holding over it a ſtopper moiſtened with acetous acid; white fumes will be formed, which indicate that ammoniac is preſent.

Pure acid of amber is a cryſtalline white ſalt, of an acid taſte, ſoluble in twenty-four times its weight of cold water, but in eight when boiling hot, and is volatilized on an ignited iron, leaving neither aſhes nor any other reſidue behind.

 Acid

Acid of Benzoin,

WHICH is commonly known by the name of flowers of benjamin, is not liable to be easily adulterated.

The best flowers of benjamin are brilliant white, possess a peculiarly grateful smell; are totally soluble in boiling water and ardent spirit, and leave no residue or ashes when laid upon a red-hot iron, or on ignited coals.

V.

A Model proposed for the Construction of a Satellitian, or Instrument for explaining the Phenomena of Jupiter and his Satellites, with an Account of its Use. By the Rev. W. PEARSON, *of Lincoln* *.

SINCE the doctrine of brightness has been particularly attended to by Mr. Herschell in his observations of the heavenly bodies, this celebrated astronomer has been able, by means of the excellence of his telescopes, to announce to the world, what his predecessors could only conjecture, not only that the satellites of Jupiter have each a rotatory motion on their axes, but also the exact time of each rotation. In Part II. of the Philosophical Transactions of the Royal Society of the last year [1797], is an article in which Mr. Herschell has proved, I think, in a satisfactory manner, that each of these four satellites has, like our moon, just one rotation in every periodical revolution round its primary, viz. The first or nearest, in 1 day 8 hours and $26\frac{6}{10}$ minutes; the second, in 3 days 13 [by a typographical error made 18] hours and $17\frac{9}{10}$ minutes; the third, in 7 days 3 hours and $59\frac{3}{10}$ minutes; and the fourth, in 16 days 18 hours and $5\frac{1}{10}$ minutes.——This discovery of a rotation in each revolution renders the analogy so striking between the Jovian, as a detached part of the solar system, and that of our own earth and moon, that we can hardly any longer doubt whether or not Jupiter be inhabited. Such a consideration, in my opinion, renders the Jovian portion of the solar system a subject which merits more particular attention than is usually allotted to it in lectures upon astronomy; and on this account I have contrived the model of an instrument of a simple construction for the purpose of explaining the different phenomena thereof, which, for want of a more appropriate name, I shall take the liberty of calling a satellitian. Other instruments to answer the same purpose may have been constructed and used; but as I have no knowledge of any, except that Mr. Rowley's grand orrery is said to represent Jupiter's satellites moving by wheel-work †, I feel myself warranted in presenting to the public a plan and description of the constituent parts of this detached instrument, and of the method of using it, in hopes that it may prove not unacceptable to the cultivators of science, or at least that it may suggest to some more ingenious artist the means of making a more eligible one.

* Communicated by the author.

† A machine for this purpose is also described in Harris's Lexicon Technicum. N.

In

In figure 1 of plate VI. is exhibited a lateral view of the wheels of this instrument, which are calculated for producing the mean motions of the four satellites, and contained in the brass box ABCD, $8\frac{1}{10}$ inches long within, 4 broad, and $1\frac{1}{10}$ deep. Figure 2, EF, represents the cover of this box, on which the different faces for the hour, week, and month-hands are graduated and silvered: the small concentric wheels marked 8, 18, 27, and 39 in fig. 1, are all fixed on the same revolving axle, placed on the point H, which carries the hour-hand on its upper end once round in 24 hours; but the larger corresponding wheels, 134, 129, 96, and 69, are fixed on the ends of as many separate tubes or hollow axles of brass, which just turn easily round within one another, and are supported by a shoulder in the perpendicular fixed stem that holds Jupiter over the point S; the innermost being the longest, and the next to the innermost the next in length, as in a common planetarium: these tubes have each a broad ring of a proper diameter to slide a little way upon their upper ends, to which are riveted the crooked arms which support the satellites; and the rings are turned round the tubes in the adjustment of the satellites to their relative situations: the tubes also have shoulders each for the support of its next superior wheel, and are kept in their places by the lower edges of the rings.

All the parts of the satellitian are laid down in the plate in their proper proportions and full size, according to the following table, from which an artist of but little ingenuity may easily construct it, provided he is furnished with, or has access to, a cutting engine for making the wheels, the plate of which is properly divided for the required numbers. An easy method for dividing a plate is laid down in Mr. Ferguson's "Select Exercises," in substance thus: As all the teeth in any wheel are to 360°, so are any odd number of teeth, to the number of degrees to be marked out, and divided into that odd number; after which the remaining even number may be very easily divided by continued halves.

Table of Dimensions *.

Wheels.	Diam. in Inches from the Pitch Line.	Distance of the Satellites from Jupiter in Diameters of him.
134	2,8	1. Satellite $2\frac{5}{8}$ diameters.
129	2,6	2. do. — $4\frac{1}{2}$ do.
96	2,3	3. do. — $7\frac{1}{6}$ do.
69	1,9	4. do. — $12\frac{2}{3}$ do.
8	0,2	
18	0,4	From S to H $1\frac{1}{2}$ inches.
27	0,7	From H to W 0,68 do.
39	1,1	From W to P 2,125 do.
56	1,2	
7	0,35	
73	3,9	

* The wheels in the table are proportioned thus:—As the sum of the teeth in any two corresponding wheels is to the distance between their centres, or sum of their semidiameters, so is the number of teeth in each, taken separately, to its semidiameter: an allowance being afterwards made for the driving wheel being somewhat larger in this proportion than the driven one, to make them work more easily.——W. P.

Though the dimenſions of the wheelwork here given are convenient for the conſtruction of the ſatellitian, yet the box may be in the form of a ſquare, an octagon, or a circle; though a parallelogram ſeems to be the moſt portable; but of whatever ſhape, there muſt be a contrivance for cramping or otherwiſe fixing it to a table when uſed.—The ſize of the ball, which repreſents Jupiter in the plate, is proportioned to the length of the arms which carry the ſatellites, and ought not to be larger; for an increaſed length of the arms would not admit the ſcreen to approach near enough to prevent the divergence of the ſhadows.—The balls which repreſent the ſatellites ſhould, however, be as diminutive as poſſible, ſo that they give a diſtinct ſhadow by candlelight. According to Mr. Herſchell's obſervations, the third is the largeſt, the firſt and fourth nearly equal, and the ſecond the ſmalleſt: their colours are, in his own words, as follows: the firſt is "white, more ſo ſometimes than others;" the ſecond "white, blueiſh, and aſh-coloured;" the third "white, differently in different ſituations;" and the fourth "duſky, dingey inclining to orange, reddiſh, and ruddy at different times."

A motion is communicated to the machinery by means of a ſingle endleſs ſcrew, on an horizontal axle, lying acroſs the box at the ſmall black circle near 73, and working with the teeth of this wheel: a handle is put upon the projecting end of this axle, which cannot appear in the plate, at ſuch a diſtance from the ſide of the box, as may prevent the hand of the perſon who turns it from touching any of the ſmall balls, or intercepting the view of a ſmall company of ſpectators.

As appendages to the ſatellitian, a ſmall ſliding ſcreen of thin paper and an appropriate candleſtick are neceſſary.—The mode of adjuſting the ſcreen is optional, and therefore requires no deſcription.—The baſe of the candleſtick, of which a ſketch is given in miniature at fig. 3, conſiſts of a circular plate of braſs 4 inches diameter, divided into the 12 ſigns, and graduated in concentric circles near the extremity: in the centre of this ſtands a ſhort perpendicular ſtem of a cylindrical form, which ſcrews into a ſolid piece ſoldered on the ſurface of the plate, and has a ſocket proper for containing a candle or lamp at the upper end: on the low end of this ſtem turns a broad ring, into which an arm is riveted 3 inches long, which has a ſimilar ſocket on its upper end, and oppoſite thereto a hand alſo riveted, which points to the graduated ecliptic oppoſite to the part on which the arm reſts at all times.

In order to eſtimate the accuracy of the mean motions of the little balls in the ſatellitian, we muſt conſider each pair of correſponding wheels as an improper fraction of a day; thus $\frac{69}{39}$ of 24 hours is equal to 1 d. 18 h. 27 min. 41 ſec.; $\frac{96}{27}$ is equal to 3 d. 13 h. 20 min.; $\frac{129}{18}$ is equal to 7 d. 4 h. 0 min.; and $\frac{134}{8}$ equal to 16 d. 18 h. 0 min.; whence it appears that the error in one day's mean motion of the firſt ſatellite is not quite $+ 37''$; of the ſecond ſomewhat more than $+ 37''$; of the third not quite $+ 3''\frac{1}{2}$; and of the fourth about $- 18''$.——The firſt and ſecond of theſe errors will amount to one hour's motion of each ſatellite in about $97\frac{1}{2}$ days' uſe with one rectification; but the third and fourth will not be perceptible by the eye in ſeveral years, by reaſon of the ſlowneſs of the ſatellites' motions, as well as ſmallneſs of their daily errors. With regard to the calculation of the other wheels, the fraction $\frac{56}{8}$ is equal to 7 days for the week-hand; and the compound one $\frac{56}{8} \times \frac{73}{7}$ is equal to 73; but the ſpiral face on the cover has five lines divided into

365 equal

365 equal parts, therefore five revolutions of the month-hand, viz. 5 × 73, will complete the year. The 365 days in the spiral are divided into calendar months, the initials of each of which stand immediately over the beginning of the first day of each; so that when the instrument is rectified for the month, day, and hour at any particular time, the different hands will preserve their respective situations to the end of the year, and so on from year to year, provided the month-hand be put back one day in the spiral at the latter end of February in every leap year. This putting back, however, will not in the least affect the motions of the satellites; for the hands, being put upon the circular ends of the axles, will turn in adjustment without giving motion to the wheels.

As the error in the first and second satellites will amount in one year to about three hours and forty-four minutes motion of each, they must also after that space be put to their right places again, which is very easily done thus: Turn the handle in a retrograde direction till all the satellites have come back 4 h. 44 min. then hold the first and second in that particular position while the handle is again turned to its original situation, and then let them be at liberty.—If this is done at a quarter of a year after rectification, the error will be somewhat less than an hour, and it may render a new rectification by the nautical almanac, as will be described hereafter, needless.

If at any time the letter pointed to in the week circle, and the corresponding day of the month in the spiral, in the first, second, third, fourth, or fifth line, as the case may be, be both attentively observed, it will appear by inspection for what particular time the satellitian stands rectified in a given year after it has been out of use; for, as the month-hand lies over the five lines, that particular line is the proper one, the numerical point of which answers to the initial of the day to which the week-hand points: for example, in the plate the rectification of the hands is for noon of Wednesday June 14, 1797. Without a regard to such an inspection, a new rectification would become necessary every time the instrument was to be used, and in some cases probably when the means of rectification might not be immediately attainable.

If the orbits of the satellites were not necessarily too large in the satellitian to be proportional to those of the earth and Jupiter, when the latter is placed at such a distance from them as that a candle in its place may produce a distinct shadow of each, $5\frac{2}{10}$ times the distance between the two sockets, viz. $15\frac{6}{10}$ inches, would be the proper distance of the central candle from Jupiter, in which position the angle at Jupiter in the satellitian subtended by the distance between the sockets, viz. 3 inches, would be equal to the parallax of the earth's annual orbit seen from Jupiter; but as the exact proportions of all their orbits cannot be preserved, a mechanical adjustment becomes requisite to preserve the due arrangement of the shadows upon the screen.

The rectification ought always to be very minutely attended to, and is thus effected: Fix the satellitian firmly on a table, and, setting the centre of the candlestick just *four feet* from Jupiter, place a steady burning candle, or rather a lamp, in the central socket, upon the same level with Jupiter in the instrument, and a similar one in the socket supported by the arm at the same height; by this means two shadows of Jupiter will fall upon the screen when placed behind the satellitian: slide the screen as near Jupiter as a revolution of the longest arm will suffer it to approach, which will be upwards of $2\frac{1}{4}$ inches; and if the

the central candle and Jupiter form a right angle at the other candle, the two shadows will cover one another just so much as is represented in fig. 4. or fig. 6.; but if this is not exactly the case, the candlestick must be brought nearer, or removed farther, till this appearance is produced; and then the screen must be secured in its place, and the candlestick be suffered to remain. In the next place, with a pair of bow-compasses describe a circle coincident with that shadow which is occasioned by the central candle; and within that, three other concentric circles, the largest of the three to touch the edge of the other shadow when the arm remains unmoved; the next to touch the same when the arm is 46° from conjunction with Jupiter and the central candle; and the smallest to touch the same when the arm is 24° from a similar situation.

Here then the central candle will represent the sun; the outermost circle in fig. 4. or 6. the diametrical section of the sun's shadow at the orbit of the first satellite; the second circle is supposed to represent the appearance of the same at the orbit of the second; the third, the appearance at that of the third; and the small one, the appearance at that of the fourth, as viewed in perspective from the earth; whilst the candle carried by the arm will be the representative of the earth. Remove now the central candle, and Jupiter's remaining shadow will be his situation as viewed from the earth, coinciding with his real shadow, represented by the concentric circles, as far as the orbit of the second satellite, and then disuniting.

Hitherto the satellites themselves have been disregarded, and must next be placed in their proper situations by the help of a * nautical almanac, thus: Look for the day, for which the satellitian is to be rectified, among the configurations given in the last page of every month, and the relative *apparent* situations of all the satellites, at the hour specified at the top, will be found to the right and left of the central cypher which represents Jupiter. When the numerical figure stands between the representative point of any satellite and Jupiter, the satellite is approaching him; but when the point is put the nearer, it is receding from him: also, when a satellite is approaching on the right hand of Jupiter, or receding on the left, it is in its superior semicircle, and is placed above the centre of the cypher; but when receding on the right, or approaching on the left, it is in its inferior one, and stands higher than the centre. The relative situations and direction of motion of each satellite being observed, put them all to their places as near as the eye can guess by the shadows compared to the points in the almanac; then, having previously marked the greatest elongation of the fourth with little points on the screen, by means of a sector or diagram of similar triangles, make the distance of each shadow, from Jupiter's centre on the screen, bear the same proportion to the distance of each point from the centre of the cypher in the almanac, that the greatest elongation of the fourth on the former bears to the greatest elongation of the same in the latter: this will be attended with no difficulty.—The rectification for the *true* places of *mean motion* will however be the more accurate the

* The rectification may be made most accurately by calculating the time of a conjunction of each satellite, *mean* or *apparent*, as may be required, by the help of Wargentin's Tables, in any given month: but every reader cannot be supposed to have such Tables; and therefore the configurations are here substituted as affording a more general, as well as familiar method.——W. P.

 smaller

ſmaller the grand equation of the ſatellites, which depends upon Jupiter's anomaly at the time of rectification.

The laſt moſt favourable time was on October 11, 1794, and the next leaſt favourable will be on July 5, 1800: the former will recur on Auguſt 21, 1806, and the latter on May 17, 1812; and two of each will follow at nearly ſix years diſtance from each other in each revolution of Jupiter afterwards.

If the diameters of the cyphers in the nautical almanac, which are too ſmall, were ſo far augmented as to bear an * exact proportion to the greateſt elongations of the ſatellites there exhibited, which appears to be not the caſe, their apparent places, meaſured by thoſe diameters, might be eaſily aſcertained without further trouble on any given day, as well as their true places of mean motion at the ſuitable times above ſpecified.

The configuration fig. 7. is for half paſt ſix P. M. on October 11, 1794, where it appears, from what has been already remarked, that the firſt ſatellite is approaching Jupiter in its ſuperior ſemicircle near its greateſt weſtern elongation; the ſecond receding in its inferior; and the third and fourth receding each in its ſuperior; the third being near its greateſt eaſtern elongation. The correſponding places of the ſhadows on the ſcreen, when the ſatellitian is rectified for this time, will appear as in fig. 8.

The ſhadows may be made to fall a little higher in their inferior ſemicircle than in their ſuperior, by giving the inſtrument a ſmall reclination with a thin wedge placed under the end next the candle, or by an adjuſting ſcrew preſſing againſt the table, which effect is greateſt with the fourth.

In this repreſentation the ſatellites are very nearly in their true places of *mean* motion, as well as in their *apparent* places: therefore, if the ſatellitian be rectified for Saturday October 11, 1794, at half paſt ſix in the evening, by this latter configuration, and the handle be turned till the month-hand comes to the end of February 1796, before it be put back a day, and thence forward to the preſent time (1798), provided the firſt and ſecond arms be alſo adjuſted for their errors in motion, as already directed, the rectification will be more accurate for *mean* motion at preſent than if made by the almanac at any other time.

From 1794 to 1800 the difference between mean and apparent motion of the ſatellites, rejecting the ſmaller equations, is increaſing; and from 1800, when it will be a maximum, to 1806, it will decreaſe, the apparent being faſter than the mean, according to the year for which the ſatellitian is to be uſed: from 1806 to 1812 it will again increaſe, after which time a decreaſe will commence; and, in a little leſs than every ſix years, the increaſe and decreaſe will continue to be alternate for the time to follow.

Laſtly: Put the index of the candleſtick to the ſun's place in the ecliptic, and the heliocentric longitude of Jupiter taken from a nautical almanac or White's Ephemeris, immediately between the central ſtem and Jupiter in the machine, by means of a thread ſtretched and tied to both, which will ſerve as an index, as well as a guide for the diſtance of the

* The diameter of Jupiter is repreſented in the oppoſite extreme in the Encyclopædia Britannica, both in fig. 18. plate 62. and in fig. 177. plate 79. though it is ſaid, in vol. ii. part 2. p. 577. that "the orbits of Jupiter's moons are drawn in true proportion to his diameter" in the latter diagram, and are evidently intended to be ſuch in the former.——W. P.

candlestick; and in this situation the representatives of the Sun, Earth, and Jupiter, as also of the satellites, will all be in their relative situations for exhibiting the general phenomena of the Jovian as a detached system.

But before I give a particular description of these phenomena as they will be exhibited by the satellitian, the reader will perhaps form a more accurate conception of the extent of its application, if a summary account be first given of those minute irregularities in the motions of the satellites, which no simple machinery can be supposed to represent.

The satellites of Jupiter were discovered by Galileo in January 1610, and called *Medicean stars*, in honour of Cosmo Medici great duke of Tuscany. This astronomer continued his observations upon them for 27 years, till unfortunately the loss of sight frustrated the fruit of his continued labours. In 1663 Borelli published a theory of the Medicean Stars, but had not acquired data sufficient for ascertaining the exact quantities and qualities of their motions.—After him Cassini, in the year 1668, favoured the world with "Tables of the Motions of Jupiter's Satellites," which were improved and edited by him again in 1693. These Tables have since that time been rendered still more accurate by Messrs. Hadley, Pound, Bradley, &c. as also by the French astronomers, and lastly by Wargentin the Swedish astronomer, whose best Tables are now used in calculating the immersions and emersions given in the nautical almanac.—The times of the mean periodical revolutions have been already mentioned; but they are subject to such inequalities of motion as require the following equations for ascertaining their apparent places: viz.

1. For the light which depends upon Jupiter's eccentricity: 2. For the light which depends upon his change of place in his orbit: 3. For his anomaly: 4. For the mutual gravitation of the three first, the period of which is upwards of 437 days: 5. For a period of 12 years for the third, accruing from an unknown cause: 6. For a similar period for the fourth, accruing from its eccentricity: 7. For the variable inclination of the orbit of the second; and, 8. for apparent time.—The greatest or grand equation, which depends upon Jupiter's anomaly, has been shewn to be different in different years: with the first satellite it is possible for it to amount to 1 h. 18 min. 16 sec.; with the second, to 2 h. 37 min. 12 sec.; with the third, to 5 h. 16 min. 32 sec.; and with the fourth, to 12 h. 20 min. 34 sec. at a certain time in some particular years above specified, though in others it may be nothing.

From measurements of the greatest elongations of these satellites, taken by a micrometer, it does not appear certain that their orbits are elliptical, except that of the fourth: the others, however, are by analogy supposed to be such, though the ellipses are so like circles that they may be considered as such, even in calculations, without any apparent error. The diameters of these orbits, when viewed from the earth, subtend but very small angles; viz. the first subtends 3′ 55″; the second, 6′ 14″; the third, 9′ 58″; and the fourth, 17′ 30″.—The diameters of each of the satellites themselves, compared to that of Jupiter, seem not to be accurately known. Mr. Herschell's observations make that of the first to be not quite $\frac{2}{10}$ of a second; whereas, before his notice, they were each considered to subtend an angle more than double this quantity; for their diameters were estimated at $\frac{1}{18}$ or $\frac{1}{20}$ of Jupiter. Future observations must determine this point.

7 In

In obferving the eclipfes of Jupiter's fatellites by his fhadow, it has been found that the duration of an eclipfe of each is longer at fome times than at others: that in fome inftances a fatellite paffes through the centre of the fhadow, which is fuppofed to be a [illegible] conical, but at others through only a chord of its circular fection; hence an inclination of each of their orbits to that of Jupiter has been proved, and calculated to be as follows: Of the firft, about 2° 55′, the afcending node being at reft near the middle of Aquarius; of the fecond, variable from 2° 50′ (in 1668), to 3° 2′ (in 1715), the afcending node being at reft about 5° of Aquarius; of the third, variable from 3° (in 1695), to 3° 24′ (in 1765), its afcending node being about 25° 58′ of Aquarius at this time (1798), and moving forwards eight minutes in a year; and of the fourth, about 2° 40′, which is very little variable, the afcending node being about the middle of Aquarius.

The apojove of the fourth is at about 24½° of Aries (1798), and moves forward about 3° in five years.

The duration of an eclipfe is the greateft at the nodes and fmalleft at the limits: that of the firft varies from 2 h. 16 min. to 2 h. 7 min. 40 fec.; of the fecond, from 2 h. 51 min. 20 fec. to 2 h. 13 min. 4 fec.; of the third, from 3 h. 35 min. 40 fec. to 1 h. 2 min. 32 fec.; and of the fourth, from 4 h. 46 min. to 0 h. 0 min. 0 fec.

The two oppofite points of the ecliptic, cut by the plane of each orbit extended, are called the geocentric nodes; and a fatellite appears to move in an exact ftraight line only when the earth is in one of thofe; for at other fituations of the earth the track of each appears, though in a fmall degree, elliptical, and the more fo the farther the earth is removed from their nodes: this is moft apparent with the fourth, notwithftanding the nodes are all in the fame figns, by reafon of the greatnefs of its orbit; for, when near either limit, it entirely efcapes an occultation, nor is eclipfed if removed above 52° from either node. This happens to be the cafe this year, and will continue fo nearly, if not quite, throughout the next; and recurs for nearly two years and a half in every fix.

The line in which the fatellites appear is nearly horizontal, as on the fcreen, when Jupiter is on the meridian, but becomes the more oblique the farther he is removed therefrom.

The greateft part of thefe inequalities and peculiarities of motion, as well as their rotations, will not be attempted to be illuftrated by the fatellitian, but fuch phenomena only as are demonftrable to the eye of an obferver independently of calculations, and which therefore may be confidered as the moft proper fubjects of illuftration by machinery.

After having given a defcription of the fatellitian and its appendages; of the method of rectifying it for ufe; and of the principal minutiæ relating to the motions of the fatellites; I come now, in the laft place, to particularize thofe phenomena to be illuftrated, which a telefcope of a moderate magnifying power for celeftial objects will prefent to the obfervation of a fpectator, and which afford a perpetual fource of amufement to any perfon who is in poffeffion of a good inftrument. It is neceffary however to make this previous remark, that if the telefcope invert the object, the pofterior furface of the fcreen muft be viewed; but if it fhew it direct, the anterior will be proper.

When the fatellitian is properly rectified, the fcreen fixed and marked, and the candleftick adjufted with only one candle, the central one being removed, each turn of the handle will produce one day's motion of every satellite; and a continuation of slow regular turns will produce a pleasing view of the following phenomena; viz.

1. The shadows will move in nearly a straight line.

2. Some in a direct and others in a retrograde direction.

3. The most distant will frequently appear the nearest to Jupiter.

4. Near Jupiter they will move the quickest, and slowest near their greatest elongation, where they become stationary for a short time.

5. Their greatest elongations will be a little before quadrature, when west; and after, when east of Jupiter.

6. Hence it will appear, that the superior portion of each orbit is greater than the inferior.

7. Whether to the east or west of Jupiter, their motions will be direct in the superior, and retrograde in the inferior, parts of their orbits.

8. When passing between Jupiter and the earth (candle) they will transit him.

9. When passing the same line in their superior semicircle they will suffer an occultation.

10. When passing through the four concentric circles they will be eclipsed.

11. The *mean* or *apparent* times, accordingly as the instrument may be rectified, of each of these phenomena, will be pointed out by the hour-hand, the name of the day by the week-hand, and the day of the month by the month-hand, in each year.

12. If the candlestick is adjusted frequently for the sun's place and Jupiter's heliocentric longitude, the solar shadow will appear to alter its situation to the right and left of Jupiter, as he approaches to or recedes from conjunction or opposition.

13. The reason will be evident, from the concentric circles, why an immersion or ingress into Jupiter's shadow, and a subsequent emersion or egress out of it, never both happen with the first and * second satellites; nor with the third, if Jupiter is within 46° of opposition to, or conjunction with, the sun; nor with the fourth, if that distance is less than 24°.

14. It will be likewise evident why an immersion only is visible of the first and second satellites from a conjunction to an opposition (fig. 6.), why an occultation and eclipse may be coincident when at opposition (fig. 5.), and why an emersion only is seen from an opposition to a conjunction (fig. 4.).

15. If a proper reclination be given to the satellitian in this and on the next year, and also on every sixth and seventh year hence, the fourth satellite will neither be eclipsed nor suffer an occultation.

16. If a candle be now placed in each socket of different lengths, as is represented in fig. 3. the shadow of the central one to cover the concentric circles, there will be two rows of shadows on the screen above one another, one of which will represent the heliocentric, and the other the geocentric, places of the satellites.

* An immersion and subsequent emersion may be seen of the second satellite, provided it be near one of its limits at the same time that Jupiter is near both his perihelion and quadrature with the sun; but this will very rarely happen.——W. P.

17. Hence

17. Hence it will appear, that an eclipſe ſeen from the earth is an occultation ſeen from the ſun.

18. That an eclipſe is never viſible at the ſun.

19. And that the ſhadow of a ſatellite, as ſeen from the earth, falls on Jupiter ſometimes before, ſometimes after, and ſometimes at its tranſit, according to the relative ſituations of Jupiter, the ſun, and earth.

20. If the central candle only be ſuffered to remain, and the little balls themſelves be viewed in an oblique direction, the reaſon will be apparent why the ſatellites are ſeen lunated from Jupiter:

21. And alſo Jupiter lunated when ſeen from them;

22. But neither of them lunated when ſeen from the earth or ſun.

23. It will alſo be evident why the ſun is frequently eclipſed to the inhabitants of Jupiter by the ſhadows of his ſatellites.

24. And, laſtly, why the ſatellites, as ſeen from the earth, do not eclipſe one another.

Theſe, and perhaps other phenomena not ſpecified here, will be illuſtrated by the ſatellitian in ſo diſtinct a manner, as to convey to a ſpectator, who may compare the apparent motions of the ſhadows to the real motions of the little balls, a clear conception of the relation that the apparent motions of Jupiter's ſatellites, as viewed from the earth with a teleſcope, have to their true circular motions, as viewed by the inhabitants of Jupiter. But though a clear conception may thus be formed by a ſpectator of the true and apparent motions of the ſatellites as they regard Jupiter himſelf, yet he muſt be informed that the *real track* in which they move round the *ſun* along with Jupiter, the progreſſive centre of their detached ſyſtem, is neither circular nor elliptical, but in a line which croſſes Jupiter's track in a ſinuoſe manner, more or leſs frequently as their periods are ſhorter or longer. As the velocity of the firſt and ſecond ſatellites exceeds the velocity of Jupiter, they are not only apparently, but really retrograde in ſome part of every revolution; on which account their tracks form loops which are alternately concave and convex towards the ſun; the concavity being greater than the convexity: but as the velocity of Jupiter exceeds that of the third and fourth, theſe are not really but only apparently retrograde at the inferior ſemicircles: on this account their tracks are always concave, except at their ſtationary points, which project and divide the concave ſpaces. Theſe real tracks will eaſily be comprehended, if, while the ſatellites are in motion, Jupiter himſelf be alſo conceived to have a direct motion ſlower than the two firſt, but quicker than the two laſt.

If now, after what has been ſaid, we conceive ourſelves, like the philoſophical poet *, conveyed into the regions of Jupiter and his ſatellites, we ſhall with him find cauſe for adoring the power, wiſdom, and goodneſs of the Almighty Creator! When we conſider that Jupiter, the diameter of which planet is more than ten times larger than that of our earth, has a rotation on its axis in the ſmall ſpace of nine hours and fifty-ſix minutes, we

* ——— "remote from day's all-cheering ſource,
"Large Jupiter performs his conſtant courſe:
"Four friendly moons with borrow'd luſtre riſe,
"Beſtow their beams benign, and light his ſkies." BAKER's *Univerſe*.

must perceive, that, without the assistance of some other luminary besides the sun, total darkness would be the lot of his inhabitants for nearly five hours in every ten.—Again, if we consider that Jupiter's year, or periodical revolution, contains 4332 d. 8 h. 51½ min. of our time, or 22936,43 + of his own days, and that he has no sensible change of seasons, by reason of the nearly perpendicular direction of his axis, the inclination of which is only 1° 20′, we must see that the number of his days in his year might frequently be lost, without some intermediate remembrancers between those two very distant extremes: accordingly, the Omnipotent Father of the creation has provided a remedy for these inconveniences: he has furnished Jupiter with these four moons to cheer his inhabitants with light in the frequent absence of the sun's rays, and to present them with four different kinds of months, like our years, months, and weeks.

Of the first kind of months in Jupiter's year there are 244,97 +, each of which contains 9,36 + of his days; of the second kind there are 121,89 +, each containing 18,81 +; of the third kind there are 60,45 +, each comprising 37,94 + ; and of the fourth kind there are only 25,85 +, each of which comprises 88,68 + of his days; so that each shorter month, particularly of the three first kinds, is very nearly *double* the next longer; and may be considered as exactly such, in counting time by divisions and subdivisions of months, by the help of intercalary days.

But besides these uses of Jupiter's satellites, and others, perhaps, which our limited capacities can never comprehend, there are three very considerable advantages, which the inhabitants of our globe possess, accruing from the observations which have already been made upon them; viz. an accurate knowledge of the parallax of the earth's annual orbit; of the real velocity of light; and of an easy method of ascertaining the longitude of places by land; all which are explained in the different books upon astronomy:—so true is it throughout the whole grand scale of nature, that no individual part is without its utility.

Lincoln,
March 10, 1798.

VI.

*Objections to the Opinion of Professor Spallanzani respecting the Cause of the Light of Natural Phosphori. Communicated to Mr. John Fabbroni, Sub-Director of the Royal Museum of Florence. By M. JOACHIM CARRADORI, M. D.**

AT length the hypothesis of Gœttling is entirely destroyed, in consequence of the refutations of various celebrated authors, and among them Spallanzani. You may perhaps recollect that I foretold its short duration, and I pointed out to Brugnatelli, at its first appearance, several proofs of its falsity, which might be deduced from the very experiments it was built upon.

Though the refutation of Spallanzani † has not the merit of being the first, its excellence

* Annales de Chimie, xxiv. 216.

† Chimico Esame del Cittadino Spallanzani. Modena. 1796.

cannot

cannot be difputed. I have read this fhort treatife with pleafure, from the many excellent and amufing points of knowledge it contains. But on perufing it with a certain degree of attention, I cannot avoid making feveral obfervations which the facts appear to demand.

It appears to me that Spallanzani fuppofes himfelf to be the firft who obferved that water has the property of abforbing oxygen from the air of the atmofphere. For at the 114th page of his book, he fays: Trovai pertanto che l'acqua e' un mezzo di decomporre l'arie, come lo fono il fosforo di Kunckel e i fulfuri alcalini; mà ella agifce con eftrema lentezza. "I found, however, that water is a medium for decompofing the air in the fame manner as Kunckel's phofphorus, or the alcaline fulphurets; but it acts with extreme flownefs."—But this fact was already known to Scheele (Treatife of Air and Fire), and confequently the honour of this difcovery is his right. He was the firft who obferved that, by keeping a bottle of atmofpheric air inverted over water for feveral days, the water gradually rifes; the volume of air is diminifhed, and azotic gas, or, as it was then called, phlogifticated air, remains alone. It is furprifing that Spallanzani fhould not be aware of this; or, if he knew it, that he fhould have omitted mentioning it.

I have very important facts to ftate againft his theory of natural phofphori. It is long fince I firft obferved that the phofphoric wood not only fhines under water and under oil, but even in the barometric vacuum; which obfervations are related in the fecond volume of my theory of heat. If the phofphoric wood require air to enable it to fhine, how can it continue to emit light under oil, where the vital air neither exifts nor can have accefs? I muft likewife remark the difference which Spallanzani has obferved between the urinous phofphorus and that of wood; namely, that when the former is entirely furrounded by any pure mephitic air, it immediately ceafes to give light; whereas the other being placed in pure azotic air continues to fhine during fix minutes, and does not entirely lofe its light till half an hour afterwards.

When Spallanzani introduced phofphoric wood into vital air, or oxygen gas, how did it happen that he omitted to obferve, whether by its fhining in that fluid for a confiderable time, there was no diminution of volume, as he obferved that this diminution took place when the phofphoric flies (lucciole) were placed therein?

The luccioloni, or glow-worms, as well as the lucciole, fhine under oil. I have obferved a luminous fly continue to fhine perfectly for a quarter of an hour in the barometric vacuum.

Spallanzani found that the phofphorus of thefe flies fhines much more in oxygen gas than in the air of the atmofphere, and is totally extinguifhed by air not capable of maintaining combuftion. But how can we explain their fhining under oil for hours together? The fact is certain, becaufe I have repeated the experiment with many variations; fometimes by putting the phofphoric flies entire beneath the oil, and fometimes the phofphorus only detached from the infect, and even crufhed.

This obfervation muft be well known to you, fince I communicated it laft year to the Royal and Oeconomical Society of Florence, in my Memoir on the Lucciole; and it will be equally known to the whole fcientific world, as it will be publifhed in the xiiith volume of the Chemical Annals of the celebrated Brugnatelli.

The

The experiment of placing the luminous flies in oxygen is not new. It was before made by Forſter; for which ſee his Theory of Heat, and the Journal de Rozier for 1784. He obſerved that they give much more heat in this fluid, not only at intervals, but continually. M. Forſter moreover aſſures us, that he diſcovered the organs of reſpiration in thoſe inſects, which M. Spallanzani could not find. He deſcribes them preciſely, by obſerving that in each of the luminous rings there are two air paſſages, furniſhed with valvules at their apertures, and that theſe paſſages are loſt in the interior ſtructure of the animal.

I have ſeen two ſpecies of luccioloni, or ſhining worms; namely, a larger and a ſmaller, of which I have given a ſlight deſcription in my Theory of Heat, where I ſpeak of phoſphori. The firſt ſpecies alſo differs from the ſecond in colour; the former being nearly grey, and the latter nearly black. The firſt have much luminous matter in the three laſt rings; and the others have leſs in the laſt ring but one. Theſe may perhaps have been the ſpecies obſerved by the Naturaliſt of Pavia. Both ſpecies conceal their phoſphorus at pleaſure with the utmoſt facility. They ſeem therefore to be aware of its effects, particularly the ſecond ſpecies.

It is not true, as certain naturaliſts, as well as M. Spallanzani, pretend, that the luccioloni and lucciole are animals of the ſame ſpecies, different only in ſex, that is to ſay, that the former are the males, and the latter the females. I can affirm that I have ſeen the lucciole pregnant, with the abdomen filled with eggs: and this part, which before was ſo luminous, had its phoſphoric part very much diminiſhed, being reduced to merely two points, and two ſmall lateral portions. The lucciole begin to diſappear preciſely at the time of their pregnancy, which is the cauſe why they conceal themſelves. If they be ſought for among the graſs and underwood, they are found in this ſtate*.

If it be certain then that phoſphoric wood, the luccioloni and the lucciole continue to ſhine under oil, it muſt be admitted that this light does not ariſe from a ſlow combuſtion, as Spallanzani pretends, becauſe oil contains no air to ſupport it. This is the legitimate and immediate conſequence which muſt follow from this experiment, and cannot be refuſed. The experiments of Spallanzani lead to peculiar conſequences, though different from thoſe he has deduced. It is poſſible that the unreſpirable air may act in a particular manner on theſe phoſphori, which may be capable of preventing the emanation of their lights; and that oxygen, by a particular action of a contrary kind, may augment it.

Why ſhould it be diſputed that the different airs may produce peculiar effects on theſe ſubſtances, with which we are not yet acquainted? To Spallanzani will be due the honour of firſt obſerving them. In the ſame manner as various fluids are pernicious to natural phoſphori, and prevent their ſhining, why may not the air produce the ſame effect? I have found by experiment, that the phoſphorus of the lucciole is ſuddenly extinguiſhed if they be plunged in alcohol, or in vinegar; but continues to ſhine in oil in the ſame manner as in water and in air.

It may be objected to me, that Spallanzani has made the experiment, that the phoſphorus of the ſhining worms produces a diminution of volume in oxygen gas, at the ſame time that their light is increaſed; whence it [illegible] be concluded that the proceſs is analogous

* See the note No. 1. p. 78. tom. ii. of my Theory of Heat. C.

to combuftion. But this confequence is not certain. How many fubftances are there which have the property by their emanations of altering vital air, in the fame manner as combuftion, which neverthelefs do not burn nor emit light? The fame may be the cafe with the phofphoric matter of thefe infects.

There is not a perfect analogy, as Spallanzani pretends, between the phofphori he has obferved and the urinous phofphorus, becaufe the latter does not fhine but at a certain heat; whereas the natural phofphori fhine at any temperature whatever, provided it be not fo great as to alter their fubftance. This proves, in my opinion, that the light is not an effect of combuftion; for every combuftion requires a more or lefs elevated degree of heat.

With regard to the explanation given by Spallanzani of the change of wood into the luminous matter, which fuppofes that the hydrogen and carbon, being fet at liberty, attract oxygen; I will venture to fay, on the ftrength of my experiments, that it is not probable. It is certain (for I have obferved it myfelf, and probably fome others before me), that wood, when luminous, has almoft totally loft its refinous part, and that confequently in that ftate it retains fcarcely any either of the carbonic or hydrogenous principle*, to which its combuftibility was owing. And in fact, fuch wood as has become luminous is with difficulty burned in the fire, and produces no flame, as every one may try. Indeed it cannot be otherwife; for the progrefs of putrefaction, which reduces it to that ftate, muft have deprived it of much of its component parts; particularly the moft volatile, fuch as hydrogen. Befides which, as I obferved fome months ago to Brugnatelli, I think very differently from Spallanzani with regard to the conftitution of thefe fubftances; namely, that they become phofphorefcent in proportion as they have loft their inflammable principle, and that the property of abforbing and retaining the light depends on that circumftance.

My opinion may be extended, in preference to that of Spallanzani, to the caufe of the phofphoric property of animals; for it is more reafonable: becaufe we cannot imagine that their luminous matter is either refinous or oily, and confequently is not inflammable; neither can it contain much carbon or hydrogen. See my Memoire on the Lucciole, Ann. Chim. et Hiftoriques de Pavie, tom. xiii.

If the lucciole fhine beneath the water, as Spallanzani maintains, becaufe the oxygen gas contained in water ferves to maintain their combuftion, why does not the phofphorus of Kunckel alfo fhine beneath that fluid? It was likewife neceffary to have exhibited fome experiments in fupport of that opinion; as for example, to have fhewn that the phofphorus of thofe animals alters or abforbs the vital air contained (uncombined) in water, and that water which does not contain it is not capable of caufing them to fhine.

Pavia,
April 15, 1797.

* Of what principles, then, is this vegetable refidue compofed? N.

VII. *Sketch*

VII.

Sketch of the Hiſtory of Sugar, in the early Times, and through the Middle Ages (a). By W. Falconer, M.D. F.R.S. &c. &c.

THE uſe of ſugar is probably of high, though not remote antiquity, as no mention of it is made, as far as I can find, in the ſacred writings of the Old Teſtament (*b*). The conqueſts of Alexander ſeem to have opened the diſcovery of it to the weſtern parts of the world.

Nearchus *, his admiral, found the ſugar cane in the Eaſt Indies, as appears from his account of it, quoted by Strabo (*c*). It is not, however, clear, from what he ſays, that any art was uſed in bringing the juice of the cane to the conſiſtence of ſugar.

Theophraſtus †, who lived not long after, ſeems to have had ſome knowledge of ſugar, at leaſt of the cane from which it is prepared. In enumerating the different kinds of honey, he mentions one that is found in reeds (*d*), which muſt have been meant of ſome of thoſe kinds which produce ſugar.

Eratoſthenes ‡ alſo is quoted by Strabo (*e*), as ſpeaking of the roots of large reeds found in India, which were ſweet to the taſte both when raw and when boiled.

The next author, in point of time, that makes mention of ſugar, is Varro §, who, in a fragment quoted by Iſidorus (*f*), evidently alludes to this ſubſtance. He deſcribes it as a fluid, preſſed out from reeds of a large ſize, which was ſweeter than honey.

Dioſcorides (*g*) ‖, ſpeaking of the different kinds of honey, ſays, that "there is a kind of "it, in a concrete ſtate, called *ſaccharon*, which is found in reeds in India and Arabia "Felix. This, he adds, has the appearance of ſalt; and, like that, is brittle when chewed. "It is beneficial to the bowels and ſtomach, if taken diſſolved in water; and is alſo uſeful "in diſeaſes of the bladder and kidneys. Being ſprinkled on the eye, it removes thoſe

(*a*) Mancheſter Memoirs, iv. 291.

(*b*) Since writing the above, I have obſerved that the *ſweet cane* is mentioned in two places of ſcripture, and in both as an article of merchandize. It does not ſeem to have been the produce of Judea, as it is ſpoken of as coming from a far country. Iſaiah, chap. xliii. v. 24. Jeremiah, chap vi. v. 20.—It is worthy of remark, that the word *ſachar* ſignifies, in the Hebrew language, inebriation, which makes it probable that the juice of the cane had been early uſed for making ſome fermented liquor.

(*c*) Ειρηκε δε περι των καλαμων οτι ποιουσι μελι, μελισσων μη ουσων. Strabon. l. xv.

(*d*) Αλλη δε εν τοις καλαμοις. Fragment of Theophraſtus preſerved in Photius. See p. 864. edit. Augſburg. 1601.

(*e*) Και τας ριζας των φυτων, και μαλιϛα των μεγαλων καλαμων, γλυκειας και φυσει και εψησει. Strabon. l. xv.

(*f*) Indica nam magna nimis arbore creſcit harundo;
Illius e lentis premitur radicibus humor,
Dulcia cui nequeant ſucco contendere mella. Iſidor. lib xvii. cap. 7.

(*g*) Eſt et aliud concreti mellis genus, quod ſaccharon nominatur. In Indiâ vero et Felici Arabiâ, in harundinibus invenitur. Salis modo coactum eſt; dentibus, ut ſal, fragile; alvo idoneum et ſtomacho utile, ſi aqua dilutum bibatur; vexatæ veſicæ, renibuſque auxiliatur. Illitum ea diſcutit, quæ tenebras oculorum pupillis offundunt. Matthioli Dioſc. cap. lxxv.

* Ante Chriſt. ann. 325. † A. C. 303. ‡ A. C 223. § A. C. 68. ‖ A. C. 35.

 "ſub-

" fubftances that obfcure the fight." The above is the firft account I have feen of the medicinal virtues of fugar.

Galen * appears to have been well acquainted with fugar, which he defcribes, nearly as Diofcorides had done, as a kind of honey, called *facchar*, that came from India and Arabia Felix, and concreted in reeds. He defcribes it as lefs fweet than honey, but of fimilar qualities, as detergent, deficcative, and digerent. He remarks a difference, however, in that fugar is not like honey injurious to the ftomach, or productive of thirft (*a*).

If the third book of Galen, " *Upon Medicines that may be eafily procured*," be genuine, we have reafon to think fugar could not be a fcarce article, as it is there repeatedly prefcribed.

Lucan † alludes to fugar, in his third book, where he fpeaks of the fweet juices expreffed from reeds, which were drank by the people of India (*b*).

Seneca ‡, the philofopher, likewife fpeaks of an oily fweet juice in reeds, which probably was fugar (*c*).

Pliny § was better acquainted with this fubftance, which he calls by the name of *faccaron*; and fays, that it was brought from Arabia and India, but the beft from the latter country. He defcribes it as a kind of honey, obtained from reeds, of a white colour, refembling gum, and brittle when preffed by the teeth, and found in pieces of the fize of a hazel nut. It was ufed in medicine only (*d*).

Salmafius, in his Plinianæ Exercitationes, fays, that Pliny relates, upon the authority of Juba the hiftorian, that fome reeds grew in the Fortunate Iflands, which increafed to the fize of trees, and yielded a liquor that was fweet and agreeable to the palate. This plant he concludes to be the fugar cane; but I think the paffage in Pliny (*e*) fcarcely implies fo much.—Hitherto we have had no account of any artificial preparation of fugar, by boiling or otherwife; but there is a paffage in Statius ‖, that feems, if the reading be genuine, to allude to the boiling of fugar, and is thought to refer immediately thereto by Stephens in his Thefaurus (*f*).

Arrian ¶, in his Periplus (*g*) of the Red Sea, fpeaks of the honey from reeds, called

(*a*) De fimplic. Medicamentis. Lib. vii.

(*b*) Quique bibunt tenerâ dulces ab arundine fuccos. Lucani Pharfaliæ lib. iii. lin. 237.

(*c*) Aiunt inveniri apud Indos mel, in arundinum foliis, quod aut ros illius cœli aut ipfius arundinis humor dulcis et pinguior gignat. Senec. Epiftol. l. i. Epift. lxxxiv.

(*d*) Saccaron Arabia fert, fed laudatius India. Eft autem mel in arundinibus collectum, gummium modo candidum, dentibus fragile, ampliffimum nucis avellanæ magnitudine, ad medicinæ tantum ufum. Plin. Hiftor. Natural. l. xii. cap. viii.

(*e*) Plin. Hift. Nat. lib. vi. cap. xxxii.

(*f*) Et quas præcoquit Ebofita cannas
Largis gratuitum cadit rapinis. Stat. Sylv. I. vi. 15.

Haud dubie (inquit Stephanus) cannas intelligit ex quibus faccharum exprimitur vel coquitur. Et fortaffe cannas pro faccharo ipfo pofuit. Sed qui Ebofitæ illi, hactenus apud neminem invenimus. Populi fortaffe funt Indiæ, ubi faccharum potiffimum nafcitur. Steph. Thef. Vox Canna. Lectio autem dubia eft. Vide Not. Marklandi in hunc locum.

(*g*) Μελι το καλαμινον το λεγομενον Σαχαρι. Page 150. Ed. Amftelod. 1683, 8vo.

* Anno poft Chrift. nat. 143. † Lucani mors, A. D. 65. ‡ Senecæ mors, A. D. 65.
§ Plinii mors, A. D. 77. ‖ A.D. circ. 80. ¶ A.D. 145.

facchar

ſacchar (Σαχαρ) as one of the articles of trade between Ariace and Barygaza, two places of the Hither India, and ſome of the ports on the Red Sea.

Ælian *, in his Natural Hiſtory, ſpeaks of a kind of honey, which was preſſed from reeds that grew among the *Praſii*, a people that lived near the Ganges.

Tertullian † alſo ſpeaks of ſugar, in his book *De Judicio Dei*, as a kind of honey procured from canes (*a*).

Alexander Aphrodiſæus (*b*) ‡ appears to have been acquainted with ſugar, which was in his time regarded as an Indian production. He ſays, "that what the Indians called "ſugar, was a concretion of honey, in reeds, reſembling grains of ſalt, of a white colour, "and brittle, and poſſeſſing a detergent and purgative power like to honey; and which "being boiled, in the ſame manner as honey, is rendered leſs purgative, without impairing "its nutritive quality."

Paulus Ægineta (*c*) § ſpeaks of ſugar as growing, in his time, in Europe, and alſo as brought from Arabia Felix; the latter of which he ſeems to think leſs ſweet than the ſugar produced in Europe, and neither injurious to the ſtomach nor cauſing thirſt, as the European ſugar was apt to do.

Achmet (*d*), ‖ a writer who, according to ſome, lived about the year 830, ſpeaks familiarly of ſugar as common in his time.

Avicenna (*e*), ¶ the Arab phyſician, ſpeaks of ſugar as being a produce of reeds; but it appears he meant the ſugar called Tabaxir or Tabarzet, as he calls it by that name.

It does not appear, that any of the above-mentioned writers knew of the method of preparing ſugar, by boiling down the juice of the reeds to a conſiſtence. It is alſo thought, the ſugar they had was not procured from the ſugar-cane in uſe at preſent, but from another of a larger ſize, called Tabarzet (*f*) by Avicenna, which is the *Arundo Arbor* of Caſpar Bauhin, the *Saccar Mambu* of later writers, and the *Arundo Bambos* of Linnæus. This yields a ſweet milky juice, and oftentimes a hard cryſtallized matter, exactly reſembling ſugar, both in taſte and appearance.

The hiſtorians of the Cruſades make the next mention of ſugar of any that have fallen under my obſervation.

The author of the Hiſtoria (*g*) ** Hieroſolymitana ſays, that the Cruſaders found in Syria certain reeds called *Cannameles*, of which it was reported a kind of wild honey was made; but does not ſay that he ſaw any ſo manufactured.

(*a*) Mella viridanti confragrant pinguia canna. Tertullian. de Judicio Dei.

(*b*) Alex. Aphrodiſæi lib. ii. Probl. 79.

(*c*) Paul. Æginetæ Vox Mel. Μελι. p. 632. Medic. Art. Princ. Ed. Henrici Stephani, 1567.

(*d*) Vide Meurſii Gloſſ. Græc. Barb. & Du Cange Gloſſ. ad Script. med. & inf. Græcitatis.

(*e*) De Zuccaro. Lib. II. Tract. II. De Melle. Lib. II. Tract. II.

(*f*) Some of the writers ſay, that it was ſo called from the name of a place Σαχαρ Ταβαρζυς, τοπος ἐλω καλμενος εις Συριαν. Conſtantinus a Secretis, MS. quoted from Du Cange Gloſſ. Græc. The word Tabarzet ſignifies white, and is tranſlated, by Du Cange, Saccar Album. Herbelot ſays, that the Perſians called by that name the hardeſt and moſt refined ſugar. Bibliothéque Orientale, p. 810.

(*g*) Pars ſecunda, p. 595.

* A. D. circ. 145. † A. D. 195. ‡ A. D. 212.

§ A. D. circ. 400. vel ſecundum Friend multo poſterior. Hiſt. Medic.

‖ A. D. 830. ¶ A. D. 980. natus. ** 1100.

Albertus

Albertus Agnenſis *(a)* * relates, that about the ſame period, "the Cruſaders found ſweet "honeyed reeds in great quantity, in the meadows about Tripoli, in Syria, which reeds "were called *Zucra*. Theſe the people (the Cruſaders army) ſucked, and were much "pleaſed with the ſweet taſte of them, with which they could ſcarcely be ſatisfied. This "plant (the author tells us) is cultivated with great labour of the huſbandmen every "year. At the time of harveſt, they bruiſe it when ripe in mortars; and ſet by the "ſtrained juice in veſſels, till it is concreted in form of ſnow, or of white ſalt. This, "when ſcraped, they mix with bread, or rub it with water, and take it as pottage; and it "is to them more wholeſome and pleaſing than the honey of bees. The people who were "engaged in the ſieges of Albaria Marra and Archas, and ſuffered dreadful hunger, were "much refreſhed hereby."

The ſame author †, in the account of the reign of Baldwin, mentions eleven camels, laden with ſugar, being taken by the Cruſaders, *(b)* ſo that it muſt have been made in conſiderable quantity.

Jacobus de Vitriaco mentions ‡, *(c)* that "in Syria reeds grow that are full of honey, by "which he underſtands a ſweet juice, which by the preſſure of a ſcrew-engine, and con-"creted by fire, becomes ſugar." This is the firſt account I have met with of the employment of heat or fire in the making of ſugar.

About the ſame period § *(d)* Willermus Tyrenſis ſpeaks of ſugar as made in the neighbourhood of Tyre, and ſent from thence to the fartheſt parts of the world.

Marinus Sanutus mentions *(e)* ‖, that in the countries ſubject to the Sultan, ſugar was produced in large quantity, and that it likewiſe was made in Cyprus, Rhodes, Amorea, Marta, Sicily, and other places belonging to the Chriſtians.

Hugo Falcandus *(f)* ¶, an author who wrote about the time of the Emperor Frederic Barbaroſſa, ſpeaks of ſugar being in his time produced in great quantity in Sicily. It appears to have been uſed in two ſtates; one wherein the juice was boiled down to the conſiſtence of honey, and another where it was boiled farther, ſo as to form a ſolid body of ſugar.

(a) Calamellos ibidem mellitos, per camporum planiciem abundanter repertos, quos vocant Zucra, ſuxit populus illorum ſalubri ſucco lætatus; et vix ad ſaturitatem prædulcedine explere hoc guſtato valebant. Hoc enim genus herbæ, ſummo labore agricolarum, per ſingulos excolitur annos. Deinde, tempore meſſis, maturum mortariolis indigenæ contundunt, ſuccum colatum in vaſis ſuis reponentes, quouſque coagulatus indureſcat, ſub ſpecie nivis vel ſalis albi. Quem raſum cum pane miſcentes, aut cum aquâ terentes, pro pulmento ſumunt; et ſupra favum mellis guſtantibus dulce ac ſalubre eſſe videtur. His ergo calamellis melliti ſaporis, populus in obſidione Albariæ Marræ et Archas multum horrenda fame vexatus, eſt refocillatus.

Geſt. Dei per Francos, p. 270.

(b) Geſta Dei, p. 353.

(c) Sunt autem calamelli, calami pleni melle ſucco dulciſſimo, ex quo quaſi in torculari compreſſo, et ad ignem condenſato, prius quaſi mel poſthæc quaſi Zuccara efficitur. Geſt. Dei, p. 1075.

(d) Per inſtitores ad ultimas orbis partes deportatur. Geſt. Dei, p. 835.

(e) Marin. Sanut. L. I. Part I. Cap. 2.—in parte ſecunda Geſt. Dei.

(f) In Præfatione ad Libr. de Calamitatibus Siciliæ.

* 1108. † 1110. ‡ 1114. § 1124. ‖ 1306. ¶ 1170.

The foregoing are all the paſſages that have occurred to my reading on this ſubject. They are but few and inconſiderable, but may ſave trouble to others, who are willing to make a deeper enquiry into the hiſtory of this ſubſtance.

Jan. 24, 1790.

The following paſſage, taken from the Viridarium Franciſci Mendozæ, Sacræ & Profanæ Eruditionis. Coloniæ Agrippinæ, 1633, ſeems to point out, though rather obſcurely, the conſtruction and principles of Balloons.

"Vas æreum, plenum aëre, aliter demergendum, in ſumma aqua ſuſtentatur, cum ea ſit naturaliter multo gravius; ergo navis lignea, aut cujuſcunque alterius materiæ in ſummâ aëris ſuperficie conſtituta, et elementari igne repleta, ſupra aërem ſuſtinebitur, nec prius in ipſo aëre ſubmergetur, quam navigii gravitas ſuperet levitatem ignis, quo plenum eſt."

Problema XLVII. Utrum aër parte aliquâ ſit navigabilis. W. F.

VIII.

Experiments and Obſervations on the Effect of Annealing a Plate of Metal, conſiſting of fine or alloyed Gold, with one twenty-fourth Part of Tin. By MATHEW TILLET.*

THOUGH the advantage be great, that men of enlightened minds and well known artiſts ſhould make inquiries into the kind of works to which their attention has been particularly applied, and ſhould render their obſervations public; it is nevertheleſs true, that their reputation, in many reſpects well founded, does in ſome inſtances cauſe the reſults drawn from their experiments to be too ſpeedily adopted; and that when they are carefully repeated, it does not always happen that they prove exact. Theſe artiſts, no doubt, well informed and faithful in their operations and reports, have not ptetended to offer them as deciſive; but by neglecting to conſider the facts before them in every point of view, they have too ſpeedily concluded that theſe facts, univerſally conſidered, were ſuch as to them they appeared at firſt ſight.

Looking over the Journal de Phyſique for the month of September 1788, I ſaw a paper entitled "Experiments and Obſervations on the Fuſion of Gold with Tin," which I read with great attention, as being the production of a man of merit well verſed in his art, namely, Mr. Alchorne, aſſay-maſter to the Engliſh mint. As the object of his experiments is intereſting to every artiſt who uſes gold and ſilver; as its aim is to remove their fears reſpecting the mixture of a certain quantity of tin with fine or alloyed gold, and as theſe artiſts do nevertheleſs retain their apprehenſions with regard to the ſmalleſt mixture of tin with the gold of various fineneſs, which they are inceſſantly melting; I have thought it proper to repeat the experiments of Mr. Alchorne: and while I admit part of the facts he has related, I muſt refute thoſe which my own trials ſhew to be erroneous, and which, if they had not eſcaped his attention, would doubtleſs have led him to make certain exceptions, in the too poſitive conſequences he has drawn from his operations.

Before I enter upon a detail of the experiments of Mr. Alchorne, as well as my own, I think it proper to take notice of the manner in which the editor of the Journal announces

* Memoirs of the Academy of Sciences at Paris for the year 1790, being the concluding volume, printed in 1797.

ees the memoir in queftion. He appears to be convinced that the world had adopted an erroneous opinion refpecting the object of the paper previous to its appearance; whence it follows, that this Journal, which is defervedly efteemed, and of wide circulation, cannot fail to produce a ftrong impreffion in this refpect, which ought to be contradicted. It is in fact founded on operations which have not been purfued with fufficient accuracy; and on this account they tend to produce a falfe fecurity in the mind of artifts who work the moft precious of metals.

It has long been a received fact among metallurgifts, obferves the editor, that tin mixed with gold in the fmalleft quantity, either in fubftance or in vapour, is totally deftructive of the malleability of that metal. But Mr. Woulfe, fellow of the Royal Society of London, communicated to that Society, in 1764, a memoir of Mr. Alchorne, affayer at the Mint in the Tower of London, and fince printed in the Philofophical Tranfactions, in which he proves, that tin may be mixed with gold in a moderate quantity without producing thefe bad effects. Thefe experiments, he adds, have not been contradicted; notwithftanding which, feveral of the moft celebrated authors have continued to follow the ancient opinion, though in no refpect founded upon fact. It may therefore be fuppofed, that the memoir of Mr. Alchorne has not been fufficiently known and attended to; for which reafon I have thought proper to give an abftract, to make it more generally known.

M. Alchorne relates, that he had long doubted this extraordinary property attributed to tin; and that an opportunity having offered, he made various experiments on that fubject. He mixed twelve ounces of fine gold with different proportions of tin from fixty grains to half an ounce. Thefe compounds were beaten under the hammer, paffed through the laminating rollers, and ftruck in the fly-prefs without fhewing any brittlenefs. He likewife attempted to expofe gold to the vapour of tin; for which purpofe he put twelve ounces of gold of 22 carats into a fmall crucible, which he placed in a larger crucible, and furrounded it with tin, and fubjected the whole to a confiderable heat for half an hour; but the gold loft nothing of its ductility. He carried his refearches ftill farther: he alloyed the foregoing mixtures with copper, and afterwards added tin to the gold thus alloyed with different proportions of copper and filver; but in all the various cafes, twelve ounces of gold alloyed with tin in the quantity of half an ounce, and of copper two ounces and a half, fuffered hammering and laminating to the thicknefs of ftrong paper, and could be wrought into fmall toys and drawn into fine wire with the fame facility as the gold of commerce.

M. Alchorne obferves, that the old opinion adopted by fo many authors owes its origin probably to the arfenic which tin commonly contains, as he found that twelve grains of that femi-metal in regulus rendered the fame number of ounces of gold brittle. Whence he concludes that tin, like the other bafer metals, does not injure gold, but in proportion to the quantity of arfenic it contains, and that there is nothing in tin which can deprive gold of its qualities, as was before obferved.

From this fhort account of the experiments and obfervations of M. Alchorne, we fee that he affirms that gold, whether pure or alloyed, being fufed with tin in the proportion of one part of the former to twenty-four of the latter, forms an alloy which preferves ductility fufficient to endure hammering and laminating to the thicknefs of ftrong paper, to be ufed in toys and drawn into fine wire with the fame facility as the common ftandard gold.

Though

Though I was persuaded that tin deprived gold of its great ductility, or at least rendered it so brittle that it could not be reduced to thin leaves, nor more especially be made to pass the wire plate but by virtue of repeated annealing, and peculiar treatment which gold of the usual ductility does not require, I nevertheless determined to repeat the experiments of Mr. Alchorne, as well from esteem for that skilful artist, as to supply such facts as might appear to have escaped his notice.

My first experiment consisted in mixing 24 grains of fine gold with one of tin, taken from an ingot of this last metal which contains no arsenic. I wrapped this grain of tin in the 24 grains of gold reduced to a very thin leaf, rendered very flexible by annealing. I placed these 25 grains upon a piece of charcoal hollowed out, upon which they could be supported during their fusion. I even sprinkled a small quantity of calcined borax upon the metal, in order that the fusion might be more sudden, that the metal might flow together, and the tin unite with the gold without allowing time for it to become calcined. This alloy was speedily fused by the enameller's lamp, and reduced into a small button without any loss of weight. It was then flattened carefully beneath the hammer; but, notwithstanding my precaution in this respect, it cracked, and at last broke into three pieces, its thickness then being a quarter of a line or thereabouts.

I repeated this first experiment with a double quantity as well of pure gold as of tin. The result was the same. This second button was brittle, and likewise broke under the hammer, though I had carefully managed the process of hammering in order that this button might have continued whole notwithstanding the cracks.

It is easily seen that these experiments, which were in some measure preparatory, tended to intimate the consequences I had reason to expect when I should repeat them more at large, and in a manner more nearly resembling the experiments with which I meant to compare them.

[*To be concluded in our next.*].

ACCOUNTS OF BOOKS.

Philosophical Transactions of the Royal Society of London, for the Year 1798. Part I. Quarto. 199 pages, with 26 pages of Meteorological Journal, and 7 plates. Sold by Elmsly, London.

THIS part contains the following papers: 1. The Bakerian Lecture. Experiments upon the Resistance of Bodies moving in Fluids. By the Rev. Samuel Vince, A.M. F.R.S. Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge.—2. Experiments and Observations, tending to shew the composition and properties of Urinary Concretions. By George Pearson, M.D. F.R.S.—3. On the Discovery of four additional Satellites of the Georgium Sidus; the retrograde motion of its old Satellites announced, and the cause of their disappearance at certain distances from the planet explained. By William Herschel, L.L.D. F.R.S.—4. An Enquiry concerning the Source of the Heat which is excited by Friction. By Benjamin Count of Rumford, F.R.S. M.R.I.A. (See Philos. Journal II. 106.)—5. Observations on the Foramina Thebesii of the Heart. By Mr. John Abernethy, F.R.S.—6. An Analysis of the earthy

earthy Subſtance from New South Wales, called Sydneia, or Terra Auſtralis. By Charles Hatchett, Eſq. F.R.S. (See Philoſ. Journal, II. 72).—7. Abſtract of a Regiſter of the Barometer, Thermometer and Rain, at Lyndon in Rutland, for the year 1796. By Thomas Barker, Eſq.—8. An Account of ſome Endeavours to aſcertain a Standard of Weight and Meaſure. By Sir George Shuckburgh Evelyn, Bart. F.R.S. and A.S.—9. A New Method of computing the Value of a ſlowly converging Series, of which all the Terms are affirmative. By the Rev. John Hellins, F.R.S. and Vicar of Potter's-Pury in Northamptonſhire. And the Appendix, containing a Meteorological Journal kept at the Apartments of the Royal Society, by Order of the Preſident and Council.

Count Rumford's Experimental Eſſays, Political, Economical, and Philoſophical. Eſſay VII.—Of the Propagation of Heat in Fluids. Part II.—An Account of ſeveral New Experiments, with occaſional Remarks and Obſervations, and Conjectures reſpecting Chemical Affinity and Solution, and the Mechanical Principle of Animal Life. Octavo. 75 pages, with 2 plates. Cadell and Davies. Price 1s. 6d.

This ſecond part accompanies a new edition of the firſt. The philoſophical world will not need any general remark on the intereſting nature of the ſubject, nor the manner in which the great author has treated it. For the preſent, I copy the abridgment of its Contents, and ſhall ſpeedily give a fuller account.

the severest frost explained on that supposition.—Probability that the metals would evaporate, when exposed to the action of the sun's rays, were they not good conductors of heat.—Mercury is actually found to evaporate under the mean temperature of the atmosphere. This fact is a striking proof that fluid mercury is a non-conductor of heat.—Probability that the heat generated by the rays of light is always the same in intensity; and that those effects which have been attributed to light ought perhaps in all cases to be ascribed to the action of the heat generated by them: a striking proof that the most intense heat does sometimes exist where we should not expect to find it.—Gold actually melted by the heat which exists in the air of the atmosphere, where there is no appearance of fire, or of any thing red-hot.—We ought to be cautious in attributing to the action of unknown powers, effects similar to those produced by the agency of heat.—The most intense heat may exist without leaving any visible traces of its existence behind it.—This important fact illustrated by the necessary result of an imaginary experiment - - page 345

Chap. IV. An account of a variety of miscellaneous experiments.—Thermometers with cylindrical bulbs may be used to show that liquids are non-conductors of heat.—Ice-cold water may be heated and made to boil standing on ice.—Remarkable appearances attending the thawing of ice, and the melting of tallow, and of bees-wax, by means of the radiant heat projected downwards by a red-hot bullet.—Beautiful crystals of sea-salt formed in brine standing on mercury.—Olive-oil soon rendered colourless by exposure to the air standing on brine.—An attempt to cause radiant heat from a red-hot iron bullet to descend in oil.—Account of an artificial atmosphere, in which horizontal currents were produced by heat.—Conjectures respecting the proximate causes of the winds - page 367

A Practical Essay on the Club-Foot, and other Distortions in the Legs and Feet of Children, intended to shew under what Circumstances they are curable or otherwise; with thirty-one Cases, and the Specification of a Patent granted the Author for his Method of Practice. By T. Sheldrake, Truss-maker to the Westminster Hospital and Mary-le-bone Infirmary. 8vo. 214 pages, with 14 plates. London: Printed for Murray and Highley, 1798.

This author shews by reasoning and induction, that the deformity which forms the chief subject of his treatise is produced by an unfavourable compression during the growth of the fœtus in utero. He adduces facts to prove, that mechanical means, if judiciously applied, will in numerous instances restore or place the organ in its proper state; and that it is of great importance that these remedies should be used as early as possible before the process of ossification is considerably advanced or completed. The contrivance and application of these means necessarily demand a knowledge of the formation of the parts, as well as of the nature of mechanical instruments. In both respects the author appears to have displayed considerable judgment, and has been rewarded by success. It is impossible to enter into any detail or description of his method within our limits. I shall therefore only say, that the nature of the cases and the respectability of the testimonies appear to deserve the attention of all who from professional pursuits or individual misfortune are urged to the consideration of this subject.

Fig. 1.

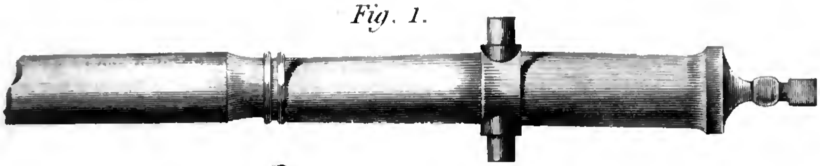


Fig. 2.

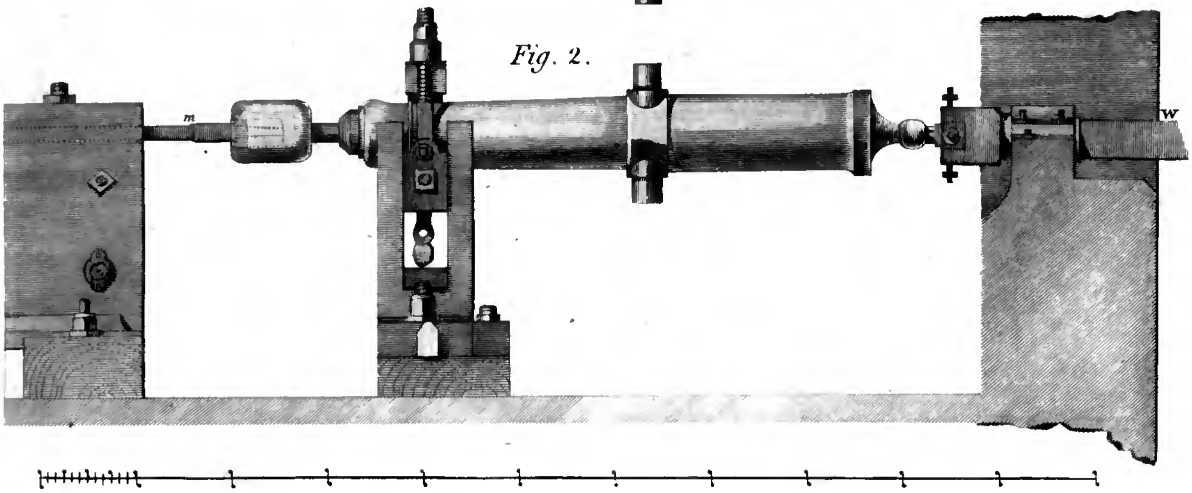


Fig. 3.

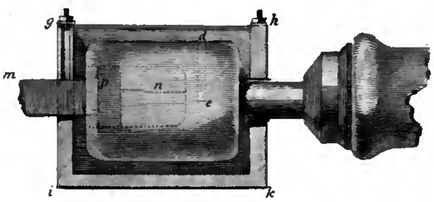


Fig. 4.

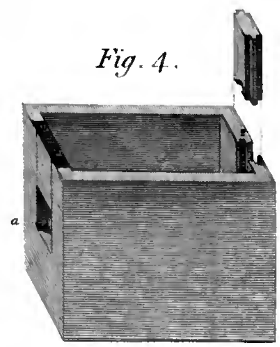
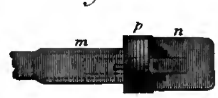


Fig. 5.

Fig. 7.

Fig. 6.

Fig. 8.



Barlow sculp.

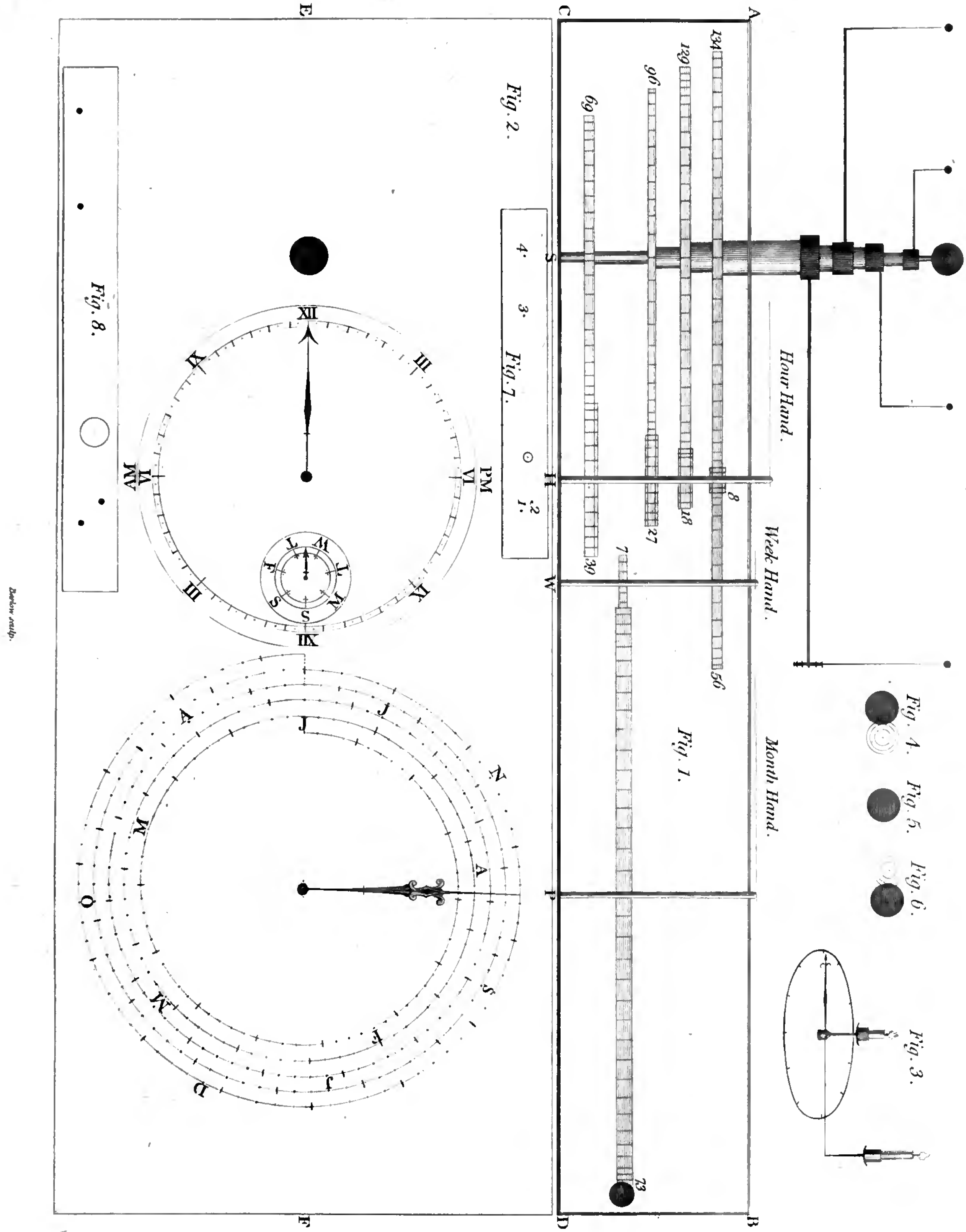


Fig. 1.
Hour Hand.
Week Hand.
Month Hand.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.
Fig. 7.
Fig. 8.
Barlow sculp.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JULY 1798.

ARTICLE I.

Memoir on a New Metallic Acid which exists in the Red Lead of Siberia. By VAUQUELIN. *Extracted from the Bulletin of the Soc. Philom.**

BY a new examination of the red lead of Siberia, Vauquelin is convinced that this mineral contains a metallic acid very different from all those which have hitherto been known. The following are the principal results of his experiments :

The red lead ore was reduced to fine powder, and boiled in a saturated solution of carbonate of potash. An effervescence of considerable duration was produced; the powder was dissolved, but soon afterwards a precipitate fell down of a yellowish white colour. The solvent had assumed a beautiful golden colour.

The precipitate proved to be carbonate of lead.

Nitric acid was poured into the alkaline fluid till the excess of carbonate of potash was saturated. The fluid exhibited an orange-red colour. It was then mixed with a solution of tin, recently prepared, with which it assumed a brown colour, that afterwards became greenish. When poured into a nitric solution of lead, it immediately generated the red lead. By spontaneous evaporation, it afforded crystals of a beautiful orange red, besides those of the nitrate of potash.

The nitric acid being poured into the solution of the red crystals occasioned no precipitate; but when, after evaporation to dryness, the crystals of nitrate of potash, which re-

* This abstract is translated from the Journal de Physique, bearing date for Nov. 1794, but lately printed.

mained at the bottom of the capfule, were wafhed with alkohol, a blue liquor was obtained, which after evaporation left a greenifh blue powder, foluble in water, of an acid tafte, and reddening the tincture of turnfol.

The red lead ore may likewife be decompofed by muriatic acid. If the latter be diluted with water, the mineralizing acid is precipitated in the form of a red powder: if it be concentrated, it re-acts on the metallic acid, deprives it of part of its oxygen, and caufes it to pafs to a deep green colour, while vapours of oxygenated muriatic acid are difengaged.

Thefe experiments are fufficient to prove that the mineralizing acid of the red lead of Siberia is a new fubftance; but as it has fome refemblance with the molybdic acid, Vauquelin has made a number of comparative experiments on their alkaline compounds, which prefented very evident differences. The following are the moft remarkable:

1. The acid of Siberian lead ore is red, when combined with potafh; the molybdate of potafh is white.

2. The molybdate of potafh affords a white precipitate with the nitrate of lead, whereas the red lead ore is regenerated of a beautiful orange colour, refembling that of the native ore when it is reduced to powder.

3. The molybdate of potafh affords a white precipitate in flocks, when added to the nitric folution of mercury. The falt formed by the fame alkali and the acid of Siberian red lead affords a precipitate of a deep cinnabar colour.

4. The former affords a white precipitate with the folution of filver; the latter a precipitate of the moft beautiful carmine red, which changes to a purple red by expofure to light.

The foregoing experiments fufficiently prove that this new acid is metallic, and differs much from the molybdic acid. It does not differ lefs from the other newly difcovered metals.

Uranium does not become acid, and cannot combine with the cauftic alkalis.

Titanium is foluble in acids, with which it forms cryftallizable falts, and does not combine with the cauftic alkalis.

Tungften becomes yellow in acids without diffolving, and affords white cryftallizable falts with the alkalis.

The author has not purfued this comparative examination, becaufe the properties of the other metallic fubftances are fufficiently known. He promifes to continue his refearches as foon as he fhall have procured more of this mineral.

P. S. Since this memoir was read to the Inftitute, Vauquelin has reduced the mineralizing acid of the red lead ore. This metal is grey, very hard, brittle, and eafily cryftallizes in fmall needles. The nitric acid acidifies it with confiderable difficulty.

H. V. C. D.

Farther

II.

Farther Experiments and Observations on the Affections and Properties of Light. By HENRY BROUGHAM, *jun. Esq.**

HAVING laid before the Royal Society an account of a courſe of experiments † on light, in which I had been engaged, and alſo of the concluſions which theſe experiments had taught me to draw; I proceed in the following paper to relate the continuation of my obſervations; which, I hope, may not prove wholly unintereſting to ſuch as honoured the former part with their attention. I am firſt to unfold a new and, I think, curious property of light, that may be indeed reckoned fourfold, as it holds, like the reſt, equally with reſpect to refraction, reflexion, inflexion, and deflexion; thus preſerving entire the ſame beautiful analogy in theſe four operations, which we have hitherto remarked. I ſhall then conſider ſeveral phenomena connected either with this, or with the properties before deſcribed, and of which they afford ſome ſtriking confirmations.

I.

Obſervation 1.—THE ſun ſhining ſtrongly into my darkened chamber, I placed, at a ſmall hole in the window-ſhut, a priſm, with its refracting angle (of 65°) upwards, ſo that the ſpectrum was caſt on a chart placed at right angles to the incident rays, and four feet from the priſm.

In the rays parallel to the chart, and two feet from it, I placed a pin, whoſe diameter was $\frac{1}{30}$ of an inch, and fixed it ſo that the axis of its ſhadow on the ſpectrum might be parallel to the ſides of the ſpectrum A ſet of images by reflexion was formed (ſimilar to thoſe deſcribed above ‡), all inclining to the violet; but what I chiefly attended to at preſent was their ſhape. I had always obſerved that the part formed out of the red-making rays was broadeſt, and that the other parts diminiſhed in breadth regularly towards the violet. I now delineated one or two, at about three inches from the ſhadow; and though (from the pin's irregularities) the ſides were by no means ſmooth, yet the general ſhape was in every pin, and with every priſm uſed, nearly as repreſented in fig. 1. Plate VII. divided in the direction R A, according to the colours of the ſpectrum in which they were formed; R O B A was red, and the broadeſt; that is, R A was broader than O B, the confines of the red and orange; and G D E V was the violet, narroweſt of all.

Obſervation 2.—Between the pin and the priſm, $\frac{1}{10}$ of an inch from the pin, was placed a ſcreen, through a ſmall hole in which, of twice the pin's diameter, the rays of the ſpectrum paſſed, and were reflected into images by the pin; theſe were pretty diſtinct and well defined, when received on a chart half a foot from the pin. They were oblong, having parallel ſides and confuſed ends; they were wholly of the colour whoſe rays fell on the pin, unleſs when the white, mixed with thoſe at the confines of the yellow and green,

* Philoſophical Tranſactions, 1797.

† See Philoſ. Journal i. 551. 585.

‡ Philoſophical Tranſactions for 1796, page 240, or Philoſophical Journal i. 557.

 cauſed

caused the images to be of all the colours. When the prism was turned round on its axis, so that different rays fell on the pin, the images changed their sizes as well as their positions: they were largest when red, and least when violet.

Observation 3.—In case it may be thought that the sides of the hole, through which the rays passed in Observation 2, by inflecting, might dispose them, before incidence, into beams of different sizes, I removed the screen, and placed the pin horizontally, the axis of the shadow being now at right angles to that of the prismatic spectrum; and moving the prism on its axis, again I observed the contraction, and dilatation of the images by reflection, though now they were rather less distinct, from the greater size of the incident beam; and to shew that there was both a change of size and of place, without any manner of deception, I placed one leg of a pair of compasses in a fixed point of the spectrum, and the other in the middle point of an image formed by the violet-making rays. The prism being then moved till the image became red, I again bisected it, and found its centre considerably beyond the point of the compasses, which was indeed evidently much nearer one end of the image than the other; besides that the red image, when measured, was longer than the rest: and this satisfied me that there were two changes, one of place, with respect to the fixed point, the other of size, with respect to the centre of the image. Lastly, as far as I could judge, the dilatation and contraction appeared even and uniform.

Observation 4.—I remarked that the fringes or images, by flexion, were always increased in size when formed out of red-making rays, and were less in every other colour, and least in violet (besides being moved farther from the edge of the shadow in the former rays than in the latter); and this agrees with an observation of Sir Isaac Newton, as far as he tried it, which was with respect to deflexion. In making several experiments with prisms, I hit on a very remarkable confirmation of this. I observed on each side of the spectrum four or five distinct fringes, like the images by reflexion, coloured in the order of the spectrum, but quite well defined at the edge, and even pretty distinct at the end: they were also much narrower than those images, but like them they inclined much to the violet, and were broadest in the red, growing narrower by degrees, and narrowest of all in the violet. I moved the prism, and they disappeared; but when the prism was brought back to its former position, they also returned. I then observed the prism in open light, and saw that it had veins, chiefly opaque and white, running through it, and that there were several of these in the place where the light passed when the prism was held as before. But in case the inclination and shape of these images might be owing to the irregular order in which the veins were laid, I held another prism, which happened to have parallel veins: in many positions of this the fringes or images returned, not indeed always so regular nor always of the same kind; for some were confused and broader, formed (as I concluded from this and their position) by reflexion; others made by transparent veins and air-bubbles were also irregular, but inclined to the red, the violet being farthest from the perpendicular, and these were obviously caused by refraction; yet all agreed in this, that they were broadest in the red, and narrowest in the violet parts.

Observation 5.—I held, in the direct rays of the sun at half an inch from the small hole in the window-shut, a glass tube, free from scratches and opaque veins, but, like most glass that is not finely wrought, having its surface of a structure somewhat fibrous. When this tube

tube was flowly introduced into the light, and fo held that none of the rays might be refracted, a ftreak, chiefly white, was feen, fimilar in fhape and pofition to thofe defcribed before *. When narrowly infpected, it was found to contain many images by reflexion in it. But thefe were much diluted by the abundance of white light, reflected without decompofition in the manner above-mentioned †. This ftreak lay wholly on one fide of the tube; but I moved the tube onward a little, and another ftreak darted through the fhadow, and extended all round on both fides: and now, when the tube was in the middle of the rays, there were two ftreaks on both fides, one a little feparated from the other and continued through the fhadow, the other on each fide of the fhadow: the former was evidently produced by refraction: it contained many images very like thofe by reflexion, only more vivid in the colours, which were all in the inverted order, the violet being outermoft, and the reft neareft the point of incidence. Images fimilar to thefe are alfo producible on the retina, as mentioned before ‡.

Obfervation 6.—I now placed a prifm at the hole, and made the fame images by refraction, out of homogeneal light. Thefe inclined to the red, not (like images by reflexion) to the violet; but they were broadeft in rhe red, and grew narrower towards the violet parts. In fhort, when viewed befide the images by reflexion, except in point of brightnefs and inclination, they differed from them in no refpect.

The three firft experiments fhew, that when homogeneal light is reflected, fome rays are conftantly difpofed into larger images than others are, that is, into images more diftended in length, though of the fame breadth. The fourth experiment fhews, that the fame takes place when light is inflected and deflected; and the two laft fhew that the fame happens when the rays are refracted in a way fimilar or analogous to that in which the other images were produced by reflexion and flexion.

We are now to fhew, that this difference of fize is not owing to the different reflexibilities and flexibilities of the rays. In order to this we fhall both demonftrate, and then prove by experience, "that inflexion and deflexion do not decompound heterogeneous rays, whofe direction is fuch, that they fall on the bending body." In fig. 2. let A B be the body, GH, EF, CD, the limits of its fpheres of deflexion, inflexion, and reflexion, refpectively; and let I P be a white ray of direct light entering at P the fphere of deflexion: through P draw LK at right angles to GH; IP will be feparated into PR red, and PV violet, and the five other colorific rays according to their deflexibilities; at R and V draw the perpendiculars ST and QO; then the alternate angles PRT, RPL; and PVQ, VPL are equal each to each. But TRP and QVP are the angles of incidence, at which the red and violet enter the fphere of inflexion; and RPL, VPL are the angles of deflexion of the red and the violet; therefore the difference of the two latter, that is RPV, is likewife the difference of the two former. Suppofe this difference equal to nothing; or that PV and PR are parallel; then rRS the angle of the red's inflexion will be lefs than vVO the angle of the violet's inflexion, by the angle RPV: (when not evanefcent) add RPV to rRS; then rRS will be equal to vVO: that is, the divergence will be deftroyed, and the rays enter the fphere of reflexion, parallel and undecompounded. It is evident, therefore, that the effect arifing from the different deflexibilities of the rays is deftroyed by the equal

* Phil. Tranf. 1796, page 236. or Philof. Journal i. 555.

† Ibid. p. 237. or Phil. Journal i. 556.

‡ Ibid. p. 243. or Phil. Journal i. 559.

and

and oppofite effect produced by their different inflexibilities; and the fame thing may in like manner be fhewn to happen in the return of the rays from the body after reflexion. But let the rays be fo reflected that they fhall pafs by the body without entering any more than one fphere of flexion; then they will be feparated by their flexibilities, as we before defcribed. It appears, then, that if the rays of light were not differently reflexible, flexion could never produce the coloured images, by feparating the compound light. And indeed, this may be eafily proved by fact. At 144 feet from the bending body, the greateft fringes by flexion are only half an inch in length, whereas the fourth or fifth images by reflexion are above half an inch at one foot from the reflecting furface: the one fort is therefore more than 144 times more diftended than the other, whereas the flexion could, at the very fartheft, only double them. Alfo the diftinctnefs, and brightnefs, and regularity of the colouring, are quite different in the two cafes: the fuppofed caufe would neither account for the order of the colours, nor for their abfence in common fpecular reflexion, and refraction through two prifms joined together with their angles the contrary ways. Laftly, if we fuppofe the images to be produced by flexion, and then reflected from the body, it would follow that light incident on a prifm fhould be decompounded, formed into feveral coloured images, and then refracted, the violet being leaft, and the red moft bent; all which is perfectly the reverfe of what actually happens. I have multiplied the proof of this propofition, perhaps beyond what is neceffary; but its great importance to the whole theory will, I hope, plead my excufe.

Let us now fuppofe that a homogeneal beam paffes through the fpheres of flexion, it will follow that no divergence can take place from the bending power of the body; fo that we have only to eftimate the effect produced by the reflexion, and to enquire whether the different reflexibilities of the rays can caufe the images to vary their fizes according as they are formed by different rays. In fig. 3. let AB be the body, CD the limit of its fphere of reflexion, and IP a beam of homogeneal rays, as red, incident at P and reflected to R, forming there the image Rr. It is evident that the greater reflexibility of the rays IP can only alter the pofition of the centre of Rr, making it nearer the perpendicular than the centre of an image formed by any other rays would be. But the greater length of Rr fhews that a greater quantity of rays is reflected, or that the fame quantity is fpread over a greater fpace, and that in the following way. Let IF fi be a beam of violet-making rays entering ABCD, and reflected fo as to form the image Rv. The force exerted by AB decreafing according to fome law (of which we are as yet ignorant) as the diftance increafes, is not fufficient to turn the rays back till they have come a certain length within ABCD. But for the fame reafon it turns back all that it does reflect before they come nearer than a certain diftance: between thefe two limits, therefore, the rays are turned back. But the limits are not the fame to all the rays; fome begin to be turned at a greater diftance from the body than others, and confequently are reflected to a greater diftance from the middle ray of the incident beam. Thus if IF fi be changed to a red-making beam, it begins to be turned back at f, and the rays fartheft from AB are reflected to r inftead of to v, where they fell when IF fi was violet-making; not but that the fame quantity of rays is reflected: the only difference is, that the moft reflexible are reflected fartheft from the body by their greater reflexibility, and fartheft from each other by this other property. Exactly the fame happens in the cafe of refraction, mutatis mutandis; but there feems to be a flight variation in the manner in which the different rays are difpofed into images of different fizes by flexion. In this cafe alfo the bending

ing body's action reaches farther when exerted on fome rays than when exerted on others: but then, the direction of the rays not paffing through the body, thofe which are fartheft off and at too great a diftance to be bent, never coming nearer, are not bent at all; and confequently as the leaft flexible rays are in this predicament at the fmalleft diftance, and the moft flexible not till the diftance is greater, the images formed out of the former muft be lefs than thofe formed out of the latter. This difference in the way in which the phenomenon appears, does not argue the fmalleft difference in the caufe: it only follows from the different pofition of the rays, with refpect to the acting body, in the two cafes. I infer then from the whole, that different forts of rays come within the fpheres of flexion, reflexion, and refraction, at different diftances, and that the actions of bodies extend fartheft when exerted on the moft flexible. It may perhaps be confiftent with accuracy and convenience to give a name to this property of light; we may therefore fay that the rays of light differ in degree of refrangity, reflexity, and flexity, comprehending inflexity and deflexity. From thefe terms (uncouth as, like all new words, they at firft appear) no confufion can arife, if we always remember that they allude to the degree of diftance to which the rays are fubject to the action of bodies. I fhall only add an illuftration of this property, which may tend to convey a clearer idea of its nature. Suppofe a magnet to be placed fo that it may attract from their courfe a ftream of iron particles, and let this ftream pafs at fuch a diftance that part of it may not be affected at all: thofe particles which are attracted may be conceived to ftrike on a white body placed beyond the magnet, and to make a mark there of a fize proportional to their number. Let now another equal ftream confiderably adulterated by carbonaceous matter, oxygen, &c. pafs by at the fame diftance, and in the fame direction. Part of this will alfo be attracted, but not fo far from its courfe, nor will an equal number be affected at all; fo that the mark made on the white body will be nearer the direction of the ftream, and of lefs fize than that made by the pure iron. It matters not whether all this would actually happen, even allowing we could place the fubjects in the fituation defcribed: the thing may eafily be conceived, and affords a good enough illuftration of what happens in the cafe of light.

Purfuant to the plan I before followed, I now tried to meafure the different degrees of reflexity, &c. of the different rays; but though the meafurements which I took agreed in this, that the red images were much larger than the reft, and the green appeared by them of a middle fize, yet they did not agree well enough (from the roughnefs of the images, and feveral other caufes of error) to authorize us to conclude with any certainty "that the action of bodies on the rays is in proportion to the relative fizes of thefe rays." This, however, will moft probably be afterwards found to be the cafe: in the mean time there is little doubt that the fizes are the caufe of the fact.

II.

SEVERAL phenomena are eafily explicable on the principles juft now laid down.

1. If a pin, hair, thread, &c. be held in the rays of the fun refracted through a prifm, extending through all the feven colours, a very fingular deception takes place: the body appears of different fizes, being largeft in the red, and decreafing gradually towards the violet. This appearance feemed fo extraordinary, that fome friends who happened to fee it as well as

 myfelf,

myfelf, fufpected the body muft be irregular in its fhape. On inverting it, however, the fame thing took place; and on turning the prifm on its axis, fo that the different rays fucceffively fell on the fame parts, the vifible magnitude of the body varied with the rays that illuminated it. This appearance is readily accounted for by the different reflexity of the rays, and follows immediately from Obfervation 2d and 3d.

2. Sir Ifaac Newton found that the rings of colours made by thin plates, and by thick plates of glafs (as he calls them), when formed of homogeneal light, varied in fize with the rays that made them, being largeft in the moft flexible rays. I have had the pleafure of obferving feveral other forts of rings, fo extremely fimilar, and formed by flexion, that I can no longer doubt of this being alfo the caufe of the phenomena obferved by Newton. I fhall firft defcribe a fpecies, to prove "that the colours by thick and thin plates are one and the fame phenomena, only differing in the thicknefs of the plates." Happening to look by candle light upon a round concave plate of brafs, pretty well polifhed, fo as to reflect light enough for fhewing an image of the candle, I was furprifed to fee that image furrounded by feveral waves of colours, red, green, and blue, difpofed in pretty regular order. This was fo uncommon in a metallic fpeculum, that I examined the thing very minutely by a variety of experiments: thefe I fhall not particularly now defcribe, but give a general idea of their refults.

It muft be obferved, for the fake of clearnefs, that in the following enquiries concerning the formation of rings or fringes, the diameter of a ring or fringe means the line paffing through the centre of that ring, and terminated at both ends by the circumference; whereas the breadth means that part of the diameter intercepted between the limits of the ring, or the diftance between its extreme colours, red and violet.

In the firft place, they were formed by the fun's light in the figure of rings, furrounding the centre of the fphere to which the plate was ground, at greater diftances increafing their breadths, the colours pretty bright, though inferior in brilliancy to thofe of concave fpecula.

Secondly, the order of the colours was in all red outermoft, and violet or blue innermoft, with a greyifh-blue fpot in the common centre of the whole; and on moving the plate from the perpendicular pofition, the rings moved and broke exactly like thofe of fpecula.

In the third place, homogeneal light made them of fimple colours; they were broadeft when red, narroweft when blue and violet.

Fourthly, they decreafed in breadth from the centre; and I found by a fimple contrivance, that they were to one another in the very fame ratio that the rays by fpecula follow.

In the fifth place, I compared the general appearance of the two forts by viewing them at the fame time, and was ftruck with their general appearance, unlefs that thefe of fpecula were moft vivid and diftinct.

Thefe things made me fufpect that they were actually caufed by the thin coat of gums with which the furface of the plate was varnifhed, called lacker. Accordingly I took it off with fpirit of wine, and found the rings difappear; on lackering it again they returned; and in like manner I caufed a well finifhed concave metal fpeculum to form the rings of which we are fpeaking, by giving it a thin coat of lacker. This is a clear proof that thefe rings

rings were exactly the fame with thofe of thick plates (to ufe Newton's expreffion); for the coat of gums is, when thin, pretty tranfparent, as may be feen by laying one on glafs plates.

But this coat is extremely thin, and cannot exceed the 200th part of an inch; fo that the colours of thick plates are in fact the very fame with thofe of thin plates, except that the two kinds are made by different fized plates. We cannot, therefore, diftinguifh them, any more than we do the fpectrum made by a prifm whofe angle is 90° from that made by one whofe angle is 20°. This kind of colours is not the only one I have obferved of nearly the fame kind with thofe of plates; we fhall prefently fee another much more curious and remarkable.

III.

IN reflecting on the obfervations and conclufions contained in my former paper, feveral confequences feemed to follow which appeared fo new and uncommon, that I began to doubt a little the truth of the premifes; but at any rate was refolved to examine more minutely how far thefe inferences might be confiftent with fact: and I am happy in being able to announce the completenefs of that confiftency, even beyond my expectations. The chief confequences were the following:

1. That a fpeculum fhould produce, by flexion and reflexion, colours in its reflected light wherever it has the leaft fcratch or imperfection on its furface.

2. That on great inclinations to the incident rays all fpecula, however pure and highly polifhed, fhould produce colours by flexion.

3. That they fhould alfo in the fame cafe produce colours by reflexion.

4. That lenfes, having the fmalleft imperfections, fhould produce by flexion colours in their refracted light.

5. That there fhould be many more than three, or even four fringes by flexion, invifible to the naked eye. And,

6. That Iceland cryftal fhould have fome peculiarities with refpect to flexion and reflexion; or if not, that fome information fhould be acquired concerning its fingular properties refpecting refraction.

The manner in which the firft of thefe proportions is demonftrated a priori, is evident from the 4th figure, where CD is the reflecting furface, vo a concavity bearing a fmall ratio to CD, Ao and AB rays proceeding to CD. The one, AB, will be feparated into Br red, and Bv violet, by deflexion from o, and will be reflected to r′ v′, forming there the fringes. The other, Ao, being reflected, will be feparated into Bx and By, by deflexion from v, forming other fringes, xy, on the fide of vo's fhadow oppofite to r′v′. Alfo when vo is convex inftead of concave, the like fringes will be produced by the rays being deflected in paffing by its fides. Laftly, when vo is a polifhed ftreak, images by reflexion will be produced, as defcribed Phil. Tranf. for 1796, p. 269. (Philof. J. i. 593.) The fame paffage will alfo fhew the reafon why, on great inclinations, colours by reflexion fhould be produced. And the fecond propofition, with refpect to flexion, follows from what was demonftrated in this paper (p. 149 and 150); it being that cafe where the rays either leave or fall on the fpeculum at fuch an inclination as to come only within the fphere of inflexion, without being deflected. The fourth propofition is merely a fimple cafe of flexion. And the two laft require no illuftration. I fhall now relate how I inquired into the truth of thefe things a pofteriori.

Obfervation 1.—Looking at a plane glafs mirror expofed to the fun's light, I obferved that up and down its furface there were minute fcratches (called hairs by workmen), and that each of thefe reflected a bright colour, fome red, others green, and others blue. On moving the mirror to a different inclination, or my eye to a different pofition with refpect to the mirror, I faw the fpecies of the colours change; the red, for inftance, became green, and the green blue. I applied my eye clofe to the mirror, and received on it the light reflected from one hair. I obferved feveral diftinct images of the fun much diftended and regularly coloured, juft like thofe defcribed above; the fame appearances were obfervable in all fpecula, metal and glafs, which had thefe hairs, and I never faw any metal one without fome: their fize is exceedingly fmall, not above $\frac{1}{1000}$ of an inch. Rubbing a minute particle of greafe on the furface of the fpeculum, images were feen on the fibrous furface; and they always lay at right angles to that direction in which the greafe was difpofed by drawing the hand along it.

Obfervation 2.—Befides thefe polifhed hairs, many fpecula have fewer or more fmall fpecks and threads, rough and black. Perhaps every polifhed furface is ftudded with a number of fmall ones, invifible to the naked eye from the quantity of regular light which it reflects. I took, from a reflecting telefcope, a fmall concave fpeculum not very well finifhed; its furface fhewed feveral fpecks to the naked eye, and many with a microfcope. Its diameter was $\frac{37}{50}$ of an inch, its focal diftance two inches, and the fphere to which it was ground eight inches diameter. I placed it at right angles to the rays of the fun, coming through a fmall hole $\frac{1}{42}$ of an inch diameter, into a very well darkened room; I then moved it vertically, fo that the rays might be reflected to a chart 12 inches from the fpeculum, and confequently 10 from the focus: and though the focus appeared white and bright, yet on the chart the broad image was very different. It was mottled with a vaft number of dark fpots; thefe were of two forts chiefly, circular and oblong. Of the former a confiderable number were diftinct and large, the reft fmaller and more confufed, but fo numerous that they feemed to fill the whole image. None were quite black, but rather of a bluifh grey, and the oblong ones had a line of faint light in the middle, juft as is the cafe in fhadows of fmall bodies. But the chief thing which I remarked was the colours. Each oblong and round fpot was bordered by a gleam of white, and feveral coloured fringes feparated by fmall dark fpaces. The fringes were exactly like thofe furrounding the fhadows of bodies, of the fame fhape with the dark fpace, having the colours in the order, red on the outfide, blue or violet in the infide—the innermoft fringe was broadeft, the others decreafing in order from the firft. I could fometimes fee four of them, and, when made at the edge of the large image, I could indiftinctly difcern the lineaments of a fifth: when two of the fpots were very near one another, their rings or fringes ran into one another, croffing.

Obfervation 3.—When the chart was removed to a greater diftance, as fix feet, the fringes were very diftinct and large in proportion; alfo the fmaller fpots became more plain, and their rings were feen, though confufedly, from mixing with one another. When the fpeculum was turned round horizontally, fo that its inclination to the incident rays might be greater, the diftance of the chart remaining the fame (by being drawn round in a circle), the

the ſpots and fringes evidently were diſtended in breadth. I have endeavoured to exhibit the ſun's image, as mottled with fringes or rings and ſpots, in fig. 5.

Obſervation 4.—I placed the ſpeculum behind a ſcreen with a hole in it, through which were let paſs the homogeneal rays of the ſun, ſeparated by refraction through a priſm; this being turned on its axis, the rays which fell on the ſpeculum were changed; the fringes were now of that colour whoſe rays fell, and when the rays ſhifted, the fringes contracted or dilated, being broadeſt in the moſt flexible rays, and conſequently in thoſe whoſe flexity is greateſt.

[*To be continued.*]

III.

Enquiries reſpecting the Colouring Matter of Vegetables, and the Action of Metallic Subſtances and their Oxides upon them; together with a New Proceſs for obtaining Lakes of the moſt intenſe and ſolid Colours. Read to the National Inſtitute (of France) 15 Vendemiaire, in the Year VI. By the C. GUYTON.*

LINNÆUS, the great naturaliſt of the north, had affirmed that the red colours of vegetables announce the preſence of an acid. It was long ago obſerved, that the juice of the violet acquires a beautiful blue ſhade in veſſels of tin, the uſe of which metal was recommended in diſpenſatories for the preparation of violet ſyrup; and the original colour of ſuch ſyrups as had been changed by keeping, was reſtored by long digeſtion in tin. Little attention however was paid to the cauſe of theſe phenomena; and our aſſociate, Berthollet, in his Elements of the Art of Dyeing, had pointed it out no otherwiſe than by conjecture, when he ſuppoſed an acid to have combined with the oxide formed at the ſurface of the tin.

Such was the ſtate of our knowledge on this ſubject, when, from the ſtriking difference of colour of two preparations of the ſame fruit, I undertook to examine the circumſtances in which theſe changes take place.

I ſuppreſs the detail of experiments to which I ſubjected almoſt all the acid coloured fruits in ſucceſſion, ſuch as the ſtrawberry, the gooſeberry, the plum, as well as the petals of flowers, turnſol, fernambouc, turmeric, &c. by treating them comparatively in veſſels of glaſs, of porcelain, of metal, and metallic alloys, or by keeping them in digeſtion on plates of metal perfectly cleaned, or upon metallic oxides. I ſhall confine myſelf at preſent to ſuch reſults as may improve the theory of vegetable colours, or afford ſome uſeful applications to the proceſſes of the arts.

Theſe experiments prove that the red colour of fruits is manifeſtly owing to the real action of their peculiar acid upon their colouring matter.

That tin, when it brightens or reſtores the colour of violets, does nothing more than reſume, by ſuperior affinity, the acid which had cauſed it to turn red.

That tin or its oxide is not, as has hitherto been thought, the only metal which exerciſes

* Tranſlated from La Decade philoſ. litt. et politique, No. II. An VI. (Jan. 1798.)

 this

this affinity; but that lead, bifmuth, antimony, and zinc, produce the fame effect; that it takes place more fpeedily and completely with iron; and that the contact of all thefe metals produces a very perceptible violet tinge in fuch infufions, as without this circumftance are of a lively and decided red colour.

That the green and acid part of fruits does not contain the colouring principle which is difpofed to become red with acids; and that the coloured part retains in combination that portion of acid which is neceffary to maintain the ftate of re-action that determines the fhade.

That although this colouring principle is modified in certain vegetables fo far as to refift acid or alkaline re-agents to a certain degree, as fernambouc with regard to acids, and turnfol with regard to alkalis, yet it may be brought to this condition; which feems to fhew that it is, if not effentially yet at leaft originally, of the fame nature.

That the metallic oxides are not all equally proper to feize and fix vegetable colours; that fome among them appear to attack them with more facility, while others retain them with very little power.

Laftly, that the new metal called tungften, carried to the laft degree of oxygenation, which has not hitherto been tried in this refpect, has a decided advantage over all the other metallic oxides; that it is capable of forming lakes of great value to painters, which perfectly refift the proofs of lime-water, of acetic acid or radical vinegar, of hydro-fulphureous gas, and even to a certain point the oxygenated muriatic acid gas, that enemy of colours, which burns them fuddenly, and, according to the expreffion of Citizen Berthollet, reprefents in a few inftants the combined action of air and light. We fhall be lefs furprifed at this affertion, when it is recollected that this is the only metallic oxide which eludes the folvent power of the three mineral acids.

The oxide of tungften eafily becomes charged with the colours of all vegetable matters. I have hitherto found no more than one exception in the petals of nic-ago, of which I have not been able to extract the fine purple red, without being yet able to fufpect the caufe of this difference.

In general, the lakes formed with this oxide become deeper inftead of fainter when they are diluted. It is neceffary to foften the fhade. I have remarked that they acquire ftill more intenfity when the oxide has been previoufly rendered blue by boiling it in vinegar.

One of our affociates, Citizen Vauquelin, being informed of the object of my refearches, directed my attention to aloes. Citizen des Fontaines had the goodnefs to procure me feveral kinds. I fhall fpeak only of that which bears the name of foccotrine. I made experiments on this plant, which is one of the moft rich in colour, though it does not exhibit the flighteft appearance while the equilibrium of its principles is maintained by the energy of vegetable life. The woody fibre, which is the external part, then ferves as the covering of a very vifcid matter, of a greenifh white colour, weakly acid; but fcarcely has this matter been expofed to the air before it affumes a very lively red purple colour, which becomes very abundant by the progrefs of fermentation. I have formed lakes of this matter with alumine, oxide of tin, and the white oxide of zinc. None of them were comparable to that prepared with tungften.

I do not doubt but that the oxide of this new metal may likewife be ufeful in the compofition of colours for dyeing, at leaft for the dyeing of filks, which are not intended to withftand

ftand alkaline proofs. The oxide of tin is ufeful in thefe proceffes, becaufe it is not eafily attacked by acids; but our oxide is abfolutely infoluble.

I fhall conclude by a reflection which may render thefe refearches more interefting. Wolfram, from which this oxide is obtained, is found in the territory of the (French) Republic. Several mines are already known, and the French chemifts have been for fome years paft employed in fimplifying the proceffes by which it is feparated from foreign fubftances*.

We may therefore hope that the properties I have here defcribed will fupply artifts with additional means of giving durability to the productions of genius, and may open a new branch of national induftry.

IV.

Abftract of a Memoir on Camphor and the Camphoric Acid, read to the Firft Clafs of the National Inftitute of France. By BOUILLON LA GRANGE.

[Concluded from Page 101, Vol. II.]

Habitudes of Nitric Acid with Camphor.

THE nitric acid has likewife a different action upon camphor from that which we have already mentioned.

The Camphoric Acid.

Kofegarten has informed us, that by diftilling nitric acid eight times in fucceffion from camphor, an acid is obtained which differs in its properties from the oxalic acid.

As thefe refults have not been confirmed by experiment, and it has not been demonftrated that this acid is peculiar in its nature and its affinity, I have repeated the experiments of Kofegarten, of which we poffefs only a fimple notice in a letter tranflated from Crell's Journal, and printed in the 27th volume of the Journal de Phyfique, page 298. The method of preparing this acid is as follows:

Firft Procefs for preparing the Camphoric Acid.

Take four ounces, or 122,284 grammes of camphor, which introduce into a glafs retort, and pour one pound, or 489,136 grammes of nitric acid, at 36 degrees, or fpecific gravity 1,33, and adapt a receiver well luted. Place the retort on a fand bath, and apply a gradual heat. Much nitrous and carbonic acid gas are difengaged; part of the camphor rifes, while another part feizes the oxigen of the nitric acid. When the vapours ceafe to rife, unlute the veffels; return the fublimed camphor into the retort, pour thereon another pound of the acid, and diftill a fecond time. This operation muft be repeated until the camphor is totally acidified. Four pounds and fourteen ounces of this nitric acid are fufficient to acidify four ounces of camphor.

When all the camphor is acidified it cryftallizes in the remaining fluid. The whole muft then be poured on a filter after previous decantation of the acid, and diftilled water

* Journal des Mines, No. XIX. G. Our Cornifh mines alfo afford the ores of tungften. N.

muft

muſt be thrown on the cryſtals in order to clear them of the portion of nitric acid which may adhere to their ſurface. The moſt certain indication that the camphor is acidified, conſiſts in its cryſtallization by the cooling of the fluid which remains in the retort.

Purification of the Camphoric Acid.

This acid is purified by ſolution in hot diſtilled water, filtration and evaporation of nearly half the fluid, or till the period at which a ſlight pellicle is formed. The cryſtals of camphoric acid ſeparate by cooling.

A Second Proceſs.

Another method of procuring the camphoric acid conſiſts in the uſe of nitric acid at 50 degrees, or ſpecific gravity 1,532. This proceſs is much ſpeedier than the former, but its inconveniences are ſuch as do not entitle it to be preferred to the other. In fact, the camphor is more ſpeedily attacked by the concentrated acid; but inſtead of ſubliming, it paſſes over into the receiver, and a portion is likewiſe carried off in the gas which eſcapes. Theſe circumſtances occaſion a real loſs in the product, as may be ſeen by the following tables of the reſults:

By the firſt method.	grammes.
Nitric acid, at 36 degrees, 4 lb. 14 oz. or - -	2114,538
Camphor, 4 oz. or - - - - -	122,284
Acid obtained, 14 gros, or - - - -	53,498
By the ſecond method.	
Nitric acid, at 50 degrees, 2 lb. 12 oz. 2 gros, or - -	1352,168
Camphor, 4 oz. or - - - - -	122,284
Acid obtained, 9 gros, or - - - -	34,392

SECT. VII.

Camphor and Oxygenous Gas.

BEING deſirous of knowing how camphor would be affected with oxygenous gas, I made the following experiment:

Oxygen gas was obtained from the ſuperoxygenated muriate of pot-aſh. After an inverted glaſs veſſel was thus filled, it was transferred to the trough containing mercury, and a ſmall portion of water was paſſed to the ſurface of the metallic fluid.

On the other hand, a ſmall piece of camphor with a particle of phoſphorus was placed in a ſmall cupel. A tube was then bended in ſuch a manner and applied, that one of its extremities was placed beneath the glaſs jar, and the other in a pneumato-chemical trough beneath a jar filled with water.

In this diſpoſition of the apparatus the phoſphorus was ſet on fire by means of a red-hot iron. The inflammation was communicated to the camphor. Much caloric was diſengaged with a very brilliant flame; the inner ſurface of the veſſel became covered with a black matter, which was detached by degrees, and floated upon the water over the mercury. A gas was collected at the ſame time, which exhibited all the characters of carbonic acid gas.

This experiment therefore confirms the reſult in Section II. for it cannot be doubted but that the matter in the retort was a true carbon.

The

The water which had been placed under the jar became very odorant, and its fmell was abfolutely the fame as that of the oil obtained by the proceffes already defcribed. It was acid, and reddened the tincture of turnfol. Lime water afcertained the prefence of carbonic acid in this water ; but upon adding an excefs of the carbonic acid the precipitate was not taken up ; which fact led to a fufpicion that another acid was prefent, and in fact a calcareous camphorate was obtained.

I obferved that it was neceffary to add a fmall portion of phofphorus to the camphor, which otherwife would not have taken fire. For an ignited body only diffipates it in vapour, and it cannot be inflamed but by the contact of a body at a much more luminous or higher degree of heat.

SECT. VIII.

Characters of the Camphoric Acid.

THE camphoric acid has a flightly acid bitter tafte, and reddens the tincture of turnfol. It is cryftallizable, and a mafs of its cryftals refembles the muriate of ammoniac. By expofure to the air it effloresces. Cold water diffolves it with difficulty. One ounce of water at the temperature of 10 or 12 degrees of Reaumur, takes up no more than fix grains; whereas at the boiling heat it diffolves 48 grains. When this acid is placed on ignited coals it emits a denfe aromatic fume, and is entirely diffipated. By a gentler heat it melts and is fublimed.

If the camphoric acid be put into a porcelain tube (heated), and oxygen gas be paffed through, the acid does not undergo any change, but is fublimed.

By mere diftillation our acid firft flows and then fublimes; by which procefs its properties are in fome refpect changed. It no longer reddens the tincture of turnfol, but acquires a brifk aromatic fmell ; its tafte becomes lefs penetrating, and it is no longer foluble either in water or the fulphuric and muriatic acids. Heated nitric acid turns it yellow and diffolves it. Alcohol likewife diffolves it; and if this folution be left in contact with the air of the atmofphere it cryftallizes.

Camphoric acid does not produce any change in fulphur ; alcohol and the mineral acids totally diffolve it ; and fo likewife do the volatile and the fat oils. It forms combinations with earths, alkalis, and metallic fubftances. We fhall give a more particular account of thefe faline combinations in another Memoir.

Among metallic folutions, it decompofes only the fulphate and the muriate of iron.

It produces no change in the folution of indigo by fulphuric acid, nor in the tincture of nutgalls. It has no action on lime-water.

Conclufion.—From all thefe facts it follows, that camphor is a volatile oil rendered concrete by carbon ; that thefe two diftinct products may be obtained by the medium of alumine; and laftly, that by treating camphor with the nitric acid, another peculiar acid is obtained which differs from all the known vegetable acids,

1. By its cryftallization. 2. Its fparing folubility in cold water. 3. Its burning without leaving any refidue. 4. Its not precipitating lime-water. 5. Its producing no change in the fulphuric folution of indigo—a difference which remarkably diftinguifhes it from the fuberic acid which turns the fame folution green; and 6. By the formation of peculiar falts, every one of which exhibit a blue flame with the blow-pipe.

V. *An*

V.

An Account of several new Experiments on Heat, with occasional Remarks and Observations; and Conjectures respecting Chemical Affinity and Solution, and the Mechanical Principle of Animal Life. By BENJAMIN, *Count of* RUMFORD *.

AT the end of a French Translation of the first edition of Count Rumford's Seventh Essay, by Professor Pictet, that Translator added the following extract of one of the Count's private letters to him, dated June 9, 1797: "I should have been much surprised if my "Seventh Essay had not interested you; for in my life I never felt pleasure equal to that I "enjoyed in making the experiments of which I have given an account in that perform- "ance. You will perhaps be surprised when I tell you, that I have suppressed a whole chap- "ter of interesting speculation, merely with a view of leaving to others a tempting field of "curious investigation *untouched*, and to give more effect to my concluding reflection, "which I consider as being by far the most important of any I have ever published." As these assertions were not originally intended for the public eye, the worthy author found his situation altered with regard to the philosophical world by this communication; and with a very honourable degree of delicacy conceived that it became his duty to let the public know with precision how far he had carried his enquiries in the investigation of the subject of that Essay, instead of giving obscure hints of important facts kept in reserve, and apparently to be brought forward, when others might make discoveries of the same kind. The work before us contains the matter in question.

The experiments and observations contained in this Second Part are as follow:

CHAP. I. When a quantity of water was frozen in a glass jar by placing the vessel in a freezing mixture, it was always observed that, as the ice first began to be formed at the sides of the jar, and gradually increased in thickness, the portion of water in the axis of the jar which last retained its fluidity, being compressed by the expansion of the ice, was forced upwards towards the end of the process, and formed a pointed projection or nipple, which was sometimes above half an inch higher than the rest of the upper surface of the ice. This fact induced the Count to make experiments relative to the defect of conducting power in fluids downwards. For, if a fluid be poured upon this cake of ice so as to cover the whole mass, and a heated solid be then suspended at a small distance from the papillary protuberance, it is evident that this last, if melted, will derive its fluidity from the transition of heat, through the fluid of which the conducting power or its absence was meant to be ascertained.

In an experiment with fine olive oil, the cake of ice or congealed water was three inches thick, four inches and three quarters in diameter, and the pointed projection rose half an inch above the upper surface. The temperature of the apartment was 31 degrees of Fahrenheit, and the external part of the jar as high as the ice was surrounded with a mixture of pounded ice and water. In this situation of the apparatus, fine olive oil, previously cooled to 32 degrees, was poured into the jar till it stood at the height of three inches above the surface of the cake of ice.

* Abridged from the Second Part of his Seventh Experimental Essay.

A solid

A ſolid cylinder of wrought iron $1\frac{1}{4}$ inch in diameter, and 12 inches long, provided with a hollow cylindrical ſheath of thick paper, was heated to the temperature of 210 degrees in boiling water, and, being ſuddenly introduced into its ſheath, was ſuſpended from the ceiling of the room, and very gradually let down into the oil, until the middle of the flat ſurface of the hot iron, which was directly above the point of the conical projection of ice, was diſtant from it only 2-10ths of an inch. The end of the ſheath deſcended 1-10th of an inch lower than the end of the hot metallic cylinder. The ice was perfectly viſible through the oil in every ſtage of this experiment. How long the metallic cylinder was kept immerſed in the oil is not ſaid, but the time was no doubt ſufficient to ſatisfy the Count that heat is not propagated downwards through this fluid. The ice was not in the ſmalleſt degree diminiſhed, or otherwiſe affected, by the vicinity of the hot iron.

A ſimilar experiment was made with a freſh cake of ice in the ſame jar, but with ice-cold mercury covering the cake to the height of about an inch. The ſurface of the mercury in the jar was cleaned with blotting paper, after which the whole was ſuffered to remain quiet for about an hour, when the hot cylinder of iron was very carefully introduced, and ſuffered to remain ſeveral minutes at the diſtance of a quarter of an inch from the point of the conical projection of ice.

In this as well as in the other experiment, the cylindrical ſheath was made to project 1-10th of an inch below the baſe of the iron, for the purpoſe of diminiſhing the internal motions of the fluid.

In this experiment alſo the ice remained unchanged. In order to ſhew whether the ice were really in the ſtate of melting with the leaſt poſſible addition of heat, the Count touched it with his finger beneath the mercury; and he found that this operation could not be ſo ſpeedily performed, but that ſigns of water having been produced became apparent on the clean and bright ſurface of the mercury.

From the reſults of theſe experimental inveſtigations, it appears to our author that water, oil, and mercury, are perfect non-conductors of heat; and that when either of theſe ſubſtances takes the form of a fluid, all interchange and communication of heat among its particles, or from one of them to the other directly, become from that moment *abſolutely impoſſible* *. In the Philoſophical Tranſactions for 1792, the Count has ſhewn the extreme imperfection of the conducting power of air, and in his Sixth Eſſay he has ſhewn how much reaſon there is to conclude that the particles of ſteam and of flame are in the ſame predicament: From all which circumſtances he is diſpoſed to conclude, that it is common to all fluids, and even eſſential to fluidity, that they ſhall not immediately or directly conduct heat from particle to particle.

This important circumſtance was applied by our author, in the Philoſophical Tranſactions above referred to, to account for the warmth of natural and artificial clothing, and of ſnow, as well as to explain various phenomena of winds. And in his Sixth Eſſay he availed himſelf of the non-conducting power of ſteam and of flame, to explain the effects of the blow-pipe, and inveſtigate the moſt advantageous forms for boilers. And, laſtly,

* On the abſolute impoſſibility of the tranſition of heat through fluids at reſt, ſee the note in our Journal, I. 291. It may however be obſerved, that moſt of the Count's concluſions will be practically true; that is to ſay, with regard to ſuch terms of time as his experiments juſtify. N.

in the third chapter of the prefent Effay, he has extended the application of his difcoveries to the means which appear to have been ufed by the Creator of the world, to render the temperatures of different climates more nearly equal by means of the falt water, which covers fo large a part of the furface of the globe. But the moft interefting application remains to be made of this doctrine to chemiftry, vegetation, and the animal economy; to the learned in which branches of fcience he begs leave moft earneftly to recommend them. From his meditations he thinks it not improbable that every change of ftate, in every kind of fubftance, may be owing to heat alone; that every concretion is a true congelation effected by cold, or the diminution of heat; that every change from a folid to a fluid form is a real fufion;—and that it may be found, that the apparent violence with which certain folids are attacked by their folvents, is not owing to any particular or elective attraction, but to the confiderable degree of heat or cold, and the great difference of fpecific gravity which enfues in the folvent, from this caufe as well as from the fubfequent change produced by combination.

If fluids be non-conductors of heat, it will neceffarily follow, that change of temperature will produce currents in every chemical folvent, in proportion to the change of fpecific gravity; and the rapidity of the procefs of folution will be proportioned to that of the currents, or to the change of temperature. And again, if the faturated folution be either heavier or lighter than the folvent itfelf, currents will be produced from this caufe alfo, which will tend to render the folution more or lefs rapid, according to the magnitude of this difference, and its direction with regard to the other difference caufed by the change of temperature. An inftance of thefe two caufes operating jointly is adduced in the folution of common falt in water. If this folid be fupported in a perforated veffel under water, but near its furface, the folution will be moft rapid: firft, becaufe the temperature is diminifhed, and confequently the water is condenfed in the procefs; and, fecondly, becaufe the folution of falt in water is itfelf more denfe than the water itfelf. On both accounts therefore the brine will rapidly defcend, and frefh portions of the folvent will continually be brought in contact with the falt.

On this occafion the author propofes a curious queftion: Whether, in a cafe where the expanfion by heat were equal to the condenfation by the procefs of chemical union, the effect of folution could take place?—He anfwers by obferving, that if chemical attraction, as has been generally fuppofed, really exifts and operates in the way of predilection beyond the point of actual contact, it is probable that the folution would take place.—But if this attraction be nothing more than has been juft pointed out, it would follow that though folution would not be abfolutely impoffible, yet it would be fo flow as hardly to be perceptible.

In the confideration of the folution of falt in water, the pofition of the folid near the furface of the fluid was ftated as a circumftance effential to its rapidity. The uniform diffufion of folids through the whole mafs of lighter fluids which diffolve them, has ufually been confidered by chemifts as a proof of attraction between the two bodies operating at a diftance from the place of contact. It feems to be a ftriking refult of the Count's reafoning, that if no other caufes fhould interfere but thofe which flow from the mutual combination of the two fubftances, the falt would not, at leaft in any moderate portion of time, be carried to the upper part of the fluid refting upon it. He made this the fubject of a particular experiment.

A cylin-

A cylindrical glafs jar $4\frac{1}{2}$ inches in diameter, and $7\frac{3}{4}$ inches high, was placed in the middle of another cylindrical glafs jar $7\frac{1}{2}$ inches in diameter, and eight inches high, which ftood in a fhallow earthen difh, nearly filled with pounded ice and water. This apparatus was placed in an uninhabited room, where the temperature was conftantly at about 36 degrees of Fahrenheit. In this fituation, a quantity of pure ice-cold water, flightly tinged with turnfol, was poured into the inner jar, to the height of more than two inches; and then by means of a glafs funnel, which ended in a long narrow tube, by introducing this tube into the frefh water, and refting it on the bottom of the jar, a quantity of the ftrongeft clear ice-cold brine of common falt, equal to that of the frefh water, was poured very flowly in. The difference of colour rendered the two fluids very diftinguifhable, and fhewed that they were not difpofed to mix together.

The fpace between the two jars was then filled with large fragments of ice and ice-cold water, becaufe pounded ice would have obftructed the view; and when this was done, the tinged water of the interior jar was carefully covered with ice-cold olive oil to the height of about an inch. This laft fluid ferved to prevent the water from being agitated by the air, or cooled by evaporation or the communication of heat.

The fluids remained in perfect tranquillity, without the fmalleft difpofition to mix together, during four days. At the end of that time the fmalleft jar was removed without agitation, and placed in the window of a room heated by a German ftove. In lefs than an hour it was perceived that the brine and the tinged frefh water began to mix, and at the end of 24 hours they were intimately mixed throughout.

The author leaves philofophers to deduce their own conclufions from this experiment; but in the mean time points out a refult, which is not only curious in itfelf but capable of affording important confequences. He thinks there are ftrong reafons to conclude, that were a lake but very deep, its waters near the furface would neceffarily be frefh, even though its bottom fhould be one folid mafs of rock-falt. He fuggefts the advantage which might accrue to an inland country where falt is fcarce, if on experiment it fhould be found that the water at the bottom of fome deep lakes is falt. And that the water at the bottom of all very deep lakes ought neceffarily to be falt, even in fituations where no mines of falt exift, appears to him probable, from the geological facts which indicate that moft of our continents have been covered by the waters of the ocean. If ever that event happened, he thinks it highly probable, that the falt water left at the bottoms of all deep lakes by the fea, on its retiring, muft be there now.

Chap. II. In the former part of this Effay, the author has availed himfelf of all the circumftances which accompany the cooling and congelation of water, to account for the ftriking effects which they produce in the economy of heat on the furface of the globe. As water contracts by cooling to the 41ft degree, and afterwards expands until it affumes the folid ftate; it fhould feem at firft confideration, that, when the heat is abftracted from the bottom of a veffel containing this fluid, the particles of water would rife and elude the refrigerating power until the whole mafs was cooled to that temperature, and afterwards continue to circulate in the fame manner as if the temperature were actually rifing, until at length the whole mafs having arrived at the temperature of 32°, it would become folid almoft inftantaneoufly. But thefe inferences fuppofe the particles to circulate with lefs impediment from inertia and refiftance than would be fufficient to detain them long enough at the lower

 furface

ſurface to effect their congelation. Whether this ſuppoſition may agree with the actual ſtate of things or not, might perhaps be aſcertained by reaſoning from former facts; but in all ſuch queſtions where the deciſion can be immediately and ſimply had from experiment, it is certainly beſt to apply to that ſource of information. Count Rumford made the trial, by pouring mercury into a glaſs tumbler to the depth of one inch, and upon this about the ſame bulk of water. Both fluids were at the temperature of 60 degrees. The tumbler was then placed in a freezing mixture of ſnow and common ſalt, which reached no higher than the upper ſurface of the mercury. The ice was formed at the bottom in contact with the mercury, not only under theſe circumſtances, but alſo when the experiment was varied by previouſly cooling the mercury to about ten degrees, and then gently pouring boiling hot water on its ſurface. This laſt fluid was inſtantly frozen, and gradually formed a thick cake of ice, covering the mercury, though almoſt the whole of the maſs of the unfrozen water which reſted on this ice remained nearly boiling hot.

Among the inferences deduced from this experiment, Count Rumford makes one, to account for the formation of ice at the bottom of rivers, which he thinks can only take place in ſuch ſtreams as do not conſtantly fill their bed, but occaſionally overflow portions of ground cooled by the atmoſphere below the freezing point*. Another important inference from the ſame facts is, that it is impoſſible any fluid ſhould be of the ſame temperature while expoſed to light, though its maſs be ever ſo ſmall, and that at the difference of heat muſt occaſion perpetual motions among its parts. This conſequence is very fully explained in detail by our author, who conſiders fluidity as the life of inanimate bodies, and congelation as the ſleep of death; and is thence diſpoſed to reject altogether the attribution of attractive powers or exertions of any kind to dead motionleſs matter. He extends his meditations to the vital principle in living animals, and demands whether their life alſo do not depend on the internal motions in their fluids occaſioned by an unequal diſtribution of heat? and whether ſtimulation be not in all caſes the mere mechanical effect of the communication of heat?—The ancient hypotheſis, that the life of an animal reſides in its blood; the evident tendency of reſpiration, digeſtion, and inſenſible perſpiration to produce and perpetuate inequalities of tem-

* I am not acquainted with the peculiar circumſtances under which ground ice is formed; but it is certainly poſſible that ſuch an event ſhould happen in a ſtream which conſtantly fills its banks. Suppoſe a ſtream to flow with very little agitation in contact with an atmoſphere eight degrees or more beneath the freezing point. It is known that water whoſe parts are relatively at reſt, or nearly ſo, may be cooled about eight degrees below 32° without aſſuming the ſolid ſtate. The middle of our ſtream might therefore continue fluid when ſo cooled, and the congelation would only take place at the ſides, where the friction againſt the banks would cauſe the requiſite internal agitation. The middle cold ſtream, on account of its expanſion beneath 41 degrees, would occupy the ſurface, and conſequently could not rub againſt the bottom except in ſhallow places, or unleſs ſome means were to offer of ſinking it. Suppoſe one or more ſprings riſing from a great depth in the earth, and thence poſſeſſing the mean temperature of the climate, to flow into the principal ſtream. This maſs of warm water would occupy the ſuperior part of the ſtream, and cauſe the cold maſs to deſcend; and wherever this touched the bottom it would be agitated, and form a coat of ice. Theſe effects would be governed by the temperatures, the maſſes, and the local requiſites, for which upon the whole we are in want of obſervations. As to the aſſumed facts, they are undoubtedly poſſible. A remarkable inſtance of a warm ſtream in winter is ſeen in the ſmall river Wandle, which burſts out of the earth near Carſhalton in Surrey, and after flowing with conſiderable velocity over a line of ten miles, and giving activity to thirteen mills, falls into the river Thames at Wandſworth without having had time to acquire the freezing temperature, even in the ſevereſt weather. N.

perature;

perature; the effects of heat and cold introduced into the fyftem in liquid foods, with various other phyfiological events, are adduced by our author in fupport of the probability of that doctrine which he recommends to the attention of philofophers.

CHAP. III. In the third chapter, the Count proceeds to point out a variety of ftriking confequences and obfervations which flow from the imperfect conducting power of fluids. One of the moft immediate of thefe is the very great degree of heat which may exift at a fmall diftance from other bodies intenfely cold. Thus it is found that ice evaporates while expofed to an atmofphere in which the thermometer ftands far below 32 degrees; an event which he rationally accounts for, by fuppofing that fome of the particles of air which come into contact with the ice are fo hot as not only to melt thofe particles of ice which they happen to touch, but alfo to reduce part of the generated water to fteam before it has time to freeze again; or otherwife by fuppofing the fame effect to be produced by the intenfe heat generated from the abforption of light by fmall projecting points of the ice. He even thinks the metals would evaporate if they were bad conductors of heat, inftead of being very good conductors as in fact they are; and in proof of this he urges the fact, that mercury, which from its fluidity is what he would call a non-conductor, is known to evaporate by the mere heat of the atmofphere.

That the moft intenfe heat is often excited in the midft of maffes of cold liquids, is not to be doubted. The fun's rays generate heat of extreme intenfity; but when circumftances are not favourable to its accumulation it is foon difperfed, and leaves no traces of its exiftence which can be meafured either by inftruments or the organs of fenfe. The Count does not think it improper to infer, that the heat excited by a ray of light in an indefinitely fmall particle of folid and opaque matter floating in a mafs of cold water, may be equally intenfe with that which is generated in the focus of the moft powerful burning mirror or lens. Hence he accounts for various effects of the fun's light, which gradually produce changes of the fame nature as thofe which arife from very elevated temperatures. Thus wood is rendered brown or fuperficially charred, luna cornea is rendered black, or as may be fuppofed fuperficially reduced, metallic oxyds are deprived of oxygen, the green leaves of vegetables emit the fame fluid, &c.

Among other familiar inftances of intenfe heat, in circumftances where no vifible figns appear, the Count adduces that of the afcending current of air from a candle. Iron is fully red hot at the temperature of about 1000° of Fahrenheit's fcale; brafs melts at 3807°, copper at 4587°, filver at 4717°, and gold at 5237°; and it muft be obvious that this laft temperature obtains where gold undergoes fufion. But fine gold, filver, or copper wire flatted, fuch as is ufed to cover thread to make lace, melts inftantly on being held in the flame of a candle, or even if held for a few feconds over the flame at the diftance of an inch, where there is no appearance of fire or ignition. The air, or many of its particles, muft therefore be heated to this intenfity, though their number may be infufficient to caufe any very elevated degree of temperature in a large mafs of metal or a thermometer.

From this hypothefis of intenfe heat in the fmall parts of fluids, or bodies fufpended in them, it will follow that chemical folutions and precipitations are or may be effected by the fole agency of heat, and will not differ from fufion and congelation;—that the points of temperature at which bodies affume the folid, the fluid and the gafeous ftates will be of the utmoft confequence with regard to thefe events, and that perhaps there may be no other effential difference

difference between one body and another but what arises from this circumstance;—that these points being changeable by combination, give rise to a prodigious number of consequences different from those which obtain in the simpler elements. We must refer to the original Essay for the more ample elucidation of these points, where chemical philosophers will see the outline traced of an immense field of research, which, if it should not include the whole of the phenomena which have been designated under the name of elective attraction, will certainly comprehend a large mass of effects which never fail to present themselves in every natural change which comes under our notice.

CHAP. IV. The last and concluding chapter contains a variety of miscellaneous experiments. 1. If a thermometer with a long cylindrical bulb being at the temperature of the air in summer, or any temperature above the freezing point, be plunged to half the length of its bulb in ice and water, the mercury will fall in the tube only half as much as if the whole bulb had been immersed. This experiment succeeds equally well, when the superior half of the bulb is covered with a sheath lined with soft fur to prevent the communication of heat from the air during the experiment. It shews that the upper half of the mercury is not cooled, or, in other words, that heat does not pass downwards in fluids.—2. Ice-cold water standing on ice at the bottom of a thin tube of glass, may be boiled by holding the tube inclined over the flame of a candle applied near the upper end, and gradually removed lower down as the ebullition proceeds. In this way all the water in the tube may be brought into the most violent ebullition, to within a quarter of an inch of the ice, before this begins to be melted.—3. The radiant heat from a red hot iron bullet was not found to make its way downwards through liquid water, nor through melted tallow, nor melted wax. The experiment was tried on a thermometer about a quarter of an inch beneath the surface of each of these fluids. These facts are analogous to the observation of Scheele, who found that radiant heat does not pass through glass. When the red-hot ball was held over a cake of ice, the heat appeared to be transmitted by communication, though very slowly. The excavation in ice was deepest near the middle. The contrary was the case both in tallow and in wax. Count Rumford does not directly explain the cause of this remarkable difference. It is the only immediate fact I recollect having seen in his Essays, in proof of the assertion that water stands alone, with regard to the property of expanding in the lower degrees of refrigeration; and consequently of contracting as its temperature increases in that part of the scale. From this property it would follow, that a warm stream of water must descend immediately beneath the bullet, while an ascending current obtains near the circumference of the cavity. But if the expansion of tallow be uniform from the earliest point of fusion, the heated fluid will not descend, but will spread out sideways, and circulate in a contrary direction to the current in water; while the ascending stream of cold fluid will be near the centre, and will defend part of the tallow in that region from being fused.—4. Beautiful crystals of sea salt were formed in brine standing on mercury in an open vessel for half a year. The Count makes it a question, whether the mercury contributed in any manner to this effect? It seems probable that it did not, and that evaporation equally slow and undisturbed would have afforded the same result in other circumstances.—5. A similar remark may be made on his observation of olive oil rendered colourless by exposure to the air for six months upon brine, in a place where the sun's rays never entered. The same effect happens to olive oil which remains in unclosed vessels in work-shops.—6. Part of the last-mentioned

tioned colourlefs oil being imperfectly congealed, an attempt was made to caufe radiant heat to defcend through its mafs. It was expected that the tranfparent or melted portion of the oil would in this cafe have been hemifpherical, or at leaft of fome convex figure; but this did not happen, for the furface beneath the fluid or melted part continued flat. Whence the Count infers, that the fufion was effected merely by the heat abforbed or generated by the fides of the tumbler.

7. The concluding experiment of this Effay affords a ftriking refult of the circulation of fluids under different circumftances. When the inftrument defcribed in our Journal, I. 342, was placed by accident in the window of a room warmed by a German ftove, the difference of temperature, on the fide neareft the window from that oppofite the internal part of the room, was fuch as to keep up a conftant circulation of the fluid. This event induced the Count to fubftitute a box of plate glafs in the place of one of the panes of his window. This apparatus, which was 13 inches high, $10\frac{1}{2}$ inches wide, and one inch within from plate to plate, was half filled (and as I fuppofe afterwards completely filled) with the faline folution with pieces of amber floating in it, as defcribed at the place laft quoted. The Count expected to behold the currents as ufual in this new veffel; but to his great furprife they proved not vertical, but horizontal, exhibiting actual winds in oppofite directions, which fpringing up in the different regions of this artificial atmofphere, prevailed for a long time with the utmoft regularity, while the fmall particles of the amber collecting themfelves together formed clouds of the moft fantaftic forms, which being carried by the winds rendered the fcene perfectly fafcinating.

Several fubordinate circumftances gave thefe motions a more intimate refemblance to the atmofpherical phenomena. One remarkable appearance never failed to prefent itfelf regularly every day during the three weeks that the experiment was continued. The clouds, after having been driven about all day by the different currents, in the liquid (of which there were fometimes as many as fix or feven running in oppofite directions at the fame time), never failed to collect themfelves together in the evening into large maffes, fometimes forming only one, and fometimes two or three ftrata at different heights, where they remained to all appearance perfectly motionlefs during the night.

The experiment was put an end to by the accidental breaking of the veffel.

VI.

An Account of certain Motions which fmall lighted Wicks acquire when fwimming in a Bafon of Oil; together with Obfervations upon the Phenomena tending to explain the Principles upon which fuch Motions depend. By PATRICK WILSON, *F.R.S. Edin., and Profeffor of practical Aftronomy in the Univerfity of Glafgow* *.

DEAR SIR, *Glafgow College, April* 28, 1795.

I NOW fit down to give you fome account of the little hydroftatical lamp which I fo briefly mentioned to you in a former letter. As I am far from being fure whether what I

* In a Letter to Mr. John Playfair, F. R. S. Edin. &c. inferted in the Edinburgh Tranfactions, vol. iv.

have

have to offer upon this fubject may be entitled to the notice of the Edinburgh Royal Society, fo I will refer this point to your determination after you have had leifure to confider the contents.

The phenomena treated of in the fequel were quite new to me a few months ago, and, fo far as I know, have not hitherto been attended to or defcribed by any body elfe. What I have called the hydroftatical lamp, confifts of a fmall circular patch of common writing paper, about three-eighths of an inch in diameter, having about a quarter of an inch of foft cotton thread ftanding up through a puncture in the middle to ferve as a wick; and the phenomena in queftion are certain motions which fuch minikin lamps acquire when lighted and made to fwim in very pure fallad oil.

A fhallow glafs bafon, with fides rifing nearly perpendicular, or a common glafs falver, will conveniently contain the oil for thefe experiments. As foon as the lamp is lighted, it will immediately fail brifkly forward in fome direction till it meets the fide of the veffel, and afterwards will take a circular courfe, always bearing up to the fides, and fo will perform many revolutions.

Sometimes the circulation is from right to left, and fometimes in the contrary direction, according as that point of the paper bafe, which in the direct failing kept always foremoft, turns away from the fide of the glafs, a little to the right or to the left hand of that which comes to be the point of contact. This turning away of what may be called the leading point of the bafe is diftinctly obfervable by a partial rotation of the lamp round the wick as an axis, as foon as it arrives at the fide of the veffel. Sometimes, though rarely, the leading point itfelf attaches itfelf to the fide, and forms the vinculum, in confequence of the well-known corpufcular attraction between the elevation of oil around the bafe and that belonging to the fides of the glafs; and when the vinculum fo correfponds to the leading point, the lamp will be found to ftand ftill without any tendency to circulate.

When the little wick has any fenfible eccentricity upon the circular paper bafe, the lamp will fail fo as to make that part of the bafe which lies neareft to the wick the ftern; and if the bafe of the lamp be clipped to an oval form, and the wick placed in the longer axis excentrical, that end of the bafe neareft the wick will alfo keep hindermoft, when the lamp fails acrofs the falver. In the fame manner, if there be an equilateral triangle, having its wick in the perpendicular which bifects any of the fides, either the vertex or fide will become the ftern, and keep hindermoft, according as the wick is placed neareft the one or the other. Lamps fo constructed are found alfo to circulate upon their arrival at the fide of the veffel, when the leading point turns away from the glafs, as it commonly happens.

Whatever be the caufe of the failing of the lamp directly forward, the perpetual circulation after it arrives at the fide feems to proceed from the force which formerly impelled it ftill acting in the fame manner, but in a direction inclined to that of the corpufcular attraction which forms the vinculum; and it is evident that this inclination will be greater or lefs according as the leading point is more or lefs averted from the glafs. When it fo happens that the leading point and vinculum coincide, it fhould feem that both forces juft now mentioned muft urge the lamp in a direction perpendicular to the fide of the glafs; in which cafe it muft ftand ftill, agreeable to obfervation.

The next thing which I had occafion to take notice of when the lamp failed in a direct courfe, was a feemingly very active repulfion between its ftern and the oil at the furface contiguous

tiguous to it. This became manifeſt when very fine charcoal duſt was lightly ſcattered around the lamp. As it then proceeded in its courſe, it marked out a ſpreading or diverging *wake* behind it entirely clear of all duſt, in conſequence of the particles being chaſed backwards and laterally with a motion much more than merely relative.

Deſirous of learning how this diſpoſition of the duſt would take place when the lamp was ſtationary, I conſtructed one of a fine wafer, and with an excentric wick conſiſting of a ſoft cotton thread doubled; and to prevent the wafer or baſe from catching fire I coated its upper ſurface with gold leaf. When this was made to reſt immoveably upon the oil, the duſt retired in all directions ſo as to leave the ſpace adjacent to the wafer quite free from every particle. But here it was obſervable, that this diſperſion of the duſt, by the ſeeming repulſion of the baſe of the lamp, was much more rapid at that ſide which lay neareſt to the wick than at any other part, and leaſt of all ſenſible at the ſide diametrically oppoſite.

The circumſtances laſt mentioned ſeem ſufficiently to account both for the progreſſive motion of the lamp, and for the general law of this motion formerly deſcribed. For, regarding this diſperſion of the duſt as yet only in a general way, and as the effect of ſome repulſion between the baſe and the oil contiguous to it, the facts above mentioned plainly indicate, that in all caſes this repulſion is ſtrongeſt at that part of the baſe neareſt the wick or flame; and as action and reaction are equal and contrary, the lamp muſt therefore be impelled, in the direction of a line drawn through the wick, towards that part of the baſe moſt remote from it, and where the reaction is the leaſt.

But in order to obtain a ſtill more competent knowledge of the phyſical cauſe of theſe motions, it ſeemed now neceſſary to enquire more particularly into this apparent repulſion, between the baſe of the lamp and the ſurrounding oil, as indicated by the diſperſion of the duſt in the manner above deſcribed: and here the following conſiderations preſented themſelves:

The oil in the baſon, when of an uniform temperature, has all its parts in a ſtate of equilibrium and of reſt. When the lamp is lighted, it is evident we have a very active cauſe introduced tending to deſtroy that equilibrium. This cauſe is the flame which broods over a ſmall portion of the oil, and is ſeparated from it only by the intervention of a piece of paper or a wafer. The oil in ſuch circumſtances, in conſequence of being violently heated, muſt ſuddenly increaſe in volume, and muſt now, on account of the decreaſe of its ſpecific gravity, be preſſed upwards by a force ſufficient to raiſe part of it above the general level. But this heated portion of oil, in its endeavour to riſe up, will meet with a reſiſtance equal to the weight of the incumbent lamp, which will determine it, in ſeeking a vent, to ſlide out from under the baſe in a thin ſuperficial ſtream; and it ſeems to follow with equal certainty, that this conſtant ſtream will flow moſt rapidly and moſt copiouſly towards that ſide of the baſe of the lamp where the reſiſtance is leaſt, or where it has the ſhorteſt way to preſs forward; that is, from under the wick or flame to the edge of the baſe which is the neareſt, according to what we have ſeen to be agreeable to the phenomena. But, from the laws of motion, it is certain that the re-action of this ſtream of rarefied oil, thus iſſuing moſt rapidly and moſt copiouſly from a particular ſide of the baſe, muſt impel the lamp in the contrary direction, and make it ſail in the manner we have ſeen. It may further be remarked, that the heated oil ſo retreating from the flame, and endeavouring to riſe ſomewhat above the general level, in conſequence of its diminiſhed ſpecific gravity may more or leſs lift up that ſide of of the base nearest the wick, and aid the re-action of the recoiling stream, by making the lamp sail in the opposite direction, as it were down hill.

That the rarefied oil under the base has really a constant tendency to rise above the general level, seems undeniable from the following facts; namely, that after any of the lamps has burned a little while, and has got its base soaked with the oil, as soon as the flame is blown out the lamp sinks to the bottom; and even a lamp with its base made of a thin lamina of talc sails very well till the flame is extinguished, and then it immediately sinks.

Agreeable to the explanation which has now been attempted, I found that when a topical heat was applied to the surface of the oil, by bringing the point of a poker dully red hot nearly into contact, there was soon produced a superficial stream or efflux from the iron in all directions, which cleared the face of the oil from the charcoal dust in a wider and a wider circle, till at last the whole particles were crowded together at the confines of the bason.

When the oil in this experiment was shallow, having gold leaf beat into very minute parts mixed with it, an opposite stream was observed below setting in towards the poker in all directions, and then rising upwards. But this general tendency of all the parts of the fluid, of moving in quest of an equilibrium, is illustrated in a very entertaining manner as follows: Into a tea cup or punch glass nearly filled with pure water, pour a dessert spoonful of very clear sallad oil with minute particles of gold leaf in it. If the water be cold, the oil when poured on at the centre, leisurely and continuedly, will rest upon the surface in the form of a lens, and remain insulated and equidistant from the sides of the vessel. A little lamp, when put upon this lens of oil and lighted, will sail and circulate as longer ones do in a bason. If it be now made to stand still, it is very amusing to observe the minute particles of the gold perpetually thrown out briskly at the stern in the superficial current, whilst the particles in the fund of the lens creep in all directions towards the lamp, and at last rise up under the base towards the flame, as the great centre of attraction, till they are caught by the retreating superficial stream, in which they rapidly trend off to some distance, when again they sink to renew the circulation.

When a patch of paper, or a wafer, or such light body, swims upon the oil in the bason, the point of a hot iron held near to it makes it flit its place, and move away by a seeming repulsion; but in reality by the heat generating a superficial stream flowing from the iron in all directions.

Again, if upon oil of turpentine, æther, alcohol, or any of the inflammable fluids possessing much tenuity, you throw a wafer much heated, it will immediately glide away and continue in motion till it cools; when the stream which issued from some part of it most copiously ceases. Double rum, melted tallow, bees-wax, and rosin, also afford the same continued efflux at the surface upon a topical application of heat, and the same phenomena as the oil does when little lamps are made to swim in them. It is somewhat remarkable, however, that though the inflammable fluids all agree in this, yet the topical application of heat at the surface of water does not produce similar effects.

For, if the point of a poker nearly red hot be held very close to the surface of water in a bason, the particles of the charcoal dust do not at all glide away, as they do in the case of oil, but seem to acquire only a slow irregular circular motion, which in time spreads wider,

whilst

whilst the floating motes, or particles of dust, keep nearly their relative places; and the same thing happens, though the point of the iron touches the water so as to make it simmer.

I do not well know how to account for this, unless it may be a consequence of the known much less expansibility of water by heat, compared to that of the inflammable fluids, and which may be so inconsiderable as not to destroy the equilibrium, so far as to produce an efflux from the lighter and expanded fluid immediately under the heated body. Possibly too the parts of the water, as soon as heated, may transmit the surplus temperature to the contiguous colder water, much more rapidly than the inflammable fluids do in like circumstances, and thereby resist the high temperature necessary to that degree of expansion which would disturb the equilibrium and produce an efflux; not to mention that the maximum of this temperature can never, at any rate, exceed 212 degrees, the boiling point of water.

That the equilibrium, however, amongst the parts of water is disturbed by the local application of heat, though in a much smaller degree than what obtains among the inflammable fluids, appears from an experiment I was led to make with a small thin cup swimming on water, and so contrived as to carry and feed with oil a wick placed a little way down from the lip in the inside, so as to be on a level with the water. The consequence of this construction was, that the cup moved upon the water very slowly, but always with the flame evidently sternmost. The same cup, when taken from the water and put into a bason of strong rum, sailed a great deal faster, and according to the same usual law.

I am much afraid that, by this time, I have wearied you by such a detail of minute facts and circumstances, and by those frequent repetitions which every new subject more or less requires.—And I ever remain, dear Sir, your most obedient faithful servant,

PAT. WILSON.

P. S. Should you be inclined to repeat any of the experiments, the following directions and miscellaneous observations may be attended to: The thread I made use of for the wicks was of that soft kind commonly employed in the flowering of muslin. After making the puncture in the base, you put through a bit of the thread, which clip short off below, and with a pin force in the burr gently round the thread, to give the base a proper hold of it. Then clip away the superfluous thread above, leaving the wick about a quarter of an inch long; and so the lamp is completed. Set it then upon the oil by taking hold of the wick, that the paper base may not be bent or distorted by handling it; and, after the wick is touched with a drop of oil, it is ready for being lighted. For this purpose a bit of packthread, which has been steeped in oil, is a cleanly and convenient match, and sheds no impurities on the oil, as a candle or wax taper would do.

When you want the lamp to circulate, the oil must be very pure, and brought into full contact with the sides of the glass. The oil and the bason, or salver, should all be allowed to come to the same temperature, between 55 and 60 degrees of Fahrenheit. For, if any part of the brim be much hotter than the rest, the lamp, on arriving there, will leave the side, by the current issuing from the heated part forcing it away.

Sometimes the lamp, when sailing, veers a little into a different direction, by the base altering or warping by the scorching heat of the flame, which determines the stream to flow out most copiously at a different part of the base.

 In

In the melted greafe which lies round the wick of a common candle, when lighted, there are fometimes obferved atoms, which have been left by the fnuffers, moving to and from the flame continually. Thefe motions have been conceived by fome as occafioned by attractions and repulfions, in confequence of an electrical quality imputed to the flame. It fhould feem, however, that they depend merely upon oppofite currents, at the furface and immediately below the furface of the melted greafe, according to the principle above explained.

VII.

*Experimental Refearches concerning the Principle of the lateral Communication of Motion in Fluids, applied to the Explanation of various Hydraulic Phenomena. By Citizen J. B. VENTURI, Profeffor of Experimental Philofophy at Modena, Member of the Italian Society of the Inftitute of Bologna, the Agrarian Society of Turin, &c.**

THE apparatus made ufe of in moft of the following experiments is the fame as that of Poleni †. It is reprefented at Fig. 1. Pl. VIII. The refervoir X, of a conical form, has forty inches diameter at C E, and 30 at O P. F P is a broad plate of copper, the plane of which is perpendicular to the horizon; it is applied to the infide of the refervoir. The valve or flap F S, moveable by the handle K, is drawn up againft the fide of the veffel above F, in order that it may not impede the courfe of the particles of the fluid contained in the refervoir to the aperture P. I have applied different ajutages to this aperture, according to the exigence of the cafe. The tubes which I applied were made of tinned iron of the beft quality; the longitudinal junction of the edges was made by immediate contact, and not by overlapping, and the whole of the workmanfhip was executed with great care. When the aperture was fimply a hole through a thin plate, the thicknefs of its edge did not exceed one fourth of a line.

The upper veffel Z ferves to maintain the water of the refervoir X at the conftant height of the line C E, while it flows out through P. The plug A B is drawn more or lefs back, in order to regulate the introduction of the fupply. The box or fhelf D L prevents this water from exciting by its fall any agitation which might influence the emiffion at P. The opening at Q difcharges the fuperfluous water which might rife above the line C E. The height of the furface C E above the centre of the orifice at P was 32,5 inches, in all cafes where it is not otherwife expreffed.

Moft of the experiments here defcribed were made in public at the Philofophical Theatre of Modena; various men of fcience were prefent at the reft; the different departments of experiment were performed by feveral perfons at the fame time. One of thefe operators repeated the feconds audibly from the clock; another drew back the valve S F; a third regulated by the means of the plug B the introduction of the fupply of water, fo that a very thin

* Since the receipt of this memoir (which is equally valuable as the fource of data for fcientific proceffes, and of ufeful practical refults, either unknown or difregarded in hydraulic works) it has remained in my hands with the intention of prefenting my readers with an abftract of its contents. But, upon ftricter confideration, I have thought it beft to give it entire from the French original. N.

† De Caftellis. This treatife is reprinted in the third volume of Hydraulic Treatifes, publifhed at Parma. V.

ſheet of water conſtantly flowed at Q. At the inſtant agreed upon, the paſſages of the water were again cloſed, Every experiment was repeated ſucceſſively for a number of times, until the agreement of the reſults had removed every ſuſpicion of error. I am aſſured that even in the moſt complicated caſes, the quantity of error could not exceed one fortieth part.

The meaſures indicated in the courſe of theſe experiments were taken from a toiſe adjuſted by that of the Academy, which Citizen Lalande ſent me in 1783. Theſe meaſures, as well as all the others of the 18th century, will undergo the fate which is prepared for them by the eſtabliſhment of the new metre. They may be reduced to this new ſtandard, by obſerving that the foot is to the metre as 100 to 308.

The wiſeſt philoſophers have their doubts with regard to every abſtract theory concerning the motion of fluids; and even the greateſt geometers avow, that theſe methods, which have afforded them ſuch ſurpriſing advances in the mechanics of ſolid bodies, do not afford any concluſions with regard to hydraulics but ſuch as are too general and uncertain for the greater number of particular caſes. Impreſſed with a conviction of this truth, I have attended to theory only when it combined with the facts, and was neceſſary to unite them under a ſingle point of view. Even this ſmall portion of theory may, if the reader pleaſes, be rejected; and he may conſider the following propoſitions ſimply as the reſults of experiment.

When I quote the eſtimable work of Citizen Boſſut, on hydrodynamics, I refer to the edition of 1786*.

Propoſition 1. The motion of a fluid is communicated to the lateral parts which are at reſt.

Newton has affirmed, that when motion is propagated in a fluid, and has arrived beyond the aperture B C, fig. 2, the motion diverges from that opening, as from a centre, and is propagated in right lines towards the lateral parts N K, as well as towards S. The ſimple and immediate application of this theorem cannot be applied to a jet which iſſues from the aperture B C at the ſurface of ſtill water. Circumſtances enter into this caſe, which tranſform the reſult of the principle into particular motions. It is nevertheleſs true, that the jet B C communicates its motion to the lateral parts N K; but it does not repel them towards P and Q, but on the contrary tranſports them along with its own ſteam towards S.

Experiment 1. The horizontal cylindric pipe A C, fig. 3, is introduced into the veſſel D E F B, which is filled with water as high as D B. Oppoſite and at a ſmall interval from the aperture C commences a ſmall rectangular channel of tinned iron, S M B R, which is open at top S R; the inclined bottom M B reſts on the edge of the veſſel B. It is 24 lines broad; the diameter of the tube A C is 14,5 lines; the extremity A is applied to the aperture P of fig. 1. The water of the reſervoir being ſuffered to flow through the tube A C, the jet riſes along the ſmall channel M B, and flies out of the veſſel in the ſtream B V. By this means a current is produced in the fluid of the veſſel D E F B; this fluid enters into the channel S R, and iſſues by M B V along with the jet A C, ſo that in a few ſeconds the water D B falls to M H.

* I conſider this treatiſe as ſuperior to all which before were extant. It is founded on a combination of the principles of experiment and of theory. I have profited by theſe principles, and ſeveral particular remarks which the ſame Citizen Boſſut and Citizen Prony have been ſo good as to communicate after peruſal of my memoir. V.

Experiment

Experiment 2. Bring fome very light or moveable bodies near the jet of water P Y, fig. 1, which iffues from the aperture P, and falls from a certain height E into the inferior veffel R T. It is feen that thefe bodies are carried along by the air which defcends with the jet P Y. Part of this air is carried along and plunged into the water of the inferior veffel.

Thefe experiments clearly prove, that the fluid which iffues by B C, fig. 2, impreffes its motion on the lateral parts N K; not by impelling them towards P Q, but by carrying them along with itfelf towards S. I call this the lateral communication of motion in fluids. Newton was acquainted with this communication, and has deduced from it the propagation of rotatory motion from the interior to the exterior ftrata of a whirlpool. Is this lateral communication of motion occafioned by the vifcidity or mutual adhefion of the parts of the fluid, or their mutual engagement or intermixture, or the divergency of thofe parts which are in motion? We may perhaps be able to give fome account of this when we fhall have feen the effects; but in the mean time, whatever may be the caufe, let us take the effect as experience points it out; let us confider it as a principle, and endeavour to apply it to fome particular cafes in order to afcertain the refult.

The firft circumftance to which I propofe to apply this principle is the increafe of expenditure of fluid iffuing out of an orifice fitted with additional tubes.

Propofition 2. If that part of an additional cylindric tube which is neareft the fide of the refervoir be contracted according to the form of the contracted vein of fluid which iffues through a hole of the fame diameter in a thin plate, the expenditure will be the fame as if the tube were not contracted at all. It is well known, that when the water of a refervoir is fuffered to flow through a circular orifice in a thin plate, the fluid vein which forms the jet becomes contracted at a fhort diftance from the orifice; and the diameter of the contracted vein is nearly 0,8 of the diameter of the orifice. Poleni firft obferved, that by applying an additional cylindric pipe to the orifice, of the fame diameter as the orifice itfelf, and from two to four times that length, the expenditure is increafed from 100 to 133. To account for this augmentation, he fuppofes that the fluid vein is lefs contracted in pipes than after paffing through the thin plate. The fuppofition was not unreafonable; but it could not apply to the cafe announced in this propofition. I fhall proceed to give the particulars in the following experiment.

Experiment 3. To the aperture P, of fig. 1, I applied a circular orifice 18 lines in diameter, pierced through a thin plate. Four cubical feet of water flowed into the veffel Y in 41 feconds.

I then applied to the orifice a cylindric tube of the fame diameter, and fifty-four lines long. The four cubic feet flowed out in 31 feconds.

Inftead of this fimple cylindric tube, I applied the compound tube of fig. 5; the parts of which have the following dimenfions in lines: $AC = GI = MN = 18$; $DF = 14{,}5$; $AB = 11$; $BG = 10$; $GM = 37$; $AM = 58$. With this compound tube the expenditure of four cubic feet of water was made in 31 feconds, as with the fimple cylindric tube.

The form of the conical portion A C D F was nearly the fame as that of the contraction of the vein which iffues through a thin plate. The vein muft therefore have paffed through a contraction nearly equal to that of the contracted vein from a thin plate; the expenditure never-

neverthelefs was more abundant, in the fame proportion as through the fimple cylindric tube. It follows, therefore, that the velocity of the fection D F, and of the whole conoid A C D F, muft have been greater than that of the contracted vein from a thin plate; and it remains to be fhewn what was the caufe of this augmentation of velocity which takes place within the tube, and does not manifeft itfelf externally.

That the conical tube A C D F does not itfelf caufe any augmentation of expenditure, is evinced by the following:

Experiment 4. The conical tube A C D F, from which the remaining part D G M N I F was feparated, was applied to the orifice P. The four cubic feet were emitted in 42 feconds, which is the time of the expence through the orifice itfelf A C in the thin plate, with the difference of one fecond only. This flight variation arifes from its being almoft impoffible to make the tube A D C F perfectly of the form of the natural contracted vein.

Propofition 3. The preffure of the atmofphere increafes the expence of water through a fimple cylindric tube, when compared with that which iffues through a hole in a thin plate, whatever may be the direction of the tube.

It has long been known, that a heavy fluid which moves in a defcending cylindric pipe tends to accelerate its motion. The inferior parts tend to feparate themfelves from the fuperior, and by that means caufe the preffure of the atmofphere to increafe the velocity of the fuperior parts. This fucceffive acceleration of gravity cannot take place in an horizontal or afcending pipe. We fhall neverthelefs find that the preffure of the atmofphere acts even in thefe laft fituations to increafe the velocity of fluid within the pipe. Certain queftions of legal right, which arofe in my country, refpecting the quantity of water fupplied by a pipe for watering lands (canal d'arrofement) directed my attention to this object.

In the year 1791 I made the following experiments publicly in the Theatre of Natural Philofophy at Modena:

Experiment 5. To the aperture P, fig. 1, I applied a cylindrical pipe 54 lines in length and 18 in diameter. At the diftance of nine lines from the interior orifice P, twelve fmall holes were made in its circumference. When thefe fmall holes were open, the four cubic feet iffued out in 41 feconds, in the fame manner as through a thin plate. Not a fingle drop paffed through any of the holes, and the ftream did not fill the tube. The holes were then clofed one after the other with wet fkin. As long as there was one hole open the expence continued the fame; but when at laft all the twelve holes were well clofed, the fluid ftream iffued out in a body which filled the pipe, and the four cubic feet were emitted in 31 feconds.

Experiment 6. To the cylindric tube K L B, fig. 6, 18 lines in diameter and 57 lines long, was joined the glafs tube Q R S T, at the diftance of eight lines from the interior orifice K. The glafs tube was plunged in coloured water contained in the veffel T. When this apparatus was applied to the aperture P, fig. 1, the four cubic feet of water flowed out in 31 feconds. The coloured liquid T rofe in the tube T R as high as S, at the height of 24 inches above the furface T.

The branch R T of the glafs tube was fhortened fo that R T was only fix inches longer than R Q. The efflux being then permitted to take place, the coloured liquor of the veffel T rofe through the tube R T, and mixed with the water which flowed from the refervoir through K V, both of which flowed out at V, and in a fhort time the veffel T was emptied.

I repeated

I repeated this experiment with the compound tube fig. 5, and the refults were the fame.

Experiment 7. The cylindrical pipe K L V, fig. 6, was applied in an afcending and nearly vertical fituation to the orifice R, fig. 8, of the veffel H I, of which the end H communicated by an opening of confiderable extent with the water of the refervoir X, fig. 1. The charge on the upper extremity V of the tube was 27,5 inches. I inclined the tube a little from the vertical direction, in order that the jet might not fall back upon itfelf. The glafs tube Q R T, fig. 6, in this new fituation was fo difpofed that its lower extremity was immerfed as before in the coloured liquid of the veffel T. When the efflux was permitted, the expenditure of four cubic feet was made in 34 feconds; and the coloured liquid rofe in the tube R T to the height of near 20 inches. With the fame charge of 27,5 inches the orifice of 18 lines in a thin plate would have afforded the four cubic feet in 45 feconds.

Experiment 8. A cylindrical veffel of 4,5 inches diameter had in its vertical fides near the bafe a circular opening of 4,5 lines in diameter, opened in a thin plate of tinned iron. The furface of the water contained in this veffel was 8,3 inches above the centre of the aperture. The water was then fuffered to flow out of this aperture in the thin plate, and its furface was depreffed feven inches in the veffel in 27,5 feconds of time.

To the fame aperture was applied a cylindric tube of the fame diameter, and in length 11 lines. The veffel was filled to the fame height as before, and, the water being fuffered to flow out, its furface was depreffed feven inches in 21 feconds.

The fame experiment was afterwards repeated in the receiver of the air-pump, under which the mercurial gauge ftood at no more than 10 lines in height. The furface of the water in the veffel was depreffed feven inches in 27,5 feconds, whether the aperture was made in a thin plate, or whether it was provided with an additional cylindric tube.

The height of the coloured water in the tube of glafs meafures the active quantity of the preffure of the atmofphere, which is exerted on the furface of the water to increafe the expenditure. For example, in the fixth experiment 32,5 + 24 inches charge on the orifice P, and we have nearly $\sqrt{32,5} : \sqrt{56,5} :: 31'' : 41''$, as is required by the common theory of the motion of fluids which iffue out of veffels by a fmall aperture. The fame obtains in Experiment 7.

Daniel Bernoulli made the 7th experiment in defcending tubes, and in diverging conical tubes, and explained the refult merely by the theory of confervation of living forces. Euler and d'Alembert obferved to him, that the preffure of the atmofphere was concerned in the effect*. Though the cafe of the defcending tube be different from that of the horizontal or afcending tube, the knowledge of the firft of thefe two cafes may neverthelefs facilitate the knowledge of the fecond. Befides which, the caufes which act in both cafes are often combined together, and it is neceffary to be well acquainted with both, in order to diftinguifh the refults. On this account it is, that in the following propofition I have turned from my principal fubject for a moment to confider the firft cafe, after which I fhall return to the fecond.

Propofition 4. In defcending cylindrical tubes, the upper ends of which poffefs the form of the contracted vein, the expence is fuch as correfponds with the height of the fluid above the inferior extremity of the tube.

* D'Alembert, Traité des Fluides, Art. 149.

The

The ancients remarked, that a defcending tube applied to a refervoir increafes the expenditure*. Mariotte eftimated that the water iffues through C Q, fig. 7, with a velocity nearly the mean proportional between the velocities arifing from the two heights A B, A C†. Guillielmini fought for the caufe of this augmentation in the weight of the atmofphere, and determined the velocity at C to be the fame as would arife from the whole height A C‡. In his reafoning he fuppofes that the preffure at C is the fame for the ftate of motion as for that of reft; which is not true. In the experiments he made upon this object, he paid no regard either to the diminution of expenditure produced by the irregularity of the inner furface of the tubes, nor the augmentation occafioned by the form of the tubes themfelves. By a fingular accidental concurrence, one of thefe errors compenfated for the other. I know of no other decifive experiment on this head fince Guillielmini. I fhall, therefore, proceed to eftablifh the propofition upon the principle of virtual afcenfion combined with the preffure of the atmofphere, and that in a manner which fhall be clear of every objection, of theory as well as of experiment.

Let B L K O reprefent a conical tube adapted to the form of the contracted vein §; the cylindrical tube L C Q K is of the fame diameter as the contracted part. The fluid ftratum, L K, continuing to defcend through L C, tends to accelerate its motion, according to the laws of gravitation; and confequently when it paffes from L K to M N, it tends to detach itfelf from the ftratum which follows, or in other words it tends to produce a vacuum between L K and M N; and the fame effect takes place through the whole length of the tube L C. The preffure of the atmofphere becomes active as far as is neceffary to prevent the vacuum; and its action is alike both at the furface of the fluid at A, and at the inferior extremity of the tube at C. At A it increafes the expenditure, and at C it deftroys the fum of the accelerations which would be produced along L C, fo that the fluid remains continuous in the tube.

Let T reprefent the time which the continuous column of fluid L C Q K employs to pafs through the tube L C, whatever may be the velocity at L, and the fucceffive acceleration from L to C. And if we fuppofe this fame column to return upwards from D to E, it will pafs through the fpace D E = L C in the fame time T; during which it will lofe all the acceleration it acquired from L to C. The preffure of the column E D, continued for the time T, is therefore the quantity required to deftroy the fucceffive acceleration from L to C, and to prevent the fluid from ceafing to be continuous in the tube L C: confequently that part of the preffure of the atmofphere which is exerted at C Q to deftroy the fum of the accelerations through L C, is equal to the preffure of a column E D of a fluid, homogeneous to that of the refervoir A B. And fince the fame preffure muft alfo be exerted on the furface A of the refervoir, if we take F A = L C, the fluid at L K will poffefs the velocity which is proper to the height F L = A C; without confidering the retardation which the external inequalities of the tube L C Q K muft produce.

* Calix devexus amplius rapit. Frontin. de aquæduct. Art. 36. See alfo the Pneumatics of Hero, in the mathem. vet. ed. 1693, page 157.

† Mouvement des eaux, part. 3, difc. 2.

‡ Epift. hydroftatic. Oper. tom. 1. page 212.

§ When I fpeak of the form of the contracted vein, I always mean to exprefs the conoid formed by the fluid iffuing from an orifice through a thin plate.

Experiment IX. 1. The orifice P (fig. 1) through a thin plate is circular, and 18 lines in diameter. The charge of fluid above the centre of the orifice is 40 inches. Four cubic feet of water were emitted in 38 feconds.

2. To the orifice P, fig. 1, I applied the tube A C D, fig. 4, the upper end of which A C had the form of the contracted vein. The diameter at A was 18 lines in length, A D 31 inches, and the fituation of the tube horizontal. The expenditure of four cubical feet was made in 48 feconds.

3. The fame orifice and the fame tube were applied to the horizontal bottom of the refervoir fig. 7, fo that the tube was vertical, and A C = 40 inches, or the height of the charge in the two former experiments. The four cubic feet flowed out in 48 feconds, as in the fecond experiment.

Experiment X. The laft defcribed experiment was repeated with a circular aperture of 11,2 lines in diameter. The extremity A C, of the tube fig. 4, had the form of the contracted vein; the end A having the fame diameter as that of the orifice. The other circumftances were as in the preceding cafes. In the difpofition, according to the firft cafe four cubical feet of water flowed out in 98 feconds; in the fecond cafe the time was 130 feconds; and in the third cafe 129 feconds.

In each of thefe two experiments the tubes and the expence of water were the fame for the fecond and the third cafes; whence it follows, that the force by which the expenditure was governed was the fame in both cafes. Now the force which acts in the fecond cafe is the fame as in the firft; and confequently the fame force likewife acts in the firft and third cafes. All the difference of the refult between the firft cafe and the two following arifes from the retardation produced by the inequalities of the internal furface of the tubes.

Experiment XI. The height A B, fig. 7, being conftantly 32,5 inches, and the orifice B O 18 lines in diameter, the tube B O C Q was applied to the orifice itfelf, the fuperior extremity of this tube having the form of the contracted vein. When the length of the tube was varied, the times of the efflux of four cubic feet of water were as in the following table.

Length of the tube B C in inches.	Time of efflux of four cubic feet by experiment.	Time according to the theory without confidering the retardation.	Difference between the theory and experiment.	Retardations computed from the following experiment.
3	41″	40″	1″	1″,3
12	38″	35″,2	2″,8	3″,4
24	35″	31″,2	3″,8	5″.

The fifth column of this table is calculated from the proportion of retardation produced by the irregularities of the internal furface of the tubes. Citizen Boffut has obferved, that thefe retardations* increafe rather in a lefs ratio than the velocity of the ftream. This is perhaps the reafon of the difference obferved between the fourth and fifth columns.

Experiment XII. I applied to the orifice P, fig. 1, the fame tubes as in the foregoing experiment one after another in an horizontal fituation, the height of the charge being conftant-

* Hydrodyn. Art. 622.

ly

ly 32,5 inches above the centre of the orifice. The times of emission were as in the following table.

Length of the tube B C in inches.	Time of efflux of four cubic feet.	Differences.
0	41″	0
3	42″,5	1″,5
12	45″	4″
24	48″	7″

I must here observe, that the viscidity or mutual adhesion of the particles of the water * is of very little consequence to the increase of expenditure through the orifice B O, fig. 7, by the additional tube B C. For as soon as a small hole is opened at K the increase of expenditure diminishes or entirely ceases, and the fluid is no longer continuous in the tube.

We will now return to tubes in the horizontal and ascending situations.

[*To be continued.*]

VIII.

Experiments and Observations on the Effect of Annealing a Plate of Metal, consisting of fine or alloyed Gold, with one twenty-fourth Part of Tin. By MATTHEW TILLET.

[Concluded from page 142.]

I HESITATED to use my gold of 24 carats, from the just fear I had that it would lose the advantage of being the ductile of metals, and that I could not restore this property but by the method of parting. For this reason I determined to begin my experiments on a more decisive scale than the former, upon gold of 22 carats, or alloyed with $\frac{1}{12}$th part of copper. In this manner I should repeat some of the experiments related by Mr. Alchorne, from which he has constantly inferred that tin, mixed in a certain proportion with gold of this same fineness of 22 carats, does not deprive it of its ductility.

I therefore alloyed 1 gros 24 grains of tin taken from the ingot deprived of arsenic, which I have mentioned, with 4 ounces of gold, the fineness of which, namely 22 carats, was perfectly well ascertained. These two metals, reduced into small pieces, were mixed together, put into a crucible, and urged by the strong heat of a forge with two pair of bellows. When their fusion appeared to be complete, I poured the metal into a small ingot-mould proportioned to the quantity.

The ingot thus obtained had lost scarcely any thing of the weight of the two metals that composed it, which was a proof that the tin had united and incorporated with the four ounces of gold. But on attempting to bend the ingot, which was about six inches long and not more than two or three lines thick, I remarked, contrary to the nature of gold of 22 carats, that it was rigid, and would have required a considerable effort to give it any degree

* Gravesande and others have attributed the increase of expenditure through descending tubes, to the natural cohesion of the particles of water. V.

of curvature, or bring it to the flexibility it would have poffeffed if no tin had entered into its compofition. Hence I clearly faw, that this rigidity announced a diminution of its ductility; that the interpofition of a fubftance, which was foreign to the gold, and incapable by its nature of maintaining the cohefion, was the caufe of this want of flexibility; and that it could be attributed only to the tin, becaufe copper alone, mixed with fine gold, though it gives a greater degree of hardnefs and rigidity than it before poffeffed, deprives it of very little of its ductility.

After this firft obfervation on the ftate of the ingot afforded by the experiment, I came to the more decifive proof by hammering, particularly with the edge of the hammer, in order that the bar might be lengthened, and by that means fubmitted to the moft decifive proof. I did not obferve during the continuation of this procefs, till the bar was reduced to about two-thirds of its firft thicknefs—I did not obferve, I fay, that its edges were cracked, or exhibited much of the appearance of brittlenefs; but as I was apprehenfive that this accident might happen by too long hammering, I divided the bar by cutting off the part which had been hammered out. This part was placed in the midft of lighted charcoal, in order that, by a moderate annealing, it might recover the ftate of malleability it poffeffed before it was hammered.

But when I went to take it out of the fire, where it had undergone no greater heat than a cherry-red, I found it divided into two parts. After having fuffered thefe to cool, I forged them again. They were extended with confiderable eafe, though with fome cracks at the edges; but they did not yet fatisfy the whole of my enquiries. I therefore annealed one of the two laft-mentioned pieces a fecond time, and referved the other in its hard hammered ftate to be paffed between the laminating rollers. The annealed part, which might have the thicknefs of about a fhilling (piece de douze fols), broke in the fire, though the heat was very gentle, into four or five portions. The longeft of thefe portions, which beft refifted the action of the fire, bent and twifted itfelf, and fhewed, by this ftate of ftrong contraction in different directions, that it had tended to break and become divided into fmall portions fimilar to thofe which had already feparated from it.

This accident gave me reafon to fufpect that the afhes, upon which I had annealed the plate of gold which broke in pieces, might alfo contain certain portions. I was not deceived; for the afhes being carefully wafhed left three or four fragments which the ignited coals had prevented from being feen.

It has been remarked, that I referved one of the two parts of the portion of the ingot which I had forged a fecond time, and that I had kept it in its hammer-hardened ftate. I could not doubt, after the laft-recited experiment, that any attempt to anneal this portion, even by the moft moderate heat, would be attended with the fame confequence, namely, that it would break in pieces. I therefore determined to extend it ftill more between the rollers, fetting them up very gradually in order that the fracture, if it fhould take place, might be principally owing to the brittlenefs of the material, and not to the force of compreffion to which it was fubjected. By this management I fucceeded in extending the metal to double its length notwithftanding its hardnefs. In this manner it was rendered as thin as ftrong paper; though it muft be confeffed that the edges were cracked through their whole length like the teeth of a faw. But this accident is not at all furprifing, when it is confidered that gold, though alloyed fimply with copper, whatever may be the caufe, does

does not possess its usual ductility, particularly when it is laminated very thin without repeated annealing as the metal becomes hard.

It might be presumed, in reflecting on the experiment I have related, that the fracture of the pieces of gold was owing to an incomplete fusion or unequal mixture of the two metals. I was aware, from this notion, that it was proper to melt the ingot over again with all the parts which had been separated from it, and to neglect no precaution, after well mixing it, that it should be poured out in perfect fusion. At the moment this state of fusion was obtained, I threw a small quantity of calcined borax upon the metal, in order that its surface might become clear, and every foreign substance might be carried to the circumference. No other consequence could arise from this use of borax than a greater softness in the compound, and consequently less risk of its breaking when it came to be subjected to the hammer.

All my precautions were useless. I forged one end, which was lengthened very well without any perceptible crack; but the extremity of this ingot, afterwards reduced to a small thickness and exposed to the annealing heat, became divided into several parts, the longest of which was contorted, and would no doubt have broken if the heat had been stronger, or continued for a greater length of time.

Though the experiment, of which I have related the result, on the mixture of one part of tin and 24 parts of gold of 22 carats, might, in strictness, have been sufficient to prove that this alloy deprives gold of great part of its ductility, and exposes it, while annealing, to an accident which artists would scarcely be able to avoid, from the necessity they are under of continually restoring the malleability of the gold they work, by annealing it after it has become hard under the hammer; I nevertheless thought it proper to lay aside the objection I had to deprive my fine gold of its ductility, which could not be restored but by a new parting essay, and to repeat the experiment by using gold of 24 carats in such a quantity that my operation might be compared with that of Mr. Alchorne, from which chiefly he has drawn his conclusion.

I therefore employed 6 ounces of fine gold and 2 gros of tin for this new experiment. The first of these metals was divided into a great number of pieces, and I had included the two gros of tin in two leaves of gold taken from the 6 ounces, rolled out very thin, and so flexible, after annealing, that they perfectly enveloped the tin. After having put one part of the gold at the bottom of a small crucible, I placed the tin wrapped up in gold upon it, and over this I put the remainder of the gold.

When the whole of the two metals was in perfect fusion, I poured them speedily into an ingot mould, which I had before used, and obtained an ingot rather longer and cleaner than the two former.

As soon as it was cold, I forged one of its extremities with the edge of the hammer. It was lengthened without any perceptible crack; and when it was reduced to the thickness of one line, or thereabouts, I cut it off for separate treatment. By moderate annealing it maintained its integrity; and, with the exception of a few cracks, it passed the laminating rollers without breaking. As I was fearful, nevertheless, that it might break in some part if I continued to laminate it, I gave it a slight annealing. It had scarcely acquired a cherry redness between the charcoal before it broke into five or six parts, some of which were simply bended or twisted, and others flat as they quitted the rollers. Among the annealed

6 pieces

pieces of this extremity of the ingot, there was one fufficiently long, though a little curled, which I laminated a fecond time, with the determination of rendering it very thin without the leaft annealing. It acquired at leaft double the length it had at firft without breaking; and, if we except the two fides of this plate which were cracked, the body, or main piece, was entire. It was fpongy, and might be confidered as if formed out of an ingot of common gold containing no tin, but not poffeffing the whole of its natural ductility.

It follows, from thefe experiments, that gold, whether fine or alloyed, when perfectly fufed with a fmall portion of the fineft tin, acquires rigidity and hardnefs by the mixture; that it lofes fomewhat of its diftinguifhing colour; and that it may, indeed, by careful management, be extended to a certain degree by the hammer, or ftill better by the rollers; but that, as it cannot be annealed without danger of breaking, it is by this defect deprived of the effential advantage of recovering its original foftnefs after it has been ftrongly hammer hardened. It is not but by careful management in the ufe of the hammer, and by frequent annealing, that artifts employed on works of gold and filver fucceed in obtaining them without cracks, and bringing them to a ftate of perfection, without being obliged to have recourfe to folder to repair the defects which exceffive hardnefs under the hammer would occafion. How much, therefore, ought gold-workers, who continually have this metal in their hands, to be attentive to prevent the introduction of tin in their workfhops, and never to employ fuch compounds of gold as are fubject to break, or even to warp, while annealing! The expence of refining, which they would pay for depurating fuch compounds, would be of lefs confequence to them than the lofs of time required for the careful management of fuch gold contaminated by tin, even if they did fucceed in ufing it, and were not often forced to abandon, after much labour, a work nearly finifhed.

I do not doubt but that Mr. Alchorne, if he had carried his experiments further, or had confidered them with regard to the methods of gold-workers, who frequently expofe to the fire fuch pieces as they propofe to raife or fafhion according to their defigns;—I do not doubt, I fay, but that Mr. Alchorne would have cautioned artifts againft the accidents to which gold alloyed with tin is fubject while annealing. He has obferved a degree of ductility in this matter, and has not prefumed that it might be taken away by means of fire, which, on the contrary, reftores to moft metals their flexibility and facility of working.

The opinion which has hitherto been maintained refpecting the danger of a very fmall alloy of tin with gold is therefore well founded. In fact, it muft have been difficult for fuch an opinion to have prevailed without foundation, when a multitude of artifts are in a fituation to verify the fact, and muft be immediately ftruck with the brittlenefs of fuch gold, and be moft ftrongly interefted to difcover its caufe.

This example of an opinion generally received and fupported on conftant facts, which the greater number of artifts have rather adopted than examined; this example proves that we ought not, without the greateft caution, to attack fuch received notions, particularly when as in the prefent cafe it is in no refpect hurtful, and only tends to render the workmen more cautious in the ufe of the moft valuable of metals.

The experiments of Mr. Alchorne have long remained upon record in the Philofophical Tranfactions, and have by that means acquired a fanction which demands a greater degree of attention. I have had no other aim in repeating them, and exhibiting an effential dependant fact not mentioned by that fkilful affayer, than to give ufeful information to artifts,

and

and contribute to the certainty of their operations. I think this purpofe will be anfwered, by leaving them in poffeffion of all the apprehenfion they have hitherto entertained refpecting the mixture of tin with gold.

If it fhould be thought that my experiments may not be as conclufive as at firft fight they appear, they will at leaft produce no other effect than that of giving artifts fome ufelefs trouble. They will with reafon attend to them, however fuperfluous they may be fuppofed, and will prefer the care I recommend to the anxiety of working upon materials rendered fufpicious by their harfhnefs, and fuppofed to be incapable of annealing on account of their containing tin.

If it be allowable to form fome conjectures on the fracture of plates of gold containing tin, when fubjected to the annealing heat, it may be prefumed, that the tin, which very fpeedily melts, while the gold requires a very ftrong heat for its fufion—it may be prefumed, I fay, that the parts of the tin intermixed in a fort of proportional equality with thofe of the gold, tend to feparate by a fpeedy fufion and at a very gentle degree of heat; that they remain without confiftence between the parts of the gold, while the latter preferve the whole of their folidity, and do not lofe it even by the annealing heat; whence it feems, that the parts of the precious metal, when ignited among the coals, having no longer the folid connection formed by the tin, but, on the contrary, having an infinite number of fmall cavities occupied by particles of that metal in fufion, muft tend to difunion; whereas the fame accident does not take place in the pieces which have refifted the annealing, and have been laminated after cooling, becaufe the particles of tin have become folid by cooling, and have recovered their original ftate of union with the gold.

This fracture of the compound does not take place with an alloy of gold and copper, for an oppofite reafon to that which has here been explained; namely, becaufe thefe two metals require nearly the fame heat for their fufion. The effect of annealing being therefore equal upon both, the metals, notwithftanding this treatment, preferve their natural confiftence, even though the heat be carried near the point of fufion.

In fupport of the opinion I have prefented, refpecting the fracture of thefe plates of gold, I muft obferve that, by examining their furfaces under the microfcope, a great number of particles of tin may be diftinguifhed, which appear to be difengaged from the pores of the gold; that thefe furfaces, being applied by an annealing heat to a plate of iron or filver, adhere ftrongly by virtue of thefe particles of tin; that they cannot be feparated but with difficulty, and even tear up fome flight portions of the metal on which they were annealed when they are fo feparated.

I fhall conclude thefe details, into which the fubject of my experiments has forced me to enter, by remarking that the plates of gold here fpoken of may be kept entire, when annealed, on a flat plate of metal, on which they may remain fupported till cold; whereas, if placed on ignited charcoal, they frequently bend or twift, and are fubject to break, particularly if an attempt is made to take them out with the tongs in their ignited ftate.

IX.

On the Knowledge of the Ancients reſpecting Gravity. By a Correſpondent.

THE reſpectable publiſhers of the "Journal of Natural Philoſophy, Chemiſtry and the Arts," are deſired to ſubmit to the inſpection of its ingenious Author the following obſervations relative to a Note at page 85 of the 15th Number. (Vol. II.)

"In the treatiſe of 'Plutarch De Placitis Philoſophorum,' occurs no paſſage referring to the 'vibration of a body through the centre of the earth to the antipodes, and to the retention of the moon in its orbit by the combination of the projectile and gravitating forces.'"

The paſſage alluded to is probably in the treatiſe "De Facie quæ in Orbe Lunæ apparet;" but there, ſo far from proving a gravitating force, the abſurdity of the ſuppoſition is endeavoured to be ſhewn.

Ed. Oxon.—8vo. T. iv. § ζ. *Φιλοσοφων δε ουκ ακουστεον, αν τα παραδοξα παραδοξοις αμυνεσθαι βουλωνται, και μαχομενοι προς τα θαυμασια των δογματων, ατοπωτερα και θαυμασιωτερα πλαττωσιν· ωσπερ ουτοι την επι το μεσον φοραν εισαγουσιν. Ἡ, τι παραδοξον ουκ ενεστιν; Ουχι την γην σφαιραν ειναι, τηλικαυτα βαθη και υψη και ανωμαλιας εχουσαν; Ουκ αντιποδας οικειν, ωσπερ θριπας, η γαλεωτας, τραπεντα ανω τα κατω τῃ γῃ προϊσχομενους; ημας δε αυτους μη προς ορθας βεβηκοτας, αλλα πλαγιους επιμενειν απονευοντας, ωσπερ οι μεθυοντες; Ου μυδρους χιλιοταλαντους δια βαθους της Γης φερομενους, οταν εξικωνται προς το μεσον, ιστασθαι μηδενος απαντωντος, μηδε υπερειδοντος; Ει δε ρυμη κατω φερομενοι το μεσον υπερβαλλοιεν, αυθις οπισω στρεφεσθαι και ανακαμπτειν απ' αυτων.* Thus tranſlated by Philemon Holland—"But we ought not to give ear unto philoſophers, if they would maintain ſtrange "paradoxes by other poſitions as abſurd, or, to confute admirable opinions, deviſe others "much more extravagant and wonderful, like as theſe here who broach and bring in a "notion, forſooth, tending unto a middle, wherein, what abſurdity is there not? Hold "not they that the earth is as round as a ball, and yet we ſee how many deep profundities, "haughty ſublimities and manifold inequalities it hath? Affirm not they that there be an-"tipodes dwelling oppoſite one unto another, and thoſe ſticking as it were to the ſides of the "earth with their heels upward and their head downwards topſy turvy, like unto theſe wood-"worms or cats (r. newts) which hang by their ſharp claws? Would not they have even us alſo "that are here, for to go upon the ground not plumb upright, but bending or inclining ſide-"long, reeling and ſtaggering like drunken folk? Do they not tell us tales, and would make "us believe, that if bars and maſſes of iron weighing a thouſand talents a piece were let fall "down into the bottom of the earth, when they came once to the middle centre thereof, "will ſtay and reſt there, albeit nothing elſe came againſt them nor ſuſtained them up? And "if peradventure by ſome forcible violence they ſhould paſs beyond the ſaid midſt, they "would ſoon *rebound back thither* again of their own accord?"

So true is the obſervation of Cleomedes, that thoſe among the ancients who treated of natural philoſophy were much confuſed, and on this head greatly erred, not being able to diſcern, that ſince the world was of a ſpherical figure, the centre muſt of neceſſity be inferior to every part of it—*αι δε λοιπαι σχεσε πολλην παρεσχον ταραχην τοις παλαιοτεροις των φυσικων—ου δυνηθεντων επιστησαι, οτι εν τῳ κοσμῳ, σφαιρικῳ τῳ σχηματι οντι, κατω μεν απο παντος αυτου, το μεσαιτατον ειναι αναγκαιον.* De Meteoris, ed. Balforei, 1605, p. 9.

Remarks

Remarks on the preceding Communication.

UPON consulting the original, to which I referred by memory, in the note at page 85 of the present volume, I perceive that both the passages alluded to are in the treatise De Facie in Orbe Lunæ. I was misled by another obscure passage in the third book De Placitis Philosophorum, under the title Περι κινησεως γης. ιγ. where it is said that Philolaus the Pythagorean held that the earth revolves in an orbit (περι το πυρ) around the fire, and that Heraclides of Pontus and Ecphantus the Pythagorean attributed motion to the earth, not progressive, but rotatory, from west to east. I did not, as my learned correspondent seems to intimate, pretend to say that either Plutarch or the speakers in his treatise exhibited the doctrines of the gravitating and projectile forces, as being worthy of adoption. My implied argument was, that since they are extant in a passage of some length, there were in fact philosophers among the ancients who had maintained and developed them; though, from a variety of well-known causes, this doctrine remained without distinction among other ill-digested or false systems. The combination of these forces in the moon, which occurs in the paragraph to which Theo replies, in my correspondent's quotation, is καιτοι τη μεν σεληνη βοηθεια προς το μη πεσειν, η κινησις αυτη και το ριζωδες της περιαγωγης· Ωσπερ, &c. "But the moon is prevented from "falling by its motion, and the violence of its revolution; as bodies placed in a sling and "whirled round do not drop out. For every body will be carried according to its natural "motion, if not diverted by some other cause. The moon is not, therefore, carried in the "direction of (or by) its weight, because its circular motion opposes this tendency."

X.

A short Mineralogical Description of the Mountain of Gibraltar. By Major IMRIE*.

THE mountain of Gibraltar is situated in 36° 9′ north latitude, and in 5° 17′ east longitude from Greenwich. It is the promontory which, with that of Ceuta upon the opposite coast of Barbary, forms the entrance of the straits of Gibraltar from the Mediterranean; and Europa Point, which is the part of the mountain that advances most towards Africa, is generally regarded as the most southern promontory in Europe. The form of this mountain is oblong; its summit a sharp craggy ridge; its direction is nearly from north to south; and its greatest length in that direction falls very little short of three miles. Its breadth varies with the indentations of the shore, but it no where exceeds three quarters of a mile. The line of its ridge is undulated, and the two extremes are somewhat higher than its centre.

The summit of the Sugar-loaf, which is the point of its greatest elevation towards the south, is 1439 feet; the Rock Mortar, which is the highest point to the north, is 1350; and the Signal-house, which is nearly the central point between these two, is 1276 feet above the level of the sea. The western side of the mountain is a series of rugged slopes, interspersed with abrupt precipices. Its northern extremity is perfectly perpendicular, except towards the north-west, where what are called the Lines intervene, and a narrow passage of flat

* Transactions of the Royal Society of Edinburgh, iv. 191.

ground that leads to the isthmus, and is entirely covered with fortification. The eastern side of the mountain mostly consists of a range of precipices; but a bank of sand rising from the Mediterranean in a rapid acclivity covers a third of its perpendicular height. Its southern extremity falls in a rapid slope from the summit of the Sugar-loaf into a rocky flat of considerable extent called Windmill-hill. This flat forms half an oval, and is bounded by a range of precipices, at the southern base of which a second rocky flat takes place similar in form and extent to Windmill-hill; and also like it surrounded by a precipice, the southern extremity of which is washed by the sea, and forms Europa Point. Upon the western side this peninsular mountain is bounded by the bay of Gibraltar, which is in length nearly eight miles and a half, and in breadth upwards of five miles. In this bay the tide frequently rises four feet. Upon the north the mountain is attached to Spain by a low sandy isthmus, the greatest elevation of which above the level of the sea does not exceed ten feet, and its breadth at the base of the rock is not more than three quarters of a mile. This isthmus separates the Mediterranean on the east from the bay of Gibraltar on the west.

This mountain is much more curious in its botanical than in its mineralogical productions. In respect to the first, it connects in some degree the Flora of Africa with that of Europe. In respect to the latter, it produces little variety; perhaps a few substances and phenomena that are rare, but none that are peculiar.

The principal mass of the mountain rock consists of a grey dense (what is generally called primary) marble; the different beds of which are to be examined in a face of 1350 feet of perpendicular height, which it presents to Spain in a conical form. These beds or strata are of various thickness, from 20 to upwards of 40 feet, dipping in a direction from east to west nearly at an angle of 35 degrees. In some parts of the solid mass of this rock I have found testaceous bodies entirely transmuted into the constituent matter of the rock, and their interior hollows filled up with calcareous spar; but these do not occur often in its composition, and its beds are not separated by any intermediate strata.

In all parts of the globe where this species of rock constitutes large districts, it is found to be cavernous. The caves of Gibraltar are many, and some of them of great extent. That which most deserves attention and examination is called St. Michael's Cave, which is situated upon the southern part of the mountain, almost equally distant from the Signal-tower and the Sugar-loaf. Its entrance is 1000 feet above the level of the sea: this entrance is formed by a rapid slope of earth which has fallen into it at various periods, and which leads to a spacious hall incrusted with spar, and apparently supported in the centre by a large massy stalactitical pillar. To this succeeds a long series of caves of difficult access. The passages from the one to the other of these are over precipices, which can only be passed by the assistance of ropes and scaling-ladders. I have myself passed over many of these to the depth of 300 feet from the upper cave; but at that depth the smoke of our torches became so disagreeable that we were obliged to give up our pursuit, and leave caves still under us unexamined. In these cavernous recesses, the formation and process of stalactites is to be traced from the flimsy quilt-like cone, suspended from the roof, to the robust trunk of a pillar, three feet in diameter, which rises from the floor, and seems intended by nature to support the roof from which it originated.

The variety of form which this matter takes in its different situations and directions renders this subterraneous scenery strikingly grotesque, and in some places beautifully picturesque.

The

The ſtalactites of theſe caves when near the ſurface of the mountain are of a browniſh yellow colour; but as we deſcended towards the lower caves we found them begin to loſe their darkneſs of colour, which by degrees ſhaded off to a whitiſh yellow.

The only inhabitants of theſe caves are bats, ſome of which are of a large ſize. The ſoil in general upon the mountain of Gibraltar is but thinly ſown; and in many parts that thin covering has been waſhed off by the heavy autumnal rains, which have left the ſuperficies of the rock for a conſiderable extent bare and open to inſpection. In thoſe ſituations an obſerving eye may trace the effects of the ſlow but conſtant decompoſition of the rock, cauſed by its expoſure to the air, and the corroſion of ſea ſalts, which in the heavy gales of eaſterly winds are depoſited with the ſpray on every part of the mountain. Thoſe uncovered parts of the mountain rock alſo expoſe to the eye a phenomenon worthy of ſome attention, as it tends clearly to demonſtrate, that, however high the ſurface of this rock may now be elevated above the level of the ſea, it has once been the bed of agitated waters. This phenomenon is to be obſerved in many parts of the rock, and is conſtantly found in the beds of torrents. It conſiſts of pot-like holes of various ſizes hollowed out of the ſolid rock, and formed apparently by the attrition of gravel or pebbles ſet in motion by the rapidity of rivers or currents in the ſea. One of thoſe, which had been recently laid open, I examined with attention. I found it to be five feet deep and three feet in diameter; the edge of its mouth rounded off as if by art, and its ſides and bottom retaining a conſiderable degree of poliſh. From its mouth for three feet and a half down it was filled with a red argillaceous earth, thinly mixed with minute parts of tranſparent quartz cryſtals: the remaining foot and a half to the bottom contained an aggregate of water-worn ſtones, which were from the ſize of a gooſe's egg to that of a ſmall walnut, and conſiſted of red jaſpers, yellowiſh white flints, white quartz, and blueiſh white agates, firmly combined by a yellowiſh brown ſtalactitical calcareous ſpar. In this breccia I could not diſcover any fragment of the mountain rock, or any other calcareous matter, except the cement with which it was combined. This pot is 940 feet above the level of the ſea.

Upon the weſt ſide of the mountain towards its baſe ſome ſtrata occur, which are heterogenial to the mountain rock: the firſt or higheſt forms the ſegment of a circle; its convex ſide is towards the mountain, and its ſlopes alſo in that direction. This ſtratum conſiſts of a number of thin beds; the outward one, being the thinneſt, is in a ſtate of decompoſition, and is mouldering down into a blackiſh brown or ferruginous coloured earth. The beds inferior to this progreſſively increaſe in breadth to 17 inches, where the ſtratification reſts upon a rock of an argillaceous nature.

This laſt bed, which is 17 inches thick, conſiſts of quartz of a blackiſh blue colour, in the ſepta or cracks of which are found fine quartz cryſtals, colourleſs and perfectly tranſparent. Theſe cryſtals are compoſed of 18 planes, diſpoſed in hexangular columns, terminated at both extremities by hexangular pyramids. The largeſt of thoſe that I have ſeen does not exceed two-eighths of an inch in length: they in general adhere to the rock by the ſides of the column, but are detached without difficulty. Their great degree of tranſparency has obtained them the name of Gibraltar diamonds.

[*To be continued.*]

ACCOUNT OF NEW BOOKS.

Memoirs of the Literary and Philofophical Society of Manchefter, Vol. V. Part I. 8vo. 318 pages, with four plates. Price 6 fhillings in boards. Cadell and Davies, London, 1798.

THIS volume contains, 1. Curfory Remarks, moral and political, on Party Prejudice. By Samuel Argent Bardfley, M. D.—2. Extraordinary Facts relating to the Vifion of Colours; with Obfervations. By Mr. John Dalton. The intelligent author of this valuable paper is himfelf the fubject of many of his obfervations refpecting the peculiarity of his perceptions of colours. This Memoir confifts of an account of his own vifion; an account of others whofe vifion has been found fimilar to his own; and obfervations refpecting the probable caufe of the peculiarities he defcribes. The folar fpectrum, formed by the prifmatic difperfion of light, exhibits to him only two, or at moft three, colours, which he fhould call yellow and blue, or yellow, blue, and purple. This yellow comprehends the red, orange, yellow, and green of others; and his blue and purple coincide with theirs. He enters minutely into the requifite comparative obfervations on the feveral colours refpectively as they affect his organs of fight. Pink appears, by day-light, to be fky-blue a little faded; by candle-light it affumes an orange or yellowifh appearance, which forms a ftrong contraft to blue. Crimfon appears a muddy blue by day; and crimfon woollen yarn is much the fame as dark blue. Red and fcarlet have a more vivid and flaming appearance by candle-light than by day-light. There is not much difference in colour between a ftick of red fealing-wax and grafs by day. Dark green woollen cloth feems a muddy red, much darker than grafs, and of a very different colour. The colour of a florid complexion is dufky blue. Coats, gowns, &c. appear to Mr. Dalton, and the other individuals he mentions, frequently to be badly matched with linings, when others fay they are not. On the other hand, they fhould match crimfons with claret or mud; pinks with light blues; browns with reds; and drabs with greens. In all points where they differ from other perfons, the difference is much lefs by candle-light than by day-light. Mr. Dalton, by various obfervations, fhews, almoft beyond a doubt, that this affection of the eye is caufed by fome of the humours, probably the vitreous, being coloured by fome modification of blue.—3. An Enquiry into the Name of the Founder of Huln Abbey, Northumberland, the firft in England of the Order of Carmelites; with Remarks on Dr. Ferriar's Account of the Monument in the Church of that Monaftery. By Robert Uvedale, B. A. of Trinity College, Cambridge.—4. On the Variety of Voices. By Mr. John Gough. It is a fact which continually prefents itfelf to our obfervation, that founds differ from each other in other refpects as well as in mufical tone and their intenfity. The oboe, the violin, and the flute, however perfectly they may be made to agree in thefe refpects, are ever found to differ in fomething which may be called the character of the tone itfelf. The author of this paper conceives that the variety of human voices is of this laft kind; and, in his reafoning on the philofophy of found, he confiders every tone as compounded. So that the fame cotemporaneous founds which are heard in the bell, though practically confidered as if it emitted the fundamental note only, being conceived to exift in all other tones, thefe will be found to differ according to the number, the nature or relation, and the intenfity of their component parts. That circum-

ftances

ſtances of this kind may modify the aggregate of ſonorous undulation, is ſcarcely to be doubted; but Mr. Gough ſeems to have overlooked that modification, which may be proved to be the diſtinguiſhing circumſtance between tone and tone when they are in unifon. If a toothed wheel be made to revolve very ſwiftly with its teeth againſt a ſpring, which ſhall ſtrike each tooth in ſucceſſion, a muſical tone will be produced, which will be more acute the ſwifter the rotation. But this tone will differ accordingly as the material of the wheel is itſelf diſpoſed to give a tone of greater acuteneſs or gravity by each ſingle or individual ſtroke. If the ſame note be afforded by a braſs and by a wooden wheel revolving on one axis, the character of each note will differ in the manner here ſtated. Or more ſimply, if a cord of thirty or forty feet in length, or ſhorter if more convenient, be ſtretched, and in this ſtate ſtruck with the edge of the hand, the vibrations will be ſlow enough to be counted, and will evidently appear to be of two kinds. When the ſtring is ſtruck near the end or bridge, a wave or undulation will be propagated to the other bridge, and immediately return; which proceſs will continue as long as the elaſticity of the ſtring can maintain the motion. The waves of this undulation will be larger or ſmaller, the more remote or the nearer the place of percuſſion is to the bridge; but the times of tranſmiſſion of the waves backwards and forwards along the ſtring will be invariable. The tone of ſuch a ſtring, ſuppoſing its dimenſions and tenſion to be ſuch as would produce ſound, may therefore be conſidered as if the tone produced by the ſyſtem of waves were interrupted as often as the re-action of each bridge cauſes the retrograde motion of the ſyſtem; that is to ſay, it is afforded by a proceſs ſimilar to that of the experiment with the wheels, where the peculiar ſound of a blow upon each tooth was inceſſantly repeated and ſuſpended at regular intervals. Now, the muſical acuteneſs or gravity of the tone depends altogether on the time of theſe intervals, and not at all on the nature of the ſound which is thus repeated; but the character of the tone is governed by this laſt circumſtance. And accordingly it is found by thoſe who make harpſichords, and other ſimilar inſtruments, that the character of the tone of the ſame ſtring is wonderfully changed, accordingly as the jacks or hammers are made to operate nearer to, or further from, the bridge.—5. On the Benefits and Duties reſulting from the Inſtitution of Societies for the Advancement of Literature and Philoſophy. By the Reverend Thomas Giſborne, M. A.—6. On an Univerſal Character. By James Anderſon, LL.D. F.R.S. &c. By this letter, which bears date February 20, 1795, it appears that the Doctor had then made conſiderable progreſs in the inveſtigation of the means of writing language by characters regularly conſtructed on thoſe principles of mental operation which conſtitute univerſal grammar, and do not require the intervention of ſound. To thoſe who have conſidered this ſubject, particularly with reſpect to the practice of the Chineſe, whoſe written words are known to repreſent, for the moſt part, things inſtead of words, and conſequently are intelligible to nations who do not ſpeak the ſame language, the practicability of this ſcheme will be a matter of no doubt. But whether the difficulties be ſuch as to operate more ſtrongly againſt its introduction in Europe than the immediate motives of convenience in favour of its reception, is a queſtion which can only be decided by minute and continued attention to the whole ſubject. That theſe difficulties are comparatively ſmall, and would yield to the induſtry of active and intelligent men, in the firſt inſtance, is ſcarcely to be doubted, provided the exertions of the firſt promoters of this ſcheme were carried ſo far as to afford a grammar, a dictionary, and one or two well written intro-

ductory

ductory books on morals, political economy, or the more confined ſciences of mechanics, chemiſtry, or aſtronomy. The facility and accuracy of enunciation, the ſpeed and brevity of deſcription, and the extreme rapidity of peruſal, would ſoon realize thoſe expectations upon which our author and a few men of reflection now meditate as the objects of enjoyment deſtined for remote future ages. The Doctor has not entered into the particulars of his own ſcheme; and the preſent occaſion does not admit of a detail of the multiplied means and advantages which preſent themſelves in the contemplation of this intereſting ſubject. From the date of his letter, it is to be feared that other avocations may have ſuſpended this purſuit, as well on his own part as that of his able friend whom he mentions as being actively engaged in it.—7. The Inverſe Method of Central Forces. Communicated by Dr. Holme.—8. Obſervations on Iron and Steel. By Joſeph Collier. After a conciſe account of the methods of ſmelting and refining iron, a deſcription is given of the manufacture of ſteel by cementation, with a good drawing exhibiting the plan and ſection of a furnace for performing this operation. I do not recollect meeting with a drawing of this kind in any Engliſh author. Mr. Collier's paper exhibits a ſummary of the moſt eſſential facts related by Reaumur, Duhamel, Vandermonde, Monge, and Berthollet, and others*.—9. Remarks on Dr. Prieſtley's Experiments and Obſervations relating to the Analyſis of Atmoſpherical Air, and his Conſiderations on the Doctrine of Phlogiſton and the Decompoſition of Water. By Theophilus Lewis Rupp. The modern chemiſtry which rejects phlogiſton is ably defended in this paper, of which, from the cloſe method of quotation and remark according to which it is written, it is impoſſible to give any fair account in this place.—10. An Account of three different Kinds of Timber Trees, which are likely to prove a great Acquiſition to this Kingdom, both in point of Profit, and as Trees for Ornament and Shade. By Charles White, Eſq. F.R.S. The trees are the black American birch with broad leaves, the Athenian poplar, and the iron oak with prickly cups. For the minute particulars of deſcription, as well as the perſpicuous detail of advantages, recourſe muſt be had to the paper itſelf.—11. An Analyſis of the Waters of two Mineral Springs at Lemington Priors, near Warwick, including Experiments tending to elucidate the Origin of the Muriatic Acid. By William Lambe, M. A. late Fellow of St. John's College, Cambridge.—12. Some Account of the Perſian Cotton Tree. By Matthew Guthrie, M.D. F.R.S. &c. — 13. Experiments and Obſervations on the Preparation and ſome remarkable Properties of the Oxygenated Muriate of Potaſh. By Thomas Hoyle, jun.—14. Experiments and Obſervations on Fermentation and the Diſtillation of Ardent Spirit. By Joſeph Collier. On the chemical papers laſt mentioned, as well as the ſubſequent paper on bleaching, I make no remarks, not only for the ſake of brevity, but becauſe I hope on a future occaſion to give a more ample account of their contents.—15. Hints on the Eſtabliſhment of an Univerſal Written Character. By William Brown, M.D. Notwithſtanding the great value of this paper, and the ability of its author, who has occaſionally turned his thoughts during three years to Dr. Anderſon's project, I cannot help wiſhing that he may not be the friend to whom that author refers. His hints contain an outline of the proceſs by which an univerſal written character is to be eſtabliſhed; but he ſeems rather diſpoſed to conſider the pro-

* The author mentions certain omiſſions in my Chemical Dictionary, article Iron, which he would have found under the word Steel, if he had turned to that article.

ject

ject as being very laborious, and in its practical application difficult to be eftablifhed. Under fuch impreffions, much is not to be expected in the progrefs. It feems neverthelefs that mankind have rejected univerfal characters when offered to them, not becaufe they were averfe from the means of facility and perfpicuity, but becaufe thofe means were not in reality difplayed. The Arabic numerals, the notation of mufic, and the fymbols of quantity and operation ufed by algebraifts, are univerfal characters confined to no language, and have been univerfally received becaufe they are fimple and appropriate; but the characters of chemifts, as well as the few attempts at the more ample defignation of things in the fame manner, have not been received, becaufe their authors have ftopped fhort in their ufe and adaptation. If Bifhop Wilkins, and the great men of the laft century who favoured his univerfal character, had ftudied and ufed it to fuch an extent as to have written treatifes according to that notation; or if the great Bergman had attempted a reform in the characters of chemiftry, and adopted fuch as could be readily formed, eafily diftinguifhed, and compounded, with all the advantages of pofition, of which he was fo well aware; if he had completed a fet of tables exhibiting the whole of chemical fcience, and expreffing things, quantities, actions, temperatures, and other habitudes, in a few pages, which thofe who attentively meditate on his compound tables of elective attraction may without difficulty conceive to be poffible; the lovers of that fcience would have learned to read his work, and to write others according to the fame method; and that for reafons of the fame kind as have induced arithmeticians, muficians, and algebraifts, to become mafters of the univerfal characters proper to their refpective fciences.—16. On the Procefs of Bleaching with the Oxygenated Muriatic Acid, and a Defcription of a new Apparatus for Bleaching Cloths with that Acid diffolved in Water, without the Addition of Alkali. By Theophilus Lewis Rupp.—17. Account of a remarkable Change of Colour in a Negro. By Miers Fifher. This man's father was the fon of a native African and an Indian of Philadelphia. His mother was the daughter of an African man and an Irifh woman. By a certificate here exhibited, it appears, that until the month of February 1792, being then at leaft 30 years old, he was of as dark a complexion as any African; at which period his fkin began to change white, like that of an European, commencing at his fingers' ends, and proceeding, chiefly in the fummer or warm weather, over the reft of his body. The greater part of the furface was thus changed at the time of the account being written, which was the 22d of November 1796. The change was not uniform over the whole furface, but gradually progreffive along the fkin; the black and white parts being feparated by an irregular line. No facts are mentioned in explanation of this remarkable change.

Tranfactions of the Royal Society of Edinburgh, Vol. IV. Quarto. 470 pages, with 12 plates. Printed for Cadell and Davies in London; and Dixon and Balfour, Edinburgh. 1798.

This work is divided into three parts.

Part I. contains the Hiftory of the Society, or Reports concerning Memoirs prefented, and Communications made, with a Lift of the Officers and Members, Donations, and other fimilar Particulars. It alfo contai[ns] Biographical Accounts of Lord Abercromby; of William Tytler, Efq. of Woodhoufelee; of Mr. William Hamilton, late Profeffor of Anatomy and Botany in the Univerfity of Glafgow; and of John Roebuck, M. D.

Part II.

Part II. contains, I. Papers of the Phyfical Clafs.—1. Account of a Mineral from Strontian, and of a peculiar Species of Earth which it contains. By Thomas Charles Hope, Profeffor* of Medicine in the Univerfity of Glafgow.—2. Obfervations on the Natural Hiftory of Guiana. By William Lochead, Efq.—3. On the Principles of the Antecedental Calculus. By James Glenie, Efq.—4. Obfervations on the Trigonometrical Tables of the Brahmins. By John Playfair, Profeffor of Mathematics in the Univerfity of Edinburgh.—5. Some Geometrical Porifms, with Examples of their Application to the Solution of Problems. By Mr. William Wallace, Affiftant Teacher of the Mathematics in the Academy of Perth.—5†. On the Latitude and Longitude of Aberdeen. By Andrew Mackay, LL.D. & F.R.S. Edin.—6. An Account of certain Motions which fmall lighted Wicks acquire when fwimming on a Bafon of Oil. By Patrick Wilfon, F.R.S. Edin. and Profeffor of Practical Aftronomy in the Univerfity of Glafgow. (See Philof. Journal, II. 167.)—7. Account of a fingular Halo of the Moon. By William Hall, Efq. of Whitehall, F.R.S. Edin.—8. A New Series for the Rectification of the Ellipfis, with Obfervations on the Evolution of a certain Algebraic Formula. By James Ivory, A.M.—9. Mineralogical Defcription of the Mountain of Gibraltar. By Major Imrie.—10. Defcription of a Thermometer, which marks the greateft Degree of Heat and Cold from one Time of Obfervation to another. By Alexander Keith, Efq. F.R.S. & F.A.S. Edin.—11. Defcription of a Barometer which marks the Rife and Fall of the Mercury from two different Times of Obfervation. By Alexander Keith, Efq. F.R.S. & F.A.S. Edin.—12. Meteorological Abftract for the Years 1794, 1795, 1796.———II. Papers of the Literary Clafs.—1. On the Origin and Principles of Gothic Architecture. By Sir James Hall, Bart. F.R.S. & A.S.S. Edin.—2. M. Chevalier's Tableau de la Plaine de Troye illuftrated and confirmed from the Obfervations of fubfequent Travellers and others. By Andrew Dalzel, M.A. F.R.S. Edin. Profeffor of Greek in the Univerfity of Edinburgh.

* Now Joint Profeffor of Chemiftry in the Univerfity of Edinburgh.
† By miftake, No. 5. is repeated.

ig. 1.

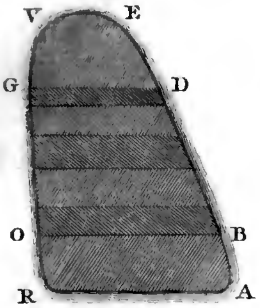


Fig. 2

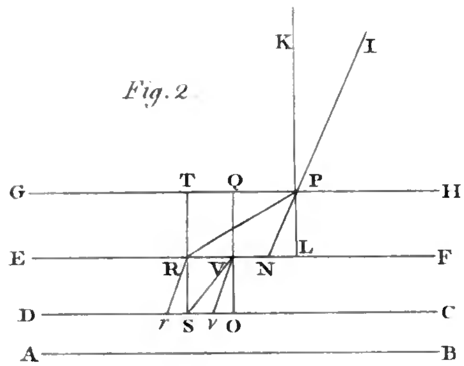


Fig. 3.

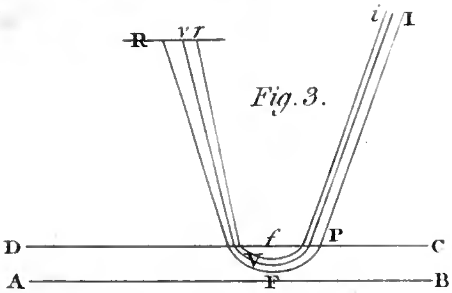
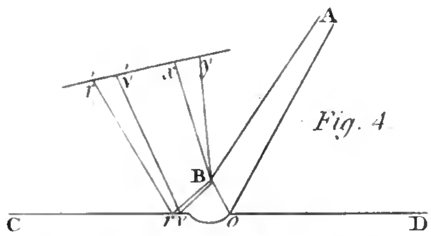


Fig. 4



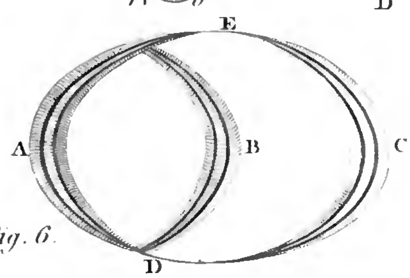


Fig. 6.

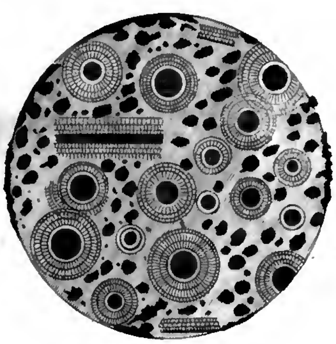


Fig. 5.

Fig. 7.

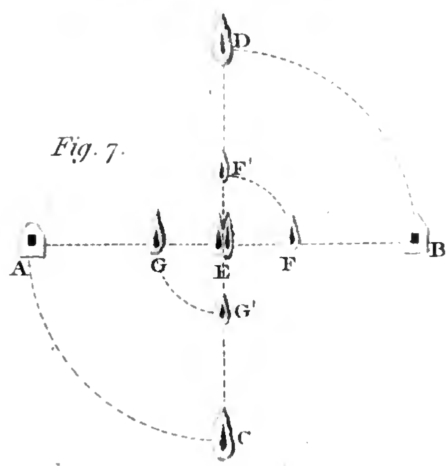
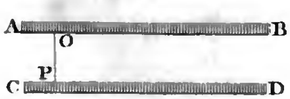


Fig. 8.

Fig. 9.



Barlow sculp.

Fig. 1.

Fig. 2.

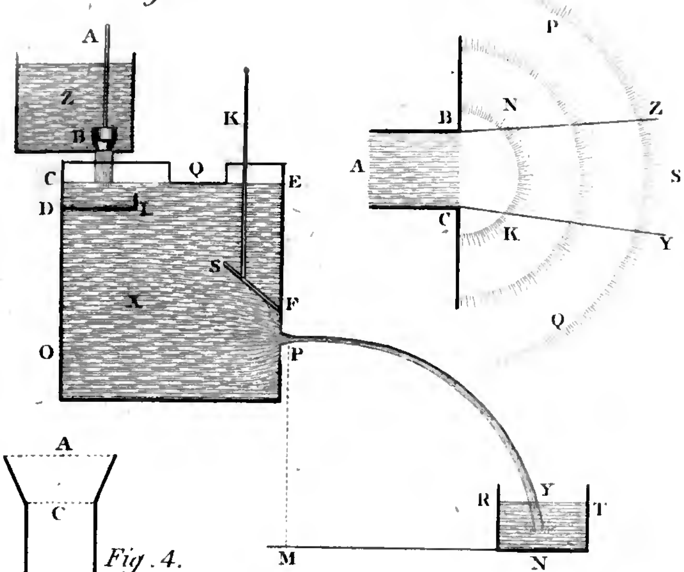


Fig. 3.

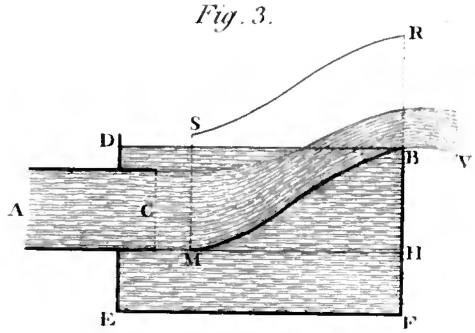


Fig. 4.

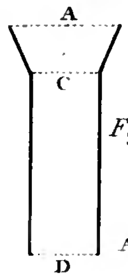


Fig. 5.

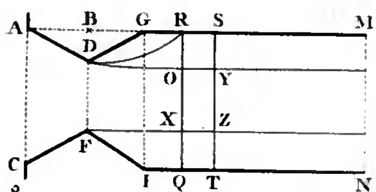


Fig. 6.

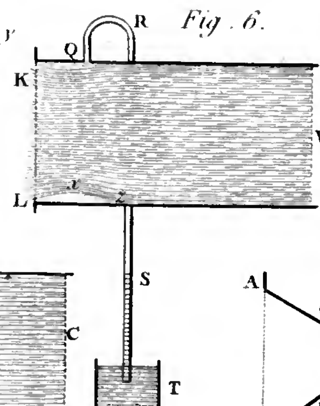


Fig. 7.

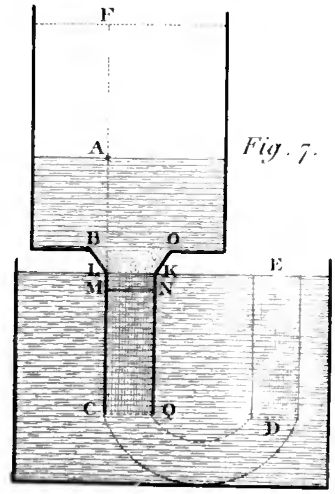


Fig. 8.

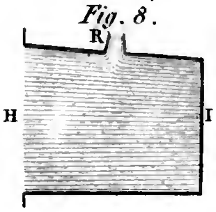


Fig. 9.

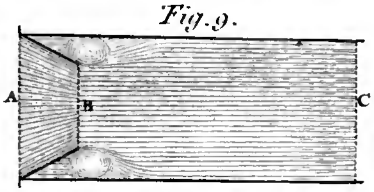
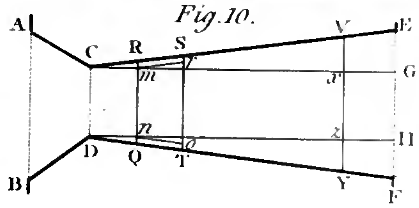


Fig. 10.



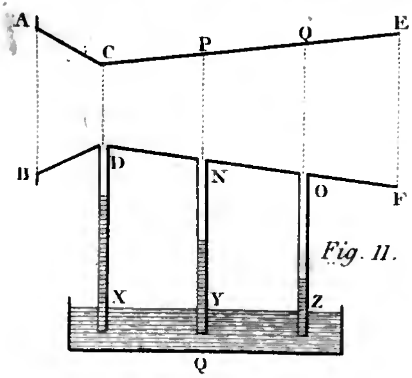


Fig. 11.

Fig. 12.

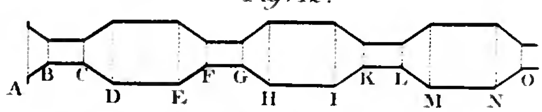
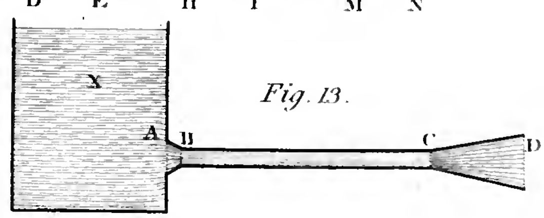


Fig. 13.



Barlow sculp.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

AUGUST 1798.

ARTICLE I.

Farther Experiments and Obſervations on the Affections and Properties of Light. By HENRY BROUGHAM, *jun. Eſq.*

[Concluded from Page 155 of the preſent Volume.]

Obſervation 5.

THE direct light falling on the ſpeculum, and part of the reflected light on the horizontal white ſtage of a very accurate micrometer, I meaſured the breadth of the fringes, ſpots, &c. Theſe, with the diſtance of the ſpeculum from the window and micrometer, and the ſize of the ſun's image, are ſet down in the following table, all reduced to inches and decimals.

	Inches. Parts.
Diſtance of the ſpeculum from the hole in the window ſhut - -	24.
Diſtance of the ſpeculum from the ſtage of the micrometer - -	18.
Tranſverſe axis of the ſun's image - - - -	2.6
Conjugate axis of the ſun's image - - - -	1.4
Length of the oblong dark ſpot - - - -	.4
Breadth of the oblong dark ſpot - - - -	.0074
Breadth of its firſt fringe - - - -	.0022
Elliptic ſpot's tranſverſe axis - - -	.0016
——— conjugate axis - - - -	.0068
Breadth of its firſt fringe - - -	.0034
Tranſverſe axis of a larger elliptic ſpot - - -	.013
Conjugate axis of the ſame ſpot - - -	.0076

In the image where these meafures were taken, there were feven other elliptic fpots, a little lefs and nearly equal; all the others were much fmaller and more confufed.

Obfervation 6. On viewing the furface of the fpeculum attentively in that place whence the rays formed the oblong and firft mentioned elliptic fpots, I faw a dark but very thin long fcratch, and a dark dent, fimilar in fhape to the dark fpaces on the image; the dark fpot meafured lefs than $\frac{1}{250}$ of an inch; which makes its whole furface to the whole polifhed furface, as 1 to 34225, fuppofing the former circular or nearly fo. All thefe meafures will be found to agree very well, for their fmallnefs and delicacy: thus, the ratio laft mentioned is nearly the fame which we obtain by comparing the image and the fpot: the like may be faid of the two fpots mentioned in the table, i. e. their axes are proportional. I now could produce what fpots I pleafed, by gently fcratching the fpeculum, or by making lines, dots, &c. with ink, and allowing it to dry; for thefe laft formed convex fibres, which produced coloured fringes as well as the concavities, agreeably to what was deduced *a priori.*

Obfervation 7. The whole appearance which I have been defcribing bore fuch a clofe and complete refemblance to the fringes made round the fhadows of bodies, that the identity of the caufe in both cafes could not be doubted. In order, however, to fhew it ftill further, I meafured the breadths of two contiguous fringes in feveral different fets: the meafurements agreed very well, and gave the breadth of the firft fringe .0056, and of the fecond .0034; or of the firft .0066, and of the fecond .0034. The ratio of the breadths by the firft is 28 to 17; by the fecond, 30 to 17; of which the medium is 29 to 17: and this is precifely the ratio of the two innermoft fringes made by a hair, according to Sir Ifaac Newton's meafurement; the firft being, according to him, $\frac{1}{170}$ of an inch; the fecond, $\frac{1}{290}$ of an inch*. Farther, the two innermoft rings made by plates have their diameters (not breadths) in the ratio of $1\frac{11}{16}$ to $2\frac{3}{8}$†, and the diftance between the middle of the innermoft fringes (made by a hair), on either fide the fhadow, is to the fame diftance in the fecond fringes as $\frac{1}{38}$ to $\frac{2}{47}$: therefore, the diameters of the two firft rings made by the fpecks in the fpeculum, are as $\frac{209}{363}$ to $\frac{627}{1363}$; which ratio differs exceedingly little from that of $1\frac{11}{16}$ to $2\frac{3}{8}$, the ratio of the diameters of rings made by plates, either thofe called by Newton thick, or thofe which he names thin: for fuppofe this difference nothing, $2\frac{3}{8} \times \frac{209}{363} = 1\frac{11}{16} \times \frac{627}{1363}$; and the difference between thefe two products (now ftated equal) is not much above $\frac{9}{9}$ in reality.

Obfervation 8. The laft thing worth mentioning in thefe phenomena was this: I viewed the fringes through a prifm, holding the refracting angle upwards, and the axis parallel to that of the dark fpace; then moving it till the objects ceafed defcending, I faw in that pofture the fringes much more diftinct and numerous; for I could now fee five with eafe, and feveral more lefs diftinctly. This led me to try more minutely the truth of the 5th propofition, with refpect to the number of the fringes furrounding the fhadows of bodies in direct light. Having produced a bright fet of thefe by a blackened pin $\frac{1}{25}$ of an inch in diameter, I viewed them through a well made prifm, whofe refracting angle was only 30°, and held this angle upwards, when the fringes were on the fide of the fhadow oppofite to me. I then moved the prifm round on its axis; and when it was in the pofture between the afcent and defcent of the objects, I was much pleafed to fee five fringes plainly, and a great number beyond, decreafing in fize and brightnefs till they became too fmall and confufed for

* Optics, book iii. obf. 3. † Book ii. parts 1 and 4.

 light,

fight. In like manner, thofe formed by a double flexion of two bodies, and thofe made out of homogeneal light, were feen to a much greater number when carefully viewed through the prifm. And this experiment I alfo tried with all the fpecies of fringes by flexion which I could think of.

Obfervation 9. The fame appearances which were occafioned by the metal fpeculum, might be naturally expected to appear when a glafs one was ufed. But I alfo found the like rings or fringes of colours and fpots in the image beyond the focus of a lens; nor was a very excellent one belonging to a Dollond's telefcope free from them. The rings with their dark intervals refembled thofe floating fpecks fo often obferved on the furface of the eye, and called "mufcæ volitantes," only that the mufcæ are tranfparent in the middle, becaufe formed by drops of humor: they will, however, be found to be compaffed by rings of faint colours, which will become exceedingly vivid if the eyes be fhut and flowly opened in the fun's light, fo that the humor may be collected; they alfo appear by reflexion, mixed with the colours defcribed in Phil. Tranf. for 1796, p. 268, (or Philof. Journal I. 593.)

Obfervation 10. The fun fhining ftrongly on the concave metal fpeculum, placed at fuch a diftance from the hole in the window that it was wholly covered with the light; upon inclining it a little, the image on the chart was bordered on the infide with three fringes fimilar to thofe already defcribed: on increafing the inclination thefe were diftended, becoming very bright and beautiful: when the inclination was great, and when it was ftill increafed, another fet of colours emerged from the fide next the fpeculum, and was concave to that fide. Here I ftopped the motion, and the image on both fides the focus had three fets of fringes, and four fringes in each fet; but when viewed through a prifm (as before defcribed), the numbers greatly increafed, both the fringes and the dark intervals decreafing regularly. The appearance to the naked eye is reprefented in fig. 6, plate VII. where A D C being the image, A and C are the fets of fringes at the edges, and B the third fet, there being none at E and D the fides, fince the light which illuminates thefe quarters comes not from the edges of the fpeculum in fo great inclinations. I now viewed the furface of the fpeculum, and faw it in the place anfwering to B in the image, covered with fringes exactly correfponding with thofe at B; and on changing the figure of that part of the fpeculum's edge between them and the fun, the fringes likewife had their figure altered in the very fame way. On moving the fpeculum farther round, B came nearer to A in the image, according as the fringes on the fpeculum receded from that fide which formed them; and before they vanifhed alike from the fpeculum and image, they mixed with the colours at A in the image, and formed in their motion a variety of new and beautiful compound colours: among thefe I particularly remarked a brown chocolate colour, and various other fhades and tinges of brown and purple. Juft before the fringes at B appeared, the fpace between A and C was filled with colours by reflexion, totally different in appearance from the fringes; but I could not examine them fo minutely as I wifhed in this broad image. I therefore made the following experiment:

Obfervation 11. At the hole in the window-fhut I held the fpeculum, and moved it to fuch an inclination that the colours by reflexion might be formed in the image: they were much brighter and far more diftended than the fringes, and were in every refpect like the images by reflexion in the common way, only that the colours were a little better and more regular. They were alfo feen on the fpeculum, as the third fet of fringes had before

 been

been in Obſ. 10; but by letting the rays fall on the half next the chart, and inclining that half very much, I could produce them, though leſs diſtinctly, by a ſingle reflexion. I now held a plain metal ſpeculum, ſo that the rays might be reflected to form a white image on a chart. On inclining the ſpeculum much, I ſaw the image turn red at the edge; it then became a little diſtended; and laſtly, fringes emerged from it well coloured, and in regular order, with their dark intervals. This may eaſily be tried by candle-light with a piece of looking-glaſs; and thoſe who without much trouble would ſatisfy themſelves of the truth of the whole experiment contained in this and the laſt obſervation, may eaſily do it in this way with a concave ſpeculum; but the beauty of the appearance is hereby quite impaired. After this detail it is almoſt ſuperfluous to add, that the fringes at B, fig. 6, are formed by deflexion from the edge of the ſpeculum next the ſun, and then falling on it are reflected to the chart; that the images by reflexion are either formed by the light being decompounded at its firſt reflexion, and then undergoing a ſecond, or, in other inſtances, without this ſecond reflexion; and that the other fringes are produced exactly as deſcribed above, from the neceſſary conſequences of the theory. I ſhall only add, that nothing could have been more pleaſing to me than the ſucceſs of this experiment; not only becauſe in itſelf it was really beautiful from its variety, but alſo becauſe it was the moſt peremptory confirmation of what followed from the theory *a priori*, and in that point where the ſingularity of its conſequences moſt inclined me to doubt its truth.

Let us now attend to ſeveral concluſions to which the foregoing obſervations lead, independently of the propoſitions (viz. the five firſt) which they were made to examine.

I. We muſt be immediately ſtruck with the extreme reſemblance between the rings ſurrounding the black ſpots on the image made by an ill-poliſhed ſpeculum, and thoſe produced by thin plates obſerved by Newton; but perhaps the reſemblance is ſtill more conſpicuous in the colours ſurrounding the image made by any ſpeculum whatever, and fully deſcribed in Obſ. 10 and 11. The only difference in the circumſtances is now to be reconciled. The rings ſurrounding the black ſpot on the top of a bubble of water, and thoſe alſo ſurrounding the ſpot between two object-glaſſes*, have dark intervals (exactly like thoſe rings I have juſt now deſcribed, and the fringes ſurrounding the ſhadows of bodies); but theſe intervals tranſmit other fringes of the ſame nature, though with colours in the reverſe order; from which Sir Iſaac Newton juſtly inferred, that at one thickneſs of a plate the rays were tranſmitted in rings, and at another reflected in like rings. Now it is evident, that neither reflexibility nor refrangibility will account for either ſort of rings; becauſe the plate is far too thin for ſeparating the rays by the latter, and becauſe the colours are in the wrong order for the former; and alſo becauſe the whole appearance is totally unlike any that refrangibility and reflexibility ever produce. To ſay that they are formed by the thickneſs of the plates, is not explaining the thing at all. It is demanded, in what way? And indeed we ſee the like dark intervals and the ſame fringes formed at a diſtance from bodies by flexion, where there is no plate through which the rays paſs. The ſtate of the caſe then ſeems to be this: "When "a phenomenon is produced in a particular combination of circumſtances, and the ſame "phenomenon is alſo produced in another combination, where ſome of the circumſtances "before preſent are wanting; we are entitled to conclude, that the latter is the moſt gene-

* Optics, book ii. p. 1.

"ral

"ral cafe, and muft try to refolve the other into it." In the firft place, the order of the colours in the Newtonian rings is juft fuch as flexion would produce; that is, thofe which are tranfmitted have the red innermoft, thofe which are reflected have the red outermoft: the former are the colours arranged as they would be by inflexion, the latter as they would be by deflexion; and here by outermoft and innermoft muft be underftood relative pofition only, or pofition with refpect to the thicknefs of the plate, not of the central fpot. Secondly, the thinneft plate makes the broadeft ring (the diameter of the rings being in the inverfe fubduplicate ratio of the plate's thicknefs): juft fo is it with fringes by flexion; nearer the body of the fringes are broadeft, and their diameters increafe in the fame ratio with the diameters of the rings by plates whofe thicknefs is uniform; each diftance from the bending body therefore correfponds with a ring or fringe of a particular breadth, and the alternate diftances correfpond with the dark intervals. The queftion then is, what becomes of the light which falls on or paffes at thefe alternate diftances? In the cafe of thin plates, this light is tranfmitted in other rings: we fhould therefore be led to think, that in the cafe of the light paffing by bodies, it fhould be at one diftance inflected, and at another deflected: and in fact the phenomena agree with this; for fringes are formed by inflexion within the fhadows of bodies; they are feparated by dark intervals; the fringes and the intervals without the fhadow decreafe in breadth according to the fame law, fo that the fringes and intervals within the fhadow correfpond with the intervals and fringes without refpectively. Nor will this explanation at all affect the theory formerly laid down; it will only (if found confiftent with farther induction) change the definite fpheres of inflexion and deflexion into alternate fpheres. At any rate, the facts here being the fame with thofe defcribed by Newton, but in different circumftances, teach us to reconcile the difference, which we have attempted to do, as far as is confiftent with ftrictnefs; and what we have feen not only entitles us to conclude that the caufe is the fame, but alfo inclines us to look for farther light concerning that caufe's general operation: and I truft fome experiments which I have planned, with an inftrument contrived for the purpofe of inveftigating the ratio of the bending power to the diftances at which it acts, will finally fettle this point.

II. Another conclufion follows from the experiments now related, viz. that we fee the great importance of having fpecula for reflectors delicately polifhed, not only becaufe the more dark imperfections there are on the furface the more light is loft, and the more colours are produced by flexion (thefe colours would be moftly mixed, and form white in the focus), but alfo becaufe the fmalleft fcratches or hairs being polifhed produce colours by reflexion, and thefe diverging irregularly from the point of incidence are never collected into a focus, but tend to confufe the image. Indeed it is wonderful that reflectors do not fuffer more from this caufe, confidering the almoft impoffibility of avoiding the hairs we fpeak of: however, that they do actually fuffer is proved by experience. I have tried feveral fpecula from reflecting telefcopes, and found that though they performed very well from having a good figure, yet from the focus (when they were held in the fun's light) feveral ftreaks diverged, and were never corrected; others had the hairs fo fmall that it was very difficult to perceive the colours produced by them unlefs they fell on the eye. Glafs concaves were freer from thefe hairs, but they were much more hurt by dark fpots, &c. In general the hairs are fo fmall in well wrought metals that they do little hurt; but when enlarged by any length of expofure to the light and heat in folar obfervations, they produce irregularities

round

round the image. Such at leaft I take to be the explanation of the phenomenon obferved at Paris by M. de Barros during the tranfit of mercury in 1743, and recorded in Phil. Tranf. for 1753. But there is another more ferious impediment to the performance of reflectors, and which it is to be feared we have no means of removing. In making the experiments of which the hiftory has been given, on viewing attentively the furface of the fpeculum, every part of it was feen covered with points of colours, formed by reflexion from the fmall fpecular particles of the body. I never faw a fpeculum free in the leaft from thefe; fo that the image formed in the focus muft be rendered much more dim and confufed by them than it otherwife would be.

III. The laft conclufion which may be drawn from thefe experiments is a very clear demonftration in confirmation of what was otherwife fhewn, concerning the difference between coloured images produced by reflexion and thofe made by flexion. This complete diverfity is moft evident in the experiments with fpecula, the colours produced by which, in the form of fringes and rings, ought, as well as the others defcribed as images by reflexion in Obf. 11, to be the fame in appearance with thofe formed by pins; whereas no two things can be more diffimilar.

It remains to examine the 6th propofition. For this purpofe I made the following obfervations:

Obfervation 1. Having procured a good fpecimen of Iceland cryftal, I fplit it into feveral pieces, and chofe one whofe furface was beft polifhed. I expofed this to a fmall cone of the fun's light, and received the reflected rays on a chart; nothing was obfervable in the image farther than what happens in reflexion from any other polifhed body. Some pieces indeed doubled and tripled the image, but only fuch as were rough on the furface, and confequently prefented feveral furfaces to the rays. When fmooth and well polifhed, a fingle image was all that they formed. The fame happened if I viewed a candle, the letters of a book, &c. by reflexion from the Iceland cryftal.

Obfervation 2. I ground a fmall piece of Iceland cryftal round at the edge, and gave it a tolerable polifh here and there by rubbing it on looking-glafs, and fometimes by a burnifher (it would have been next to impoffible to polifh it completely). I then placed the polifhed part in the rays near the hole in the window-fhut, and faw the chart illuminated with a great variety of colours by reflexion, irregularly fcattered as defcribed above*. I therefore held the edge in the fmoke of a candle and blackened it all over, then rubbed off a very little of the foot, and expofed it again in the rays. I now got a pretty good ftreak of images by reflexion, in no refpect differing from thofe made in the common way. Nor could I ever produce a double fet or a fingle fet of double images by any fpecimen properly prepared, either on a chart by the rays of the fun, or on my eye by thofe of a candle.

Obfervation 3. I ground to an even and pretty fharp edge two pieces of Iceland cryftal, and placed one in the fun's rays. At fome feet diftance I viewed the fringes with which its fhadow was furrounded, and faw the ufual number in the ufual order. I then applied the other edge fo near that their fpheres of flexion might interfere in the manner before defcribed†, and thus the fringes might be diftended: ftill no uncommon appearance took place, nor when other bodies were ufed with one edge of cryftal, nor when polifhed pieces

* Phil. Tranf. for 1796, p. 270, or Phil. Journal I. 594. † Ibid. p. 256, or Phil. Journal I. 587.

of

of different ſhapes and ſizes were employed. The ſame things happened by candle-light, and alſo by refracted homogeneal light. In ſhort, I repeated moſt of my experiments on flexion with Iceland cryſtal, and found that they were not changed at all in their reſults.

Obſervation 4. Having great reaſon to doubt the accuracy of an experiment tried by Mr. Martin, and in which, by a priſm of Iceland cryſtal, he thought ſix ſpectra were produced, I was not much ſurpriſed to find that a priſm made by poliſhing the two contiguous ſides of a parallelopiped of Iceland cryſtal produced only two equal and parallel images, in whatever poſition the priſm was held. But though, from the imperfect account which Martin gives of this appearance, it was impoſſible to diſcover his error from his own words, yet chance led me to find out what moſt probably had miſled him; for, looking at a candle through the oppoſite ſides of a ſpecimen of Iceland cryſtal, I ſaw four coloured images (beſides two white ones) of the candle. Theſe were parallel to one another, and in the ſame line as repreſented in fig. 7, plate VII. where E repreſents the two regular images, G and F two others coloured very irregularly, and changing colours as the cryſtal was moved horizontally, ſometimes appeared each two fold, and its two parts of the ſame or different colours. A and B were regularly coloured, and evidently formed by refraction, and reflected back from the ſides. On turning the cryſtal round ſo that its poſition might be at right angles to its former poſition, the images moved round, and were in a line perpendicular to A B, as C D. All this happened in like manner in the ſun's rays; and on viewing the ſpecimen I found it was ſplit and broken in the inſide, ſo as to be lamellated in directions parallel or nearly ſo to the ſides: on theſe plates there were colours in the day-time by the light of the clouds; and it is evident that it was theſe fractures which cauſed the irregular images G and F, for other ſpecimens ſhewed no ſuch appearance. I would therefore conclude, that Iceland cryſtal ſeparates the rays of light into two equal and ſimilar beams by refraction, and no more*.

As to the cauſe of the ſeparation, I would hope that ſome information may be obtained from the experiments I have related: for from them it appears, that this ſingular property extends no farther than to the action of the particles of Iceland cryſtal on the particles of light in their paſſage through the body; and from Obſ. 4, it is farther evident, that it is not owing to the different properties which Sir Iſaac Newton conjectures the different ſides of rays to have; for, if this were the cauſe, when the rays paſs between two pieces of cryſtal an uncommon flexion would take place. Laſtly, another fact (miſ-ſtated by Bartolin † and Romé de Liſle ‡) ſhews that the unuſual refraction takes place within the body, while the other, like all refractions, begins at ſome ſmall diſtance before the rays enter.

The writers juſt now quoted aſſert, that if the cryſtal be turned round ſo as to aſſume different poſitions, there is one in which the line appears ſingle. The fact is very different,

* Mentioning this account of Martin's miſtake to Profeſſor Robiſon of this univerſity, I was pleaſed to find a full confirmation of it. It was that excellent philoſopher who ſhewed the appearance to Martin; but he not underſtanding it, took the liberty of publiſhing the obſervation as his own, after firſt mangling it in ſuch a way as to give him indeed ſome pretext for the appropriation. The Profeſſor merely mentioned his having communicated it to Mr. Martin: how the latter uſed it we have ſhewn in the text: the theory of the appearance is ſomewhat more complex than appears by my obſervations. I was therefore pleaſed to find that the Profeſſor was in poſſeſſion of the true account of it, which is however foreign to the preſent purpoſe. B.

† Experimenta Cryſtalli abridged in Phil. Tranſ. vol. v.

‡ Criſtallographie, vol. i.

as follows: When the cryſtal is turned round, the unuſual image turns round alſo, and appears above the other: the greateſt diſtance between the two images is when they are parallel to the line biſecting one of the acute angles of the parallelogram through which the rays paſs: when the images are parallel to a line biſecting one of the obtuſe angles they ſeem to coincide; but they will be found, if obſerved more nearly, to coincide only in part. Thus (in fig. 9, plate VII.) A B and C D are the two black lines at their greater diſtance, and their extremities A and C, B and D, are even with one another; that is, the figure formed by joining A and C, B and D, is a rectangle. But in the other caſe (fig. 8), A B and C D being the lines, the ſpace C B (equal in depth of colour to the real line on the paper) is the only place in which the lines (or images) coincide. The ſpace A C of A B, and B D of C D are ſtill of a light colour, and the two lines A B and C D do not coincide, by the difference A C or B D; that is, by the difference O P, the greateſt diſtance (fig. 9). In ſhort, the unuſual line's extremities deſcribe circles (in the motion of the cryſtal) whoſe centres are the extremities of the uſual line, and whoſe radii are the greateſt diſtance. From this it appears evident, that the unuſual image is formed within the cryſtal, and turns round with the ſide of the particle or rhomboidal maſs of particles which forms it. Farther, it is evident that the power which produces the diviſion of the incident light is very different from common refraction, from the motion, and the effect taking place when the rays are perpendicular. Suſpecting, therefore, that it might be owing to flexion, I made the following experiment, which undeceived me:

Obſervation 5. I covered one ſide of a ſpecimen of Iceland cryſtal three inches deep with black paper, all but a ſmall ſpace $\frac{1}{20}$ of an inch in diameter, and placed a ſcreen with a hole of the ſame ſize, ſix feet from the hole in the window-ſhut of my darkened chamber, ſo that the rays might paſs through the ſcreen and fall on a priſm placed behind, to refract them into a ſmall and well defined ſpectrum, which was received on a chart two feet from the priſm. This ſpectrum I viewed through the cryſtal, and of courſe ſaw it doubled; but the two images were by no means parallel: the unuſual one inclined to the red, and its violet was conſiderably farther removed from the violet of the other, than the two reds were from one another; which ſhews that the moſt refrangible or leaſt flexible rays were fartheſt moved from their courſe by the unuſual action, and proves this to be very different from flexion*.

From all theſe obſervations this concluſion follows: that the remarkable phenomenon in queſtion ariſes from an action very different from either refraction or flexion, and whoſe nature well deſerves to be farther conſidered. It may poſſibly belong to the particles of Iceland cryſtal, and in a degree to thoſe of rock cryſtal, from the form and angles of the rhomboidal maſſes whereof theſe bodies are compoſed. Nor is this conjecture at all diſproved by the fact, that glaſs ſhaped like theſe bodies wants the property; for we cannot mould the particles of glaſs, we can only ſhape large maſſes of theſe; whereas we cannot doubt that in cryſtallization the ſmalleſt maſſes aſſume the ſame form with the largeſt. But then other hypotheſes may perhaps alſo account for the fact, ſuch as atmoſpheres, electric fluid, &c. &c.; ſo that till farther obſervations are made we ought to reſt contented with barely ſuggeſting the query. In the mean time, reſerving to a future opportunity ſome inquiries concerning the chemical

* When a candle or line is viewed through a deep ſpecimen, the unuſual image is tinged with colours.

properties

properties of light, and the nature of the forces which bodies exert on it internally, I conclude at present with a short summary of propositions. But, first, may I be permitted to express a hope, that what has been already attempted (and for which no praise can be claimed farther than what is due to attentive observation, according to the rules of the immortal Bacon) may prove acceptable to such as love to admire the beautiful regularity of nature, or more particularly to trace her operations, as exhibited in one of the most pleasing, most important, and most unerring walks of physical science.

Proposition I. The sun's light consists of parts which differ in degree of refrangity, reflexity, inflexity, and deflexity; and the rays which are most flexible have also the greatest refrangity, reflexity, and flexity—or are most refrangile, reflexile, and flexile.

Proposition II. Rays of compound light passing through the spheres of flexion, and falling on the bending body, are not separated by their flexibility, either in their approach to, or return from, the body.

Proposition III. The colours of thin and those of thick plates are precisely of the same nature, differing only in the thickness of the plate which forms them.

Proposition IV. The colours of plates are caused by flexion, and may be produced without any transmission whatever.

Proposition V. All the consequences deducible from the theory *à priori* are found to follow in fact.

Proposition VI. The common fringes by flexion (called hitherto the "three fringes") are found to be as numerous as the others.

Proposition VII. The unusual image by Iceland crystal is caused by some power inherent in its particles, different from refraction, reflexion, and flexion.

Proposition VIII. This power resembles refraction in its degree of action on different rays; but it resembles flexion within the body, in not taking place at a distance from it, in acting as well on perpendicular as on oblique rays, and in its sphere or space of exertion moving with the particles which it attends.

II.

Observations on Bituminous Substances, with a Description of the Varieties of the Elastic Bitumen. By CHARLES HATCHETT, *Esq. F.R.S. Lond. and Edin. F.L.S. &c.**

SECT. I.

IT is now generally believed that the bituminous substances are not of mineral origin, but that they have been formed from certain principles of substances belonging to the organized kingdoms of Nature, which, after the loss of animal and vegetable life, have suffered considerable changes by long contact and union with mineral bodies.

These changes have been however so considerable, that the bitumens can no longer be referred to their first origin, and they are therefore regarded by general consent as forming part of the present mineral system.

* Read before the Linnæan Society in June and July 1797.

The bituminous ſubſtances are :

Naptha,	Jet,
Petroleum,	Pit Coal,
Mineral Tar,	Bituminous Wood,
Mineral Pitch,	Turf,
Aſphaltum,	Peat, and

thoſe combinations of the oxides of certain metals with bitumen called Bituminous Ores*.

Thoſe who are acquainted with the nature of theſe ſubſtances will immediately perceive, that they may be formed into two diviſions : the firſt of which conſiſts of ſimple ſpecies, or unadulterated bitumens : and the ſecond is compoſed of bitumen mixed or combined with the earths, vegetable matter, and metallic oxides ; ſo that theſe appear to merit the name of compound ſpecies.

I ſhall now firſt conſider how the ſimple ſpecies are connected with each other.

SECT. II.

IT has been the opinion of ſome eminent Naturaliſts and Chemiſts, that naptha is an ethereal oil produced from the more compact and ſolid bitumens by a ſort of natural diſtillation. This however appears to be an hypotheſis founded upon analogy, and ſupported only by a few local facts, which may often be queſtioned. But many facts and obſervations concur to prove that the contrary moſt frequently happens, and that the compact bitumens are often, if not always, formed from naptha and petroleum by inſpiſſation. I will not however now inſiſt upon the proofs of this, as the varieties of the elaſtic bitumen, which I ſhall ſoon deſcribe, will be ſufficient for the purpoſe †.

NAPTHA.

NAPTHA is a ſubſtance well known to Mineralogiſts, as a light, thin, often colourleſs oil, highly odoriferous and inflammable, which is ſometimes found on the ſurface of the waters of ſprings, and at other times iſſuing from certain ſtrata.

When expoſed to the air, it becomes at firſt yellow, afterwards brown, and in the like proportion it thickens, and paſſes into

PETROL OR PETROLEUM.

THIS has a greaſy feel, is thicker than the preceding ſubſtance, is tranſparent or ſemi-tranſparent, and of a reddiſh or blackiſh brown colour. By air it becomes like tar, and then is called

* As I intend only here to notice the modifications of naptha and petroleum, I have not mentioned amber and the honey-ſtone.

† Bergman was of opinion, that the liquid bitumens were often, if not always, formed from thoſe which are ſolid, by the means of ſubterraneous heat; and expreſſes himſelf thus: " Cæterùm ad fidem pronum eſt, naptham, petroleum, bituminoſoſque liquores, quibus abundat Aſia, plures harum materierum exhibens non tantùm ſcaturigines, ſed rivulos quoque, quibus etiam, parcius licet diſtributis, Auſtralis Europa non caret : probabile, inquam, eſt, has pinguedines liquidas variis antea terris inhæſiſſe exſiccatas, et mediante calore ſubterraneo, ſi non ſemper, ſæpe tamen fluiditatem recuperâſſe. Novimus ignem in alto haud rarò agere, quamvis in ſuperficie vix obſcura ejuſdem indicia inveſtigare liceat: novimus præterea e ſicco aluminari ſchiſto petroleum extorqueri juſto caloris gradu, cui arte exponitur.—Bergman de Productis Vulcaniis Opuſcula, tom. iii. p. 238.

MOUN-

MOUNTAIN OR MINERAL TAR, BITUMEN PETROLEUM TARDE FLUENS.

THIS substance is viscid, and of a reddish or blackish brown or black. When burned, it emits a disagreeable bituminous smell, and by exposure to the air it passes into

MOUNTAIN OR MINERAL PITCH—BITUMEN MALTHA.

THE mineral pitch much resembles common pitch, and, when heated, emits a strong unpleasant odour, like the former substance. When the weather is cold, it may be broken, and then exhibits, internally, a glassy lustre; but when warm, it is softened, and possesses some tenacity. It is however susceptible of a superior degree of induration, and then becomes

ASPHALTUM—BITUMEN ASPHALTUM—PETROLEUM INDURATUM.

THIS is a light, brittle substance, of a brownish black, or black. When broken, it shews a conchoidal fracture with a glassy lustre. It has little of the bituminous odour, unless it is rubbed or heated. It easily melts, is very inflammable, and, when pure, burns without leaving any ashes.

In this manner, naptha, by inspissation, passes successively through different states until it becomes asphaltum, which appears to be the ultimate degree of induration which the pure bitumens derived from naptha can receive.

I have at this time specimens before me which prove these gradations; and I have seen a remarkable instance in a bitumen brought from the Island of Trinidad, which exhibits mineral tar passing into mineral pitch, and lastly into asphaltum*.

SECT. III.

THE division which comprehends the simple bituminous substances derived from naptha, may therefore be considered as terminating in asphaltum; but nature appears to have glided on by an uninterrupted chain which connects the simple bitumens with those which we have called compound; and this effect is produced by the gradual increase of the carbonic principle, and the introduction of extraneous matter, the different quantity of which, together with the greater or less degree of mixture or of chemical union, occasion considerable changes in these substances, so that they are gradually removed from those characters which distinguish the pure bitumens.

To form an accurate table of these gradations, it would be necessary to have comparative analyses of the different bituminous substances, and also to contrast the analyses with the properties of these bodies. But at present these analyses, for the greater part, are wanting; and although at some future time I intend to attempt a series of such experiments, I must now content myself with the observations and facts which I have been able to collect†.

* The progressive changes of naptha into petroleum, mineral tar, mineral pitch, and asphaltum, appear to be caused by the gradual dissipation of part of the hydrogen of the bitumen, and the consequent development or disengagement of carbon. Hence, I am inclined to believe, arise the changes of colour, the degrees of inspissation, and the increased proportion of carbon found in those substances by chemical analysis. I would be understood however to mean that the carbon is only relatively increased, in respect to the other ingredients, in a given quantity of these bitumens, and that it predominates in proportion to the dissipation of a certain portion of the hydrogen, which was originally necessary to the forming of the bitumen in conjunction with the carbon.

† This paper was written and read before I had seen the ingenious experiments which the celebrated Mr. Kirwan has published, in the last edition of his Elements of Mineralogy.—Vide vol. ii. p. 514, (or Philos. Journ. I. 487.)

From thefe I am of opinion, that the moft immediate gradation from afphaltum (which is the laft of the fimple bitumens) into thofe which are compound, takes place in the fubftance called

JET.

Jet is a fubftance well known to be of a full black, fometimes however inclining to brown. It is confiderably harder and lefs brittle than afphaltum. It breaks with a conchoidal fracture, and the internal luftre is glaffy. It has no odour except when heated, and it then refembles afphaltum. It melts in a ftrong heat, and, when burned, leaves an earthy refiduum.

Wallerius confidered jet as afphaltum which had become indurated by time, and Mr. Fourcroy is of the fame opinion *. Others again have arranged it with the varieties of coal †. I am inclined however to believe, that it is neither afphaltum nor coal, but an intermediate fubftance which may be regarded as the firft gradation from the fimple bitumens into thofe which are compound. The matter of afphaltum undoubtedly enters into it in a large proportion, and has confequently ftamped feveral of its characters upon it; but the increafe of carbon, and of the extraneous or earthy matter which is intimately mixed or rather combined with it, has had fo much influence, that the characters of coal are alfo in fome meafure apparent, and are rendered the more ftriking by the fimilarity of certain local circumftances which attend thefe two fubftances. The characters of coal are however by no means fully eftablifhed in jet; but from this we pafs immediately to another, in which thefe characters cannot be queftioned.

This is the fubftance called

CANNEL COAL,

which is of a full black, of a fmooth, folid, even texture; it breaks in any direction, and the tranfverfe fracture is conchoidal. It burns well, and is fo compact that it is often employed, like jet, to be formed into trinkets.

The great refemblance which cannel coal has to jet in many of its properties, induces me to regard it as the next gradation of the compound bituminous fubftances, and as the leading variety of coal from which the others follow according to the degree of their bituminous character.

The limits of this paper will not allow me to enter into a circumftantial account of all the other varieties of pit-coal; neither is it neceffary, after the gradations of afphaltum to jet, and of jet to coal, have been noticed. I fhall not therefore defcribe the varieties of coal known by divers names in different countries, and even in different provinces, fuch as thofe called in England caking coal, rock coal, fplent coal, &c. &c.; but fhall only obferve, that the pit coals in general appear to be compofed of bitumen intimately mixed, or rather combined, with various proportions of carbon and earthy matter; and according to the intimacy of the union, and the excefs of one or other of the ingredients, fo the compound poffeffes more or lefs the characters of perfect coal, or, by various fhades, paffes into certain earthy or ftony fubftances, which, although impregnated with bitumen, do

* Elémens d'Hift. Nat. et de Chimie, tom. iii. p. 456.

† Widenmann's Handbuch der Mineralogie, p. 628.

not

not merit the appellation of coal, and theſe alſo at length gradually loſe the bituminous character *.

It is likewiſe worthy of notice, that the quantity of earthy matter does not appear to be the principal cauſe why pit-coals do not burn with the rapidity which is to be perceived in ſome other earthy ſubſtances impregnated with bitumen. For we may conclude, that the ſlow combuſtion of coal proceeds from the joint effects produced partly by the relative proportions of the bituminous, carbonaceous, and earthy ingredients, and partly by the more or leſs perfect degree of mixture which connects them together, and which degree of mixture, I believe, in many caſes, nearly approaches to chemical union, if not actually ſo: when, therefore, the degree of mixture is ſo perfect as that every particle of bitumen is connected with much carbon or earthy matter, it is not ſurpriſing that the rapid combuſtible property of the former ſhould be checked in a conſiderable degree; and, by a parity of reaſoning, when the mixture is groſs and imperfect, ſo that it conſiſts of a ſtony or earthy ſubſtance, which has ſimply imbibed bitumen, it is natural to expect that the bitumen (although leſs abundant than in coal) ſhould enter readily into combuſtion, which is vehement in proportion to the ſhortneſs of its duration; and this we find to be the caſe in many earthy ſubſtances, and looſe ſand-ſtones which are ſimply impregnated with bitumen.—To return, however, to the varieties of coal, I muſt obſerve, that, from the cauſes above-mentioned, the different characters and properties of coal appear to me to be produced. That in this manner, perfect pit-coal paſſes into ſchiſtoſe or ſlaty coal; and this again, by certain gradations, paſſes into the varieties of combuſtible or bituminous ſchiſtus; which alſo, by the gradual decreaſe of the bituminous ingredient, become at length confounded with the varieties of the common or argillaceous ſchiſtus.

We have a remarkable example of this in the gradations of bituminous ſchiſtus into argillaceous ſchiſtus, which are to be obſerved at Kimmeridge, on the coaſt of Dorſetſhire, where a peculiar bituminous ſchiſtus is found, which is uſed as fuel by the inhabitants, and is improperly called Kimmeridge coal.

By the ſeries of gradations which have been noticed in the foregoing pages, the ſimple bituminous ſubſtances appear to paſs into thoſe which are compound; and theſe alſo, by declining ſhades, at laſt paſs into ſubſtances appertaining to the claſs of earths and ſtones.

In the compound bituminous ſubſtances the prevalent earthy ingredient is for the greater part generally, if not always, argillaceous; and although certain calcareous grits (ſuch as the Portland ſtone †) as well as limeſtones and marbles are found impregnated with bitumen, yet I know not of any inſtance in which this happens to the degree requiſite to form a combuſtible ſubſtance.

This curſory view of the ſimple bitumens, and of their combinations, would be ſufficient as an introduction to the principal ſubject of this paper; but, to complete the ſeries, I ſhall make ſome obſervations on the vegetable ſubſtances which contain bitumen, and ſhall afterwards mention the mixtures of bitumen with metallic oxides.

* From Mr. Kirwan's experiments it appears that carbon is a conſtituent principle of coal, and that the preſence of it is a principal cauſe of thoſe modifications which produce the ſpecies. It even ſeems chiefly to form the Kilkenny coal.—Kirwan's Elements of Mineralogy, vol. ii. p. 521.

† The Portland ſtone, when recently broken in the quarries, emits a ſtrong bituminous odour, like the bituminous limeſtone or ſtink-ſtone. It is alſo full of extraneous foſſils, or at leaſt the veſtiges of them.

SECT. IV.

SECT. IV.

WHEN we conſider the facts which apparently prove that vegetables have contributed principally to the formation of bitumen, we have every reaſon to expect that mixtures of vegetable matter with bitumen ſhould frequently occur. But by the mixture of bitumen with the parts of vegetables, we underſtand the remains and parts of vegetables mixed and connected with the bitumen which they themſelves have produced.

This ſeems to be the nature of the ſubſtance called

BITUMINOUS WOOD, as well as of TURF and PEAT.

BITUMINOUS or foſſil wood is found in many places; but in reſpect to that which is found at Bovey, near Exeter, and which is therefore called Bovey coal, there are ſome peculiarities which deſerve to be mentioned. The Bovey coal is a dark brown, light, brittle ſubſtance, which in texture and other external properties much reſembles wood which has been half charred. It is not found as ſcattered logs or trunks, but forms regular ſtrata.

The pits are on a heath which is flat and ſandy; the ſtratum of ſand is however but thin, after which a pale browniſh grey clay is found mixed with quartz pebbles. This prevails to about ſix feet, at which depth the firſt ſtratum of the coal commences. The quality of this is however much inferior to that of the ſubſequent ſtrata, which in all amount to ſeventeen, producing a depth of nearly ſeventy-four feet from the ſurface. Between each ſtratum of coal is a ſtratum of clay. The direction of the ſtrata is from eaſt to weſt, and the inclination or dip is from north to ſouth. The inferior ſtrata are thought to afford the beſt coal, and the coal is more ſolid and of a better quality towards the ſouth. The thickeſt ſtratum of coal is from ſix to eight feet *.

The Bovey coal burns readily with a flame like half-charred wood: it does not crackle, and, if but moderately burned, forms charcoal; or if completely burned, it leaves a ſmall quantity of white aſhes exactly ſimilar to thoſe of wood. The ſmell of it when burning alſo reſembles that of wood, with a faint diſagreeable odour. It is certainly very remarkable that this ſubſtance ſhould form regular ſtrata, although it poſſeſſes the texture and moſt of the properties of wood; and that theſe ſtrata do not exhibit any of thoſe irregularities on their ſurfaces, which might be expected, on the ſuppoſition that they were formed by the roots, trunks, and branches of trees long buried in the earth. It is alſo difficult to imagine wood to have been tranſported and depoſited in this place at ſeventeen different periods, and yet it muſt be allowed that theſe ſtrata have been formed by ſucceſſive operations. I muſt confeſs, that after having twice viſited and examined the ſpot expreſsly for the purpoſe, I ſtill find myſelf utterly unable to offer any opinion upon the ſubject.

The characters of bitumen are but little apparent in the Bovey coal, and the ſuperior ſtrata even appear to have loſt a portion of their combuſtible principle, while the inferior ſtrata poſſeſs it. The lower parts alſo of theſe ſtrata are more compact and more combuſtible than thoſe parts which are immediately upon them †.

Another

* In the winter, twelve men can raiſe about 120 tons of this coal in a week, the whole of which is employed in a neighbouring pottery.

† At about 100 yards to the weſt of the pits, is a bog of conſiderable extent, where peat is cut, and decayed roots and trunks of trees are found, which do not, however, in the leaſt approach to the nature of the Bovey coal.

Another remarkable ſort of foſſil wood, which much reſembles the Bovey coal, and in like manner is arranged among the bituminous woods, is that found in Iceland, which is called by the inhabitants Surturbrand. This is rather harder than the Bovey coal, but in every other reſpect is the ſame. It alſo forms ſtrata many feet in thickneſs; but it is very extraordinary that theſe ſtrata appear to be formed of trunks of trees, which, in their tranſverſe ſection, exhibit the concentric circles of their annual growth, with this difference, that the trunks have been ſo compreſſed as to be nearly flat, ſo that the circles appear like parallel lines connected at their extremities by a ſhort curve.

I did not obſerve ſuch an appearance at Bovey; but this would depend upon the poſition of the trunks of the trees, in reſpect to the ſection of the ſtrata.

Chaptal *, Troil †, Bergman ‡, and many others, have been of opinion that the ſurturbrand is wood which has been charred by the heat of the lava. But I cannot diſcern why it ſhould be ſuppoſed that it has been acted upon by fire, any more than that the Bovey coal has been ſubjected to the effects of the ſame agent. The qualities of the two ſubſtances are the ſame; and as (from Archbiſhop Troil's and Profeſſor Bergman's account) the ſurturbrand is ſtratified, I think we may venture to pronounce that the circumſtances under which they are found, are alſo ſimilar §. The whole, therefore, of the opinion in favour of fire, appears

coal. Whether this bog has been in any manner connected with the formation of the above-mentioned ſubſtance, I do not pretend to determine.

A yellowiſh brown compact ſubſtance, which in colour and fracture reſembles ferruginous clay, is alſo found occaſionally with the Bovey coal: it is brittle, and is highly inflammable; it melts like a bitumen, and emits a ſmoke which in ſmell reſembles amber. This ſubſtance is but rarely found.

* Elements of Chemiſtry, vol. iii. p. 199.

† Von Troil's Letters, p. 43.

‡ Quid de ligno foſſili Iſlandiæ ſentiendum ſit, gnaro in loco natali contemplatori decidendum relinquimus. Interea, ut cum Vulcani operationibus nexum credamus, plures ſuadent rationes, quamvis huc uſque modum ignoremus, quo ſitum texturamque adquiſiverunt hæc ſtrata. Scilicet truncis arborum perquam craſſis conſtant, qualis in Iſlandiâ nullibi reperiuntur, et ne quidem hoc tempore creſcere poſſe videntur. Hi ſitu horizontali in ſtratis multorum pedum craſſitiei congeſti ſunt et petroleo plus minus penetrati, non jam molli, ſed optimè indurato, a quo tam nigrorem, quam flammæ ſub deflagratione qualitatem mutuantur. Sed quod in primis attentionem meretur, eſt truncorum in lamellas planas compreſſio.

Ponamus truncum arboris cujuſdam tranſverſim ſectum, hinc, uti notum eſt, figura oritur in orbem rediens circiter circularis, quæ omnia monſtrat annotina incrementa, extimo propemodum parallela.. Fingamus jam talem ſectionem in tenuem laminam compreſſam, et veram habebimus ligni foſſilis, de quo heic agitur, ideam; nam in magnis hujus materiæ fruſtis, tranſverſim ſectis, quemlibet annotinorum orbium viſu perſequi licet, ita plerumque coactum, ut duas lineas fere parallelas exhibeat, quarum extrema brevi flexura ſunt adunata.—Quæ autem immanis requiritur vis, ut truncus cylindricus ita complanetur? Nonne antea particularum nexus putredinis quodam gradu fuerit relaxatus? Certe, niſi compages quodammodo mutatur, quodlibet pondus incumbens huic effectui erit impar. Cæterùm idem obſervatur phænomenon in omni ſchiſto argillaceo.

Orthoceratitæ, quæ in ſtrato calcareo conicam figuram perfectè ſervant, in ſchiſto planum fere triangulare compreſſione efficiunt. Idem valet de piſcibus, conchis, inſectiſque petrifactis. Cauſa adhuc latet, ſed in utroque caſu ſine dubio eadem eſt, et digna quæ exploretur. Obſervatu quoque dignum eſt, quod idem reperiatur effectus, quamvis ſtratum calcareum ſub ſchiſto collocatum ſit et majori ideo pondere comprimente onuſtum.——Bergman de Productis Vulcaniis Opuſcula, tom. iii. p. 239.

§ "It is found (the ſurturbrand) in many parts of Iceland, generally in the mountains, in horizontal beds; ſometimes more than one is to be met with, as in the mountain of Lack in Bardeſtrand, where four ſtrata of ſurturbrand are found alternately with different kinds of ſtone."—Troil's Letters, p. 42.

So

to reſt on the volcanic nature of Iceland; but it ſurely would be going too far were we to aſcribe to fire all the phenomena which are obſerved in volcanic countries.

Bovey coal, like the ſurturbrand, reſembles half-charred wood; and I will allow, and indeed am diſpoſed to believe, that it is in a ſtate nearly ſimilar; but from this it does not follow that fire has been the cauſe.

Carbon is known to be one of the grand principles of vegetables, and alſo as that which is the moſt fixed, excepting the ſmall portion of the earths contained in them. As a fixed principle, carbon appears to form, in great meaſure, the vegetable fibre; and after a certain degree of combuſtion, (by which the other principles have been diſſipated,) it remains, and the particles of it keep the ſame arrangement which they poſſeſſed when the vegetable was complete. If, however, the combuſtion has been carried on with the free acceſs of air, the carbon enters into combination with oxygen and caloric, and forms carbonic acid.

We have many examples in which carbon is formed or rather liberated from thoſe ſubſtances with which it was combined in vegetables; and theſe are now explained as effects ſimilar to thoſe of combuſtion, although fire has not been the cauſe. In both caſes the carbon has been freed from the more volatile principles; and under circumſtances not favourable to the union of carbon with oxygen, the former muſt neceſſarily remain more or leſs undiminiſhed.

During the combuſtion of vegetable matter, the more volatile principles contained in the vegetable fibre (which with carbon alſo form the reſinous and other ſimilar ſubſtances) appear to be firſt ſeparated; and in proportion to this ſeparation, the other more fixed ſubſtance, which we call carbon, is developed.

Thus, by the progreſs of combuſtion, wood becomes brown, and afterwards black; ſo that the ſtate of the wood ſhews the degree of combuſtion to which it has been ſubjected, or, in other words, how far the ſeparation of the other principles from carbon has been effected.

Combuſtion is therefore a ſpecies of analyſis by which the principles of vegetables are ſeparated, according to their affinities, and according to their degree of volatility. By this operation hydrogen and azote (if it be preſent in the vegetable) are firſt diſengaged and form new combinations, while the carbon is the laſt which is acted upon; ſo that unleſs a ſufficient quantity of oxygen be preſent, it remains fixed and unchanged.

But the ſame ſeparation of the vegetable principles happens whenever vegetables in the full poſſeſſion of their juices are expoſed to circumſtances which favour the putrid fermentation.—As in combuſtion, ſo by the progreſs of putrefaction does the vegetable loſe its colour, become brown, and afterwards black; at the ſame time a gas is diſcharged, which is compoſed of hydrogen, azote, and carbonic acid.

When combuſtion is long continued with the free acceſs of air, the whole of the carbon is diſſipated in the ſtate of carbonic acid; but in the proceſs of putrefaction a conſiderable portion of carbon commonly remains even long after the putrid fermentation has ceaſed. Although, therefore, it is as readily developed by putrefaction as by combuſtion, it is not, however, when liberated from the other principles, ſo ſpeedily diſſipated by the former as by the latter proceſs.

According to the degree of combuſtion within certain limits the carbon is more or leſs apparent, and the like prevails according to the degree of putrefaction; ſo that whenever

the

the caufes which have promoted this fpecies of fermentation have ceafed, the vegetable fubftance will remain with more or lefs of its firft principles, and with more or lefs vifible carbonic matter, according to the degree of putrefaction which has prevailed, and the vegetable fubftance will confequently have the appearance and properties of wood which has been charred more or lefs.

To this caufe, therefore, I am inclined to attribute the formation and appearance of the Bovey coal and furturbrand; and I believe that the portion of oily and bituminous matter, which I have obtained from them by diftillation, is nothing more than the remainder of the vegetable oils and juices which have been partly modified by mineral agents *.

[*To be continued.*]

III.

New Methods of affording, at an inconfiderable Expence, the Heat and the Water required for performing Experiments in Chemiftry. By Citizen GUYTON †.

THERE is but one fure road to arrive at truth in natural philofophy, namely, by confulting nature herfelf by experiments. Independent of the fagacity neceffary to direct thefe to objects precifely determinate, and to combine the means of operating, there is likewife an art of performing them, or, to fpeak more properly, of giving facility without diminifhing the certainty of their refults. To awaken the induftry of philofophers with regard to fuch refources as may be obtained for the multiplication of experiments at the leaft poffible coft, muft therefore be a labour of utility to the advancement of fcience. When Franklin was afked how he could afford the charges of his experiments on electricity, at a time when he was far from being in circumftances of independence, he replied, that a man who could not faw with a gimblet, and bore with a faw, was not fit for an experimental philofopher. The fervices which Bergman has rendered to chemiftry, and particularly to mineralogy, by the introduction of the blow-pipe, are well known. What a number of valuable obfervations would ftill be wanting, if he had not put this inftrument into the hands of thofe who were unable to procure, or have accefs to the furnaces of the elaboratory!

It is in confequence of thefe reflections, and the invitation I have received for that purpofe, that I have determined to defcribe the fmall manipulations by which I obtain a very confiderable faving of fuel and of diftilled water in chemical experiments, to which I may add the faving of time, that moft ineftimable of all the defiderata for experimental refearch.

* "Coal not only forms the refiduum of all vegetable fubftances that have undergone a flow and fmothered combuftion, that is, to which the free accefs of air has been prevented, but alfo of all putrid vegetable and animal bodies: hence it is found in vegetable and animal manures that have undergone putrefaction, and is the true bafis of their ameliorating powers: if the water that paffes through a putrefying dunghill be examined, it will be found of a brown colour; and if fubjected to evaporation, the principal part of the refiduum will be found to confift of coal. All foils fteeped in water communicate the fame colour to it in proportion to their fertility; and this water being evaporated, leaves alfo a coal, as Meffrs. Haffenfraz and Fourcroy atteft."—Kirwan on Manures, p. 154, vol. v. of the Tranfactions of the Royal Irifh Academy.

† Read before the National Inftitute of France, the 26th Brumaire, in the year 6, and inferted in the 24th volume of the Annales de Chimie, page 311.

In the second volume of the memoirs of the ancient academy of Dijon, I gave a description of a box containing a kind of portable laboratory, composed of a lamp with three wicks, disposed in the figure of an equilateral triangle, to form an internal current of air, with supports for the different vessels of digestion, distillation, evaporation, &c. I made a solution of silver with common aqua fortis and the metal in an alloyed state, which answered very well as a re-agent, without having occasion for any other utensils but this box and apothecary's phials, which are every where to be found. A number of these boxes have been fitted up by Citizen Dumoutier, more particularly for travellers, and I have reason to think that they have proved useful. But this apparatus must necessarily be confined in its application, and is different from the object I purpose to describe at present.

Ten years ago, I constructed a lamp, on the principles of Citizen Argand, with three concentric circular wicks, each having an interior and exterior current of air. The effect surpassed my expectations with regard to the intensity of the heat; but it was difficult to prevent the destruction of the hard solder round the wicks; and the glass retorts were frequently melted at the bottom, and disfigured. It may easily be imagined, that the quantity of oil consumed was considerable; and as it could not be used at the same time for giving light, it had, to say the truth, no more than a remote application to the object of the present communication.

A short time afterwards it occurred to me, to substitute, instead of the glass chimney of Argand's lamp, a cylinder of copper with an indented part or ledge a few millimetres * above the flame, to perform the office of the indented chimney of glass, and by that means to render it practicable to raise the wick to a certain height without smoking. This cylinder has three branches like a chaffing-dish. By this apparatus two or three decilitres of water (about half an English wine pint) may be brought to boil in a copper or glass vessel in about six or seven minutes. It has served, and I still use it, for a number of operations; but it was not till after I had observed the degree of heat obtained from the lamp in its ordinary state, and particularly since I have substituted instead of the metallic tube a chimney of glass cut off at the length of three centimetres (rather more than one English inch) above the contraction, that I perceived all the advantages it was capable of affording; and that by means of a moveable support for the reception of the different vessels, which may be fixed at pleasure by a thumb-screw, this lamp furnace, at the same time that it gives light, and consequently without any additional expence, may with facility be used for almost every one of the operations of chemistry; such as digestions, solutions, crystallizations, concentrations; the rectification of acids; distillations on the sand-bath, or by the naked fire; incinerations of the most refractory residues; analyses with the pneumatic apparatus, or of minerals by the saline fusion, &c. &c. I have not hitherto met with any exception but for complete vitrifications and cupellations; for even the distillations to dryness may be performed with some precautions, such as that of transferring the matter into a small retort blown by the enameller's lamp, and placing its bottom on a little sand-bath in a thin metallic dish.

The support, here mentioned, is simply a copper ring eight centimetres (3,15 inch) in diameter, which is raised or lowered by sliding on a stem of the same metal. It is described in the memoirs of the academy of Dijon as part of the portable laboratory; for which reason

* For the value of the new measures and weights of France, see Philos. Journal, I. 332.

it

it is unneceſſary to ſpeak more largely of it in this place*. Nothing more was required but to adapt it to the ſquare iron ſtem which paſſes through the reſervoir of the lamp. The connection is made by a piece of wood, in order that leſs of the heat might be diſperſed. As the lamp itſelf is capable of being moved on its ſtem, it is eaſy to bring it nearer or remove it at pleaſure from the veſſels, which remain fixed; a circumſtance which, independent of the elevation or depreſſion of the wick, affords the means of heating the retorts by degrees, of moderating or ſuppreſſing the fire inſtantly, or of maintaining it for ſeveral hours at a conſtant or determinate intenſity, from the almoſt inſenſible evaporation of cryſtallizable ſolutions to the ebullition of acids; properties never poſſeſſed by the athanor, of which chemiſts have boaſted ſo much. The advantage of theſe will be properly valued by thoſe operators, who know that the moſt experienced and the moſt attentive chemiſts meet with frequent accidents, by which both their veſſels and the products of their operations are loſt, for want of power in the management of the fire.

I muſt here enter into ſome detail, in order to eſtabliſh upon poſitive facts the poſſibility of applying the heat of a lamp to the operations I have enumerated, as well as to communicate the reſults of my experience to thoſe who in preference, or for want of more extenſive means, may be inclined to uſe this apparatus. I do not heſitate to ſay in preference; for, in the beſt appointed laboratory, the lamp will alſo be uſed in ſuch operations as may be made with equal facility on the ſame quantities, in much leſs time and more conveniently than by the fire of a furnace, by burning in the former inſtance one or two decimes (or pennyworths) of oil inſtead of five or ſix decimes of charcoal. The proof of this has been made in the laboratory of the Polytechnic School, at the concluſion of my laſt courſe.

For the analyſis of ſtones, ſuch as the cryſtals of tin, on which I operated before the claſs at the ſeſſion of the firſt Meſſidor laſt, I uſe the ſhortened chimney of glaſs. I begin by placing the mixture in a capſule of platina or ſilver of ſeven centimetres ($2\frac{3}{4}$ inches Engliſh) in diameter. I place this capſule on the ſupport, and regulate the heat in ſuch a manner that ebullition ſhall take place without throwing any portion of the matter out of the veſſel. As ſoon as its contents are perfectly dry, I transfer them into a very thin crucible of platina, of which the weight is not quite eleven grammes ($252\frac{1}{2}$ grains Engliſh), and its diameter forty-five millimetres ($1\frac{3}{4}$ inch Engliſh). This crucible reſts on a ſmall ſupport of iron-wire, which ſerves to contract the ring; and the wick being at its greateſt elevation, with the ring lowered to the diſtance of twenty-five millimetres ($9\frac{3}{4}$ inches Engliſh) from the upper rim of the glaſs chimney, I produce in leſs than twenty minutes the ſaline fuſion to ſuch a degree, that from the commencement of the operation the decompoſition proceeds as far as to 0,70 of the mineral †.

The ſame apparatus, that is to ſay, with the ſhortened chimney, ſerves for oxidations, incinerations, torrefactions, and diſtillations to dryneſs.

In ſuch operations as require a leſs heat, I leave the lamp with its large chimney abſolutely in the ſame ſtate as when it is uſed for illumination; and by raiſing and lowering either

* Several philoſophers who have ſeen this apparatus at work, having requeſted me to give a drawing, I have accordingly annexed a deſcription of the figures, which repreſent the whole together at the end of this memoir. I think it may be called the Economical Laboratory.

† See the Annales de Chimie, xxiv. 132; or our Journal, I. 545.

 the

the ring which ſupports the veſſel, or the body of the lamp if the veſſels be fixed in communication with others, I graduate the heat at pleaſure. Vinegar diſtils without interruption at ſix centimetres ($2\frac{1}{3}$ inches Engliſh) from the upper termination of the chimney, that is to ſay, at 19 centimetres ($7\frac{1}{2}$ inches Engliſh) from the flame. Water is made to boil in eight minutes, at the ſame height, in a glaſs veſſel containing five decilitres (one wine pint Engliſh), and is uniformly maintained at the diſtance of twenty-two centimetres ($8\frac{2}{3}$ inches) from the flame. It will ſoon be ſhewn, that I have another method of ſupplying chemiſts with diſtilled water; ſo that I ſcarcely ever repeat this operation but when I have no other ſupply at hand, or am deſirous of avoiding all trouble whatſoever. In this caſe, I obtain two or three decilitres (or quarter pints) of water in the courſe of a winter evening, without the leaſt portion of my time being employed in attending to the operation.

I muſt not in this place omit to mention a ſlight obſervation which this proceſs has afforded, becauſe it may lead to uſeful applications, and tends to point out one great advantage of this method of operating; namely, that an infinity of circumſtances may be perceived, which might not even be ſuſpected when the whole proceſs is carried on within a furnace. I have remarked, as did likewiſe ſeveral of my colleagues who were then preſent, that a column of bubbles conſtantly roſe from a fixed point of the retort on one ſide of the bottom. We were of opinion, that ſome particle of matter was in that place incorporated with the glaſs, which had a different capacity (probably conducting power) for heat from that of the reſt of the glaſs. In order to verify this conjecture, I endeavoured the following day to diſtil the ſame quantity of the ſame water in the ſame retort, after having introduced a button of cupelled ſilver, weighing nine decigrammes ($20\frac{1}{2}$ grains). At the commencement of the operation there was a ſmall ſtream of bubbles from the ſame point as before; but a ſhort time afterwards, and during the whole remaining time of operating, the largeſt and moſt inceſſant ſtream of bubbles roſe from the circumference of the button, which was often diſplaced by the motion; and in proportion to the time the product of the diſtillation was ſenſibly greater. Whence we may conclude, that metallic wires or rods, diſtributed through a maſs of water required to be kept in a ſtate of ebullition, and placed a little below its ſurface, would produce, without any greater expence of fuel, nearly the ſame effect as thoſe cylinders filled with ignited matter which are made to paſs through the boilers *.

It now remains to ſhew the economical method of ſupplying the water neceſſary for experiments.

When water is mentioned in chemiſtry, pure water is always meant. In medical preſcriptions it is uſual to preſcribe ſpring water, though in many places the water of ſprings or ſtreams is more loaded with ſelenite or ſulphate of lime than the well water of other places. The ſame remark may be applied to river water, which is no doubt more wholeſome than

* When we attempt to reaſon on this curious fact of the metallic button, there are various circumſtances which require to be conſidered. It does not ſeem probable, that a larger quantity of a fluid can be rendered elaſtic by a given quantity of heat, unleſs we ſuppoſe part of the heat to have been waſted in the former proceſs. Does the metal, by the excellence of its conducting power, convey a portion of the heat more readily into the maſs of liquid than might have been done by the ordinary proceſs of circulation? Or is the thin ſtratum beneath the button ſo far inſulated as to become more ſuddenly elaſtic, and, by riſing in that ſtate to the ſurface, to increaſe the rapidity of circulation, and the number of points at which vapour can eſcape? Would a tuft of ſilver wire produce the ſame effect? &c. &c. N.

the

the water of wells in places where selenite or plaister abounds, but which are nevertheless far from being pure, and are necessarily subject to vary according to the quantity of rain water which dilutes that which has remained upon the soluble matter.

Recourse is therefore had to distillation, to purify the water employed in laboratories; but if on the one hand we consider the labour and expence it requires, and on the other hand the quantity which it is necessary to have in readiness for the smallest operations; it will not appear surprising, when I assert, that there are few days in which a chemist does not avoid making some experiment, or obtain uncertain results, for want of having this article in his power. It is only with distilled water that perfect reagents can be prepared; distilled water is consumed in infusions, macerations, solutions, and edulcorations; repeated lotions demand a large quantity of this fluid; it ought to be used even for rinsing vessels; and in order to avoid deceitful conclusions, it is even proper to use it in the hydropneumatic vessels.

I have long been in the habit of supplying most of these demands with rain water; not with that which is directly received, though in fact it is of considerable purity, particularly in countries where there is no reason to fear that plaister should rise with the dust; but the quantity of this would be too small. I have, therefore, used water collected with care from the roofs of houses after the rain has washed the surface. I filter it without delay; and in this manner from time to time I obtain a considerable provision, without much labour and at no expence. But it is evident, that in order to depend on the purity of this water, it is necessary that there should be no gypseous matter in the composition of the mortar of the roof, or the plaistering of the chimneys. This condition does not obtain at Paris; insomuch that I have sometimes found the first water of the gutters more selenitous than that of the Seine in the time of floods. I have thought of a method of supplying this resource, by a process which may be used in all countries; and the success which has attended my trials renders it a duty to communicate it, in order to place the instruments of analysis in the hands of a greater number of operators.

Rain water collected from the roofs of houses which have been previously washed, cannot, and in fact does not, contain any thing but the very small portion of sulphate of lime which it has taken up during its contact with the plaister of the chimneys and the pointing of the ridges. It is necessary, therefore, to deprive it of this, in order to have water in a very pure state. For this purpose I prepare a solution of barytes according to the process * by which our colleague Vauquelin has rendered so easy what Bergman attempted with imperfect success. I pour this solution into the filtered rain water, until, after the precipitation has subsided, the last drop exhibits no alteration of transparency. I even add a small quantity in excess, which shews itself by the vinous colour it gives to paper coloured with fernambouc or brazil wood. This excess soon falls down in the state of carbonate of barytes, by simple exposure to the air. The precipitation may be very suddenly determined, by the addition of water impregnated with carbonic acid. Too much must not however be added, because it would take up a portion of the precipitate. But the spontaneous evaporation of the excess of gaseous acid in the open air would in this case soon restore its purity.

To judge with what facility and trifling expence the whole of the pure water for experi-

* Philosophical Journal, I. 535. Dr. Hope in the Edinburgh Transactions, iv. 36. informs us, that, contrary to the assertions of many chemists, the native carbonate of barytes may be deprived of its acid by mere heat (of a smith's forge, in a black lead crucible). N.

ments

ments may be procured, it is sufficient to obferve, that by ufing an aqueous folution of barytes of which the fpecific gravity was no more than 1,0205, the quantity of 15 grammes in weight (344½ grains Englifh), or 0,1473 cubic metres (4,1 cubic inches Englifh), proved fufficient completely to purify eleven decilitres (2⅓ wine pints Englifh) of water. Confequently one decilitre or part of the fame aqueous folution of barytes will ferve to afford 74,62 parts, or about 8 (French) pints of diftilled water.

I muft add an obfervation, which is very proper to fhew that the collection of rain-water is worth the trouble and care of procuring it in the circumftances moft favourable to its purity. By a comparative operation on rain-water, I found that river water acquired 60 grammes, or four times the quantity of the fame folution of barytes.

I have no doubt but that the aqueous folution of barytes for the purification of water will, in the courfe of time, be introduced into manufactories for dyeing. It will ferve, at a very moderate expence, to render the artift mafter of the fhades he means to produce, without waiting for the feafon in which he confiders his water as moft pure. I gave this advice to a manufacturer who requefted me to analyfe the water of a fmall ftream which fupplied his works.

Thefe obfervations may alfo probably be applied to another purpofe. It is known that water faturated with fulphate of lime is much lefs putrefcible than purer water: would it not be of advantage, in long voyages, to take a fupply of water exprefsly loaded with this earthy falt, and, when it is wanted for ufe, to purify it three or four days before hand by a fmall portion of the folution of barytes?

This folution would occupy little room, and be attended with inconfiderable expence. If it were apprehended that a fmall portion of barytes might remain in folution, which would not in fact be without danger, as this earth is perceptibly noxious, a proof might be made; or, to fpeak more correctly, the depuration might be rendered abfolute by the addition of a few drops of the folution of carbonate of foda. All thefe manipulations are among thofe which may be very eafily practifed by perfons of no chemical experience.

Explanation of the Figures of the Economical Laboratory, Plate IX.

Figure 1. reprefents the whole apparatus ready mounted for diftillation, with the tube of fafety and a pneumatic receiver.

A is the body or refervoir of the ufual lamp of Argand, with its fhade and glafs chimney. The lamp may be raifed or lowered at pleafure by means of the thumb fcrew B *, and the wick rifes and falls by the motion of the fmall toothed wheel placed over the wafte cup. This conftruction is moft convenient, becaufe it affords the facility of altering the pofition of the flame with regard to the veffels, which remain fixed; and the troublefome management of bended wires above the flame for the fupport of the veffels is avoided, at the fame time that the flame itfelf can be brought nearer to the matter on which it is intended to act.

D, a fupport confifting of a round ftem of brafs, formed of two pieces which fcrew together at about two-thirds of its height. Upon this the circular ring E, the arm F, and the nut G flide, and are fixable each by its refpective thumb-fcrew. The arm alfo carries a

* In the lamps of this conftruction made in London there is a fpring in the focket, fufficiently rigid to prevent the lamp A from falling by its weight when B is unfcrewed. I fuppofe Citizen Guyton's lamp was provided with the fame convenience, but that by overfight it may not have been mentioned. N.

moveable

moveable piece H, which ſerves to ſuſpend the veſſels in a convenient ſituation, or to ſecure their poſition. The whole ſupport is attached to the ſquare iron ſtem of the lamp by a piece of hard wood I, which may be fixed at any required ſituation by its ſcrew.

K repreſents a ſtand for the receivers. Its moveable tablet L is fixed at any required elevation by the wooden ſcrew M. The piece which forms the foot of this ſtand is fixed on the board N; but its relative poſition with regard to the lamp may be changed by ſliding the foot of the latter between the pieces O O.

P, another ſtand for the pneumatic trough. It is raiſed or lowered, and fixed to its place, by a ſtrong wooden ſcrew, Q.

R is the tube of ſafety, or reverſed ſyphon, invented by Citizen Welter, and deſcribed in the third cahier of the Journal of the Polytechnic School, p. 437 *.

Fig. 2. ſhews the lamp furnace diſpoſed to produce the ſaline fuſion; the chimney of glaſs ſhortened; the ſupport D turned down; the capſule of platina or ſilver, S, placed on the ring very near the flame.

Fig. 3. The ſame part of the apparatus, in which, inſtead of the capſule, a very thin and ſmall crucible of platina, T, is ſubſtituted, and reſts upon a triangle of iron wire placed on the ring.

Fig. 4. exhibits the plan of this laſt diſpoſition.

IV.

An Account of ſome Experiments made by Mr. JOHN CUTHBERTSON, with a View to determine an unequivocal Method of aſcertaining the Power of electrical Machines.

HOWEVER great the influence and probable importance of electricity may appear in a large claſs, and perhaps in the whole of natural phenomena, we ſtill find that a number of fundamental experiments remain to be made. Among theſe there is ſcarcely a more deſirable object than to determine the degree of excitation or quantity of electricity afforded by machines, in proportion to the ſurface expoſed to friction. When philoſophers endeavour to communicate to each other the indications of power in their reſpective electrical apparatus, they either deſcribe the length and appearance of the ſimple ſpark from a conductor, or the exploſion from a certain meaſure of coated ſurface, or elſe the diſtance to which the attractive power of the prime conductor is rendered perceptible upon a thread or pendulous body. The firſt of theſe methods is ſubject to variation from the magnitude of the conductor itſelf, the figure of its termination, and particularly that undulation of

* And alſo in the 2d vol. of the Annales de Chimie, p. 311. This apparatus ſerves, in a great meaſure, to prevent the bad effects of having the veſſels either perfectly cloſed or perfectly open. Suppoſe the upper bell-ſhaped veſſel to be nearly of the ſame magnitude as the bulb at the lower end of the tube, and that a quantity of water, or other ſuitable fluid, ſomewhat leſs than the contents of that veſſel, be poured into the apparatus: In this ſituation, if the elaſticity of the contents of the veſſels be leſs than that of the external air, the fluid will deſcend into the bulb, and atmoſpheric air will follow and paſs through the fluid into the veſſels: but, on the contrary, if the elaſticity of the contents be greater, the fluid will be either ſuſtained in the tube, or driven into the bell-ſhaped veſſel; and if the force be ſtrong enough, the gaſeous matter will paſs through the fluid, and in part eſcape. N.

which the first account was given in our Journal, vol. i. page 83: and the last method, being subject to modification, not only from the structure of the less essential parts of the machine, but also from the dimensions and figure of the apartment in which the experiments are made, has been accordingly very little used. Electricians have, therefore, with considerable reason, been disposed to avail themselves of the second method, according to the simple computation described in our work last referred to, page 87. But to this method also Mr. Cuthbertson, the constructor of the great Teylerian machine at Haerlem, offers serious objections; in consequence of which he took the trouble to repeat some experiments at my request. These experiments, together with his observations and such remarks as have occurred to myself, will form the subject of the present memoir.

The Honourable Henry Cavendish, Esq., from a series of experiments upon the charges of electrical jars *, has deduced, that the quantities of electricity which coated glass of different shapes and sizes will receive with the same degree of electrization, are directly as the area of the coating, and inversely as the thickness of the glass; and that, when the intensities vary, the quantities of electricity in like circumstances are nearly as the length of the spark. Mr. Cuthbertson's great experience has led him to modify these general conclusions. I have found that in great intensities the length of the spark is much more than in proportion to the charge †; and from some facts hereafter to be related, there is reason to think that a real charge of low intensity cannot be measured either by the length of its very short spark, or even by the number of turns of the machine. Electricians, in general, use the best glass they can procure in their vicinity; whence their conclusions are, for the most part, applicable to one kind of glass only. But Mr. Cuthbertson has observed, that the different kinds of white glass, and still more the green, will require very different quantities of electricity to charge equal surfaces and thicknesses to the same height. He showed me a jar, of which the coating had been cut away until its capacity, as determined by the number of turns of the machine, became equal to that of another similar jar of the same thickness. The coated surface of the former of these two jars might be estimated at more than one-third part less than that of the former. Hence the necessity of some other test of electrical power, different from that which includes the dimensions of the jar as one of its elements, is evident. Mr. C. offers the explosion of steel wire for this purpose; the result of the facts observed by him being, that equal quantities of electricity, in the form of a charge, will cause equal lengths of the same steel wire to explode, whether the jar made use of be of greater or less capacity, within certain limits very easily comprehended in a loose verbal description. The primary object of his experiments was directed to the establishment of this proposition.

July 6th, 1798, I waited on Mr. Cuthbertson at seven in the evening, and found two electrical machines in a state of activity. The first consisted of a glass-plate 24 inches in diameter, rubbed by two pair of cushions, each 5 inches long, proceeding from the extreme of the circumference towards the centre upon opposite radii, whence the whole surface rubbed in each turn was 1193,8 square inches, or 8,29 square feet. The other machine was of the same dimensions, but had two plates and four pair of cushions. A jar fitted up with Lane's electrometer, the balls of which were invariably fixed, was used to ascertain the steadiness of the excitation. When this jar was applied to the prime conductor of the

* Philosophical Transactions, lxvi. p. 196. † Phil. Trans. lxxvi.

single

single plate-machine, it exploded five times in $7\frac{1}{4}$ turns of the winch. Upon applying the same test to the double plate-machine it was found to afford very nearly, or rather more than twice the quantity of electricity. All the following experiments were made with the single plate.

A jar *, No. 1, was applied to the prime conductor: after four turns some ramified flashes struck into the uncoated part, and at the sixth turn a spontaneous explosion took place over the clear glass. It must be remarked, that the mouths of none of the jars had any stopper or covering. Mr. C. then took a glass tube, which he inserted into the jar nearly to the bottom, and breathed twice through it, in order, as he said, to render it more capable of retaining its charge. This process, so contrary to the received opinion of electricians, who carefully avoid all dampness, is considered by him as of the same nature as the experiment of Brook, who found that a jar, the naked part of which was soiled by handling, would retain six times the charge without exploding, which it would have held if perfectly clean †. I had always considered this last experiment as a proof that glass, when greased by handling, does not attract vapour from the air as readily as when clean; and accordingly I have often used tallow as a good extemporaneous varnish for glass pillars. Mr. Cuthbertson's process, however, had the same effect; for the jar did not afterwards exhibit any flash or disposition to explode during the whole evening, though it was occasionally charged with ten and twelve turns. A brass ball supported on a stand by a stick of glass was placed so as to receive the explosion from the jar No. 1, when thus connected with the prime conductor. From this ball hung a small piece of pendulum wire ‡, confined at each end between small forceps constructed for that purpose. The clear portion of the wire which formed part of the circuit was exactly five inches in length. An explosion was passed through a moderate interval or length of spark; and this interval was gradually increased till, at last, the shock became so strong as completely to ignite the wire and divide it near the positive end, where a sparkling globule or two flew out: ten turns of the machine were required to produce this effect, and the length of the explosive spark was $1\frac{1}{4}$ inch.

Another jar, No. 2 §, was applied to the prime conductor along with No. 1, and their outside coatings were connected together. The same quantity of wire as in the last experiment was placed in the circuit, and by the same method an explosion was obtained, which ignited and broke the wire with nearly the same appearances as before. In this case the number of turns were $9\frac{2}{7}$, and the length of spark $\frac{3}{4}$ of an inch.

These experiments evidently confirm the result Mr. Cuthbertson obtained from his former trials; namely, that the quantity of electricity to disperse a given portion of wire will be

* Height of the coating $7\frac{1}{2}$ inches, diameter $6\frac{1}{4}$ inches, both outside measure. Uncoated part $4\frac{1}{4}$ inches perpendicular, though the mouth, in reality, was somewhat smaller than the body of the jar. Thickness 0,16 inch. Hence the surface was about 188 square inches, or $1\frac{1}{3}$ square foot.

† Miscellaneous Experiments and Remarks on Electricity, &c. by A. Brook. Norwich, 1789.

‡ This wire is sold at the watch-tool makers. It is of steel flattened, and is used to make the small spring of the balance in watches. The wire here mentioned is 0,005 inch in breadth and one grain in weight; measures $44\frac{1}{2}$ inches long.

§ Height of the coating $8\frac{1}{4}$ inches, diameter 6,2 inches, both outside measure. Uncoated part $3\frac{1}{2}$ inches perpendicular, though the mouth was, as in No. 1, somewhat smaller than the body of the jar. Thickness 0,17 inch. Hence the surface was about 190 square inches, or $1\frac{1}{3}$ square foot, nearly the same as the other jar.

 the

the same, even though the charged surface should be greatly varied. But it appeared desirable to ascertain the proportional quantities of electricity required to explode different lengths of the same wire. For this purpose 2,3 inches of wire were exploded by the jar No. 1, by five turns of the winch; which was probably too great a quantity, for the ignition was very strong, and deflagrating globules were thrown about. But by a more careful repetition of the experiment with $2\frac{1}{2}$ inches of wire, the ignition and partial melting of the wire were very nearly the same as in the experiments which had been made on lengths of five inches. From these experiments there seems reason to suppose that the quantities of electricity may be as the lengths of the wires exploded.

A battery of fifteen jars, containing about 17 square feet of coated surface, was then used to explode five inches of the wire. Nineteen turns rendered the wire faintly red hot; but 20 turns caused it to explode in the same manner as in the experiments with the jars.

Half the length of wire, namely $2\frac{1}{2}$ inches, was in the next place submitted to the explosion of the battery. Twelve turns ignited the wire, and 15 turns caused it to explode with somewhat more violence than in the experiments with the jars.

At this stage of the process, as the battery had required twice the quantity of electricity to produce the effect which had before taken place with a smaller quantity of coated surface, it became a question whether the length of the circuit, which was six feet on each side, might have influenced the results; and also whether the state of excitation had become less intense. For these reasons, the disposition of the apparatus was altered so that the circuit with the battery was the same as had before been used with the jars; and the trial jar, with Lane's electrometer, was again applied to ascertain the power of the machine. Five explosions were afforded by $7\frac{3}{4}$ turns, as at first: this experiment was twice repeated, and shewed that the action of the machine continued to be the same as at first.

In this new disposition of the battery five inches of the wire were exposed to the explosion. No effect was produced by 12 turns; but by 21 turns the whole of the wire was dispersed in globules by a strong ignition. I suppose that 20 turns might have dispersed it in the same manner as in the first experiment.

Two inches and a half of the wire were then placed in the circuit. This portion was not affected by an explosion of 12 turns; but by 13 turns it was ignited and broken by the dissipation of a globule or two. Lastly, one inch of the wire was exposed to the shock; it was ignited, and partly dispersed in globules by 10 turns.

The whole of these experiments employed upwards of three hours, during the greatest part of which time the machine was in action. At the conclusion the trial jar was applied: it exploded five times in a little more than eight turns. The trial was repeated twice, and serves evidently to show the steadiness of the excitation, which had diminished only about $\frac{1}{16}$th part during this course of work.

Upon a review of the foregoing experiments, it is obvious that they would require to be repeated and extended, if the general course of Mr. Cuthbertson's former processes were not to be admitted in confirmation of the position, that equal quantities of electricity will ignite and disperse equal quantities of the same wire, without requiring any particular adjustment of the quantity of coated surface, provided the intensity be considerable. From the experiments with the battery it seems reasonable to conclude, that the quantities of electricity required to produce like effects upon wire will be greater, the lower the intensity, when the quantity

quantity of surface is greatly increased; in which case the velocity of the electric fluid may be supposed insufficient for the whole charge to exist in the conducting wire at one and the same time; or its impetus may be less; or, lastly, there may be a considerable waste from the conducting power of the air through the very thin stratum of air through which the explosion at last passes. This last supposition is far from being merely conjectural. For by some experiments on the charge of a plate of air in connection with the gold leaf electrometer, of which I may hereafter give an account, I find that even in very low intensities the electric matter will pass in a constant stream through intervals of about one inch; a conclusion which might likewise be deduced from the very small duration of the usual charge in the prime conductor.

With regard to the curious fact of the spontaneous explosion of a clean jar; which, when damped by breathing into it, was found much more capable of retaining its charge, there appears to be some difficulty in the theory. I have mentioned my supposition that Mr. Brook's experiment of soiling the jar with the hand, might have succeeded from the perspirable matter having operated as a varnish to the expulsion of moisture from the air. Mr. Cuthbertson's experiment overthrows this notion. I am now inclined to think that it depends upon undulation. The bare surface of clean glass may become charged in successive zones of the contrary electricities, as Priestley and others have shewn. When these zones have acquired the requisite intensity, they may explode into each other, and produce an undulation in the whole charge, which may greatly favour its flight through the interval from coating to coating. But when the surface is covered with distinct insulated particles of moisture, the escape from particle to particle must be by smaller leaps; the tranquillity of the charge will be scarcely at all disturbed; and the spontaneous explosion will not take place until the intensity has become so great, as to carry it through a space equal to the sum of the intervals between any one row of particles which form a line from coating to coating.

V.

A short Mineralogical Description of the Mountain of Gibraltar. By Major IMRIE.

[Concluded from Page 187, Vol. II.]

AT no great distance from where these crystals are found, upon the same slope of the mountain, but rather nearer to the level of the sea, a stratum of argillaceous matter has been laid open, divided into many thin beds, the broadest of which does not exceed a foot in thickness. Its general colour is of a whitish grey, with a small mixture of yellow; and it is divided transversely by straight septa or cracks, both sides of which are covered with dendritical figures of a yellowish brown colour, beautifully representing the objects of landscape. At the western base of the mountain, on a level with the sea, by which it is washed, a very extensive stratum occurs of the same nature as the last described, bearing from north to south, parallel with and dipping towards the mountain nearly at an angle of 40 degrees.

In some parts of the western slope of the mountain, towards the south, are found nests of a dark red shivery clay, in which are imbedded flints of a dirty sap green colour: of those no regular stratum is to be perceived; many of them are unshapely masses; but they in general tend to the rhomboidal form, and are from three to four inches long by two or three broad, and an inch and a half thick. They are not incrufted as the flints found in chalk, nor have they the appearance of having been worn by attrition.

Upon different parts of the mountain, towards its bafe, are found large quantities of fand compofed of different materials, and affuming various appearances as to colour. The largeft bank of this arenaceous matter is upon the weftern fide of the mountain, and confifts of fmall particles of cryftallized quartz, colourlefs, and perfectly tranfparent per fe, but of an ochreous colour in the mafs on account of a red argillaceous earth which adheres to them. The fand of this bank is perfectly loofe and uncombined: one half of it has been levelled into an extenfive parade, its furface having been combined by the lime and rubbifh from the ruins of the town. The fouthern extremity of the bank is ftill to be feen in its natural ftate, and forms the burying-ground of the garrifon.

Upon the eaft fide of the mountain is found another of thefe banks, of confiderable extent, and, as I mentioned before, rifing from the Mediterranean in a rapid acclivity, and reaching to one-third of its entire elevation. This bank is compofed of fmall particles of cryftallized quartz, of teftaceous bodies rounded by attrition, and of a few minute particles of the calcareous rock; the whole has a whitifh grey colour. The rain-water which falls from the bare mountain rock above the fand brings along with it calcareous matter, which is depofited upon the bank, and combines its furface into a cruft, which in fome places is fo much indurated as to bear the preffure of the foot.

In other parts of the mountain where this fand is furrounded by the calcareous rock, and covered in and protected from the action of the air and corrofion of fea-falts, it is found in a perfect indurated ftate, combined by ftalactitical fpar, and forming a minute breccia. A quarry of this arenaceous ftone has been opened upon the fouth-eaft quarter of the mountain, and is made ufe of with great propriety to line the embrafures of fome of the new works belonging to the garrifon. Its inaptitude to fly off in fplinters when ftruck by a ball gives, in fuch fituations, additional fafety to the defenders of the place.

The weftern fide of the mountain's bafe around Rofia Bay, and the New Mole, is a rock compofed of an aggregate of fmall fragments of every foffil that has been here defcribed, with the addition of two different fpecies of marble, that are probably adventitious, as their native beds have not been found in the mountain. The one of thofe is black, and the other of an olive green colour. The whole of this mixture produces a moft beautiful breccia, and is firmly combined by a calcareous cement of a yellow verging towards an orange colour. It is fufceptible of a high polifh, except where fragments of the argillaceous ftrata occur: thefe can be eafily fmoothed down, but cannot be brought to a perfect polifh. The fragments in this breccia are angular, and none of them have the appearance of being water-worn.

It only now remains for me to mention, what are generally called the foffil bones found in the rock of Gibraltar. Thefe have been much talked of, and by fome looked upon as a phenomenon beyond the power of explanation. The general idea which exifts concerning them is, that they are found in a petrified ftate and inclofed in the folid calcareous rock; but thefe are miftakes which could only arife from inaccurate obfervation and falfe defcription.

In the perpendicular fiffures of the rock, and in fome of the caverns of the mountain (all of which afford evident proofs of their former communication with the furface), a calcareous concretion

concretion is found of a reddish brown ferruginous colour, with an earthy fracture and considerable induration, inclosing the bones of various animals, some of which have the appearance of being human. These bones are of various sizes, and lie in all directions intermixed with shells of snails, fragments of the calcareous rock, and particles of spar; all of which materials are still to be seen in their natural uncombined states, partially scattered over the surface of the mountain. These having been swept by heavy rains at different periods from the surface into the situations above described, and having remained for a long series of years in those places of rest, exposed to the permeating action of water, have become enveloped in, and cemented by, the calcareous matter which it deposits.

The bones in this composition have not the smallest appearance of being petrified; and if they have undergone any change, it is more like that of calcination than that of petrifaction, as the most solid parts of them generally admit of being cut and scraped down with the same ease as chalk.

Bones combined in such concretions are not peculiar to Gibraltar: they are found in such large quantities in the country of Dalmatia, and upon its coasts in the islands of Cherso and Osero, that some naturalists have been induced to go so far as to assert, that there has been a regular stratum of such matter in that country, and that its present broken and interrupted appearance has been caused by earthquakes or other convulsions experienced in that part of the globe. But of late years, a traveller (Abbé Alberto Fortis) has given a minute description of the concretion in which the bones are found in that country: and by his account it appears, that with regard to situation, composition, and colour, it is perfectly similar to that found at Gibraltar. By his description it also appears that the two mountain rocks of Gibraltar and Dalmatia consist of the same species of calcareous stone; from which it is to be presumed that the concretions in both have been formed in the same manner and about the same periods.

Perhaps, if the fissures and caves of the rock of Dalmatia were still more minutely examined, their former communications with the surface might yet be traced, as in those described above; and in that case, there would be at least a strong probability that the materials of the concretions of that country have been brought together by the same accidental cause, which in my opinion has collected those found in the caverns of Gibraltar. I have traced in Gibraltar this concretion, from the lowest part of a deep perpendicular fissure up to the surface of the mountain. As it approached to the surface, the concretion became less firmly combined; and when it had no covering of the calcareous rock, a small degree of adhesion only remained, which was evidently produced by the argillaceous earth in its composition having been moistened by rain and baked by the sun.

The depth at which these materials had been penetrated by that proportion of stalactitical matter, capable of giving to the concretion its greatest adhesion and solidity, I found to vary according to its situation, and to the quantity of matter to be combined. In fissures narrow and contracted, I found the concretion possessing a great degree of hardness at six feet from the surface; but in other situations more extended, and where a larger quantity of the materials had been accumulated, I found it had not gained its greatest degree of adhesion at double that depth. In one of the caves where the mass of concretion is of considerable size, I perceived it to be divided into different beds, each bed being covered with a crust of the stalactitical spar from one inch to an inch and a half in thickness; which seems to indicate,

that

that the materials had been carried in at various periods, and that those periods have been very remote from each other.

At Rosia Bay, upon the west side of Gibraltar, this concretion is found in what has evidently been a cavern, originally formed by huge unshapely masses of rock, which have tumbled in together. The fissure or cavern formed by the disruption and subsidence of those masses has been entirely filled up with the concretion, and is now exposed to full view by the outward mass having dropped down in consequence of the encroachments of the sea. It is to this spot that strangers are generally led to examine the phenomenon; and the composition having here attained to its greatest degree of hardness and solidity, the hasty observer, seeing the bones enclosed in what has so little the appearance of having been a vacuity, examines no farther, but immediately adopts the idea of their being incased in the solid rock. The communication from this former chasm to the surface, from which it has received the materials of the concretion, is still to be traced in the face of the rock; but its opening is at present covered by the base of the line wall of the garrison. Here bones are found that are apparently human; and those of them that appear to be of the legs, arms, and vertebræ of the back, are scattered among others of various kinds and sizes, even down to the smallest bones of small birds. I found here the complete jaw-bone of a sheep; it contained its full complement of teeth, the enamel of which was perfect, and its whiteness and lustre in no degree impaired. In the hollow parts of some of the large bones was contained a minute crystallization of pure and colourless calcareous spar; but in most the interior part consisted of a sparry crust of a reddish colour, scarcely in any degree transparent.

At the northern extremity of the mountain the concretion is generally found in perpendicular fissures. The miners there employed upon the fortifications, in excavating one of those fissures, found, at a great depth from the surface, two skulls, which were supposed to be human; but to me one of them, if not both, appeared to be too small for the human species. The bone of each was perfectly firm and solid; from which it is to be presumed that they were in a state of maturity before they were enclosed in the concretion. Had they appertained to very young children, perhaps the bone would have been more porous, and of a less firm texture. The probability is, that they belonged to a species of monkey which still continues to inhabit in considerable numbers those parts of the rock which are to us inaccessible.

This concretion varies in its composition according to the situation in which it is found. At the extremity of the Prince's lines, high in the rock which looks towards Spain, it is found to consist only of a reddish calcareous earth, and the bones of small birds cemented thereby. The rock around this spot is inhabited by a number of hawks, that in the breeding season nestle here and rear their young: the bones in this concretion are probably the remains of the food of those birds. At the base of the rock below King's lines the concretion consists of pebbles of the prevailing calcareous rock. In this concretion, at a very considerable depth under the surface, was found the under part of a glass bottle, uncommonly shaped, and of great thickness; the colour of the glass was of a dark green.

In many parts of the rock I have found concretions in which there are no bones of any kind; and on the elevated parts of the mountain, where the slopes are rapid, I have found a breccia (if I may so call it) entirely consisting of snail-shells combined in a mass of opaque stalactitical spar of a yellowish brown colour. The various progressive augmentations of this matter

matter were to be traced in various shades of the same colour, which, like the zones of the antique alabaster, curve round and follow the form of the shell. The purer matter of this spar has penetrated the shells, and in their interior hollows has formed a lining of small crystals, generally colourless, and perfectly transparent.

I have bestowed more time in endeavouring to describe the composition and the real situation in this concretion of bones than the subject, in the estimation of many, will seem to deserve, and, indeed, more than it deserves in my own opinion; but where an erroneous opinion has obtained a footing in consequence of inaccurate observations and partial description, it is the duty of every new observer to endeavour to correct it.

VI.

Accounts of the Discovery of Native Gold in Ireland.

THE public attention was a few years ago greatly excited by the important mineralogical discovery of native gold in Ireland; soon after which accounts were sent to the Royal Society. (Phil. Transf. 1796.) It may easily be imagined, that these works must have been suspended by the convulsions which at present agitate that unhappy kingdom. But as I hope by enquiry to ascertain what has been done since the original discovery, which from its interest and curiosity is highly deserving of attention, I shall here present the substance of these accounts to my readers with very little abridgement.

John Lloyd, Esq. of Havodynos, gave the following account, dated November the 4th, 1795:

About seven miles westward of Arklow, in the county of Wicklow, there is a very high hill, perhaps six or seven hundred yards above the sea, called Croughan Kinshelly, one of whose N. E. abutments or buttresses is called Balinnagore, to which the ascent may be made in half or three quarters of an hour. In Jacob Nevill's map of the county of Wicklow, published in 1760, by casting your eye on the river Ovo, which runs by Arklow, at about four miles above the latter place, you will perceive the conflux of two considerable streams, and of a third about half a mile higher up, close to a bridge. By tracing this last to its source, you will come to a place set down in the map Ballinvally; this is a ravine between two others, that run down the side of the hill into a semi-circle, or more properly semi-elliptical valley, which extends in breadth from one summit to the other of the boundary of the valley, and across the valley three quarters of a mile or somewhat less. The hollow side of the hill forms the termination of the valley, and down which run the three ravines abovementioned. At their junction the brook assumes the name of Ballinasloge: at this place the descent is not very rapid, and so continues a hanging level for about a quarter of a mile or somewhat more, when the valley grows narrower and the sides of the brook become steeper; and it should seem that some rocky bars across the course of the brook have formed the gravelly beds, above, over, and through which the stream flows, and in which the gold is found. The bed of the brook, and the adjacent banks of gravel on each side, for near a quarter of a mile in length, and for 20 or 30 yards in breadth, have been entirely stirred and washed by the peasants of the country, who amounted to many hundreds at work at a time whilst they were permitted to search for the metal.

A gentleman who ſaw them at work aſſured Mr. Lloyd, that he counted above three hundred women at one time, beſides great numbers of men and children.

The ſtream runs down to the N. E. from the hill, which ſeems to conſiſt of a maſs of ſhiſtus and quartz; for, on examination of the principal ravine, which is now waſhed clean by the late heavy rains, the bottom conſiſted of ſhiſtus interſected at different diſtances and in various places by veins of quartz, and of which ſubſtances the gravelly beds at the bottom, where the gold is found, ſeem to conſiſt.

Large tumblers of quartz are thickly ſcattered over the ſurface of the top of the hill, under a turbary of conſiderable thickneſs, upon the removal of which theſe tumblers appear.

The gold has been found in maſſes of all ſizes, from thoſe of ſmall grains to that of a piece of the weight of five ounces; which beautiful ſpecimen is intended for the cabinet of a nobleman adored in this country, and not leſs reſpected by his friends in England, and which I dare to ſay you will ſhortly have an opportunity of ſeeing in London. One piece of twenty-two ounces has been taken up, and which I am told is to be preſented to his Majeſty.

Mr. Graham of Ballycoage informed Mr. Lloyd, that about twenty-five years ago, or more, one Dunaghoo a ſchoolmaſter, reſident near the place, uſed frequently to entertain them with accounts of the richneſs of the valley in gold; and that this man uſed to go in the night and break of day to ſearch for the treaſure; and theſe gentlemen with their ſchool-fellows uſed to watch the old man in his excurſions to the hill, to frighten him, deeming him to be deranged in his intellects. However, the idea of this treaſure did at laſt actually derange him.

Mr. Lloyd learned alſo from John Byrne, that about eleven or twelve years ago, when he was a boy, he was fiſhing in this brook, and found a piece of gold of a quarter of an ounce, which was ſold in Dublin; but that, upon one of his brothers telling him it muſt have been dropped into the brook by accident, he gave over all thoughts of ſearching for more. Charles Toole, a miner at Cronbane, ſaid, that he heard of this diſcovery at the time, but gave no credit to it, as he never found any gold, and lives very near the place; and Mr. Lloyd was alſo credibly informed, that a goldſmith in Dublin has every year for eleven or twelve years bought four or five ounces of gold brought conſtantly by the ſame perſon, but not John Byrne.

The name of the brook where the gold is found is in Iriſh Aughatinavought.

The account of the mineralogy and other circumſtances by Abraham Mills, Eſq. is as follows:

The workings, which the peaſantry recently undertook, are on the north-eaſt ſide of the mountain Croughan Kinſhelly, within the barony of Arklow and county of Wicklow, on the lands of the Earl of Carysfort, wherein the Earl of Ormond claims a right to the minerals, in conſequence of a grant in the reign of King Henry the Second by Prince John, during his command of his father's forces in Ireland; which grant was renewed and confirmed by Queen Elizabeth, and again by King Charles the Second *.

The ſummit of the mountain is the boundary between the counties of Wicklow and Wexford; ſeven Engliſh miles weſt from Arklow, ten to the ſouth-weſtward of Rathdrum, and ſix ſouth-weſterly from Cronbane mines; by eſtimation about ſix hundred yards above

* It was afterwards found that this grant had been annulled by an act of the Iriſh Parliament near the beginning of the preſent century. Mr. Mills has ſince worked this mine on account of Government. N.

6 the

the level of the ſea. It extends W. by N. and E. by S. and ſtretches away to the north-eaſtward to Ballycoage, where ſhafts have formerly been ſunk, and ſome copper and magnetic iron ore has been found; and thence to the N. E. where extends a tract of mineral country eight miles in length, running through the lands of Ballymurtagh, Ballygahan, Tigrony, Cronebane, Connery, and Kilmacoe, in all which veins of copper ore are found; and terminating at the ſlate quarry at Balnabarny.

On the higheſt part of the mountain are bare rocks, being a variety of argillite *, whoſe joints range N. N. E. and S. S. W. hade to the S. S. W. and in one part include a rib of quartz three inches wide, which follows the direction of the ſtrata. Around the rocks for ſome diſtance, is ſound ground, covered with heath; deſcending to the eaſtward there is ſpringy ground abounding with coarſe graſs; and below that a very extenſive bog, in which the turf is from four to nine feet thick, and beneath it in the ſubſtratum of clay are many angular fragments of quartz, containing chlorite and ferruginous earth. Below the turbary the ground falls with a quick deſcent, and three ravines are obſerved. The central one, which is the moſt conſiderable, has been worn by torrents, which derive their ſource from the bog; the others are formed lower down the mountain by ſprings, which uniting with the former, below their junction the gold has been found. The ſmaller have not water ſufficient to waſh away the incumbent clay ſo as to lay bare the ſubſtratum; and their beds only contain gravel conſiſting of quartz, with chlorite and other ſubſtances of which the mountain conſiſts. The great ravine preſents a moſt intereſting aſpect; the water in its deſcent has in a very ſhort diſtance from the bog entirely carried off the clay, and conſiderably worn down the ſubſtrata of rock, which it has laid open to inſpection.

Deſcending along the bed of the great ravine, whoſe general courſe is to the eaſtward, a yellow argillaceous ſhiſtus is firſt ſeen; the laminæ are much ſhattered, are very thin, have a ſlight hade to the S. S. W. range E. S. E and W. N. W. Included within the ſhiſt is a vein of compact barren quartz, about three feet wide, ranging N. E. and S. W.; below this is another vein about nine inches wide, having the ſame range as the former, and hading to the northward, conſiſting of quartz including ferruginous earth. Lower down is a vein of a compact aggregate ſubſtance, apparently compounded of quartz, ochraceous earth, chert, minute particles of mica, and ſome little argillite of unknown breadth, ranging E. and W. hading faſt to the ſouthward, and including ſtrings of quartz from one to two inches thick, the quartz containing ferruginous earth. The yellow argillaceous ſhiſtus is again ſeen with its former hade and range; and then adjacent to a quartz vein is laminated blue argillaceous ſhiſtus, ranging N. E. and S. W. and hading S. E.; which is afterwards ſeen varying its range and hade, running E. N. E. and W. S. W. and hading N. N. W. Lower down, the blue ſhiſt is obſerved more compact, though ſtill laminated. The ground, leſs ſteep, becomes ſpringy, is incloſed, and the ravine, ſhallower, has depoſited a conſiderable quantity of clay-ſand and gravel. Following the courſe of the ravine, or, as it may now more properly be called, the brook, you arrive at the road which leads to Arklow; here is a ford, and the brook has the Iriſh name of Aughatinavought (the river that drowned the old man): hence it deſcends to the Aughrim river, juſt above its confluence with that from Rathdrum, which after their junction take the general name of the

* Kirwan, edit. 1794, p. 234.

Ovo, that, discharging itself into the sea near the town of Arklow, forms an harbour for vessels of small burthen.

The lands of Ballinvally are to the southward, and the lands of Ballinagore to the northward of the ford, where the blue shistus rock, whose joints are nearly vertical, is seen ranging E. N. E. and W. S. W. including small strings of quartz which contain ferruginous earth. The same kind of earth is also seen in the quartz, contained in a vein from ten to twelve inches wide, ranging E. N. E. and W. S. W. and hading to the southward, which has been laid open in forming the Arklow road.

There the valley is from twenty to thirty yards in width, and is covered with substances washed down from the mountain, which on the sides have accumulated to the depth of about twelve feet. A thin stratum of vegetable soil lies uppermost; then clay mingled with fine sand composed of small particles of quartz, mica, and shist; beneath which the same substances are larger, and constitute a bed of gravel that also contains nodules of fine grained iron-stone, which produces 50 per cent. of crude iron: incumbent on the rock are large tumblers of quartz, a variety of argillite and shistus; many pieces of the quartz are perfectly pure; others are attached to the shistus; others contain chlorite, pyrites, mica, and ferruginous earth; and the arsenical cubical pyrites frequently occurs imbedded in the blue shistus. In this mass of matter, before the workings began, the brook had formed its channel down to the surface of the rock, and between six and seven feet wide, but in times of floods extended itself entirely over the valley.

Researches have been made for the gold amidst the sand and the gravel along the run of the brook for near half a mile in length; but it is only about one hundred and fifty yards above, and about two hundred yards below the ford, that the trials have been attended with much success: within that space the valley is tolerably level, and the banks of the brook have not more than five feet of sand and gravel above the rock: added to this, it takes a small turn to the southward, and consequently the rude surfaces of the shistus rock in some degree cross its course, and form natural impediments to the particles of gold being carried further down the stream, which still lower has a more rapid descent. Besides, the rude manner in which the country people worked seldom enabled them to penetrate to the rock in those places where the sand and gravel were of any material depth. Their method was to turn the course of the water wherever they deemed necessary, and then with any instruments they could procure to dig holes down to the rock, and, by washing in bowls and sieves the sand and gravel they threw out, to separate the particles of gold which it contained; and from the slovenly and hasty way in which their operations were performed, much gold most probably escaped their search: and that indeed actually appears to have been the case; for, since the late rains washed the clay and gravel which had been thrown up, gold has been found lying on the surface. The situation of the place, and the constant command of water, do however very clearly point out the great facility with which the gold might be separated from the trash, by adopting the mode of working practised at the best tin stream works in the county of Cornwall; that is, entirely to remove (by machinery) the whole cover off the rock, and then wash it in proper buddles and sieves. And by thus continuing the operations, constantly advancing in the ravine towards the mountain, as long as gold should be found, the vein that forms its matrix might probably be laid bare.

The discovery was made public, and the workings began early in the month of September last,

last (1795), and continued till the 18th of October, when a party of the Kildare militia arrived, and took possession by order of government; and the great concourse of people, who were busily engaged in endeavouring to procure a share of the treasure, immediately desisted from their labour, and peaceably retired.

Calculations have been made, that, during the foregoing period, gold to the amount of three thousand pounds Irish sterling was sold to various persons; the average price was three pounds fifteen shillings per ounce; hence eight hundred ounces appear to have been collected within the short space of six weeks.

The gold is of a bright yellow, perfectly malleable; the specific gravity of an apparently clean piece 19,000. A specimen assayed here by Mr. Weaver in the moist way produced from 24 grains, 22 $\frac{58}{101}$ grains of pure gold, and 1 $\frac{43}{101}$ of silver. Some of the gold is intimately blended with, and adherent to quartz; some (it is said) was found united to the fine-grained iron-stone, but the major part was entirely free from the matrix; every piece more or less rounded on the edges, of various weights, forms and sizes, from the most minute particle up to 2 oz. 17 dwt.; only two pieces are known to have been found of superior weight, and one of those is five and the other twenty-two ounces.

The bearings are all taken by the compass, without allowing for the variation.

William Molesworth, Esq. of Dublin, in a letter to Richard Molesworth, Esq. F. R. S. writes, that he weighed the largest piece of gold in his balance, both in air and water, and that its weight was 20 oz. 2 dwt. 21 gr. and its specific gravity to that of sterling gold, as 12 to 18. Also, that Richard Kirwan, Esq. F. R. S. found the specific gravity of another specimen to be as 13 to 18. Hence, as the gold was worth 4l. an ounce, Mr. William Molesworth concludes that the specimens are full of pores and cavities which increase their bulk, and that there are some extraneous substances, such as dirt or clay, contained in those cavities.

This opinion was discovered to be well founded, by cutting through some of the small lumps.

Stanesby Alchorne, Esq. his Majesty's assay-master at the Tower of London, assayed two specimens of this native gold. The first appeared to contain in 24 carats,

21 $\frac{6}{8}$ of fine gold.
1 $\frac{7}{8}$ of fine silver.
$\frac{3}{8}$ of alloy, which seemed to be copper tinged with a little iron.

The second specimen differed only in holding 21 $\frac{5}{8}$ instead of 21 $\frac{6}{8}$ of fine gold.

Major John Brown, of the royal engineers, transmitted to the Right Hon. Thomas Pelham a sketch of the spot where the gold was found, which Mr. Pelham permitted to be engraved for the use of the Royal Society, and of which fig. 5. plate IX. is a copy, one third of the size of the original.

VII.

An Account of the Principles and Effect of Steam Engines, which act by means of a Piston. With Descriptions of the Atmospherical Engine of NEWCOMEN and CAWLEY; the Engine of WATT, which works in Vacuo; and a new Engine by Mr. SADLER, in which the direct Action of Steam and the Pressure of the Atmosphere are combined.

SOME account of the original steam-engine of the Marquis of Worcester, and its subsequent improvements, has been given in a former paper in this work*. One of the chief imperfections of that engine was then shewn to consist in the direct action of the steam, for forcing, being necessarily more than equal to the weight of the column of water required to be raised; in consequence of which, it became in most cases an indispensable condition, that the boiler and vessels should be very strong, as well as that a large quantity of fuel should be consumed, to produce steam sufficiently dense. It is probable that these inconveniences may have early directed the thoughts of various ingenious men to the application of a piston, though the difficulties of the undertaking seem to have retarded this pursuit for a considerable time. The first steam-engine, with a piston, made in 1707 by Papin †, was little calculated to remove these difficulties; and it is to Newcomen and Cawley that we are indebted for the application of a piston with machinery, by which the indirect action of steam little stronger than the atmosphere, or rather the direct action of the atmosphere, is made to act with safety and effect against the most severe pressures. It appears that they had brought their engine, about the year 1713 ‡, to a degree of perfection little different from those which are to be seen at present. A particular description of this engine, with drawings of its parts, and a considerable portion of the history of its invention, are to be met with in Desaguliers's Course of Experimental Philosophy, and it has also been described in many other books. For these reasons, I shall in this place give merely a verbal account of its principles and mode of operation.

Suppose a very large syringe to be set upright, and a piston or plug inserted at the upper end, the usual aperture being supposed to be at the lower extremity. If this last aperture be open, the piston will descend by its own weight, neglecting the effect of friction at its circumference. But let it be imagined, that the piston is supported by a counter-weight at the opposite extremity, by a lever or by any other means. In this case the piston will not descend unless more weight be added to it. Among the various ways of applying such a weight, there is one which consists in exhausting the air from the internal part of the cylinder beneath the piston. For, if this were done, it is evident that the whole pressure of the atmosphere, which amounts to about twelve pounds on every circular inch, will become active upon the upper surface. If the vacuum were to be produced by means of an air-pump, it may easily be allowed, that the labour of effecting it would be at least equal to that of any work which might be performed by the subsequent descent of the piston. We have seen that, in Savery's engine, the operation of steam is twofold; namely, by the direct pressure from its

* Philosophical Journal, I. 419.

† Loco citato, p. 421.

‡ Desaguliers in his Lectures, ii. 467, says that it had been in use near thirty years at the time he wrote, and the imprimatur to his work bears date November 17, 1743. See also p. 532 of the same volume.

elasticity,

elafticity, and by the indirect confequence of its condenfation, which affords a vacuum. This laft is the only principle difplayed in Newcomen's engine. In order to produce the vacuum at pleafure, it becomes requifite that various apertures fhould be formed at the bottom of the cylinder or fyringe we have been fpeaking of:—one to communicate fteam from a boiler; another to admit a jet of cold water, to condenfe that fteam during the interval in which the communication from the boiler is cut off; a third provided with a pipe called the eduction-pipe, to carry off the condenfed fteam and injection water; and laftly, a fmall lateral aperture or valve through which the permanently elaftic fluid, which cannot defcend through the eduction-pipe, may be driven out. This laft is called the fnifting clack. By thefe provifions the operation is made to take place as follows: The pifton being up, the fteam-cock is opened, and fteam iffues from the boiler, which being lefs than half the weight of common air, rifes to the top and expels the air through the eduction-pipe, of which the lower extremity is covered with a flap valve in a trough of water. When the noife of its efcape is heard the fteam-cock is fhut, and the injection-pipe being opened throws a ftream of cold water againft the bottom of the pifton. The fteam becomes immediately condenfed, and the preffure of the atmofphere forces the pifton down into the vacuum. Upon its progrefs downwards the injection pipe is clofed; and when it has arrived nearly to the bottom of the cylinder, the fteam-cock is again opened. The elaftic fteam then not only fills the fmall fpace between the cylinder and the bottom, but its preffure affifts the eduction water to pafs off through its pipe, and drives the difengaged elaftic fluid through the fnift. In this ftate therefore the fteam is fomewhat ftronger than the atmofphere, and counterpoifes its action on the upper furface of the pifton; whence the pifton itfelf rifes by the action of the counter-weighr, and regains its original pofition at the top of the cylinder. A fecond repetition of the procefs, namely, of fhutting off the fteam and injecting cold water, caufes it again to defcend, and in this manner the alternations may be continued without limit.

As the preffure requifite to work an engine of this kind with fpeed, and to overcome the friction of its parts, may be eftimated at about $7\frac{1}{2}$ pounds for every round inch, the effect of a pifton three feet in diameter, with ten ftrokes of fix feet each per moment, will be to raife a weight of near half a ton at the rate of one foot per fecond. Such a power will therefore be very confiderable.

It is to be underftood, that the opening and fhutting of the fteam and injection cocks are performed by apparatus fixed to the working beam, in fuch a manner as to ftrike the levers of thofe cocks at the precife inftants of time when their effects are required to be produced. The attendant has no other office to perform than keeping up the fire.

This curious and moft eminently ufeful engine became an object of the refearches of James Watt, then of Glafgow, but fince affociated with Matthew Boulton of Birmingham. It would be fuperfluous for me to enter into any general account of the fcience and intelligence of either of thefe engineers, or of the great advantages which fociety muft ever derive when the fpirit of enterprife is joined to the exertions of mental ability. Neither can I here attempt to detail the variety of new refults which are exhibited in the fpecifications lodged by Watt in the Chancery offices concerning this object. My prefent limits and the attention which the world has already paid to thefe eminent men, and the products of their enlightened

lightened activity, render both unneceſſary. I ſhall therefore confine myſelf to remark, that the great features of improvement made in the engine of Newcomen by Watt are, firſt, that the elaſticity of the ſteam itſelf is uſed as the active power in his engine; and ſecondly, that beſides various other judicious arrangements for the economy of heat, he condenſes the ſteam, not in the cylinder, but in a ſeparate veſſel.

In the great cylinder or ſyringe, concerning which we have ſpoken, in deſcribing the engine of Newcomen, let us ſuppoſe the upper part to be cloſed, and the piſton-rod to ſlide air-tight through a collar of leathers. In this ſituation, it is evident, that the piſton might be depreſſed by throwing the ſteam upon its upper ſurface, through an aperture at the ſuperior end of the cylinder. But if we ſuppoſe the external air to have acceſs to the lower ſurface of the piſton, we ſhall find, that ſteam no ſtronger in its elaſticity than to equal the weight of the atmoſphere would not move the piſton at all; and conſequently that this new engine would require much denſer ſteam, and conſume much more fuel than the old engine. The remedy for this evil is to maintain a conſtant vacuum beneath the piſton. If ſuch a vacuum were originally produced by ſteam, it is certain that its permanency could not be depended on, unleſs the engine contained a proviſion for conſtantly keeping it up. Mr. Watt's contrivance in his ſimpleſt engine is as follows: The ſteam is conveyed from the boiler to the upper part of the cylinder through a pipe, which alſo communicates occaſionally with the lower part, and beyond that ſpace with a veſſel immerſed in a trough of water; in which veſſel the condenſation is performed by an injected ſtream of cold water. This water is drawn off, not by an eduction-pipe but by a pump, of which the ſtroke is ſufficiently capacious to leave room for the elaſtic fluid, ſeparated during the injection, to follow and be carried out with the injection water. Suppoſe now the piſton to be at its greateſt elevation, and the communication from the boiler to the upper as well as to the lower parts of the cylinder to be opened. The ſteam will then paſs into the whole internal part of the engine, and will drive the air downwards into the condenſer, and thence through the valves of the air-pump. In this ſituation, if the communication from the boiler to the lower part of the cylinder be ſtopped, and an injection be made into the condenſer, a vacuum will be produced in that veſſel, and the ſteam contained in the lower part of the cylinder and communication pipe will expand itſelf with wonderful rapidity towards the condenſer, ſo that in a period of time too minute to be appretiated, the whole of the ſteam beneath the piſton will be practically condenſed. The ſteam which continues to act above the piſton will immediately depreſs it into the vacuum beneath; at the ſame time that by connection with the external apparatus the piſton of the air-pump alſo deſcends in its barrel. When the ſtroke is nearly completed downwards, the requiſite part of the apparatus ſhuts the communication with the boiler; opens that between the upper and lower parts of the cylinder and condenſing veſſel; and turns the injection-cock. At this very inſtant the piſton loſes its tendency to deſcend, becauſe the ſteam preſſes equally on both ſurfaces, and continues its equality of preſſure while the condenſation is performed. It therefore riſes; the injection is ſtopped; and the air-pump making its ſtroke ſuffers the injection water and a conſiderable part of the elaſtic fluid to paſs through its lower valve. The vacuum is thus kept up through the whole internal capacity of the engine. As ſoon as the piſton has reached the upper part of the cylinder, the communication to the under part of the cylinder is ſtopped, and that with the boiler opened as before; the confequence of which is, that the pifton again defcends, and in this manner the alternations repeatedly take place.

The principal augmentation of power in this engine, compared with that of Newcomen, appears to arife from the cylinder not being cooled by the injection water, and its being practicable to ufe fteam fomewhat ftronger than the preffure of the atmofphere. In general thefe engines are worked by fteam, which would fupport a column of four or five inches of mercury befides the preffure of the atmofphere, and I have fometimes feen the gage as high as eight inches. But whether the comparative profit of the additional work was fuch as to repay the extraordinary confumption of fuel in fuch cafes may be doubted; and indeed I fuppofe this practice is never adopted but when the work of a manufactory is fuch as to exceed the power or rate of the engine conftructed for performing it.

The moft ample information refpecting thefe improved fteam engines, which is to be met with in print, is found in the Nouvelle Architecture Hydraulique of Prony.

When we contemplate thefe engines together with the fimpler engine of Savery, one of the moft ftriking differences, is, that the latter is not embarraffed with the weight and expence of the beam and counterpoife. I believe that the fly was firft introduced by Meffrs. Watt and Boulton, as an equalizer of the action in fteam engines, which in various conftructions renders a counterpoife unneceffary, but is faid to diminifh the power by loading the work with friction and refiftance. Of this however I can fay nothing from my own experience or obfervation. The counterpoife is alfo unneceffary in the engines called double engines, in which the fteam is made to act alternately on each fide of the pifton, by proper communication from the boiler, while the fpace into which the pifton is to move is made to communicate with the condenfing veffel, where a conftant injection is maintained. I was alfo informed, in the year 1794, that fome engines had been conftructed without the beam; but the architect from whom I had the information did not ftate the particulars.

The injection water and elaftic fluid are difpofed of in Newcomen's engine with fcarcely any lofs of power, becaufe the former flows fpontaneoufly through the eduction-pipe, and the latter is driven out by a very flight effort of the fteam againft the atmofphere. Watt's engine could not have an eduction-pipe unlefs the column were at leaft 34 or 35 feet long, to exclude the atmofphere, and even in that cafe there could be no fnift to deliver the elaftic fluid. It was therefore neceffary there fhould be an air-pump to carry off both; and this is a load upon the engine equal to the amount of the friction of thefe additional parts and the whole weight of the eduction water. I am not well informed of this laft quantity; but fuppofe it to be about one eightieth * part of the power of the engine.

The fteam-engine for which a patent has lately been granted to Mr. Sadler, is worked without a lever or beam, and confequently has not the inertia of that heavy mafs to overcome. The greateft part of its action is performed by the elafticity of fteam; but the moft remarkable character of his invention is, that part of the fteam previous to its condenfation is employed a fecond time in another cylinder, the pifton of which is fubject to the preffure of the atmofphere. By this fecond application it not only performs the office of an air-pump in a very accurate manner, but likewife adds to the total amount of force in the machine.

* Suppofing the injection water to be about $\frac{1}{120}$ of the magnitude of the cylinder within, and that the ftroke would raife $\frac{2}{3}$ of the fame magnitude of water through its own length.

Plate X.

Plate X. exhibits a projection of part of this engine, the boiler and the working parts being left out. R S U T reprefent a frame of wood. A is the larger cylinder, of which the pifton-rod is kept vertical by a wheel I, which runs between parallel plates with a very fmall degree of fhake. The rife and fall of this wheel moves the arm N O of a crank, which drives the fly P Q, and gives motion to the pumps or other working parts. B is the fecond cylinder open at top, the pifton-rod of which is kept vertical in like manner by a wheel K. A connection is formed between the two piftons by the lever L M, which caufes them to make their ftrokes together. D is a valve in the pifton of A, which, by means of its tail beneath, is opened when that pifton is down, and continues fo during the whole afcent, until it becomes clofed by the preffure of its crown againft the top of the cylinder.

Suppofe the piftons to be near the tops of their refpective cylinders, and the valve D to be open: if C be opened, the fteam will pafs through the cylinder A, and enter B by the paffage E, driving the air before it through the valve G. If then the valve D be clofed by raifing the pifton or otherwife, and an injection of cold water be made through F into B, the fteam in that cylinder and beneath the pifton of A will be condenfed, and both piftons will defcend; that in A by the action of the fteam, and that in B by the preffure of the atmofphere. Near the termination of this defcent the valve D will be opened, and the pifton in A will be left at liberty to rife, in confequence of the equal action of the fteam on both fides, at the fame time that, the valve C being fhut, there will be no farther fupply from the boiler. The fteam which occupied the fpace above the pifton in A will expand through E, and affift the rife of the pifton in B. If we overlook the effect of refrigeration in the paffage E, and againft the pifton at B, the fteam will begin to act beneath this pifton with a force not greatly different from its laft action upon that in A. It would not be eafy to calculate the whole effect of the fteam in its fecond application, not only for want of an accurate ftatement of the elafticity of this fluid during its expanfion; but becaufe the practical effect of the temperature of the fecond cylinder in diminifhing that force is ftill more difficult to be appretiated. I fhall therefore only obferve, that the pifton in B having defcended fo as to touch the furface of the injection water, and the elaftic fluid refting upon it, not only caufes that air but a portion of the hot water to pafs through the valve W, which it carries up and difcharges through a conveyance at the upper part of the cylinder to feed the boiler. The fteam which follows it through E, drives all the elaftic fluid through the valve of that paffage, whence it remains in B till the next ftroke carries it off. The quantity of water which fhall rife above the pifton through W, may be regulated by the height of the external furface of the ciftern into which the pipe G difcharges itfelf. The excefs of the action of the fteam beneath the pifton in B, beyond the weight of that pifton and the water it carries, will be an acquifition to the power of the engine. When we confider the facts, we may obferve, that, if B were too fmall, the action of the fteam under its pifton would approach more nearly to equality with the laft action in A; but that the ftroke would be infufficient to carry off the elaftic fluid, and the addition to the force of the engine inconfiderable; and on the other hand, if it were too large, the greateft part of the rifing ftroke would be performed againft the preffure of the atmofphere, with very little affiftance from the expanded fteam. Whence it follows, that there is a certain definite proportion under like circumftances of preffure, temperature and work, between the two cylinders, which will produce a greater effect than any other. To determine this would be the object of computation, if the data were at hand; but it is probable

 that

that an experimental result with regard to those dimensions might be obtained with as little trouble as the mere data for computation.

VIII.

On the supposed Improvement of Achromatic Lenses, when rendered more transparent by cementing them together.

AN achromatic glass, consisting of one concave and two convex lenses, has six surfaces at which the reflexion of light can take place. Two evils of considerable magnitude are produced by this reflexion. The focal image becomes obscure from the loss of the light so diverted, and the ground or field of view is rendered misty by the irregular illumination afforded by part of this light, which enters the tube. These consequences are strikingly observed by holding an achromatic lens in such a manner as to throw the image of a window upon a sheet of paper, at the same time that a like image is afforded by a simple convex lens of the same focal length. The latter image will be bright and clear, while the former is very faint. As the reflexion of light is strongest at surfaces which confine mediums most different in density, it may naturally be imagined that the mischievous effect of the four surfaces, at which the lenses are applied to each other, might be in a great measure removed by the interposition of a denser fluid than air. * Many years ago the Abbé Rochon made some experiments to improve achromatic lenses by this invention, and Mr. Grateloup afterwards made use of a transparent solid for the same purpose. He took a very pure tear of mastic, and interposed a piece of sufficient magnitude between the lenses of an achromatic glass. These being immersed under water of which the temperature was gradually raised, the mastic at length became soft, and by pressure of the glasses was made to occupy the whole of the two spaces between the concave and the exterior lenses. After this process the compound lens was beautifully transparent; the reflected images could scarcely be discerned; and upon trial in its tube the telescope was found to be greatly improved.

My attention to this contrivance was excited by the approbation bestowed upon a telescope of this kind, of 27 inches long and 23 lines aperture, by Citizen Lalande, in the Connoissance de Tems for the fourth year of the French Republic, page 364. It appeared to me, as it certainly will to the reader, to be a matter of no small surprise, that an improvement

* Count Cassini, in the Memoirs of the Parisian Academy for 1787, p. 29, informs us, that a memoir *sur les moyens de perfectionner les lunettes achromatiques par l'interposition d'un fluide entre les objectifs*, by the Abbé Rochon, was read before that Academy in January 1774, and inserted in the Recueil des Memoires de Mechanique et de Physique, which I have not seen; and in a note on this passage he quotes the Report of the Commissioners of the Academy, who express their satisfaction at the result of their trials for correcting great part of the defects arising from the surfaces of the glasses by the interposition of a fluid between them, but recommend trials with the telescopes of astronomers on celestial objects; which it should therefore seem had not then been made. The same author proceeds to acquaint us, that Mr. Grateloup used mastic in 1785, after which the Sieur Putois made excellent lenses of this kind; and that the mastic answered well with compound lenses, whose interior surfaces were only ground (doucies) but not polished. In this last experiment, however, it can hardly be imagined that the lens would have supported an high power; because the mastic, though it might afford transparency, must have left the confines of the mediums as imperfect as before.

of such value should never have been adopted in this kingdom. As it is no where said that lenses were constructed with a particular regard to the density and figure of the interposed mastic; but there seems reason to conclude, that in every case an achromatic lens of the usual construction was stated to have been rendered more perfect by this treatment,—I thought it best to overlook the theoretical objections, and confine myself in the first instance to experiment and enquiry. For this purpose I took an achromatic lens, consisting of two convex lenses and one concave, of which the radii need not here be detailed. When together they formed a compound lens of $7\frac{3}{4}$ inches focus, with an aperture of nearly $1\frac{1}{2}$ inch, which, upon a rough trial with a magnifying power of 30 times, exhibited scarcely any colour. Upon filling the interstices of this lens with water, the focal image became much brighter; but the focal distance was diminished to somewhat less than six inches, and much colour was produced with as low a magnifying power as ten times. It is evident, therefore, that the lens was greatly injured by the expedient made use of to prevent the loss by reflexion; and it may easily be inferred that a denser substance, such as mastic, would have shortened its focus and disturbed its compensations in a still greater degree.

Some time after making this experiment, I applied to the celebrated Mr. Ramsden, to whose unequalled ability and invention the arts of philosophical observation are so highly indebted. I did not expect to receive any information tending to shew that a combination of lenses, differing in curvature at the surfaces of apposition and achromatic in the air, could retain the same property when either partially or totally immersed in another transparent medium of different refractive or dispersive power. My principal object was, to know whether the French philosophers had constructed lenses of six parts duly adapted to each other; that is to say, considering the two portions of mastic as lenses, and not merely as a medium to prevent loss by reflexion. From him I learned that the supposed improvement had long since been communicated to him by some gentlemen of the Paris academy, who affirmed that an achromatic lens of Dollond's make had been much improved by the interposition of mastic between its parts, at the same time that they asserted in general terms that the same might be done with any other object glass of this kind. He put a good object glass into their hands for this purpose. The experiment was made, and the result proved similar to that which I have already related. The aberrations from figure as well as colour became very perceptible;—and in a word the lens, though much clearer, had lost its most valuable qualities.

Such is the result of a process which has been in the possession of our learned neighbours for about four-and-twenty years, and is still esteemed by them; but which has never been adopted by our opticians:—a process, the value of which, it should appear, might be ascertained in an instant by mere inspection through the telescope in question. We seem as if driven to the supposition that something might yet remain unexplained. Has the mischief of this process been overlooked by men so acute and enlightened?—Or has it happened that the interior curvatures of the lenses, on which they operated, were nearly parallel to each other; so that the addition of light (with the magnifying power commonly used with an index telescope) might be thought to offer an advantage, in angular observations, more than sufficient to compensate for the other defects?

IX. De-

IX.

Description of a very simple Apparatus for performing the great Experiment of producing Water by the Combustion of Hydrogen Gas. Invented by Mr. JOHN CUTHBERTSON *.

A D B C, fig. 6, plate IX., represents the instrument standing in a vessel, a b c d, nearly filled with water.

A D is a large glass bottle, holding about 1000 cubic inches of water, mounted at the top with a brass cap, which screws off at A. The rising part at the bottom is bored through and mounted with brass, in which screws a piece represented by a b, fig. 7, having a part at one end which fits into the hole, and the other end a shank with a screw at the bottom. E F, fig. 6, is a straight bar of brass; B C are two glass receivers, mounted at the top with brass, each having a shank which passes through the brass bar, and by means of a female screw is screwed fast to it: these mountings are perforated perpendicularly, and have also a side hole, which corresponds with a hole in the brass bar, passing from the shank of each receiver; which hole also corresponds with two holes represented in fig. 7, communicating with the inside of the large bottle. At L, M, in the brass bar, are two air cocks, with holes drilled through them in such a direction, that when the blades of the cocks stand lengthwise with the bar they correspond with the holes in it. F R and E N are two straight flat pieces of brass; in each of them is a hole, through which pass the ends of two screws, that are screwed fast into the sides of the vessel containing the water, and confine the bars by means of two female screws. O P is a long brass wire, with a piece of platina wire at the end P: this wire is fixed to the mounting at the top of the bottle, and is bended so that the end P stands as near as possible, but not to touch the small hole which passes through the piece a b, fig. 2.

When this instrument is to be used, the large glass must be first filled with oxygen gas (or common air when mere exhibition is intended): this may be done either by means of an air pump, or by filling it with water, inverting it and suffering the air to ascend. When either of these operations is to be performed, it must first be detached from the receivers by unscrewing the two female screws Q Q. All the other parts remain annexed to it, and care must be taken that the two air-cocks are shut. When it is filled with the air required, it must be set upon the receivers, the mountings of which are marked, as well as the bar, and care must be taken that they are not misplaced: the whole instrument must then be placed in the water vessel, as is represented. The receiver B has a hole about the middle at t: This receiver must be filled with oxygen gas, and C with hydrogen gas. When the airs or gases are to be lighted, electric sparks must be made to pass from the end of the platina to the hole in a continual succession; and while these sparks are passing, the cock L must be opened by degrees till the air is seen to take fire; the electric sparks must then cease, and the size of the flame may be regulated by turning the cock either one way or the other. The other cock must then be set either quite open, or partly so, accordingly as you find the flame affected by it. As you find the gases decrease in the receivers, fill them by certain known measures, and continue the operation †.

SCIENTIFIC

* Communicated by George Pearson, M.D. F.R.S. &c.

† The above description was drawn up by Mr. Cuthbertson. I find, upon examination of the apparatus at Dr. Pearson's house, that the hole through which the hydrogen is introduced is made very small, but that the other hole

through

SCIENTIFIC NEWS.

Account of the Public Sitting of the National Institute of Sciences and Arts, held at Paris the 15th Messidor, in the Year VI. (July 3, 1798.)

CITIZEN Bitaubé took the chair at half past five o'clock, the citizens Villars and Andrieux performing the office of secretaries. Citizen Villars ascended the tribune, and gave an account of the operations of the class during the last trimestre, of which the following is an abstract:

A memoir of Cit. Camus on the typographic art, polytipage, and the art of composing geographical charts, such as have been executed by the celebrated Haas at Basle.

A memoir of Cit. Langles on the Arabian poets who preceded Mahomet. This young and laborious student shewed that, even at a period so remote, the greater part of these writings possessed considerable poetical merit.

Cit. Monges read a memoir on the ruins of Persepolis, in which he proves that the destruction by Alexander was not total, but that it subsisted long afterwards.

Cit. Papon, associate correspondent of the class of moral and political sciences, read to the class of literature a memoir on the advantages which may be obtained from the study of Greek and Roman inscriptions, in order to ascertain a variety of historical facts.

Cit. Lefevre-Gineau gave a short account of the object of the various mathematical memoirs which had been read during the last trimestre.

A memoir of Cit. Lamarck concerning the action of the moon upon the atmosphere. It has been observed, that when this satellite is to the north of the equator, the winds blow, for the most part, from the north; and that they pass to the west or south-west, which in these climates are attended with rain, when the moon is to the south of the equator.

Cit. Flangaques, associate correspondent at Viviers, transmitted to the class a memoir on the refraction of light.

Cit. Lebossut communicated a memoir on the integral calculus, which, besides the perspicuous disposition of its parts, exhibits some original methods invented by the author.

A memoir of the laborious and indefatigable Messier, on the comet of the 23d Germinal, (April 12). The orbit of this comet, which, after having been seen for 43 days, disappeared in the constellation Ursa Major, has been calculated according to the method of Laplace. Fifty comets have already been observed by Messier, of which 21 were discovered by him.

Cit. Lassus, who was appointed to give an account of the memoirs on natural philosophy and chemistry, mentioned a memoir of Cit. Guyton on the affinities and the decomposition of salts at a temperature beneath the freezing point.

through which the oxygen passes is of considerable magnitude. I conclude that these dimensions of the apertures were ascertained by experiment, as the best suited to the complete though slow combustion of the hydrogen. As the bulk of hydrogen gas required to be introduced, for perfect combustion, is more than twice that of the oxygen, and the friction through the small aperture is very great, it becomes necessary that the pressure to extrude the former should be the strongest. The hole t renders it impossible to extrude the oxygen by the reaction of any longer column of water, than from that hole to the surface of the water in the tub; but the pressure which can be exerted in the vessel B is nearly twice as much. N.

Cit.

Cit. Clouet, aſſociate correſpondent at Ouzour, near Gien, tranſmitted a memoir to the claſs, deſcribing a method of converting iron into ſteel without cementation. He makes uſe of ſilex, alumine, &c.

Cit. Baumé, likewiſe aſſociate correſpondent, read to the claſs a memoir on the material of which artillery is made, and the cauſe of the degree of heat they acquire after a few diſcharges. He offers a new compoſition leſs ſuſceptible of becoming heated.

Cit. Gibert treated of fleece-bearing animals, or ſheep, and the advantages which commerce may derive from their produce. Lamarck gave an account of the cuttle fiſh; and Cit. Cuvier, of the internal diſpoſition of the throat of birds, by means of which they utter ſounds more or leſs acute. Cit. Deſeſſarts was the author of an excellent memoir on the ſmall-pox, with a ſhort account of Galvaniſm.

The claſs of mathematical and phyſical ſciences had propoſed as the ſubject of a prize, to be determined on the 15th Vendemiaire, in the year VII, (October 6, 1798) for which the productions of candidates were to be received until the firſt of Germinal laſt, (March 21, 1798) the following queſtion:

What are the uſes of the liver in the various claſſes of animals?

One memoir only has been received, which does not apply to the queſtion. The claſs has therefore thought proper to divide it, in order to facilitate the ſolution. In this form it becomes the object of two prizes. The queſtion for the firſt prize is,

To aſcertain the form, ſituation, magnitude, comparative weight, texture of the veſſels, communications, and appendices to the liver, conſidered in the principal claſſes of animals from man to inſects, molluſcæ, and worms.

The object of the ſecond prize is,

The analyſis of the hepatic or cyſtic bile in the ſeveral claſſes of animals here mentioned.

Memoirs will be received till the firſt Nivoſe, in the year VIII, (December 21, 1799) and the prize will be aſcertained on the 15th of Germinal following (April 4). The prize for each queſtion will be a medal of gold weighing one kilogramme (22966 grains, or 47 oz. 17 dwts. troy). The Inſtitute has publiſhed a program, exhibiting the conditions to which candidates muſt conform.

A prize was decreed. It was divided between two different works; but in effect it proved but one, becauſe the ſame perſon was entitled to both. In the year IV the claſs of mathematical and phyſical ſciences propoſed the following object:

To conſtruct a pocket watch proper to determine the longitude at ſea, by obſerving ſuch diviſions as indicate the decimal parts of the day, namely, tenths, thouſandths, and ten thouſandths; or the ſyſtem in which the day ſhall be divided into ten hours, the hour into one hundred minutes, and the minute into one hundred ſeconds.

The Inſtitute divided the prize between two watches, No. 1. bearing the epigraph "Ma liberté fait ma conſtance;" and No. 2. having the inſcription "Au tems qui inſtruit."

Cit. Louis Berthout conſtructed both watches. His name was proclaimed amidſt the applauſes of the aſſiſtants.

The claſs of moral and political ſciences propoſed the two following queſtions, one relative to the ſocial ſcience, and the other to geography:

What ought to be the extent and the limits of power in the father of a family in a well-conſtituted republic?

The reception of memoirs will terminate on the 15th Meſſidor, in the year VII, (July 3, 1799) and the prize will be diſtributed the 15th Vendemiaire, in the year VIII (October 6, 1799).

To determine the great changes which have taken place in the globe of the earth, and are either indicated or proved by hiſtory.

The concurrence will ceaſe on the 5th Vendemiaire, in the year VIII, (October 6, 1799) and the prize will be diſtributed on the 15th Nivoſe following (January 4, 1800).

The ſame claſs had two prizes to diſtribute at this ſitting. One on the queſtion, 1. For what objects, and on what conditions, is it proper that a republican ſtate ſhould open a public loan? The other on this queſtion: 2. What are the inſtitutions moſt proper to eſtabliſh the morals of a people?

The memoirs tranſmitted not having complied with the conditions of the program, the Inſtitute propoſes again the queſtion reſpecting loans for the year VII. The concurrence will cloſe on the 7th Meſſidor (June 25, 1799), the prize will be given on the 15th Vendemiaire following (October 6, 1799); and the claſs of moral and political ſciences, at its next public ſitting, will give a new program for the development of the queſtion reſpecting inſtitutions. Among the memoirs which have been received, three are noticed as worthy of diſtinction, reſpectively bearing the epigraphs, 1. Si forte neceſſe eſt . . . fingere . . . non exaudita . . . contingent; dabiturque licentia ſumpta prudenter. 2. Nec enim ulla res vehementius rempublicam continet, quam fides. 3. On n'honore pas la vertu; on la reſpecte. The prize propoſed for theſe four queſtions will be a gold medal of the weight of five hectogrammes (11483 grains, or 23 oz. 18 dwts. troy).

Cit. Lacuée, ſecretary, in a ſhort account of the labours of his claſs, ſpoke of two memoirs of Cit Deliſle-Deſalles, and another of Cit. Rœderer, all three concerning the government of China. It cannot be diſſembled, that it will always be very difficult in France to acquire a perfect knowledge of the government of a nation, of which the laws cannot be ſtudied in the country itſelf, or at leaſt in the writings of that people. Depending on the report of other writers, who themſelves may not have been perfectly informed, the two members of the Inſtitute have maintained different opinions, and each refers to his authorities. Cit. Deſalles affirms that this government is tyrannical, and brings facts in proof of this aſſertion. Cit. Rœderer, from the ſtability of the government, deduces a proof that its baſis is republican. He eſtabliſhes, with much acuteneſs, a difference between the patriarchal government, in which a ſingle chief influences the whole family, and the paternal government, in which the authority is divided into as many branches as there are married individuals. He likewiſe treats of the ſyſtem of the Chineſe writing.

Cit. Villeterque, aſſociate correſpondent, read a memoir ſomewhat abſtracted on the difference between experiment and reaſoning in philoſophy.

Cit. Goſſelin was mentioned as the author of another memoir, in which he has developed the geographical ſyſtem of Polybius. It is known that this expreſſion denotes the ideas which that hiſtorian entertained reſpecting the mathematical diviſions of the ſurface of the globe, and the ſituations of places. The ſame ſkilful geographer has before proved that Eratoſthenes, Strabo, and Ptolemy, had very incorrect notions of this ſcience.

Certain

Certain memoirs of Cit. Bouchad, on the legiſlation of the Romans, and their numiſmatic hiſtory, were mentioned; and after theſe accounts the preſident ſucceſſively announced the readings.

Cit. Lefevre-Gineau read for Cit. David Leroy a memoir on the ſhips of the ancients, in which he proves that we may derive uſeful inſtruction from them. Cit. Delambre read a memoir on the meaſure of a baſe taken in the neighbourhood of Melun, for determining the length of the meridian.

Cit. Fleurieu read a memoir on the civilization of the north-weſt parts of America, where the traces are found of acquiſitions which do not belong to nations abſolutely ſavage; ſuch as ingenious conſtructions, ſculptures and indifferent paintings repreſenting animals, the parts of the human body, &c. He concludes that their anceſtors fled to theſe countries from the ravages of the Spaniards during the conqueſt of Mexico.

Cit. Chaptal read a memoir on the yellow colour obtained from vegetables.

Cit. Monges diſcuſſed the queſtion, whether the two antique ſtatues known by the names of the gladiator and the dying gladiator do really repreſent gladiators. He clearly eſtabliſhes the difference which exiſted between the gladiators of the Romans and the athletics of the Greeks. The firſt were barbarians, whoſe lives were mercileſsly expoſed; the ſecond, Greeks, and ſometimes heroes, who diſtinguiſhed themſelves by their ability in perſonal activity. The author does not believe that the ſtatues in queſtion are entitled to the denominations they bear.

Cit. Colin d'Harleville himſelf read a dialogue, entitled, "Man and his conſcience," a very well written philoſophical production. Cit. Lalande read a piece on the variation of the magnetic pole.

The ſitting was terminated by Cit. Molé, who delivered a very happy tale written by Cit. Andrieux, entitled, The Dean of Badajoz.

THE public attention has lately been much excited by a project of Mr. R. Dodd, engineer, for a communication between the counties of Kent and Eſſex, by a tunnel or ſubterraneous road beneath the Thames, from Graveſend to Tilbury. The particulars as they appear in the newſpapers, inſerted as I apprehend by the engineer himſelf, are as follow:

The tunnel is to be cylindrical, and lined on all ſides with ſtone keyed together in the manner of an arch; the internal diameter to be ſixteen feet, which Mr. Dodd imagines will be ſufficient for foot, horſe, and carriage paſſengers. The paſſage muſt of courſe be illuminated with lamps, and a ſteam-engine is propoſed to be erected in a proper ſituation to draw off the drainage water if any ſhould accumulate. The eſtimate is ſtated as follows:

To 900 yards (running meaſure) of tunneling, including excavations, vaulting with key-ſtones, &c. at 12l. per yard - - - -	£. 10,800
To relaying the bottom with new made ground 900 yards at 1l. each -	900
To placing lamps and lamp irons through the tunnel, collectors' rooms, and gates at each end - - - - -	400
To making good the entrance roads at each end of the tunnel - -	160
To a ſteam-engine to draw off drainage water - - -	1,780
Neceſſary machinery during the execution - - -	500
To ten per cent. upon the whole for contingencies - - -	1,415
Total	£. 15,955

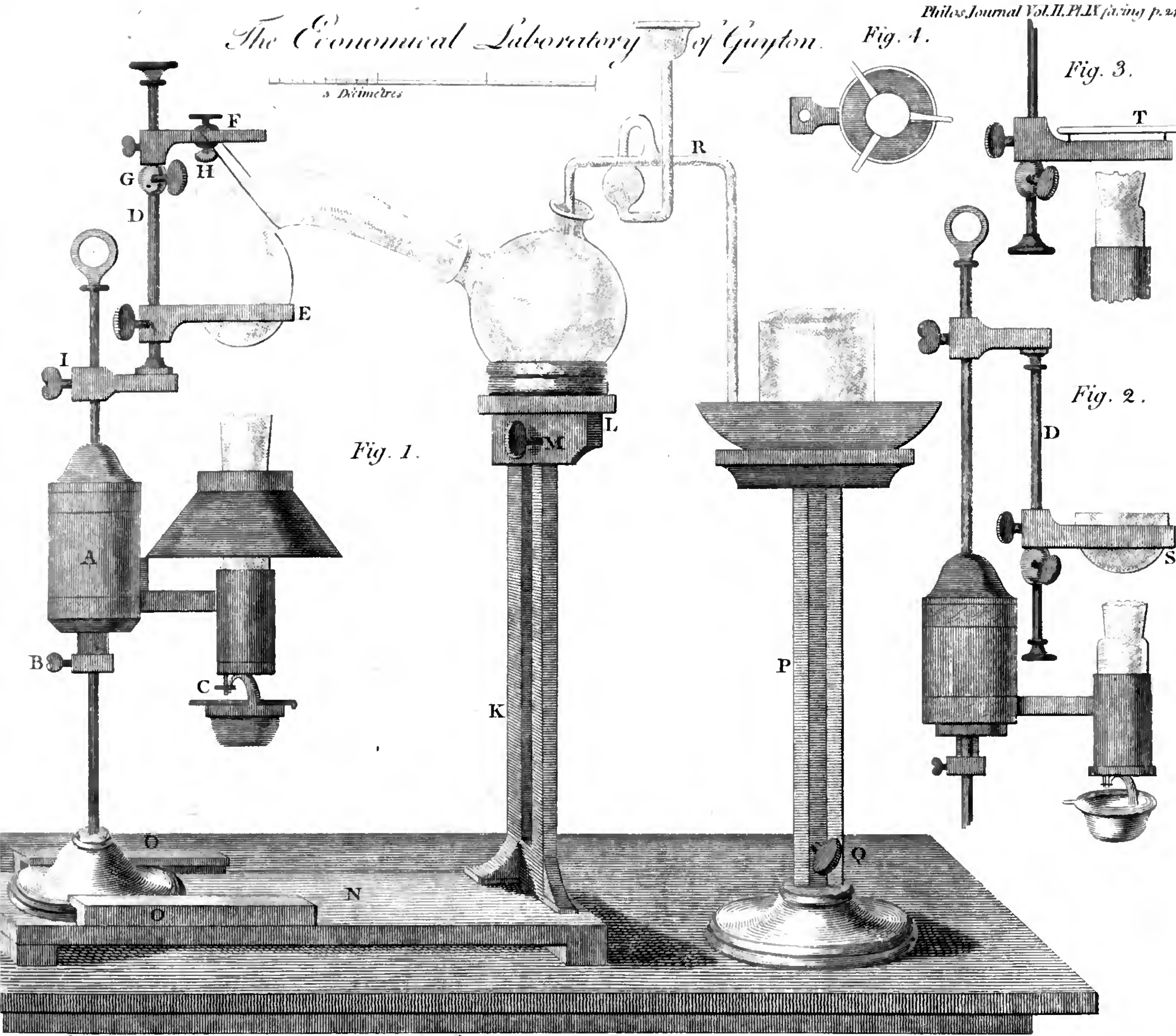
I do not find, from the enquiries I have yet been able to make, that any effective ſteps have been taken towards carrying this plan into execution. It may, as Mr. D. obſerves, be done either at the expence of the two counties, or by a private ſharing company under an act of Parliament of the ſame tenor as is uſually obtained for canals. But whatever may be the mode, there can be no doubt of the very great commercial advantages of ſuch a communication. It would demand a greater degree of attention and reſearch into the circumſtances of local ſituation; ſuch as the depth and poſition of the main channel, the elevations of the banks, the nature of the ground, with the prices of materials and workmanſhip, than on the preſent occaſion can be thought of, to form any judgment of the eſtimate or the difficulties which may attend this important work. To the great public, Mr. Dodd's propoſal will appear intereſting in a more general point of view. When it is conſidered that the practice of making tunnels has now been known for a conſiderable number of years in this kingdom; that in many, if not moſt inſtances, a tunnel will be much cheaper, and full as ſafe, durable and convenient as a bridge; that the tunnel may be made (at leaſt in favourable circumſtances of the ground) in ſituations where a bridge would be impracticable, and will leave that moſt valuable deſideratum the clear navigation above unimpeded:—this happy thought of Mr. Dodd may be allowed to promiſe beneficial conſequences of the greateſt magnitude to the community.

Dr. Herschel has diſcovered four additional ſatellites of the Georgium Sidus. His paper on this object is inſerted in the late publication of the Royal Society *. By recurring to his former communication on this ſubject †, the two old ſatellites are found to revolve, the firſt in 8 days 17 h. 1 m. 17 ſec. at the diſtance of 33″ from its primary, and the ſecond in 13 d. 11 h. 5 m. 1,5 ſec. at the diſtance of 44″,23. The planes of their orbits form ſuch large angles with that of the planet itſelf, and conſequently to the ecliptic, as to be almoſt perpendicular to it. To this remarkable departure from the analogy of the old planets another ſtill more ſingular is now announced. *They move in a retrograde direction!* The new ſatellites revolve as follows, the periodical times being inferred from their greateſt elongations: The interior ſatellite in 5 d. 21 h. 25 m. at the diſtance of 25″,5.—A ſatellite intermediate between the two old ones in 10 d. 23 h. 4 m. at the diſtance of 38″,57.—The neareſt exterior ſatellite at about double the diſtance of the fartheſt old one, and conſequently its periodical time 38 d. 1 h. 49 m.—And the moſt diſtant ſatellite full four times as far from its primary as the old ſecond ſatellite. Whence it will take at leaſt 107 d. 16 h. 40 m. to complete its revolution. Whether the motions of theſe four be direct or retrograde, is, I ſuppoſe, not yet determined.

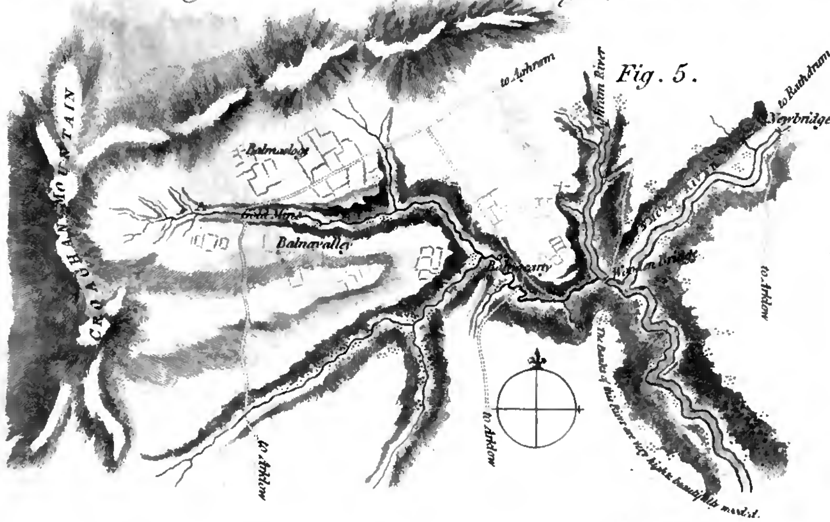
From ſome obſervations of the Doctor, with an excellent ſeven foot teleſcope, certain appearances reſembling that of two rings ſurrounding the planet, and croſſing each other at right angles, were ſeen on ſeveral different days. They were not altered in poſition by turning the ſpeculum in its cell; but there is little doubt that they were optical deceptions, becauſe they kept their poſition with reſpect to the tube, after the relative poſition of the parallel had been much changed by the earth's rotation, and becauſe they did not appear with larger teleſcopes applied during the courſe of ten years. The diſk of the Georgium Sidus is flattened. It therefore revolves with conſiderable rapidity on its axis. From the very faint light of the ſatellites, they are obſerved to diſappear in thoſe parts of their orbits which bring them apparently neareſt the planet. This does not ariſe from an atmoſphere; for the effect is the ſame, whether the ſatellite be within or beyond the planet.

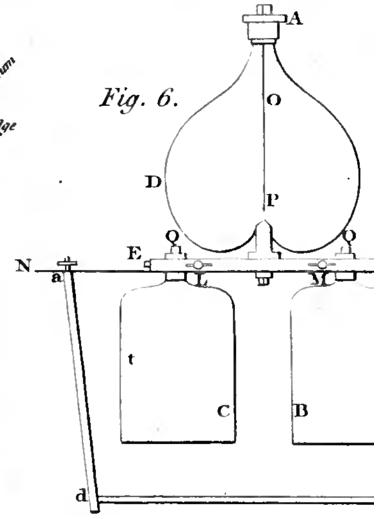
* Phil. Tranſ. 1798, p. 47. † Ibid. 1788.

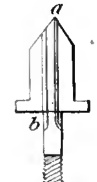
The Economical Laboratory of Guyton.



Sketch of the Gold Mine in the County of Wicklow.

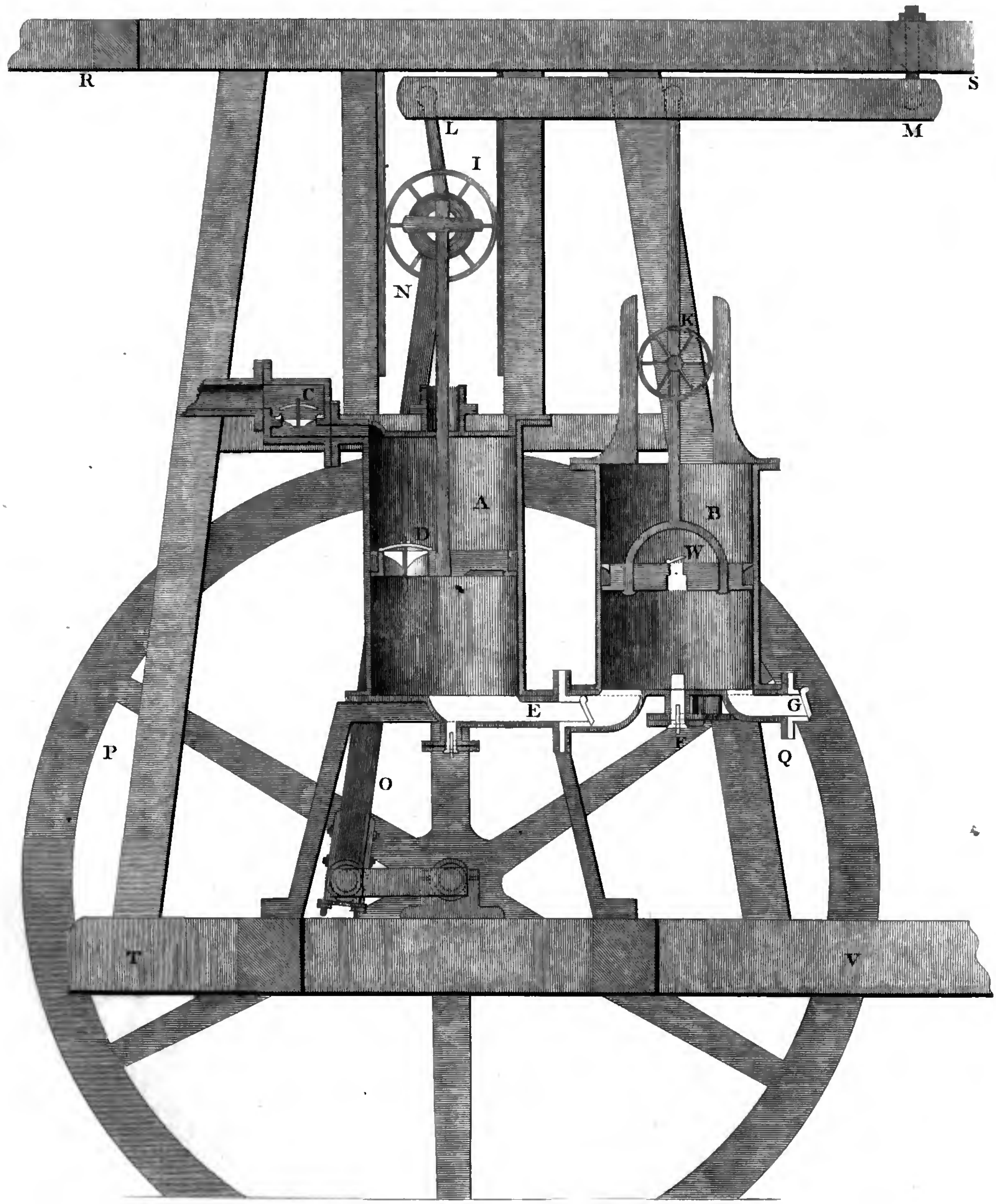






Barlow sculp.

Steam Engine by J. Sadler Esq.r



Barlow sculp.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SEPTEMBER 1798.

ARTICLE I.

*Experiments on carbonated Hydrogenous Gas; with a View to determine whether Carbon be a simple or a compound Substance *. By Mr. WILLIAM HENRY.*

THE progreſs of chemical ſcience depends not only on the acquiſition of new facts, but on the accurate eſtabliſhment, and juſt valuation, of thoſe we already poſſeſs: for its general principles will otherwiſe be liable to frequent ſubverſions; and the mutability of its doctrines will but ill accord with the unvaried order of nature. Impreſſed with this conviction, I have been induced to examine a late attempt to withdraw from its rank among the elementary bodies, one of the moſt intereſting objects of chemiſtry. The inferences reſpecting the compoſition of charcoal, deduced by Dr. Auſtin from his experiments on the heavy inflammable air †, lead to changes ſo numerous in our explanations of natural phenomena, that they ought not to be admitted without the ſtricteſt ſcrutiny of the reaſoning of this philoſopher, and an attentive repetition of the experiments themſelves. In the former, ſources of fallacy may, I think, be eaſily detected; and in the latter there is reaſon to ſuſpect that Dr. Auſtin has been miſled by inattention to ſome collateral circumſtances. Several chemiſts, however, of diſtinguiſhed rank have expreſſed themſelves ſatisfied with the evidence thus produced in favour of the compoſition of charcoal; and amongſt theſe it may be ſufficient to mention Dr. Beddoes, who has availed himſelf of the theory of Dr. Auſtin in explaining ſome appearances that attend the converſion of caſt into malleable iron ‡.

The heavy inflammable air, having been proved to conſiſt of a ſolution of pure charcoal

* From the Phil. Tranſ. 1797. † Phil. Tranſ. vol. lxxx. p. 51.

‡ Phil. Tranſ. vol. lxxxi.

in light inflammable air, is termed in the new nomenclature, carbonated hydrogenous gas. By repeatedly paffing the electric fhock through a fmall quantity of this gas, confined in a bent tube over mercury, Dr. Auftin found that it was permanently dilated to more than twice its original volume. An expanfion fo remarkable could not, as he obferves, be occafioned by any other known caufe than the evolution of light inflammable air.

When the electrified air was fired with oxygenous gas, it was found that more oxygen was required for its faturation than before the action of the electric fluid; which proves that by this procefs an actual addition was made of combuftible matter.

The light inflammable air difengaged by the electrization proceeded without doubt from the decompofition of fome fubftance within the influence of the electric fluid, and not merely from the expanfion of that contained in the carbonated hydrogenous gas: for, had the quantity of hydrogen remained unaltered, and its ftate of dilatation only been changed, there would not, after electrization, have been any increafed confumption of oxygen.

The only fubftances in contact with the glafs tube and mercury, in thefe experiments, befides the hydrogen of the denfe inflammable gas, were carbon and water; which laft, though probably not a conftituent of gafes, is however copioufly diffufed through them. If the evolved hydrogen proceeded from the decompofition of the former of thefe two fubftances, it is evident that a certain volume of the carbonated hydrogenous gas muft yield, after electrization, on combuftion with oxygen, lefs carbonic acid than an equal volume of non-electrified gas; or, in other words, the inflammation of 20 meafures of carbonated hydrogen expanded by electricity from 10, fhould not afford fo much carbonic acid as 10 meafures of the unelectrified.

From the fact which has been before ftated, refpecting the increafed confumption of oxygen by the electrified air, it follows, that in determining the quantity of its carbon by combuftion, fuch an addition of oxygen fhould be made, to that neceffary for the faturation of the gas before expofure to the electric fhock, as will completely faturate the evolved hydrogen. For, if this caution be not obferved, we may reafonably fufpect that the product of carbonic acid is diminifhed, only becaufe a part of the heavy inflammable air has efcaped combuftion. It might indeed be fuppofed, that in confequence of the fuperior affinity of carbon for oxygen, the whole of the former fubftance contained in the denfe inflammable gas would be faturated and changed into carbonic acid, before the attraction of hydrogen for oxygen could operate in the production of water. But I have found that the refidue, after inflaming the carbonated hydrogenous gas with a deficiency of oxygen, and removing the carbonic acid, is not fimply hydrogenous, but carbonated hydrogenous gas.

In the 2d, 5th and 6th of Dr. Auftin's experiments, in which the quantity of carbon in the electrified gas was examined by deflagrating it with oxygen, the combuftion was incomplete becaufe a fufficiency of oxygen was not employed; and Dr. Auftin himfelf was aware that in each of them "a fmall quantity of heavy inflammable air might efcape unaltered." It is obfervable alfo, that the product of carbonic acid from the electrified gas increafed in proportion as the combuftion was more perfect. We may infer, therefore, that if it had been complete there would have been no deficiency of this acid gas, and confequently no indication of a decompofition of charcoal. A ftrong objection, however, is applicable to thefe as well as to moft of Dr. Auftin's experiments, that the refidues were not examined with fufficient attention. In one inftance, we are told, that the remaining gas was inflammable,

mable, and in another that it supported combustion like vital air. I need hardly remark, that a satisfactory analysis cannot be attained of any substance, without the most scrupulous regard not only to the qualities, but to the precise quantities of the products of our operations.

To the 8th and 9th experiments the objection may be urged with additional weight, which has been brought against the preceding ones, that the quantity of oxygen, instead of being duly increased in the combustion of the electrified gas, was on the contrary diminished. Thus, in the 8th experiment 2,83 measures of carbonated hydrogen were inflamed with 4,58 measures of oxygenous gas; but in the 9th, though the 2,83 measures were dilated to 5,16, and had therefore received a considerable addition of combustible matter, the oxygen employed was only 4,09. To the rest of Dr. Austin's experiments, either one or both of the above objections are applicable.

The first and most important step, therefore, in the repetition of these experiments, is to determine whether the carbonated hydrogenous gas really sustains by the process of electrization a diminution of its quantity of carbon; because, should this be decided in the negative, we derive from the fact a very useful direction in ascertaining the true source of the evolved hydrogen. The following experiments were therefore made with a view to decide this question, and the error of Dr. Austin in employing too little oxygen was carefully avoided *.

Experiment 1. In a bent tube standing inverted over mercury, 94,5 measures of carbonated hydrogenous gas from acetite of pot-ash were mixed with 107,5 of oxygen. The total, 202, was reduced by an explosion to 128,5, and was further contracted by lime water to 54. A solution of hepar sulphuris left only 23 measures.

The diminution by lime water, viz. 74,5 measures, makes known to us the quantity of carbonic acid afforded by the combustion of 94,5 measures of carbonated hydrogenous gas. And the residue after the action of hepar sulphuris, viz. 23 measures, gives the proportion of azotic gas contained in the carbonated hydrogen; for the oxygenous gas employed, which was procured from oxygenated muriate of pot-ash, was so pure, that the small quantity used in this experiment could not contain a measurable portion of azotic gas.

Experiment 2. The same quantity of carbonated hydrogen was expanded by repeated electrical shocks to 188 measures. The addition of hydrogenous gas therefore amounted to 93,5. The gas thus dilated was fired at different times with 392,5 measures of oxygenous gas; and the residue after these several explosions was 203 measures. Lime water reduced it to 128,5, and sulphure of pot-ash to 19,5. In this instance, as in the former one, the product of carbonic acid is 74,5 measures.

* The apparatus employed in these experiments was the ingenious contrivance of Mr. Cavendish, and is described in the lxxvth vol. of the Philosophical Transactions. In dilating the gas, I sometimes used a straight tube, furnished with a conductor in the manner of Dr. Priestley (see his Experiments on Air, vol. i. plate 1. fig. 16.) The bulk of the gases introduced, and their volume after the various experiments, were ascertained by a moveable scale, and by afterwards weighing the mercury which filled the tube to the marks on the scale; by which means I was spared the trouble of graduating the syphons. Each grain of mercury indicates one measure of gas; and though the smallness of the quantities submitted to experiment may be objected to, yet this advantage was gained, that the electrified gas could be fired at one explosion, as was done in the 4th, 6th and 8th experiments. Errors from variations of temperature and atmospherical pressure were carefully avoided.

 Finding

Finding from the firft experiment and other fimilar ones, that the carbonated hydrogenous gas which was the fubject of them contained a very large mixture of azotic gas, I again fubmitted to diftillation a quantity of the acetite of pot-afh, with every precaution to prevent the adulteration of the product with atmofpherical air. Such an adulteration, I have obferved, impedes confiderably the dilatation of the gas, and for a time even entirely prevents it. This explains the failure, which fome experienced chemifts have met with in their attempts to expand the carbonated hydrogenous gas by electricity. Gas which is thus vitiated becomes, however, capable of expanfion after expofure to the fulphure of pot-afh.

Experiment 3. Carbonated hydrogen 340 meafures were exploded with the proper proportion of oxygenous gas. The carbonic acid produced amounted to 380 meafures, and the refidue of azotic gas was 20 meafures.

Experiment 4. The fame quantity, when expanded to 690, gave on combuftion 380 meafures of carbonic acid, and 19,8 of azotic gas.

Experiment 5. Three hundred and fifteen meafures of carbonated hydrogen yielded 359 meafures of carbonic acid, and 18,5 meafures of azote.

Experiment 6. The fame quantity, after expanfion to 600, afforded the fame products of carbonic acid and azotic gafes.

Experiments 7 and 8. As much carbonic acid was obtained by the combuftion of 408 meafures of carbonated hydrogenous gas, expanded from 200, as from 200 meafures of the non-electric fired gas; and the refidue of azotic gas was the fame in both cafes.

It is unneceffary to ftate the particulars of feveral other experiments fimilar to thofe above related, which were attended with the fame refults. They fufficiently prove that the action of the electric fpark, when paffed through carbonated hydrogenous gas, is not exerted in the decompofition of carbon; for the fame quantity of this fubftance is found after as before electrization. Even granting that charcoal is a compound, the conftituents of which are held together by a very forcible affinity, it does not appear likely that the agency of the electric fhock, which feems in this inftance analogous to that of caloric, fhould effect its decompofition under the circumftances of thefe experiments. For it is a known property of charcoal to decompofe water, when aided by a high temperature; and its union with oxygen is a much more probable event when this body is prefent, than a feparation into its conftituent principles. As an argument alfo that water is the fource of the light inflammable air in this procefs, it may be obferved that the dilatation in Dr. Auftin's experiments could never be carried much farther than twice the original bulk of the gas *. This fact evidently implies that the expanfion ceafed only in confequence of the entire deftruction of the matter whofe decompofition afforded the light inflammable air; and this fubftance could not be carbon, becaufe Dr. Auftin admits that a large portion, and I have fhewn that the whole of it, ftill remains unaltered.

If the dilatation of the carbonated hydrogenous gas arofe from the decompofition of water, the effect fhould ceafe when this fluid is previoufly abftracted. To afcertain whether

* "After the inflammable air has been expanded to about double its original bulk," fays Dr. Auftin, "I do not find that it increafes further by continuing the fhocks. Conceiving that the progrefs of the decompofition was impeded by the mixture of the other airs with the heavy inflammable, I paffed the fpark through a mixture of the heavy inflammable air and light inflammable; but the expanfion fucceeded nearly as well as when the heavy inflammable was electrified alone." Phil. Tranf. vol. lxxx. p. 52.

this

this consequence would really follow, I exposed a portion of the gas for several days before electrization to dry caustic alkali. On attempting its expansion, I found that it could not be carried beyond one sixth the original bulk of the gas. By 160 very strong explosions it attained this small degree of dilatation, but 80 more produced not the least effect; though the former number would have been amply sufficient to have dilated the gas in its ordinary state to more than twice its original volume. A drop or two of water being admitted to this portion of gas, the expansion went on as usual; and I may here observe, that when a little water gained admission into the tube along with the gas, in any experiment, which often happened before I had acquired sufficient expertness in transferring the air from water to mercury, the dilatation went on with remarkable rapidity.

Carbonic acid gas, according to the discovery of M. Monge *, undergoes, when submitted to the electric shock, a change similar to that effected on the carbonated hydrogen; and the expansion has been shewn by Messieurs Landriani, and Van Marum †, to be owing to the same cause, viz. the extrication of light inflammable air. The added gas, M. Monge ably contends, cannot proceed from any other source than the water held in solution by all aeriform bodies, the oxygen of which he supposes to combine with the mercury. That the decomponent of the water, however, in the experiments which I have described, is not a metallic body, will appear highly probable, when we reflect that there is present in them a combustible substance, viz. charcoal, which attracts oxygen much more strongly than metals; and the following experiments evince, that the mercury by which the air was confined had no share in producing the phenomena.

Experiment 9. A portion of carbonated hydrogenous gas was introduced into a glass tube closed at one end, into which a piece of gold wire was inserted, that projected both within and without the cavity of the tube. The open end of the tube was then closed by a stopper perforated also with gold wire, so that electric shocks could be passed through the confined air without the contact of any metal that has the power of decomposing water. On opening the tube with its mouth downwards under water, a quantity of air immediately rushed out.

Experiment 10. The dilatation of the gas was found to proceed very rapidly when standing over water, and exposed to the action of the electric fluid, conveyed by gold conductors.

We have only, therefore, in the two preceding experiments, one substance in contact with the gas which is capable of decomposing water, viz. charcoal. The union of this body with the oxygen of the water would be rendered palpable by the formation of carbonic acid; but Dr. Austin did not observe that any precipitation was occasioned in lime water by agitating it with the electrified gas. On passing up syrup of violets to the electrified air, with the expectation of its indicating the volatile alkali, as in the experiments of Dr. Austin, no change of colour took place, though the test was of unexceptionable purity. On examining, however, whether any alteration of bulk had been produced in the air by the contact of this liquid, it appeared that of 709 measures 100 had been absorbed. Suspecting that the absorption was owing to the presence of carbonic acid, I introduced some lime water to a volume of the expanded gas, amounting to 556 measures, when they were immediately reduced to 512. The contraction would probably have been still more remark-

* Journal de Physique, xxix. 277. † Annales de Chimie, ii. 273.

able,

able, if the gas had been farther expanded before the admiſſion of the liquid. The change in the lime water was very trifling; but my friend Mr. Rupp, who witneſſed this as well as ſeveral of the other experiments, and who is much converſant in the obſervation of chemical facts, was ſatisfied that after a while he ſaw ſmall flocculi of a precipitate on the ſurface of the mercury. This contraction of bulk cannot be aſcribed to any other cauſe than the abſorption of carbonic acid; for, beſides the fact, that the colour of ſyrup of violets and of turmeric, which I alſo tried, were not affected by expoſure to the electrified gas, I have this objection to the abſorbed gas being ammoniac, that no diminution either of bulk or tranſparency occurred on the admixture of muriatic acid gas with the electrified air; whereas ammoniac would have been exhibited under the form of a neutral ſalt. When water was paſſed up to this mixture of the two gaſes, there was an abſorption not only of the muriatic gas but of ſomething more.

Conceiving that the demolition of charcoal, by the action of the electric fluid, was ſufficiently proved by his experiments, Dr. Auſtin aſſigns the evolved hydrogen as one of its conſtituents, and the other he concludes to be azote. This inference, however, reſts almoſt entirely upon eſtimates in which material errors may be diſcovered. Some of theſe it may be well to point out for the ſatisfaction of ſuch as have acquieſced in Dr. Auſtin's opinion.

The carbonated hydrogenous gas ſubmitted to Dr. Auſtin's experiments, clearly appears from his own account to have been largely adulterated with azotic gas. One ſource of its impurity he has diſcloſed, by informing us that the gas "had been very long expoſed to water*;" for Dr. Higgins has ſomewhere ſhewn, that the heavy inflammable air, after ſtanding long over water, leaves a larger reſidue of azote, on combuſtion, than when recently prepared †. It is probable alſo, that the proportion of azote derived from the water would increaſe with the time of its expoſure; and thus a fertile ſource of error is ſuggeſted, which appears wholly to have eſcaped Dr. Auſtin's attention. In repeating his experiments, I was careful that comparative ones, on two equal quantities of the electrified and unelectrified gas, ſhould be made without the intervention of any time that could vary the proportion of azote in either of the gaſes.

To the 9th experiment, in which the quantity of azote ſeems to have been increaſed to electrization, I muſt repeat the objection, that a ſufficiency of oxygenous gas was not uſed in the combuſtion. In the 8th experiment, 2,83 meaſures of the unelectrified air were fired with 4,17 oxygenous gas, and only 0,15 of the latter remained above what was ſufficient for ſaturation; but in the 9th, though the 2,83 meaſures were expanded to 5,16, the quantity of oxygen employed was 0,08 leſs than in the former experiment; and it may therefore be preſumed, that a ſmall quantity of inflammable air might eſcape unaltered, and might add apparently to the product of azote. In the 8th experiment, alſo, the portion of oxygenous gas that was more than ſufficient to ſaturate the carbonated hydrogen, would probably combine in part with the remaining azote, as in the experiments of Dr. Higgins ‡ and Dr. Prieſtley §. But in the 9th, the quantity of oxygenous gas was hardly ſufficient to ſaturate

* Phil. Tranſ. lxxx. 54.

† Similar facts reſpecting the deterioration of other gaſes by ſtanding over water may be ſeen in Dr. Prieſtley's Experiments on Air, vol. i. p. 59. 158. I found that oxygenous gas from oxygenated muriate of pot-aſh acquired by expoſure a few weeks to water, .125 its bulk of azotic gas.

‡ Experiments and Obſervations on Acetous Acid, &c. p. 295.

§ Phil. Tranſ. lxxix. 7.

both kinds of inflammable air after electrization, and could not therefore diminiſh the azotic gas. When the proportion of oxygen is duly increaſed, and the inflammation of the electrified air is performed in ſmall portions, there is no augmentation, but on the contrary a decreaſe of the quantity of the azote, as will appear on comparing the 1ſt and 2d of the experiments which I have related.

Two circumſtances were obſerved in the experiments of Dr. Auſtin, which have not been noticed in the preceding account of the repetition of them, viz. the appearance of a depoſit from the carbonated hydrogenous gas during its electrization, and the formation of ammoniac by the ſame proceſs. In ſome experiments which I made on the firſt portion of gas, both theſe facts were ſufficiently apparent; but neither of them occurred on electrifying the gas which was afterwards procured. Suſpecting that the ceſſation of them aroſe from the ſuperior purity of the latter portion from azotic gas, I paſſed the electric ſhock through a mixture of carbonated hydrogen, with about one fourth of its bulk of azote, and thus again produced the precipitate, which would have been of a white colour, if it had not been obſcured by minute globules of mercury, that were driven upwards by the force of the exploſion. An infuſion of violets was tinged green, when admitted to the electrified gas; but the change of colour did not occur inſtantly, as happens from the abſorption of ammoniacal gas; and required for its production, that the liquid ſhould be brought extenſively into contact with the inner ſurface of the tube. From this effect on a blue vegetable colour, we may infer that the precipitate was an alkaline ſubſtance, and probably the carbonate of ammoniac; but the quantity was much too minute to be the ſubject of a more deciſive experiment.

I ſhall conclude this memoir with a brief ſummary of the facts that are eſtabliſhed by the preceding experiments *. Thoſe included under the firſt head are deducible from the experiments of Dr. Auſtin.

1. Carbonated hydrogenous gas, in its ordinary ſtate, is permanently dilated by the electric ſhock to more than twice its original volume; and as light inflammable air is the only ſubſtance we are acquainted with, that is capable of occaſioning ſo great an expanſion, and of exhibiting the phenomena that appear on firing the electrified gas with oxygen, we may aſcribe the dilatation to the production of hydrogenous gas.

2. The hydrogenous gas, evolved by this proceſs, does not ariſe from the decompoſition of charcoal; becauſe the ſame quantity of that ſubſtance is contained in the gas after as before electrization.

3. The hydrogenous gas proceeds from decompoſed water; becauſe, when this fluid is abſtracted as far as poſſible from the carbonated hydrogenous gas, before ſubmitting it to the action of electricity, the dilatation cannot be extended beyond one-ſixth its uſual amount.

4. The decomponent of the water is not a metallic ſubſtance, becauſe carbonated hydrogenous gas is expanded when in contact only with a glaſs tube and gold, a metal which has no power of ſeparating water into its formative principles.

5. The oxygen of the water (when the electric fluid is paſſed through carbonated hydrogenous gas, that holds this ſubſtance in ſolution) combines with the carbon, and forms car-

* Since this paper was written, I have extended the inquiry to phoſphorated hydrogenous gas, which expands equally with the carbonated hydrogen; loſes its property of inflaming when brought into contact with oxygenous gas, and affords evident traces of a production of phoſphorous or phoſphoric acid.

bonic

bonic acid. This production of carbonic acid, therefore, adds to the dilatation occaſioned by the evolution of hydrogenous gas.

6. There is not, by the action of the electric matter on carbonated hydrogenous gas, any generation of azotic gas.

7. Carbon, it appears, therefore, from the united evidence of theſe facts, is ſtill to be conſidered as an elementary body; that is, as a body with the compoſition of which we are unacquainted, but which may nevertheleſs yield to the labours of ſome future and more ſucceſsful analyſt.

II.

Obſervations on Bituminous Subſtances, with a Deſcription of the Varieties of the Elaſtic Bitumen. By CHARLES HATCHETT, *Eſq. F.R.S. Lond. and Edin. F.L.S. &c.*

[Concluded from Page 209, Vol. II.]

THE characters of bitumen are much more apparent in turf and peat, than in the greater part of the foſſil woods. Turf is well known to be compoſed of the parts of vegetables, ſuch as ſmall roots, twigs, &c. mixed with a portion of petroleum; and peat is the ſame, excepting that it generally contains more of earthy matter, or that the vegetables have undergone a more complete decompoſition.

The boggy nature of the places in which they are found, proves that a certain degree of maceration is neceſſary to form the bituminous matter which they contain; and I have already noticed, that every fact appears to demonſtrate, that the bitumen is a product of thoſe vegetables, the remains of which conſtitute the other ingredient of turf and peat.

The different proportion of vegetable matter, of bitumen, and of earth, together with the different ſtate of the bitumen, as well as the degree of perfection reſpecting the formation of it from the vegetable principles, contribute to alter the properties and characters of the compound, and thus produce varieties. It is believed that theſe ſubſtances have been materially concerned in the formation of pit-coal, and ſome eminent mineralogiſts maintain that there is an uninterrupted ſeries which connects the varieties of turf and peat with thoſe of coal *.

SECT. V.

LITTLE need be ſaid concerning thoſe mixtures of bitumen with metals or their oxides which are ſometimes called the bituminous ores of mercury, copper and iron, for they ſhould rather be arranged with the adulterated or impure bitumens. Few of them contain the metallic ingredient in a proportion ſufficient to cauſe the compound to be worked as an ore; and the only exception with which I am acquainted, is the ſubſtance found at Idria, in Carniolia, compoſed of mercury mixed with bituminous matter, a quintal of which, according to Mr. de Born, affords from fifteen to twenty pounds of mercury †.

* Man findet in der natur einen ununterbrochenen übergang von dem raſen und papiertorf durch den moor oder ſumpftorf in den pechtorf, und von dieſem in die braun ſchiefer und pechkohle.—Widenmann, p. 630.

† Catalogue de la Collection des Foſſiles de Mlle. de Raab, tom. ii. p. 294, 348, and 400.

SECT. VI.

SECT. VI.

FROM the preceding obfervations it will appear, that although I have firft mentioned naptha in order that I might be better underftood in refpect to the degree of connection prevailing between the bituminous fubftances; yet, to have followed them from their origin and the period of their formation, I fhould rather have begun with thofe fubftances which moft clearly point out how much the vegetable kingdom has contributed to the production of them, with the probable occafional concurrence of animal fubftances.

That the latter have contributed in fome meafure to the forming of bitumen, we can only infer from the veftiges and exuviæ of animals, which fo commonly accompany bituminous fubftances: but no doubt can be entertained in refpect to vegetables; for it appears that bitumen is formed from them by long maceration, and by other proceffes at prefent unknown to us:

That when certain portions of vegetable matter remain undecompofed, and are mixed with the petroleum thus produced, the varieties of turf and peat are formed:

That wood in general contributes to the production of bitumen; but does not feem to retain it, after the formation of it, in fo confiderable a proportion as the foregoing fubftances:

That the bituminous matter thus formed, and occafionally feparated, is in different ftates according to the degree of infpiffation:

And laftly, with various proportions of carbonic and earthy matter, it forms jet, coal, and bituminous fchiftus; and with metallic fubftances it produces thofe compounds called bituminous ores.

SECT. VII.

ABOUT the year 1786 a new fpecies of bitumen was difcovered near Caftleton, in Derbyfhire, which much refembles, in elafticity and colour, the fubftance known by the name of cahout-chou, or Indian rubber.

M. de Born was, I believe, the firft who mentioned it *; but, as he appears to have known only one variety of this fingular fubftance, I am induced to hope that a defcription of many other varieties, which have fince been found, will not be unacceptable to this Society.

The elaftic bitumen, which refembles the cahout-chou, was firft difcovered in the cavities of a vein in the lead-mine called Odin, which is near the bafe of Mamtor, to the north of Caftleton. The ore of this mine (which is fuppofed to be one of the moft ancient in England) is galena, accompanied by fluor, calcareous and heavy fpars, quartz, blende, calamine, felenite, afphaltum, and the elaftic bitumen, although the latter is now rarely found †. Another fpecies of the elaftic bitumen has within about three years been found in a neighbouring rivulet; but I fhall not at prefent notice it, as I intend firft to defcribe the varieties of that which was firft difcovered, and which refembles the cahout-chou. In order to do this with more perfpicuity, I fhall defcribe the fpecimens belonging to my collection, according to the mode in which I have arranged them.

* Catalogue de la Collection de Mlle. de Raab, tom. ii. p. 77.

† I am indebted to the ingenious Mr. White Watfon, of Bakewell, for much information refpecting the local circumftances which attend this bitumen.

SPECIES THE FIRST.

A, No. 1.—Elaſtic bitumen of a yellowiſh brown colour, part of which is almoſt liquid like petroleum, and adheres to the fingers; the other part is of a darker colour, of a mammillary form, does not adhere to the fingers, and is ſoft and elaſtic. This is on a grey bituminous limeſtone, with white calcareous ſpar in the figure of hexaedral pyramids, forming that which is called the dog-tooth ſpar.

A, No. 2.—Bitumen of a yellowiſh brown, partly liquid, and partly elaſtic, which, however, adheres to the fingers; on pale grey limeſtone, with cryſtals of white fluor ſpar, blende, and galena.—On another part of the limeſtone are ſome globules of bitumen of a reddiſh brown, perfectly hard and brittle.

A, No. 3.—Dark brown bitumen of a ſtalactitical form, hard, but in ſome degree elaſtic.

A, No. 4.—Bitumen of a reddiſh brown, in the form of globules, ſome of which are elaſtic, and others hard: on browniſh-grey limeſtone, accompanied by cryſtallized white fluor, dogtooth calcareous ſpar, and pyrites in ſmall cryſtals, ſome of which are on the ſurface of the globules of bitumen.

A, No. 5.—The ſame of a darker brown, of a ſtalactitical form, hard and brittle; on pale brown calcareous ſpar, impregnated with bitumen.

A, No. 6.—Bitumen of a dark reddiſh brown, very hard; on pale brown ſparry ſtinkſtone, with grey limeſtone, in which are ſome coralloides.

A, No. 7.—Bitumen of a dark yellowiſh brown, elaſtic, but very ſoft, ſo that it adheres to the fingers.

A, No. 8.—The ſame thinly ſpread over grey ſparry ſtink-ſtone.

A, No. 9.—Bitumen of a browniſh olive colour, which becomes reddiſh brown by the air, but when oppoſed to the light it appears ſemi-tranſparent, and of a yellowiſh brown inclining to orange. It is ſoft, very elaſtic, and (when recently cut) adheres to the fingers.

A, No. 10.—The ſame of a darker brown, and harder in a ſmall degree. The ſpecific gravity of this ſpecimen is 0,9053; water being eſtimated at 10,000 (q. 1,000?) at temp. 60°.

A, No. 11.—Bitumen of a dark brown, harder than the former. This exactly reſembles the cahout-chou in the degree of elaſticity, and in the property which it poſſeſſes of removing the traces of black-lead.

A, No. 12.—The ſame, but rather harder.

A, No. 13.—The ſame of a blackiſh brown, which is ſlightly elaſtic when the weather is warm, but is brittle when cold.

A, No. 14.—The ſame of a blackiſh brown, nearly black, which ſcarcely poſſeſſes any elaſticity; it breaks, and reſembles aſphaltum in luſtre, colour and fracture.

A, No. 15.—The ſame of a reddiſh brown, perfectly hard and brittle. The characters of aſphaltum are complete in this ſpecimen. The ſpecific gravity is 10,233. (q. 1,233?)

The other ſpecies of elaſtic bitumen, which I ſhall diſtinguiſh by the letter B, has been found during the laſt three years in a rivulet which runs at the baſe of Mamtor, from weſt to eaſt, at a ſmall diſtance from Odin mine.—The varieties of it, in my poſſeſſion, are as follow:——

SPECIES THE SECOND.

B, No. 1.—Elaſtic bitumen, which, recently cut, exactly reſembles fine cloſe cork in colour and texture, but, by the air, in a few days it becomes of a pale reddiſh brown.—This forms a thin coat, which completely covers a maſs of elaſtic bitumen, which is ſoft, and of a browniſh olive colour, like A, No. 9.

B, No. 2.—The ſame, excepting that the coat or cruſt is much thicker.

B, No. 3.—The ſame, but the coating is thicker than that of No. 2, and the browniſh olive-coloured bitumen much leſs in quantity.

B, No. 4.—The ſame, excepting that the greater part of the maſs reſembles cork, ſo that only a very ſmall nucleus of the brown bitumen remains *.

B, No. 5.—The ſame, excepting that the bitumen, which is coated, is in the ſtate of aſphaltum. The ſpecific gravity of this ſpecimen is 0,9881.

B, No. 6.—Elaſtic bitumen, the whole maſs of which reſembles fine cork.—The ſpecific gravity is 0,9748.

B, No. 7.—The ſame, but friable, and apparently paſſing by decompoſition into an ochraceous coloured powder.

THE varieties of the firſt ſpecies of the elaſtic bitumen, or that which is like the cahoutchou, evidently appear to be formed from a naptha or petroleum, which, like that which produces the other ſimple bituminous ſubſtances formerly mentioned, is ſuſceptible of various degrees of inſpiſſation.

All the varieties of the firſt ſpecies, from No. 1, to No. 15, may be regarded as thus formed; for in theſe we can trace all the modifications comprehended between petroleum and aſphaltum: with this difference, that the intermediate modifications of this ſpecies have the remarkable property of elaſticity which is the moſt complete in the variety which occupies the middle place between petroleum and aſphaltum.

The ſecond ſpecies B, or that which reſembles cork, appears ſo different from that marked A, that it is not at firſt eaſy to conceive how they are connected, or at leaſt the difficulty muſt appear great to thoſe who have only ſeen ſpecimens of each ſpecies complete in their reſpective characters. But, from an attentive examination of many ſpecimens, and particularly of thoſe which I have deſcribed, I am convinced that the varieties of the ſpecies B are only modifications of the ſpecies A, produced probably by long maceration in the water of the rivulet in which this ſpecies is found, to the effects of which we may, with ſome appearance of reaſon, add the viciſſitudes of the ſeaſons, of air, and of the weather in general, as well as thoſe of reiterated moiſture and dryneſs occaſioned by the riſe and fall of the water of the rivulet; and what ſeems to corroborate this opinion is, that the ſubſtance, like cork, incruſts the ſpecies A, and appears to be only a change which has penetrated deeper into the ſubſtance of it in proportion to the duration of the cauſes which I have mentioned, ſo that at length the original ſubſtance no longer remains in its primitive ſtate. I do not believe, however, that this change ariſes from any alteration in the conſtituent principles, but merely

* One of the ſpecimens in my poſſeſſion, ſimilar to B, No. 4. weighs between 13 and 14 pounds.

from a partial and minute diſ-union or diſintegration of the particles of the original ſubſtance, as both ſpecies melt into one which is perfectly ſimilar. I muſt alſo add, that the ſpecies A burns eaſily, and with rapidity; but the ſpecies B burns with ſome difficulty, and crackles as if it had imbibed a quantity of water.

I have remarked, when the different varieties of the elaſtic bitumen were melted, that they completely loſt the elaſtic property, and a quantity of air or gas appeared to be diſengaged, particularly from the ſpecies B. I alſo obſerved, that the ſubſtances which remained after this operation, correſponded, in reſpect to conſiſtence, with thoſe which had been employed, as the following table will ſhew:

A, No. 7 and 8 . . . . . produced a thick liquid petroleum, not apparently different from that which is commonly known.

A, No. 9 . . . . produced a thicker petroleum, approaching to mineral tar.

A, No. 11 and 12 . . . produced mineral tar.

B, No. 6 . . . . produced the ſame, approaching to mineral pitch.

A, No. 13 . . . . produced mineral pitch.

A, No. 14 and 15 . . . did not ſuffer any change, but remained as at firſt, with all the characters of aſphaltum.

From what I have related, I ſuſpect that the elaſtic property is occaſioned by the interpoſition of very minute portions of air or ſome other elaſtic fluid between the parts of the bitumen, and that this takes place by reaſon of ſome unknown cauſe at the time of formation; but when theſe bitumens are melted, the elaſtic fluid is liberated, and the maſs loſes that fine ſpongy texture which I ſuſpect to have been the cauſe of the elaſtic property *.

Derbyſhire is well known as a country which exhibits, in the moſt ſtriking manner, the remarkable changes which our globe has ſuffered. In every part of it, the moſt indiſputable evidences appear of ſome great and extraordinary revolution; and there is not any place where extraneous foſſils, ſuch as the remains and impreſſions of vegetables and animals, are more abundant.

Bitumen, in other countries, is moſt commonly found where theſe preſent themſelves; and, in like manner, there are few countries which abound ſo much with bitumen as Derbyſhire.

Whoever has examined the limeſtone rocks about Matlock, and moſt other places in this county, muſt be convinced of the truth of this aſſertion.

The limeſtone and calcareous ſpars alſo, where the elaſtic bitumen is found, are, for the greater part, in the ſame ſtate; ſo that no doubt can be entertained but that this bitumen has had the ſame origin as thoſe which are more generally known; and it would undoubtedly have been confounded with them, had it not been diſcovered when paſſing from the liquid to the ſolid ſtate.

* The elaſtic bitumen, A, No. 9. when digeſted in ſulphuric ether in a temperature of about 55°, is partly diſſolved. The ſolution is yellowiſh brown when oppoſed to the light; but, when otherwiſe viewed, is like the bitumen, that is, of a browniſh olive colour. By ſpontaneous evaporation, the etheric ſolution leaves a yellowiſh brown bitumen, which is totally devoid of elaſticity. The undiſſolved portion (like the cahout-chou under ſimilar circumſtances) is ſoftened, and is much increaſed in bulk.

The ſpecies B, No. 6. cut into very thin ſlices, communicates a yellow tinge to ſulphuric ether; in other reſpects it is but little affected.

The

The elementary principles of bitumen are, hydrogen, carbon, fometimes azote, and probably fome oxygen, which, by its action on the other principles, tends to form the concrete bitumens, and alfo produces that portion of acid obtained by chemical operations. Thefe fame principles, hydrogen and carbon, conftitute the vegetable oils and refins; and the fame, with fome azote, form the oils and greafe of animals. Now it is known that very fmall changes in the refpective proportions of thefe ingredients, and in the circumftances which attend the combination of them, will caufe confiderable variations in the nature of the products; and in like manner, it appears very probable, that when the organized bodies in their recent ftate, and in the full poffeffion of the above-mentioned principles, have been buried in a fituation where thefe principles have been long elaborated under certain favourable circumftances, and fubjected to the action of mineral bodies; I fay that it appears highly probable, that a new combination, which we call bitumen, may be formed, which, although different in fome refpects from the vegetable and animal products, ftill, however, retains many characters of thofe fubftances from the principles of which it has been formed.

HAMMERSMITH,
April 26, 1797.

III.

Obfervations on the Phyfical and Political Geography of North Africa. By JAMES RENNEL, *Efq. F. R. S.* *

TO our view, North Africa appears to be compofed of three diftinct parts, or members. The firft and fmalleft is a fertile region along the Mediterranean, lying oppofite to Spain, France, and Italy (commonly diftinguifhed by the name of Barbary); and which, could we fuppofe the weftern bafon of the Mediterranean to have once been dry land (bating a lake or recipient for the furrounding rivers), might be regarded as a part of Europe; as poffeffing much more of the European than the African character.

The fecond part is what may be deemed the body of North Africa, comprifed between Cape Verd and the Red Sea, on the eaft and weft; and having the Great Defert (or Sahara) and its members, on the North; the Ethiopic ocean and South Africa, on the oppofite fide. The prominent feature of this immenfe region is a vaft *belt of elevated land* of great breadth, often fwelling into lofty mountains, and running generally from weft to eaft, about the tenth degree of latitude. Its weftern extremity feems to be Cape Verd; the mountains of Abyffinia, the eaftern. To the north, its ramifications are neither numerous nor extenfive, if we except the elevated tract which turns the Nile to the northward beyond Abyffinia. Towards the fouth no particulars are known, fave that a multitude of rivers, fome of them very large, defcend from that fide and join the Atlantic and Ethiopic feas, from the Rio Grande on the weft to Cape Lopez on the eaft; proving inconteftably that by far the greateft proportion of rain water falls on that fide during the periodical feafon of the

* Copied by permiffion from his "Geographical Illuftrations of Mr. Park's Journey," in the Proceedings of the African Affociation, 1798. On this fubject fee likewife our Account of Books in the prefent Number N. S. W.

S. W. winds; which correfponds in all its circumftances with the fame monfoon in India *.

To the north of this belt, with the exception of the Egyptian Nile, the waters conform generally to the direction of the high land; paffing at no great diftance (comparatively) from its bafe to the right and left; as if the furface of the Sahara had a general dip to the fouthward †. Thefe rivers moreover receive all their fupplies from the fouth; no ftreams of any bulk being collected in the Defert.

In order to produce this effect, there muft neceffarily be a vaft hollow in the interior of Africa, between the high land of Nubia on the eaft, and Manding on the weft; and of which the mountains and defert form the other two fides. Nor is this ftate of things unexampled in the other continents. In Afia, the hollow, to whofe waters the Cafpian and Aral ferve as recipients, is no lefs extenfive than the one juft mentioned; reckoning from the fources of the Wolga to thofe of the Oxus; (which latter has ever communicated with the Cafpian, either throughout the year or during a part of it:) the difference is, that in Afia a greater portion of the hollow is filled up with water than in Africa.

The third part is of courfe the Great Defert (or Sahara) and its members; confifting of the leffer deferts of Bornou, Bilma, Barca, Sort, &c. This may be confidered as an OCEAN OF SAND ‡, prefenting a furface equal in extent to about *one half of Europe*, and having its gulfs and bays; as alfo its iflands fertile in groves and paftures, and in many inftances containing a great population fubject to order and regular government. The great body or weftern divifion of this OCEAN, comprifed between Fezzan and the Atlantic, is no lefs than 50 caravan journeys acrofs from north to fouth; or from 750 to 800 G. miles; and double that extent in length: without doubt the largeft defert in the world. This divifion contains but a fcanty portion of iflands (or oafes), and thofe alfo of fmall extent: but the eaftern divifion has many; and fome of them very large. Fezzan, Gadamis, Taboo, Ghanat, Agadez, Augela, Berdoa, are amongft the principal ones: befides which there are a vaft number of fmall ones. In effect this is the part of Africa alluded to by Strabo ‖, when he fays from Cneius Pifo, that Africa may be compared to a leopard's fkin. I conceive the reafon why the oafes are more common here than in the weft, is, that the ftratum of fand is fhallower from its furface to that of the earth which it covers. In other words, that the water contained in that earth is nearer to the furface; as in moft of the oafes it fprings up fpontaneoufly §. Can any part of the caufe be affigned to the prevalent eafterly winds, which, by driving the finer particles of fand to leeward, may have heaped it up to a higher level in the Sahara than elfewhere?

The

* A ridge ftretches to the fouth through the middle of South Africa, and forms an impenetrable barrier between the two coafts. M. Correa de Serra informs me, that the Portuguefe in Congo and Angola have never been able to penetrate to the coaft of the Indian Ocean.

Mr. Bruce learned (vol. iii. p. 668.) that a high chain of mountains from 6° runs fouthward through the middle of Africa. He fuppofes the gold of Sofala to be drawn from thefe mountains. (p. 669.)

† Circumftances have fhewn, that it declines to the eaftward alfo.

‡ "A wild expanfe of lifelefs fand and fky!" THOMSON.

‖ Page 130.

§ Water is found at the depth of a few feet in Fezzan (African Affoc. Q. p. 96. O. p. 146.) The fame is faid

The springs no doubt have produced the oases themselves, by enabling useful vegetables to flourish, and consequently population to be established. That the Desert has a dip towards the east as well as the south, seems to be proved by the course of the Niger also. Moreover the highest points of North Africa, that is to say, the mountains of Mandinga and Atlas, are situated very far to the west.

The Desert for the most part abounds with salt. But we hear of salt mines only in the part contiguous to Nigritia, from whence salt is drawn for the use of those countries as well as of the Moorish states adjoining; there being no salt in the Negro countries south of the Niger *. There are salt lakes also in the eastern part of the Desert.

The great ridge of mountains and its branches are very productive in gold; but more particularly in the quarters opposite to Manding and Bambouk on the west, and Wangara on the east. It may perhaps admit of a doubt, whether the gold is brought down at the present time by the numerous fountains that form the heads of the Niger and Senegal rivers; or whether it has been deposited in the lower parts of their beds at an earlier period of the world; and that the search, instead of being facilitated by the periodical floods, is on the contrary only to be pursued with effect when the waters are low.

Tombuctoo is reckoned the mart of the Mandinga gold, from whence it is distributed over the northern quarters of Africa by the merchants of Tunis, Tripoly, Fezzan, and Morocco; all of whom resort to Tombuctoo. Most of it no doubt afterwards finds its way into Europe. It may be remarked also, that the gold coast of Guinea (so called doubtless from its being the place of traffic for gold dust) is situated nearly opposite to Manding: but whether the gold brought thither has been washed out of the mountains by the northern or southern streams, I know not: it may be both †. Degombah, another country said to be very productive

said by Pliny, concerning this quarter of Africa; lib. v. c. 5. But farther to the N. W. on the edge of the Desert, and in the country of Wadreag in particular, (Shaw, p. 135.) wells are dug to an amazing depth, and water mixed with fine sand springs up suddenly, and sometimes fatally to the workmen. The Doctor tells us that the people call this abyss of sand and water, "the sea below ground." Exactly the same state of things exists in the country round London, where the sand has in several cases nearly filled up the wells. (See Phil. Transf. for 1797.) The famous well lately dug by Earl Spencer (at Wimbledon), of more than 560 feet in depth, has several hundred feet of sand in it.

* This quality of the African Desert was familiarly known to Herodotus (Melpom. c. 181, et seq.) He knew also that there was salt in abundance in the northern parts. But, as the inhabitants in that quarter can furnish themselves with salt of a better quality from the sea, the mines are not wrought.

† Some writers have said, that there are gold mines in the neighbourhood of Mina, on the gold coast; others that the gold is rolled down by the rivers to that neighbourhood. Both may be true. But, on the other hand, it is said that the gold of Wangara is also brought for sale to the southern coast.

It is difficult to conceive any other adequate cause, than the exchange of the gold of the inland countries for the introduction of so vast a quantity of kowry shells, which are carried from Europe to the coast of Guinea, and pass for small money in the countries along the Niger from Bambara to Kassina, both inclusive.

I am informed from authority, that about 100 tons of kowries are annually shipped from England alone to Guinea. These are originally imported from the Maldive islands into Bengal; and from Bengal into England. In Bengal 2400 more or less are equal to a shilling: and yet, notwithstanding the incredible smallness of the denomination, some article in the market may be purchased for a single kowry. But in the inland parts of Africa they are about ten times as dear, varying from 220 to 280. Mr. Beaufoy was told, that in Kassina they were at the rate of about 250. And Mr. Park reports, that they are about the same price at Sego: but cheaper ductive in gold *, must by its situation lie directly opposite to the gold coast: for it lies immediately to the east of Kong (the Gonjah of Mr. Beaufoy and the Conche of D'Anville †.) The people of Fezzan trade to Kong.

The triangular hilly tract above commemorated, (p. 71 of the "Illustrations") which projects northward from the highest part of the belt, and contains Manding, Bambouk, &c. is also abundant in gold; particularly in the quarter towards Bambouk, where it is found in mines; and that chiefly in the middle level ‡. (See also p. 71.)

Wangara appears to have been in its time nearly as rich as Manding in this metal. The Arabs name it *Belad al Tebr*, or the *country of gold* ||. Edrisi, Ibn Al Wardi, and Leo, bear testimony to its riches. They say that the gold is found in the sands after the periodical inundation of the Niger (which is generally over the country) is abated §. Leo alone ** says, that the gold is found in the southern quarter of the kingdom; which appears very probable, as the mountains lie on that side: so that it may be concluded, that the gold sand has not been brought there by the Niger, but by smaller rivers that descend immediately from these mountains. That a part of Wangara is bounded by mountains, we learn from Edrisi: for the lake on which Reghebil stands has mountains hanging over its southern shore ††.

It is supposed that most of the countries bordering on these mountains share in the riches contained within them, by means of the rivulets ‡‡. But considering how amazingly productive in gold the streams of this region are, it is wonderful that Pliny should not mention the Niger, as one of the rivers that rolls down golden sands: for although he speaks of the Tagus and others in different quarters, no African river is mentioned ||||. And yet Herodotus knew that the Carthaginians bartered their goods for gold, with the Africans on the sea coast beyond the pillars of Hercules; which was contrived without the parties seeing each other §§.

The common boundary of the Moors and Negroes in Africa forms a striking feature, as well in the moral as the political and physical geography of this continent. The Moors descendants of Arabs, intermixed with the various colonists of Africa from the earliest to the

cheaper at Tombuctoo, which is about the centre of the kowry country; dearer towards Manding, which is the western extremity of it. Hence they are probably carried in the first instance to Tombuctoo, the gold market; and thence distributed to the east and west. Their circulation seems to be confined between Bornou and Manding. In Bornou they have a coinage of base metal.

* African Assoc. Q. p. 176. O. p. 264.

† Mr. Park says, that Kong signifies mountain in the Mandinga language; which language is in use from the frontier of Bambara to the western sea.

‡ Labat, vol. iv. ch. 2.

|| Bakui, and Herbelot; article Vankara.

§ See Edrisi in particular, pages 11 and 12.

** Page 254.

†† Edrisi, page 12.

‡‡ Mr. Bruce, vol. iii. p. 647, says the same of the mountains of Dyre and Tegla, which are a continuation of the great belt, towards Abyssinia.

|||| Pliny, lib. xxxiii. c. 4.

§§ Melpomene, c. 196.

Dr. Shaw (p. 302) speaks of the same mode of traffic at present between the Moors and Negroes; whence the place of traffic ought to be very far removed from the Mediterranean. There is a similar story related by Cadamosta of the exchange of salt for gold in Melli; and by Dr. Wadstrom on the windward coast of Guinea.

latest

lateſt times overſpread the habitable parts of the Deſert, and the oaſes within it; and have puſhed their conqueſts and eſtabliſhments ſouthward; preſſing on the Negro aborigines, who have in ſeveral inſtances retired to the ſouthward of the great rivers; but in others preſerve their footing on the ſide towards the Deſert; according to the ſtrength or openneſs of the ſituation. It is probable, however, that the Negroes, who are an agricultural people, never poſſeſſed any conſiderable portion of the Deſert, which is ſo much better ſuited to the paſtoral life of the Moors. It appears as if matters had not undergone much change in this reſpect ſince the days of Herodotus; who fixes the boundary of the Libyans and Ethiopians, in other words, of the Moors and Negroes, near the borders of the Niger; and he apparently pointed to the quarter in which Kaſſina or Ghana are now ſituated *.

The Negroes in the weſtern quarter of the continent are of two diſtinct races, of which the leaſt numerous are named Foulahs or Foolahs. Theſe, although they partake much of the Negro form and complexion, have neither their *jetty* colour, *thick* lips, or *criſped* hair. They have alſo a language diſtinct from the Mandinga, which is the prevailing one in this quarter.

The original country of the Foulahs is ſaid to be a tract of no great extent along the eaſtern branch of the Senegal river; ſituated between Manding and Kaſſon; Bambouk and Kaarta: and which bears the name of Foola-doo, or the country of the Foulahs. But whether this be really the caſe, or whether they might not have come from the country within Serra Leona (called alſo the Foulah country), may be a queſtion; of which, more in the ſequel. The Foulahs occupy at leaſt as ſovereigns ſeveral provinces or kingdoms, interſperſed throughout the tract comprehended between the mountainous border of the country of Serra Leona on the weſt, and that of Tombuctoo on the eaſt; as alſo a large tract on the lower part of the Senegal river; and theſe provinces are inſulated from each other in a very remarkable manner. Their religion is Mahomedaniſm, but with a great mixture of Paganiſm; and with leſs intolerance than is practiſed by the Moors.

The principal of the Foulah ſtates is that within Serra Leona; and of which Teemboo is the capital. The next in order appears to be that bordering on the ſouth of the Senegal river, and on the Jaloffs: this is properly named Siratik. Others of leſs note are Bondou, with Foota-Torra adjacent to it, lying between the rivers Gambia and Falemé; Foola-doo and Brooko along the upper part of the Senegal river; Waſſela beyond the upper part of the Niger; and Maſſina lower down on the ſame river, and joining to Tombuctoo on the weſt.

The Moors have not in any inſtance eſtabliſhed themſelves on the ſouth of the great rivers. They have advanced fartheſt to the ſouth in the weſtern quarter of Africa; ſo that the common boundary of the two races paſſes, in reſpect of the parallels on the globe, with a conſiderable degree of obliquity to the north, in its way from the river Senegal towards Nubia and the Nile. Mr. Park arranges the Mooriſh ſtates, which form the *frontier* towards Nigritia, together with the Negro ſtates oppoſed to them on the ſouth, in the line of his progreſs, in the following order:

The ſmall Mooriſh ſtate of Gedumah, ſituated on the north bank of the Senegal river, and the laſt that touches on it †, is oppoſed to the ſmall Negro kingdom of Kajaaga, on the

* See Euterpe, c. 32; and Melpomene, c. 197.

† The Moors appear to be maſters of the northern bank of the Senegal, through the greateſt part of its navigable courſe; the Foulahs the ſouthern bank.

ſouth. This latter occupies the extremity of the navigable courſe of the Senegal, terminated in this place by the cataract of F'low.

From this point the Negro and Foulah ſtates occupy both banks of the Senegal river to its ſource; and beyond that both banks of the Niger (or Joliba) likewiſe, to the lake Dibbie, ſituated beyond the term of Mr. Park's expedition. This ſpace is divided unequally between Kaſſon, a hilly ſtrong country, but of ſmall extent; and which has the Moors of Jaffnoo on the north: Kaarta a conſiderable ſtate, which has Ludamar for its oppoſite (a country held by Ali, a Mooriſh prince, who is loaded with infamy on the ſcore of maltreatment of the only two Europeans who appear to have entered his country in latter times): Bambara of ſtill more conſideration, having the Mooriſh kingdom of Beeroo to the north: and Maſſina, a Foulah ſtate bordering alſo on the ſouth of Beeroo.

Here Mr. Park's perſonal knowledge ends; but he learnt that Tombuctoo and Houſſa, which ſucceed in order to Maſſina, and occupy both ſides of the Niger, are Mooriſh ſtates, though with greateſt proportion of Negro ſubjects: ſo that the river may be conſidered as the boundary of the two races in this quarter *.

Of the countries between Houſſa and Kaſſina we are ignorant. The Deſert ſeems to approach very near the river (Niger) in that quarter, whence a Mooriſh population may be inferred. South of the river, we hear of Kaſſaba, Gago, and other Negro countries; but without any diſtinct notices of poſition; and beyond theſe Melli.

Kaſſina and Bornou, two great empires on the north of the river, appear to divide the largeſt portion of the remaining ſpace to the borders of Nubia; and extend a great way to the north; this region being compoſed of deſert and habitable country intermixed; but perhaps containing the largeſt proportion of the latter. In both theſe empires, the ſovereigns are Mahomedans, but the bulk of their ſubjects are ſaid to adhere to their ancient worſhip; that is to ſay, the lower orders are almoſt univerſally Negroes †.

From what has appeared, perhaps the boundary of Nigritia as it reſpects the Negro population may be expreſſed generally, and with a few exceptions, as follows: beginning from the weſt, the extent upwards of the navigable courſe of the Senegal river, generally—thence a line drawn to Silla; from Silla to Tombuctoo, Houſſa, and Beriſſa, along the river Niger; and thence through Afouda, Kanem, and Kuku, to Dongola on the Nile.

Leo ‡ enumerates 12 ſtates or kingdoms of Nigritia: but amongſt theſe he includes Gualata, a tract only 300 miles S. of the river Nun: as alſo Cano (Ganat), adjacent to Fezzan; and Nubia. Kaſſina, Bornou, and Tombuctoo, are included of courſe §.

The kingdom of the Foulahs, before mentioned, ſituated between the upper part of the Gambia river and the coaſt of Serra Leona, and along the Rio Grande, has alſo a Maho-

* The emperor of Morocco is ſaid to have held at one period the ſovereignty of ſome of the countries on the northern banks of the Senegal and Niger rivers. Labat, vol. iii. p. 339, ſpeaks of incurſions made by his troops.

† African Aſſoc. Q. p. 126. O. p. 191.

‡ Page 4.

§ The Arabs and Moors call Nigritia by the general name of Soudan. By Belad Soudan, or the country of Soudan, Abulfeda includes all the known part of Africa, ſouth of the Great Deſert and Egypt. With him Soudan is the ſouthern quarter of the globe. D'Herbelot alſo allows it a wide range. Affnoo is another term for Nigritia, in uſe among the natives themſelves. (See alſo Proceedings Afric. Aſſoc. Q. p. 164. O. p. 246.)

medan

medan sovereign, but the bulk of the people appear to be of the ancient religion. It has been already said, that although they are a black people, they are less black than the Negroes generally, and have neither crisped hair nor thick lips: as also that they have a language distinct from the Mandinga. From these circumstances, added to that of situation, they appear clearly to be the *Leucæthiopes* of Ptolemy and Pliny. The former places them in the situation occupied by the Foulahs; that is, in the parallel of nine degrees north; having to the north the mountains of *Ryssadius*, which separate the courses of the Stachir and Nia rivers (Gambia and Rio Grande), and which therefore answer to the continuation of the great belt of high land in our geography; in which there is moreover another point of agreement, the *Caphas* of Ptolemy being the Caffaba of the map *.

Ptolemy by the name evidently meant to describe a people *less* black than the generality of the *Ethiopians*; and hence it may be gathered that this nation had been traded with, and that some notices respecting it had been communicated to him. It may also be remarked, that the navigation of Hanno terminated on this coast; probably at Sherbro' river, or sound. And as this was also the term of the knowledge of Ptolemy, it may be justly suspected that this part of the coast was described from Carthaginian materials †.

Those who have perused the Journal of Messrs. Watt and Winterbottom, through the Foulah country in 1794, and recollect how flattering a picture they give of the urbanity and hospitality of the Foulahs, will be gratified on finding that this nation was known and distinguished from the rest of the Ethiopians at a remote period of antiquity ‡.

The contrast between the Moorish and Negro characters is as great as that between the nature of their respective countries; or between their form and complexion. The Moors appear to possess the vices of the Arabs without their virtues; and to avail themselves of an intolerant religion, to oppress strangers: whilst the Negroes, and especially the Mandingas, unable to comprehend a doctrine that substitutes opinion or belief for the social duties, are content to remain in their humble state of ignorance. The hospitality shewn by these good people to Mr. Park, a destitute and forlorn stranger, raises them very high in the scale of humanity: and I know of no fitter title to confer on them than that of the Hindoos of Africa: at the same time by no means intending to degrade the Mahomedans of India by a comparison with the African Moors.

* The Soluentii of Ptolemy may also be meant for the Solimani of Mr. Park.

† And it may also have been the scene of traffic mentioned in page 155; as Dr. Wadstrom speaks of such a custom in this quarter at the present day.

‡ Pliny (lib. v. c. 8.) also speaks of the Leucæthiopes, but seems to place them on this side of Nigritia. May it not be that certain tribes of Foulahs were then established, as at present, along the Senegal river?

IV.

Obſervations on Metallic Money; chiefly directed to aſcertain the moſt advantageous Diſtribution and Figure of Gold, Silver, and Copper in Coins.

IF the value of ſcience be meaſured by its utility, there is no part of human knowledge that will rank higher than political economy. If we juſtly applaud the inventor, who by the conſtruction of a machine, or the improvement of a chemical proceſs, has added to the ſum of our enjoyments, in one ſmall part of the great ſcheme of ſocial life; how much more is due to thoſe, who, by inveſtigating the proceſſes upon which that ſcheme itſelf depends, have added to the facilities of mutual intercourſe, and given vigour to every department of active induſtry?

When we contemplate the ſtate of man, ſupported merely by the art of the hunter, or by the immediate products of the earth; aſſociated leſs for the purpoſe of mutual aſſiſtance in the arts than for predatory enterpriſe or direct reſiſtance againſt the oppreſſion of his neighbours; we are almoſt inclined to think him of a different ſpecies from the ſame creature in the civilized ſtate. Upon examination, however, it appears, that the inevitable neceſſity of events has produced the difference. The variety of productions, of wants and of fabrications, has given riſe to barter or exchange. Mutual ſupply has increaſed the ſubdiviſion of labour, and improved the means of conveyance. Streams, roads, ſhips and carriages, have extended this beneficial intercourſe. Confidence between man and man has advanced the moral principles of ſociety, and afforded a progreſſion, of which the paſt gradations may indeed be traced, but to the future part of which the imagination can ſcarcely afford a probable outline.

Among the impediments to commerce, the greateſt undoubtedly is the charge of conveyance from place to place. This is the great obſtacle which limits the exchange of commodities from one extremity of the world to the other. Whenever the charges of carriage ariſe to ſuch an amount as to equal the effectual return in any remote market, the motive for conveying merchandize to that place ceaſes. If goods were always exchanged for goods, it is clear that the conveyance, under the uncertainty of diſpoſal, would take place to a very ſmall diſtance indeed; and the labour required to diſcover the perſons willing to exchange would greatly enhance the charge. It would require a volume to enumerate and deſcribe the expedients, moral as well as mechanical, by which theſe difficulties are in part ſubdued, and ſtill more to deduce their origin and general effects. One of the chief of theſe expedients conſiſts in the uſe of ſome article of merchandize, as the medium of exchange, which ſhall be acceptable to every man, and will therefore be received and held by the ſeller of any commodity until he ſhall meet with another individual, who he knows will again take it for the article he wants.

In the iſland of Madagaſcar, it is ſaid, that the exchangeable value of goods is reckoned in hatchets, bullocks, and ſlaves; theſe commodities being univerſally vendible, and for that reaſon every where received. Smith affirms, that nails anſwer the ſame purpoſe in ſome parts of Great Britain. Theſe and other inſtances may ſerve to ſhew how a preferable medium of exchange becomes adopted, and it will without difficulty be ſeen that the ſcarceſt and leaſt deſtructible metals muſt have at length become the univerſal ſubſtitutes. For their value does not depend on their figure; they may be ſubdivided and joined again without

 loſs;

loſs; they receive no injury by keeping; and the labour of conveying them from place to place forms a leſs part of their value than of almoſt every other article.

The firſt moneys were mere quantities of metal aſcertained by weight, as the names of moſt ſpecies ſtill indicate. The interference of governments was found neceſſary to aſſure the weight, and more particularly the fineneſs of determinate portions of metal; and this has given riſe to an opinion, that a part of the value of coin muſt depend on the edict of the State which iſſues it. Whether ſtateſmen themſelves have in reality thought this to be the caſe, is little to the purpoſe; but it is certain that they have, from time to time, yielded to the temptation of diminiſhing the quantity of precious metal iſſued under a given denomination, either by openly deducting from the weight, or ſecretly debaſing the coin *. Tranſactions of this nature muſt have operated to the loſs of all the creditors in the State; but they have never deceived the ſellers, who have always regulated their prices by their knowledge of the real quantities of the metal, and not by the denomination or the ſuppoſed weight or fineneſs it might denote. The imaginary coin, or money of account, to be found in the mercantile books of almoſt every commercial nation, muſt have ariſen partly from this cauſe.

I was led to the preſent examination by hearing that a committee of the Royal Academy has been appointed to take into conſideration a proper deſign for a new coinage; and, upon enquiry, I find that his Majeſty's Privy Council have repeatedly deliberated on this ſubject, and referred the ornamental part to the Royal Academy for their diſcuſſion and report. On ſuch occaſions it has always, and, I think, rightly, been conſidered as a becoming tranſaction on the part of individuals to give their thoughts to the world by the medium of the preſs; and under this conviction it is that I have ventured to place the ſubject in the light it appears to me to require.

The metals uſed for coinage are gold, ſilver and copper. According to the exchangeable value of gold, half a grain of this metal would purchaſe as much bread as a man could eat at one meal. This ſmall piece of gold, if as thin as paper, would not meaſure above the tenth part of an inch in breadth, and would therefore be perfectly inconvenient for uſe. It has, in fact, been found, that the gold coin of the weight of 32 grains (or the quarter guinea) was too ſmall to be conveniently uſed. The ſame obſervations will apply to the ſmaller ſub-diviſions of the ſhilling of ſilver; whence upon the whole it appears, that coins of all the three metals are required to facilitate our commerce of buying and ſelling.

Gold, ſilver and copper, like every other produce of human induſtry, depend for their value principally on the labour employed in producing and bringing them to market, and

* This diminution has taken place throughout Europe. With us the pound of money, which about the year 1087 contained a pound weight of ſilver, has continued at leſs than one-third (or $\frac{20}{62}$) of that quantity ever ſince the reign of Elizabeth. Our neighbours however have univerſally exceeded us in this reſpect. Thus the pound Flemiſh is leſs than eleven ſhillings; the French livre is ten pence, and the Italian lire is leſs than 2¼d.

The Chineſe ſtill uſe fine ſilver, which they actually cut and weigh at every ſingle payment. They are ſaid to have formerly poſſeſſed ſilver coin; but whether they were urged to their preſent practice by uncertain variations in its value cauſed by their rulers, or by the difficulty of otherwiſe reſiſting the artifices of coiners, I know not.

in a confiderable degree upon the actual demand. As thefe articles are not employed merely in the fabrication of coins, the demand will vary in each according to circumftances, which admit of no permanent ratio of exchange between them. If the State were to coin certain pieces of known weight and finenefs out of each of thefe metals, and determine that a certain number of the filver pieces, for example, fhould in all cafes be equivalent to one piece of the gold, it would naturally follow, fuppofing the individual to pay nothing for the coinage, that a debt might be difcharged with moft facility to the debtor, and confequently lofs to the creditor, in the cheapeft of thefe two metals, whenever by the fluctuation of the market either of them fhould come to reprefent a larger portion of the other than the edict of the government had determined. This confequence of fixing the relative value of coins would fhow itfelf in a variety of ways, which need not be enumerated; becaufe it is certain that the dearer metal would occupy the greater part of the circulation, while the cheaper pieces would either be melted down or diminifhed, if their rated value were too high, or they would be fabricated by individuals if it were too low, in defiance of every public regulation which might be adopted. If we therefore admit, from confiderations of this nature, that no government does in reality poffefs the means of fixing a ratio between two articles of commerce, intended to be applied as the tickets of transfer or mediums of exchange, we fhall be naturally led to the adoption of one of the metals only, as the reprefentative fign, while the two others are applied merely as inftruments of accommodation for the convenient fub-divifions of value.

With regard to the queftion of preference in thefe three metals, experience has fhewn that fociety is difpofed to affume the deareft; namely, gold. With a fingle ftandard of value, the fluctuations of the market price of the metal, when compared with other commodities, will be nearly imperceptible, becaufe they confound themfelves with the rife and fall in the prices of all other articles to which the ftandard is thus applied. If a cheaper metal were to be adopted by the State, and gold were left to circulate at the election of individuals, the changes of price in this metal of high value would operate fo as to produce an uncertainty in the amount of large fums, and greatly difturb the general tranfactions of commerce. Merchants would therefore confider the gold coinage as mere bullion, and the community would in a great meafure be deprived of its ufe as a coin; as is actually the cafe in Holland and other countries where filver is the legal medium*. Hence it appears moft eligible, that gold in pieces of determinate weight and finenefs fhould conftitute the effective coin of the State, or legal tender of payment; that filver and copper fhould be formed into money for the purpofe of reprefenting fractions of the fmalleft gold coin; and that the creditor or feller fhould have the option to refufe all payments in thefe laft metals for any fum exceeding the fmalleft unity of the gold coin.

By this diftribution, though the coins of filver and copper would in ftrictnefs be fubject to fluctuations arifing from the ftate of the market with regard to thofe metals, yet the dif-

* A ftill more defective fcheme was propofed in the Report prefented by Prieur, de la Côte d'Or, from a Committee of the Council of Five Hundred, of which a very full abftract is given in the Moniteurs of 6 and 7 Floreal in the year VI. Nos. 216, 217. It is, that filver coin fhould be unchangeable in weight and denomination of value; but that the price of gold (alfo coined) fhould be fettled every fix months by a declaration from the National Treafury, deduced from the medium price of that metal during the preceding half-year. It was rejected by the Council of Ancients.

ference

ference would be disregarded in the discharge of accounts, because it could never amount to a sum of any importance. The only inconvenience which offers itself under such an arrangement is, that these subordinate coins would also be melted and sold when the metal was dear, or they would be fabricated if the metal ever happened to be so cheap as to afford an adequate motive of profit to the illegal coiner. The State, in its deliberations on this subject, might determine that the coins of silver and copper should pass either for less than the medium market price of the metal, or for more, or for that value precisely. It is evident that the first of these dispositions would afford coin which would continually vanish in the melting-pot, and is therefore altogether unadvisable. The medium rate of intrinsic value would produce a similar effect whenever the market price was low. Whence it follows, that the metal contained in such auxiliary money ought to be of less value than the gold it represents; and to prevent the introduction of a similar coinage from private manufacturers, it would be necessary that the difference between the value of the metal and that represented by the coin should be somewhat less than the cost of workmanship. Under these circumstances the public would be supplied with an useful implement or ticket of exchange, which would operate as a pledge of value, very nearly to the amount of its denomination, and would be afforded cheaper from the extensive manufactories of government than it could possibly be made by private workmen.

Coin, like every other utensil or tool, is subject to wear, and will in process of time be more or less deprived of its distinctive figure, and rendered less valuable by the loss of weight. When new, it is the real pledge or measure it pretends to be; but, if it be suffered to circulate after its weight is considerably diminished, it may become a desirable object to the coiner to fabricate pieces apparently in the worn state, or otherwise he may exercise his industry in speedily reducing the new coin to that state, for the sake of the precious metal he may thus acquire.

If, on the contrary, the Legislature should forbid the currency of pieces worn beyond a certain small or moderate loss, the consequence will be, that all such pieces will return to the Mint to be recoined; and the charge of coinage may become so heavy as to absorb a considerable part of the value of the whole circulating medium in the course of a few years.

To diminish this last inconvenience as much as possible, it becomes necessary to attend to the nature of the metal as well as to the figure of the piece. Whether the Dutch ducat, of fine gold, or the English guinea, of twenty-two carats, may, under like circumstances, be most disposed to lose by wear, has not I believe been determined; but it seems to be generally understood, that our standard gold in watch-cases and other trinkets is less durable than the coarser and harder gold allowed to be wrought in France and Geneva. If this be true, it should seem as if there existed no motive for raising the standard of our gold, and perhaps the same argument may apply still more to our silver: and the advantage, if any, in lowering the standard without diminishing the intrinsic value, has not yet been shewn with sufficient evidence to justify the offence against established use and public prejudice which such a proceeding might afford. Admitting these observations to be conclusive against altering the standard, it would follow, that the greater durability of coin must be sought for in its figure.

Let us imagine a coin to possess the figure of an equilateral triangle; let it be thin, in order

order that it may prefent a large furface; let its edges have the figure of a faw, and its faces that of a file. Under thefe conditions we fhould fabricate one of the worft or leaft durable coins that could be chofen. For the angles would be eafily broken and worn, and the edges and faces would mutually operate on each other, with a degree of rapidity which, it may be concluded, would very foon take away all the fharp prominences, and greatly diminifh the weight. On the other hand, let us fuppofe the leaft poffible furface, and we fhall obtain the fpherical figure *. Againft this it appears to be an objection, that if it be nearly perfect, the impreffions diftinctive of its purity and denomination muft be indented, and will not therefore fufficiently limit its apparent magnitude; and if they be prominent it will no longer be a fphere, but a figure prefenting fharp angular parts with fmall bearings very liable to deftruction.—What then is the figure which fhall partake fo much of the plane, as to prefent furfaces of broad contact or bearing, and afford the leaft quantity of angular prominence? It is evidently the cylinder: and this is the figure moft generally adopted for money. The edge of the cylinder affords the fmalleft bearing. It muft therefore be very fhort or flat, in order that the weight of the piece may be difpofed to reft on the bafe, and not on the edge.

If the whole furface of a piece of metal were covered with figures or impreffions, it would be immediately feen whether any part had been abraded by accident or defign. If the impreffions were concave, they might eafily be renewed by the punch or the graver; but if they were in relief, it would be almoft impoffible to reftore them when once worn or obliterated. For this reafon, the preference in coinage has moftly been given to figures in relief.

It is however a very ferious inconvenience, that when the diftinctive marks are thus rendered prominent, the face of the coin no longer fuftains the preffure and wear of the piece; but the marks themfelves are made to fupport the whole. Thus, in our gold money, particularly of the laft recoinage, the edge is a faw, and the numerous minute prominencies on the face conftitute a file, the operations of both which are feverely felt in the rapid deftruction of the piece †.

Hence we may obferve, that neither kind of mark alone is fuited to a coin intended to poffefs durability, and at the fame time to be difficult either to imitate or diminifh. A com

* The pagoda and fanam of India are the only coins I recollect which approach towards this figure.

† To place this in a more ftriking light, it may be obferved that the amount of gold coined between the years 1762 and 1772. both inclufive, was 8, ,203 l. 15s. 6d. and between 1782 and 1792, both inclufive, was 19,675,666 l. 14s. 6d. and between 1773 and 1777, both inclufive, was 10,591,833 l. 1s. During the middle period laft mentioned the great recoinage of gold took place. I am aware that other caufes may have occafioned a demand for coin befides the mere wear of the old pieces, and that the increafe of commerce and manufactures has in fact produced fuch a demand; but as this laft event (diftinguifhable by its gradual progrefs) does not appear, from the numbers in the account, to have influenced the coinage in any great proportion, I fhall difregard it in the prefent rough ftatement With this liberty, we may proceed to remark, 1ft. That as moft of the old pieces difappeared during the middle term of time the number of nineteen, or fay twenty millions muft nearly reprefent the whole of our gold money. 2d. That the national lofs by wear in the firft period, when the gold was old and fmooth, reckoned at ¼ per cent. on the fum recoined, was 3708 l. per annum; and in the latter period 8943 l. per annum: and 3d. That the whole national ftock of gold coin, under the regulations and figure of the laft period, wears out and is recoined every eleven years. The account of coinage is to be found in the *Report of the Lords' Committee of Secrecy*, printed April 28, 1797.

bination

bination of both methods is neceffary. If a coin be ftruck with indentations or parts depreffed beneath the common furface, and in thefe there be prominent objects or defigns, not more elevated than that furface, the general advantage, with regard to wear, will approach towards that of the plain furface itfelf, and the impreffion will be at leaft as difficult to imitate, if not more fo than that of a defign rifing totally above the common furface *. The late copper coinage of pieces of one and of two pennies are of this kind †.

To fum up the foregoing conclufions in a few words, we may remark, that, 1. The State is unable (from the natural impracticability of the thing) to appoint two diftinct articles of commerce as the circulating mediums of exchange. 2. The meafure of value or legal tender ought to confift in the metal which bears the higheft price, namely, gold. 3. Coin of filver and copper are required for fmaller fractions than the actual fubdivifions of the gold coin, but fhould be optional in the receipt for any larger fums. 4. Thefe laft-mentioned coins ought to reprefent a value in gold equal to their own quantity of metal, at the higheft (or perhaps medium) market price added to the charge of fabrication. 5. No fufficient reafon has yet been given, to fhew that the ftandard of gold coin fhould be changed in order to render it more durable. 6. The beft figure of coin is a fhort cylinder or flat round plate; and 7. The diftinctive marks or impreffions fhould be made neither altogether hollow nor altogether in relief, but by a combination of both forms, fo as to leave a flat bearing furface on each fide.

V.

An eafy Method of cleaning and bleaching Copper-Plate Impreffions or Prints. Extracted from a Letter of Sig. GIO. FABBRONI, *Subdirector and Superintendant of the Royal Cabinet of Philofophy and Natural Hiftory of his Royal Highnefs the Grand Duke of Tufcany, to Sig.* D. LUIGI TARGIONI *at Naples* ‡.

SINCE the happy invention of engraving in copper, which no doubt owes its origin to the revival of the art of chafing and ornamenting plate, collectors have availed themfelves of this means to accumulate and preferve copies of the moft valuable pictures and drawings. This object of refearch becomes every day more prevalent, and prints of the early and moft celebrated mafters are now fought for with the utmoft avidity.

Ancient prints are valuable, not only for their own intrinfic merits, but as monuments of

* Few coins have been made of this figure. The Chinefe coin of mixed copper called the cafh is the moft remarkable, and perhaps the only one of extenfive circulation.

† Of copper, by M. Boulton, Efq. for Government. The penny is rather more than 1.4 inch in diameter, and about 0.13 inch thick at the edge, and weighs 1 oz. avoirdupois. A circular part of the face on each fide rather more than 1.1 inch in diameter is depreffed by the ftroke of the dye, in one of which is feen the head of the King in relief, and in the other a figure of Britannia. Upon the prominent rim on one fide are the words "Georgius III. D. G. Rex," and on the other "Britannia 1797" in funken letters. The edge or cylindrical furface is plain. The two-penny piece refembles the penny; but its diameter is 1.4 inch: funk face 1.25 inch, and weight 2 oz. avoirdupois wanting 20 grains in the piece before me. I fuppofe the average weight to be 2 ounces.

‡ Tranflated from the Italian. Communicated by Andrew Duncan, jun. M. D. of Edinburgh, who received it from the author. The original is inferted in the Giornale Letterario di Napoli, No. 85.

 the

the history of the art. But their scarcity renders them still more valuable. Most of those which are still extant are defaced by negligence, during the time of their remaining suspended against walls exposed to smoke, vapor, and the excrements of insects. Collectors of prints have not, however, shewn the same partiality as antiquarians for the patina; but on the contrary they have sought and practised a method of clearing prints from these impurities.

This method consists in simple washing with clear water, or a ley made of the ashes of vine stalks or reeds, and lastly by a long exposure to the dew. Aqua fortis is also used for the same purpose, but with a degree of risque at least equal to its advantages. The ley dissolves not only the impurities but likewise the oil of the printing ink, and either discharges it totally, or leaves a cloudy appearance. The aqua fortis acts on the vegetable fibre, of which the paper itself is composed, and produces a dark colour, which cannot be removed by means of this liquid, but by an action which would considerably injure the paper itself.

The discovery of Priestley, of the fluid erroneously named by him, but since known by the name of oxygen; and the information we have obtained from Scheele, of the effects of its combination with muriatic acid, have led Berthollet to the useful application of its properties to the act of bleaching cloths, Chaptal to that of bleaching prints and books, and Giobert to the art of painting. But the method of making this preparation is too inconvenient for a mere amateur and collector of prints, and the oxygenated muriatic acid is not yet to be purchased ready prepared in Italy. It may not, therefore, be unacceptable to describe an easy method of effecting this purpose without the difficulties of chemical processes, and within the ability of any person to perform.

It is known that oxygen is abundantly contained in the combinations called metallic calces, though in a state of inactivity; and it is equally well ascertained, that these substances have a very strong attraction for it. On the other hand it is a fact, that some of the metallic calces of very moderate price are capable of easily yielding the whole or the greatest proportion of this constituent part. Manganese is not very well adapted for this purpose; but minium is much better. Nothing more is required to be done, but to provide a certain quantity of the common muriatic acid, for example, three ounces, in a glass bottle, with a ground-stopper, of such a capacity that it may be only half full. Half an ounce of minium must then be added; immediately after which the stopper is to be put in, and the bottle set in a cold and dark place. The heat, which soon becomes perceptible, shews the beginning of the new combination. The minium abandons the greatest part of its oxygen with which the flu d remains impregnated, at the same time that it acquires a fine golden yellow, and emits the detestable smell of oxygenated muriatic acid. It contains a small portion of muriate of le d; bu this is not at all noxious in the subsequent process. It is also necessary to be observed, that the bottle must be strong, and the stopper not too firmly fixed, otherwise the active elastic vapor might burst it. The method of using this prepared acid is as follows:

Provide a sufficiently large plate of glass, upon which one or more prints may be separately spread out. Near the ed es let there be raised a border of soft white wax half an inch high, adhering well to the glass and flat at top. In this kind of trough the print is to be placed in a bath of fresh urine, or water containing a small quantity of ox gall, and kept in this situation for three or four hours. The fluid is then to be decanted off, and pure warm water poured on, which must be changed every three or four hours until it passes limpid and clear.

clear. The impurities are sometimes of a resinous nature, and resist the action of pure water. When this is the case the washed print must be left to dry, and alcohol is then to be poured on and left for a time. After the print is thus cleaned, and all the moisture drained off, the muriatic acid prepared with minium * is to be poured on in sufficient quantity to cover the print; immediately after which another plate of glass is to be laid in contact with the rim of wax, in order to prevent the inconvenient exhalation of the oxygenated acid. In this situation the yellowest print will be seen to recover its original whiteness in a very short time. One or two hours are sufficient to produce the desired effect; but the print will receive no injury if it be left in the acid for a whole night. Nothing more is necessary to complete the work, than to decant off the remaining acid, and wash away every trace of acidity by repeated affusions of pure water. The print being then left to dry (in the sun if possible) will be found white, clear, firm, and in no respect damaged either in the texture of the paper or the tone and appearance of the impression.

VI.

On the Propagation of the Zebra with the Ass †.

AN experiment was made in the year 1773 with a zebra, in the collection of the late Lord Clive, the result of which, though of considerable interest to the natural historian, is no where upon record in any public journal or printed work. A set of questions were proposed at the time of the event to Mr Parker ‡ by Sir Joseph Banks; which, together with the answers, he has at my request permitted me to make use of.

The zebra was first covered by an Arabian horse. For this purpose it was found necessary to bind her, and she shewed great disgust. As she did not conceive, an English ass was procured; to which she shewed a degree of aversion, scarcely if at all less than to the horse, and was subjected to him by the same means. The result of this trial not being more favourable than the other, recourse was had to the extraordinary expedient of painting another ass so as to resemble the zebra. Complete success attended this deception. When the animals were put together, the zebra at first appeared shy; but she received the embraces of the painted ass, and conceived. The offspring was a fine large male foal, which was just turned of six months old at the time of enquiry, namely, December 1773. It resembled both parents; the father as to make, and the mother as to colour; but the colour was not so strong, and the stripes on the shoulders were more conspicuous than on any other part. In answer to a question directed to that object, the relator states it as his opinion, that it would very probably propagate its species, as it did not appear at all like a mule.

In the course of the year after this information was received, his lordship died suddenly, and

* As I have not repeated this process, I cannot estimate how far the presence of the lead may weaken the corrosive action of the acid on the paper; but I should be disposed to recommend a previous dilution of the acid with water. Whoever uses this process will of course make himself master of the proportion of water required to dilute the acid, by making his first trials with an old print of no value. N.

† Communicated by the Right Hon. Sir Joseph Banks, Bart. K. B. P. R. S. &c. &c.

‡ He was either Steward or in some other confidential employ to Lord Clive.

the collection of animals was difpofed of. Sir Jofeph Banks was then abfent from town; and upon his return he was prevented, by this circumftance, either from purchafing the animals or acquiring any further information refpecting the foal. I have lately endeavoured to obtain fome intelligence on this behalf among the dealers in animals, but hitherto without fuccefs.

VII.

On the Procefs of Bleaching with the Oxygenated Muriatic Acid; and a Defcription of a new Apparatus for Bleaching Cloths with that Acid diffolved in Water without the Addition of Alkali. By THEOPHILUS LEWIS RUPP*.

THE arts which fupply the luxuries, conveniences and neceffaries of life have derived but little advantage from philofophers. A view of the hiftory of arts will evince the juftice of this obfervation. In mechanics, for inftance, we find that the moft important inventions and improvements have been made, not through the reafonings of philofophers, but through the ingenuity of artifts, and not unfrequently by common workmen. The chemift in particular, if we except the pharmaceutical laboratory, has but little claim on the arts: on the contrary, he is indebted to them for the greateft difcoveries, and a prodigious number of facts, which form the bafis of his fcience. In the difcovery of the art of making bread, of the vinous and acetous fermentations, of tanning, of working ores and metals, of making glafs and foap, of the action and applications of manures, and in numberlefs other difcoveries of the higheft importance, though they are all chemical proceffes, the chemift has no fhare. But no branch of the ufeful arts is lefs indebted to him than that of changing the colours of fubftances. The art of dyeing has attained a high degree of perfection without the aid of the chemift, who is totally ignorant of the rationale of many of its proceffes, and the little he knows of this fubject is of a late date. The procefs of dyeing the Turkey red has been known and practifed from time immemorial by the moft uncultivated nations, but its theory is not yet underftood by philofophers. The manufacture of indigo and its application have been long known to the planter and the dyer; but it is not more than ten years fince a true theory of them has been formed. The art of printing or topical dyeing is of the greateft antiquity; but the theory of this procefs, and of adjective colours in plain dyeing, was unknown till Mr. Henry developed it in the Memoirs of this Society †. The bleaching or whitening of vegetable fubftances has been long practifed; but the knowledge of its theory could not be antecedent to the æra of pneumatic chemiftry. We might even at this moment have been unacquainted with the caufe of the deftruction of the colouring matter of vegetable fubftances, if the difcovery of the oxygenated muriatic acid, and its effects on colouring matter, had not pointed it out to us. For this difcovery, and its ineftimable advantages, the arts are indebted to the juftly celebrated Scheele; and I am happy to pay this tribute to chemiftry after the mortifying truths which I have ftated above.

M. Berthollet loft no time in applying the properties of this curious and highly interefting fubftance to the moft important practical ufes. His experiments on bleaching with the oxygenated muriatic acid proved completely fuccefsful, and he did not delay to communi-

* Manchefter Memoirs, vol. v. part i.

† Manchefter Memoirs, vol. iii.

cate

cate his valuable labours to the public. The new method of bleaching was quickly and succefsfully introduced into the manufactures of Manchester, Glasgow, Rouen, Valenciennes, and Courtray; and it has since been generally adopted in Great Britain, Ireland, France, and Germany. The advantages which result from this method, which accelerates the procefs of whitening cottons, linens, paper, &c. to a really surprising degree in every season of the year, can be justly appreciated by commercial people only, who experience its beneficial effects in many ways, but particularly in the quick circulation of their capitals.

Great difficulties for a time impeded its progrefs, arising chiefly from prejudice and the ignorance of bleachers in chemical procefses. These obstacles were, however, soon removed by Mr. Watt at Glasgow, and by Mr. Henry and Mr. Cooper at Manchester. Another difficulty presented itself, which had nearly proved fatal to the succefs of the operation. This was the want of a proper apparatus, not for making the acid and combining it with water (for this had been supplied in a very ingenious manner by Mr. Watt and Mr. Berthollet *), but for the purpose of immersing and bleaching goods in the liquor. The volatility of this acid, and its suffocating vapours, prevented its application in the way commonly used in dye-houses. Large cisterns were therefore constructed, in which pieces of stuff were stratified; and the liquor being poured on them, the cisterns were closed with lids. But this method was soon found to be defective, as the liquor could not be equally diffused; the pieces were therefore only partially bleached, being white in some parts and more or lefs coloured in others. Various other contrivances were tried without succefs, till it was discovered that an addition of alkali to the liquor deprived it of its suffocating effects without destroying its bleaching powers. The procefs began then to be carried on in open vefsels, and has been continued in this manner to the present period. The bleacher is now able to work his pieces in the liquor, and to expose every part of them to its action without inconvenience. This advantage is unquestionably great; but it is diminished by the heavy expence of the alkali, which is entirely lost. It is moreover to be feared, that the alkali which is added to the liquor, though it does not destroy its power of bleaching, may diminish it; because a solution of the oxygenated muriat of potash, which differs from the alkaline bleaching liquor in nothing but in the proportion of alkali, will not bleach at all. This is a well-known fact; from which we might infer, that the oxygenated muriatic acid will lose its power of destroying the colouring matter of vegetable substances in proportion as it becomes neutralized by an

* M. Berthollet's apparatus, however, is too complex for the use of a manufactory; Mr. Watt's is better; but a range of four, five or six hogsheads or rum puncheons connected with one another in the manner of Woulfe's distilling apparatus is preferable to either of them. Agitators on M. Berthollet's principle may be applied. The retort or matrafs should be of lead, standing in a water bath; its neck should be of sufficient length to condense the common muriatic acid, which always comes over; and it should form an inclination towards the body of the retort, so that the condensed acid may return into it. I beg leave to observe here, that I always found the liquor to be strongest when the distillation was carried on very slowly. I have also found that the strength of the liquor is much increased by diluting the vitriolic acid more than is usually done. The following proportions afforded the strongest liquor:

Three parts manganese.
Eight parts common salt.
Six parts oil of vitriol.
Twelve parts water.

The proportion of manganese is subject to variation according to its quality.

alkali.

alkali. But as we ſhould not content ourſelves with inferences, however plauſible, when the truth may be eſtabliſhed by experiment, and as I thought the matter of ſufficient importance, I made the following experiments on the ſubject:

I beg leave to premiſe, that in all theſe experiments I made uſe of one and the ſame acid, which was kept in a bottle with a ground glaſs ſtopper, and ſecured from the influence of light. The manner in which I made the experiments was ſimply this: I weighed firſt of all a bottle filled with the colouring ſubſtance which I meant to employ: I then weighed in a large and perfectly colourleſs bottle half an ounce of the acid, to which I immediately, but very gradually, added of the colouring ſubſtance contained in the former bottle till the acid ceaſed to deſtroy any more of its colour. The bottle with the colouring ſubſtance was then weighed again, and the difference between its preſent and original weight was noted. The ſame method was obſerved in all the experiments.

Experiment I. To half an ounce of oxygenated muriatic acid I added a ſolution of indigo in acetous acid *, drop by drop, till the oxygenated acid ceaſed to deſtroy any more colour. It deſtroyed the colour of 160 grains of the acetite of indigo.

Experiment II. A repetition of Experiment I. The colour of 165 grains of acetite of indigo was deſtroyed in this experiment.

Experiment III. A repetition of Experiments I and II. The colour of 160 grains of the acetite was deſtroyed.

Experiment IV. To half an ounce of the oxygenated muriatic acid were added 8 drops of pure potaſh in a liquid ſtate. This quantity of alkali was about ſufficient to deprive the acid of its noxious odour. This mixture deſtroyed the colour of 150 grains of the acetite of indigo.

Experiment V. A repetition of Experiment IV. The colour of 145 grains of the acetite was deſtroyed.

Experiment VI. To half an ounce of the oxygenated muriatic acid, 10 drops of the ſame alkali were added. It deſtroyed the colour of 1[illegible]5 grains of the acetite of indigo.

Experiment VII. A mixture of half an ounce of the oxygenated acid, and 15 drops of the alkali, deſtroyed the colour of 120 grains of the acetite of indigo.

Though I had taken the precaution of avoiding the ſulphuric acid for the reaſon ſtated in the foregoing note, I was not quite ſatisfied with theſe experiments, on account of errors which might have taken place through a double affinity. I therefore made the following experiments, in which I employed a decoction of cochineal in water inſtead of the acetite of indigo.

Experiment VIII. To half an ounce of the oxygenated muriatic acid, a decoction of cochineal was added till the acid ceaſed to act on its colour. It deſtroyed the colour of 390 grains of the decoction.

* It has been uſual to eſtimate the ſtrength of the oxygenated muriatic acid by a ſolution of indigo in ſulphuric acid. This method was inadmiſſible in theſe experiments on the comparative ſtrength of the bleaching liquor with and without alkali; becauſe the ſulphuric acid would have decompoſed the muriat of potaſh, and thereby produced errors. I therefore added to a ſolution of indigo in ſulphuric acid after it had been diluted in water, acetite of lead, till the ſulphuric acid was precipitated with the lead. The indigo remained diſſolved in the acetous acid.

Experiment IX.

Experiment IX. A repetition of Experiment VIII. The colour of 385 grains of the decoction was destroyed in this experiment.

Experiment X. To half an ounce of the acid six drops of the liquid alkali were added. This mixture destroyed the colour of 315 grains of the decoction.

Experiment XI. Eight drops of the alkali were mixed with half an ounce of the acid. This mixture destroyed the colour of 305 grains of the decoction.

On a comparative view of the results of these experiments, it will appear that an addition of potash to the bleaching liquor impairs its strength considerably. This diminution of power and the expence of potash are a serious loss in an extensive manufacture. It would therefore be desirable to have an apparatus for the use of the pure oxygenated muriatic acid simply dissolved in water, which is at once the cheapest and best vehicle for it. This apparatus must be simple in its construction, and obtained at a moderate expence; it must confine the liquor in such a manner as to prevent the escape of the oxygenated muriatic acid gas, which is not only a loss of power, but also an inconvenience to the workmen and dangerous to their health; and it must at the same time be so contrived, that every part of the stuff which is confined in it shall certainly and necessarily be exposed to the action of the liquor in regular succession. Having invented an apparatus capable of fulfilling all these conditions, I have the pleasure of submitting a description of it to the Society by means of the annexed drawing.

Explanation of Plate XI.

Fig. 1, is a section of the apparatus. It consists of an oblong deal cistern, A B C D, made water-tight. A rib, E E, of ash or beech wood, is firmly fixed to the middle of the bottom C D, being mortised into the ends of the cistern. This rib is provided with holes at F F, in which two perpendicular axes are to turn. The lid, A B, has a rim G G which sinks and fits into the cistern. Two tubes H H are fixed in the lid, their centres being perpendicular over the centres of the sockets F, F, when the lid is upon the cistern. At I, is a tube by which the liquor is introduced into the apparatus. As it is necessary that the space within the rim G G be air-tight, its joints to the lid and the joints of the tubes must be very close, and, if necessary, secured with pitch. Two perpendicular axes, K, L, made of ash or beech wood, pass through the tubes H, H, and rest in the sockets, F, F. A piece of strong canvas, M, is sewed very tight round the axis K, one end of it projecting from the axis. The other axis is provided with a similar piece of canvas. N, are pieces of cloth rolled upon the axis L. Two plain pulleys, O, O, are fixed to the axes in order to prevent the cloth from slipping down. The shafts are turned by a moveable handle P. Q, a moveable pulley, round which passes the cord R. This cord, which is fastened on the opposite side of the lid (see fig. 2) and passes over the small pulley S, produces friction by means of the weight T. By the spigot and fausset V, the liquor is let off when exhausted.

Fig. 2. A plan of the apparatus with the lid taken off.

The Manner of using the Apparatus.

The dimensions of this apparatus are calculated for the purpose of bleaching twelve or fifteen pieces of 4-4 calicoes, or any other stuffs of equal breadth and substance. When the goods are ready for bleaching, the axis L is placed on a frame in a horizontal position, and one of the pieces N being fastened to the canvas M, by means of wooden skewers in the manner represented in fig. 1, it is rolled upon the axis by turning it with the handle P. This

 operation

operation muſt be performed by two perſons; the one turning the axis, and the other directing the piece, which muſt be rolled on very tight and very even. When the firſt piece is on the axis, the next piece is faſtened to the end of it by ſkewers, and wound on in the ſame manner as the firſt. The ſame method is purſued till all the pieces are wound upon the axis. The end of the laſt piece is then faſtened to the canvas of the axis K. Both axes are afterwards placed into the ciſtern with their ends in the ſockets F, F, and the lid is put on the ciſtern by paſſing the axes through the tubes H, H. The handle P is put upon the empty axis, and the pulley Q upon the axis on which the cloth is rolled; and the cord R with the weight T is put round it, and over the pulley S. The uſe of the friction produced by this weight is to make the cloth wind tight upon the other axis. But as the effect of the weight will increaſe as one cylinder increaſes and the other leſſens, I recommend that three or four weights be ſuſpended on the cord, which may be taken off gradually as the perſon who works the machine may find it convenient. As the weights hang in open hooks which are faſtened to the cord, it will be little or no trouble to put them on and to remove them.

Things being thus diſpoſed, the bleaching liquor is to be transferred from the veſſels in which it has been prepared into the apparatus, by a moveable tube paſſing through the tube I, and deſcending to the bottom of the ciſtern. This tube being connected with the veſſels by means of leaden or wooden pipes provided with cocks, hardly any vapours will eſcape in the transfer. When the apparatus is filled up to the line *a*, the moveable tube is to be withdrawn, and the tube I cloſed. As the liquor riſes above the edge of the rim G, and above the tubes H, H, it is evident that no evaporation can take place except where the rim does not apply cloſely to the ſides of the box: which will, however, form a very trifling ſurface if the carpenter's work be decently done. The cloth is now to be wound from the axis L upon the axis K, by turning this; and when this is accompliſhed, the handle P and pulley Q are to be changed, and the cloth is to be wound back upon the axis L. This operation is of courſe to be repeated as often as neceſſary. It is plain, that by this proceſs of winding the cloth from one axis upon the other, every part of it is expoſed in the moſt complete manner to the action of the liquor in which it is immerſed. It will be neceſſary to turn at firſt very briſkly, not only becauſe the liquor is then the ſtrongeſt, but alſo, becauſe it requires a number of revolutions, when the axis is bare, to move a certain length of cloth in a given time, though this may be performed by a ſingle revolution when the axis is filled. Experience muſt teach how long the goods are to be worked; nor can any rule be given reſpecting the quantity and ſtrength of the liquor in order to bleach a certain number of pieces. An intelligent workman will ſoon attain ſufficient knowledge of theſe points. It is hardly neceſſary to obſerve, that if the liquor ſhould retain any ſtrength after a ſet of pieces are bleached with it, it may again be employed for another ſet.

With a few alterations, this apparatus might be made applicable to the bleaching of yarn. If, for inſtance, the pulley O were removed from the end of the axis K, and fixed immediately under the tube H; if it were perforated in all directions, and tapes or ſtrings paſſed through the holes, ſkains of yarn might be tied to theſe tapes underneath the pulley, ſo as to hang down towards the bottom of the box. The apparatus being afterwards filled with bleaching liquor, and the axis turned, the motion would cauſe every thread to be acted upon by the liquor. Several axes might thus be turned in the ſame box, and, being connected with each other by pulleys, they might all be worked by one perſon at the ſame time; and as all would turn

turn the same way, and with the same speed, the skains could not possibly entangle each other.

In order to shew the usefulness of this apparatus still more clearly, I request the society to attend to the following statement of the expence of a given quantity of bleaching liquor, with and without alkali, but of equal strength.

With ALKALI *.	£.	s.	d.
80 lb. of salt, at $1\frac{1}{2}$d. per lb. — — —	0	10	0
60 lb. of oil of vitriol, at $6\frac{1}{2}$d. per lb. — — —	1	12	6
30 lb. of manganese — — — — —	0	2	6
20 lb. of pearl-ashes, at 6d. per lb. — — — —	0	10	0
	2	15	0
But it appears by the foregoing experiments, that the liquor loses strength by an addition of alkali. The value of this loss, which on an average amounts to 15 per cent. must be added to the expence — — —	0	8	3
	3	3	3

Without ALKALI.			
80 lb. of salt — — — — —	0	10	0
60 lb. of oil of vitriol — — — — —	1	12	6
36 lb. of manganese — — — —	0	2	6
	£. 2	5	0

It appears from this calculation, that a certain quantity of the liquor for the use of any apparatus costs only 2 *l.* 5 *s.*; but, that the same quantity of the alkaline liquor costs 3 *l.* 3 *s.* 3 *d.* which is 40 *per cent.* more than the other. The aggregate of so considerable a saving must form a large sum in the extensive manufactures of this country.

VIII.

Experimental Researches concerning the Principle of the lateral Communication of Motion in Fluids applied to the Explanation of various Hydraulic Phenomena. By Citizen J. B. VENTURI, Professor of Experimental Philosophy at Modena, Member of the Italian Society of the Institute of Bologna, the Agrarian Society of Turin, &c.

(Continued from Page 179 of the present Volume.)

Proposition V.

IN an additional conical tube, the pressure of the atmosphere increases the expenditure in the proportion of the exterior section of the tube to the section of the contracted vein, whatever may be the position of the tube, provided its internal figure be adapted throughout to the lateral communication of motion.

We have seen (Proposition III.) that the pressure of the atmosphere increases the expen-

* I make no mention of the expence attending the preparation of the liquor, it being the same in both cases.

diture through additional tubes, whatever may be their pofition. We fhall in the next place examine the mode of action by which the atmofphere produces this augmentation, and determine the refult from its caufe. I fhall begin with the cafe beft adapted to favour the action of the atmofphere, which is, that of conical diverging tubes of a certain form, which we have not yet confidered.

Let the extremity AB, fig. 10, Plate VIII. of the tube A B E F be applied to an orifice formed in a thin plate. The part A B C D is nearly of the figure of the contracted vein, which form has been fhewn to make no perceptible alteration in the expenditure (Experiment IV.) The fluid which iffues through C D is difpofed to continue its courfe in the cylindrical form C D H G. But if the lateral parts of the diverging conical tube C E G, D F H, contain a mafs of the fluid at reft, the cylindrical ftream C D H G will communicate its motion to the lateral parts (by Prop. I.) fucceffively from part to part. And provided the divergence of the fides C E, D F, be fuch as is beft adapted to the fpeedy and complete lateral communication of motion, all the fluid contained in the truncated cone C D E F will at length acquire the fame velocity as that of the ftream which continues to iffue through C D. On this fuppofition, while the fluid ftratum C D Q R, preferving its velocity and thicknefs, would pafs into R Q T S, a vacuum would be formed in the folid zone R m r S Q n o T. Or otherwife, if it be fuppofed that the ftratum C D Q R, preferving its progreffive velocity, fhould enlarge in R Q T S, this cannot happen without its becoming thinner, and detaching itfelf from the ftratum which follows, and by that means leaving a vacuum equal in magnitude to the zone laft mentioned. A fimilar effect would take place through the whole of the tube C E; and if the quantity C m be fuppofed to be invariable, the fum of all thefe void fpaces will be equal to the folid zone V E x G z Y F H.

From this confideration, we fee that the lateral communication of motion caufes the fame effect in a conical tube, whether horizontal or vertical, as gravity produces in the defcending tube of Propofition IV. The atmofphere in this cafe alfo renders part of its preffure active on the refervoir, and at E F. If the action of the atmofphere upon the refervoir increafes the velocity of the fection C D, this velocity will communicate itfelf likewife to the whole fluid C D F E, and the tendency to a vacuum will take place as before; but fince the action of the atmofphere is exerted equally at E F, it will take away at E F all the velocity which it added at C D; fo that, being deducted from the fame mafs, and in the fame time, at E F, the fluid will not ceafe to be continuous in the pipe. It is found by computation, that this will happen when the velocity of C D is increafed in the ratio of CD^2 to EF^2.

By applying the general laws of motion to the lateral fluid filaments of the ftream which iffues through A B, it is found that they tend to defcribe a curve which commences within the refervoir, for example, at A, and continues towards C S E. To determine the nature of this curve, it is requifite to know, and to combine together by calculation, the mutual convergency of the fluid filaments in A B, the law of the lateral communication of motion between the filaments themfelves and their divergent progreffion from C to E. Thefe combinations and calculations are perhaps beyond the utmoft efforts of analyfis. While the tube A B F E poffeffes a different figure from this natural curve, the refults of experiment will always differ more or lefs from the theory.

Experiment XIII. The compound tube A B F E of the fame fig. 10, having the following dimenfions dimenſions in lines A B = E F = 18 ; A C = 11 ; C D = 15,5 ; C G = 49 ; and this tube being applied to the orifice P, fig. 1, under a charge of 32,5 inches, the four cubical feet of water were emitted in 27″,5.

We have ſeen that, in the third experiment, under like circumſtances, the orifice through a thin plate afforded four cubic feet of water in 41″. The contracted vein was 0,64 of the orifice. Conſequently, by following the enunciation of the theorem, the expence through the pipe A B F ought to be made in 26″,24. The experiment falls ſhort in the quantity 1″,26.

Experiment XIV. Between the two conical tubes of the preceding experiment is interpoſed a cylindrical tube three inches long and 15,5 lines in diameter. The interpoſition of the cylinder between the two cones was as in fig. 13. This addition retarded the expenditure 1″, the time now being 28″,5.

Experiment XV. The charge of the reſervoir being conſtantly 32,5 inches, the portion of the tube A B C D, fig. 11, had the ſame dimenſions as before; the tube C D F E was 78 lines in length, and its diameter 23 lines. To this horizontal tube I added three glaſs tubes; the firſt D X at C D; the ſecond N Y at the diſtance of 26 lines from the firſt; and the third O Z at 26 lines diſtance from the ſecond. The lower extremities of theſe three tubes were plunged in the mercury of the veſſel Q. When the water was ſuffered to flow through the tube A E F B the mercury roſe 53 lines in the tube D X; 20,5 in N Y, and 7 in O Z. Theſe quantities correſpond with 62 inches height of water in D X; 24 inches in N Y; and 8,1 in O Z. The expenditure of four cubic feet was effected in 25″.

I cut off the portion P N F E of the tube, and the remaining pipe A B N P emitted the ſame quantity in 31″.

In the truncated conical tube A C P B D N, the ſection P N is to the ſection of the contracted vein (namely 0,64 of the ſection A B) as 41″ to 30″. In the experiment with this laſt truncated tube the retardation is conſequently no more than 1″ leſs than the theory.

In the entire tube C D F E we have $\sqrt{62 + 32{,}5} : \sqrt{32{,}5} = 41'' : 24''$. The difference of 38 inches elevation of water in the two tubes D X, N Y, muſt ariſe from the motion of the fluid from C to P; it is 1-13th leſs than by the theory. The loſs is ſucceſſively greater in the two portions P Q, Q E. The reaſon of this is, that the ſtream deſcends as it moves from C D, ſo that the lateral communication not being made uniformly through the whole of any one ſection, the different parts of the current acquire irregular motions, and even eddies within the tube; whence the jet comes forth by leaps and irregular ſcattering.— Theſe uncertain motions cannot be reduced to the theory, and manifeſt themſelves the more, the longer or the more diverging the ſides of the tube. The effects conſequently remain to be aſcertained by experiment.

Experiment XVI. I conſtructed a tube C D F E as before, (fig. 11) 148 lines long, and 27 lines in diameter at E F, the reſt of the apparatus being the ſame as in the foregoing experiment. The expenditure of four cubical feet was effected in 21″; the inequality and irregularity of motion in the ſtream were greater in this experiment than in the foregoing.

It was uſeleſs to prolong the tube C D F E beyond 148 lines; for the ſtream did not in that caſe fill the portion of tube added beyond that length, and the expenditure remained conſtantly at 21″. This expenditure is nearly double what took place through the ſimple aperture in a thin plate; and it is the greateſt I have been able to obtain by additional tubes, the axis of which had an horizontal poſition under a charge of 32,5 inches.

 It

It is true, that by prolonging the tube CDFE to the length of 204 lines in the horizontal position, the four cubic feet flowed out in 19″. But to obtain this effect, I found it necessary to fix a prominence within the tube at O, which forced the fluid to fly upwards, and by that means to fill the whole tube.

Experiment XVII. In this experiment the horizontal tube CDFE, fig. 11, was more divergent than in the foregoing trials. It was 117 lines long, and 36 lines in diameter at EF. The rest of the apparatus was the same as before. The expenditure was made in 28″; the stream did not fill the whole section EF. The result was the same when successive portions of the pipe were cut off, until CE was no longer than 20 lines, and the external diameter 18 lines. In this case the stream filled the pipe, and the expenditure was also made in 28″.

When the length CE was 20 lines, its external diameter EF was increased to 20 lines. In this case the stream was detached from the sides of the tube, and the expence of four feet took place in 42 seconds, as in the VIth experiment.

These experiments teach us, that by varying the divergence of the sides of tubes, the lateral communication of motion has a minimum and a maximum of effect. The minimum is seen in the last experiment. It appears that the lateral communication ceases to produce its effect when the angle made by the sides of the tube with each other exceeds 16 degrees. The XIIIth experiment nearly determines the maximum of the effect when the same angle is about 3 degrees. These limits may also, perhaps, in a small degree depend upon some function of the velocity.

[*To be continued.*]

IX.

An Account of the Means employed to obtain an overflowing Well. *By Mr.* BENJAMIN VULLIAMY.*

SIR,

PERMIT me, in compliance with your request, to give you a short account of the well at Norland-house, belonging to Mr. L. Vulliamy; a work of great labour and expence, executed entirely under my direction, and finished in November 1794.

Before I began the work, I considered that it would be of infinite advantage should a spring be found strong enough to rise over the surface of the well; and though I thought it very improbable, yet I resolved to take from the beginning the same precautions in doing the work as if I had been assured that such a spring would be found. But although this very laborious undertaking has succeeded beyond my expectation, yet, from the knowledge I have acquired in the progress of the work, I am of opinion, that it will very seldom happen that the water will rise so high; nor will people, I believe, in general, be so indefatigable as I have been in overcoming the various difficulties that did and ever will occur in bringing such a work to perfection.

In beginning to sink this well, which has a diameter of four feet, the land springs were stopped out in the usual manner, and the well was sunk and steined to the bottom. When the workmen had got to the depth of 236 feet, the water was judged not to be very far off,

* In a letter to the Right Honourable Sir Joseph Banks, Bart. K. B. P.R.S. Phil. Transf. 1797.

and

and it was not thought fafe to fink any deeper. A double thicknefs of fteining was made about fix feet from the bottom upwards, and a borer of $5\frac{1}{4}$ inches diameter was made ufe of. A copper pipe of the fame diameter with the borer was driven down the bore hole to the depth of 24, at which depth the borer pierced through the rock into the water; and by the manner of its going through it muft probably have broken into a ftratum containing water and fand. At the time the borer burft through, the top of the copper pipe was about three feet above the bottom of the well: a mixture of fand and water inftantly rufhed in through the aperture of the pipe. This happened about two o'clock in the afternoon, and by twenty minutes paft three o'clock the water of the well ftood within 17 feet of the furface. The water rofe the firft 124 feet in eleven minutes, and the remaining 119 feet in one hour and nine minutes. The next day feveral buckets of water were drawn out, fo as to lower the water four or five feet; and in a fhort time the water again rofe within 17 feet of the furface. A found line was then let down into the well in order to try its depth. To our great furprife, the well was not found by 96 feet fo deep as it had been meafured before the water was in it; and the lead brought up a fufficient quantity of fand to explain the reafon of this difference, by fhewing that the water had brought along with it 96 feet of fand into the well. Whether the copper pipe remained full of fand or not, is not eafy to be determined; but I fhould rather be inclined to think it did not.

After the well had continued in the fame ftate feveral days, the water was drawn out fo as to lower it eight or ten feet; and it did not rife a ain by about a foot fo high as it had rifen before. At fome days interval, water was again drawn out, fo as to lower the water as before; which at each time of drawing rofe lefs and lefs, until after fome confiderable time it would rife no more; and the water being then all drawn out, the fand remained perfectly dry and hard. I now began to think the water loft; and confequently that all the labour and expence of finking this well, which by this time were pretty confiderable, had been in vain. There remained no alternative but to endeavour to recover it by getting out the fand, or all that had been done would be ufelefs; and although it became a more difficult tafk than finking a new well might have been, yet I determined to undertake it, becaufe I knew another well might alfo be liable to be filled with fand in the fame manner that this was. The operation of digging was again neceffarily reforted to, and the fand was drawn up in buckets until about 60 feet of it were drawn out; confequently there remained only 36 feet of fand in the well: that being too light to keep the water down, in an inftant it forced again into the well with the fame violence it had done before; and the man who was at the bottom getting out the fand was drawn up almoft fuffocated, having been covered all over by a mixture of fand and water. In a fhort time the water rofe again within 17 feet of the furface, and then ceafed to rife as before. When the water had ceafed rifing the founding line was again let down, and the well was found to contain full as much fand as it did the firft time of the water's coming into it.

Any further attempt towards recovering the water appeared now in vain; and moft people would, I believe, have abandoned the undertaking. I again confidered, that the labour and the expence would be all loft by fo doing; and I determined without delay to fet about drawing the fand out through the water by means of an iron box made for that purpofe, without giving it time to harden as before. The labour attending on this operation was very great, as it was neceffary continually to draw out the fand, and thereby to prevent the

fand

fand from hardening. What rendered this operation the more difcouraging was, that frequently after having drawn out 6 or 7 feet of fand in the courfe of the day, upon founding the next morning the fand was found lowered only one foot in the well, fo that more fand muft have come in again. This, however, did not prevent me from proceeding in the fame manner during feveral days, though with little or no appearance of any advantage arifing from the great exertions we were making. After perfevering however for fome confiderable time, we perceived that the water rofe a little nearer the furface, and I began to entertain fome hopes, that it might perhaps rife high enough to come above the level of the ground; but when the water had rifen a few feet higher in the well fome difficulties occurred, occafioned by accidental circumftances, which very much delayed the progrefs of the work; and it remained for a confiderable time very uncertain whether the water would run over the top of the well or not.

These difficulties being at length furmounted, we continued during feveral days the procefs before mentioned of drawing out the fand and water alternately; and I had the fatiffaction of feeing the water rife higher and higher, until at laft it ran over the top of the well into a temporary channel that conveyed it into the road. I then flattered myfelf, that every difficulty was overcome; but a few days afterwards, I difcovered, that the upper part of the well had not been properly conftructed, and it became neceffary to take down about ten feet of brick work. The water, which was now a continued ftream, rendered this extremely difficult to execute. I began by conftructing a wooden cylinder 12 feet long, which was let down into the well, and fufpended to a ftrong wooden ftage above, upon which I had fixed two very large pumps of fufficient power to take off all the water that the fpring could furnifh at 11 feet below the furface. The ftage and cylinder were fo contrived as to prevent the poffibility of any thing falling into the well; and I contrived a gage by which the men upon the ftage could always afcertain to the greateft exactnefs the height of the water within the cylinder. This precaution was effentially neceffary, in order to keep the water a foot below the work which was doing on the outfide of the cylinder to prevent the new work from being wetted too foon. After every thing was prepared, we were employed eight days in taking down 10 feet of the wall of the well, remedying the defects, and building it up again; during which time ten men were employed, five relieving the other five, and the two pumps were kept conftantly at work during one hundred and ninety-two hours. By the affiftance of the gage, the water was never fuffered to rife upon the new work until it was made fit to receive it. When the cylinder was taken out, the water again ran over into the temporary channel that conveyed it into the road.

The top of the well was afterwards raifed 18 inches, and conftructed in fuch a manner as to be able to convey the water five different ways at pleafure, with the power of being able to fet any of thefe pipes dry at will, in order to repair them whenever occafion fhould require. The water being now entirely at command, I again refolved upon taking out more fand, in order to try what additional quantity of water could be obtained thereby. I cannot exactly afcertain the quantity of fand taken out, but the increafe of water obtained was very great; as inftead of the well difcharging thirty gallons of water in a minute, the water was now increafed to forty-fix gallons in the fame time.

If you think, Sir, that the above account of an overflowing well, the joint production of nature and art, is deferving your attention, I feel myfelf much gratified in the pleafure I have

in

in giving you this deſcription of it; and have the honour of being with the greateſt regard, &c. &c. B. VULLIAMY.

Explanation of Plate XII.

Fig. I.

a Top of the well, with the water running over.

bb Ground line.

c Sand lying in the well.

d Copper pipe.

ffffff Steining of the well.

gg Double ſteining ſix feet from the bottom upwards.

h Stratum which the end of the copper pipe was driven into.

Fig. II and III.

Iron box for drawing ſand out of the well, weighing about 60lbs. one foot ſquare, and two feet nine inches long.

a Handle of the box.

b A flap or door which opens inwards by a joint at c. There is another door like this on the other ſide.

c The joint.

d The centre or pin of the joint.

SCIENTIFIC NEWS, AND ACCOUNTS OF BOOKS.

IN a Letter from Sig. Fabbroni to Sig. D. Luigi Targioni of Naples, inſerted in the 85th No. of the Giornale Letterario di Napoli, I find an account of a very effectual compoſition for extinguiſhing fire, invented by M. Von Aken. The compoſition is,

Burnt alum	pounds	30
Green vitriol powdered		40
Cinabreſe or red ochre in powder		20
Potters' clay, or other clay, alſo powdered		200
Water		630

With 40 meaſures of this mixture an artificial fire was extinguiſhed under the direction of the inventor by three perſons, which would have required the labour of 20 men and 1500 meaſures of common water. Sig. Fabbroni was commiſſioned to examine the value of this invention, and found in his comparative trials with engines of equal power, worked by the ſame number of men, that the mixture extinguiſhed the materials in combuſtion in one ſixth part leſs time, and three eighths leſs of fluid than when common water was uſed. He obſerved, as might indeed have been imagined from the nature of the material, that the flame diſappeared wherever the mixture fell, and that the ſaline, metallic and earthy matters formed an impenetrable lute round the hot combuſtible matter, which prevented the acceſs of the air, and conſequently the renewal of the deſtructive proceſs.

Sig. Fabbroni eſtimates the price of this compoſition at about one ſoldo (or halfpenny) per pound, but remarks, that it requires fewer hands, and affords the incalculable advantage of a ſpeedier extinction of the fire. Whence he concludes, that it might be adviſeable to keep the ingredients ready powdered to mix with water.

I have given this abridged account, becaufe it is evident that fuch inventions are worthy the attention of philofophers and economifts, even though in the firft applications they may prove lefs advantageous than their inventors may be difpofed to think. It is fcarcely probable that this practice in the large way, with an engine throwing upwards of 200 gallons (value about 3l. 10s.) each minute, would be thought of or adopted, or that a fufficient ftore of the materials would be kept in readinefs; fince at this rate the expenditure for an hour would demand a provifion to the amount of 210l. fterling. But in country places the procefs, or fome variation of it, might be applied with fufficient profit in the refult; more efpecially if it be confidered that common falt or alum, or fuch faline matter as can be had and mixed with the water, together with clay, chalk, or lime, ochreous earth or common mud, or even thefe laft without any falt, may anfwer the purpofe of the lute with more or lefs effect, and extinguifh an accidental fire with much greater fpeed and certainty than clear water would do.

Mr. Park has circulated Propofals for publifhing by Subfcription (under the Patronage of the African Affociation) his "Travels in the interior Parts of Africa, by Way of the River Gambia, performed in the Years 1795, 1796, and 1797, by the Direction and at the Expence of that Affociation." The Work will form One handfome Volume Quarto, and is expected to be ready for delivery early in the enfuing Seafon. One Guinea is to be paid at the time of fubfcribing, which it is expected will be the price of the Book; but as the charges are not at prefent afcertained, it is underftood that a further payment of Half-a-Guinea will be expected, provided the Committee of the African Affociation fhall certify that fuch demand is reafonable. Subfcriptions are taken by G. Nicol, Bookfeller, Pall-Mall.

Proceedings of the Affociation for promoting the Difcovery of the interior Parts of Africa, containing an Abftract of Mr. Park's Account of his Travels and Difcoveries, abridged from his own Minutes by Bryan Edwards, Efq. Alfo Geographical Illuftrations of Mr. Park's Journey, and of North Africa at large. By Major Rennell. London: Printed for the Affociation. Quarto, 162 pages, with the following Maps by Major Rennell. 1, The Route of Mr. Mungo Park upon a large Scale. 2. The Lines of Magnetic Variation in the Seas round Africa; and 3. A Map fhowing the Progrefs of Difcovery and Improvement in the Geography of North Africa. The Scale of this interefting Map, which comprehends the whole of Africa, from the Mediterranean Sea to the Equator, affords five Equatorial Degrees in two Inches. The Work has no Bookfeller's name, and is not vendible.

The title-page renders it needlefs to repeat, that this book confifts of two diftinct works. Of the latter, which adds no fmall portion to the well-acquired fame of its author, I have not yet been able to fatisfy myfelf that any abridgment can be offered to the Public without mutilations, which fuch a fubject cannot endure. The reader has already been prefented with the concluding chapter in our prefent number. The whole will be re-printed, together with the Maps, in Mr. Park's own work. Mr. Edwards's abftract of Mr. Park's Travels contains the following particulars:

From the houfe of Dr. Laidley at Pifania, on the banks of the river Gambia, but three degrees

degrees more wefterly than the mouth of that river, Mr. Park departed to the eaftward for the kingdom of Woolli with two Negro fervants, himfelf on horfeback and his fervants each on an afs. He carried a fmall affortment of beads, amber, and tobacco, a few changes of linen and apparel, a pocket fextant, a magnetic compafs, and a thermometer, together with two fowling-pieces, two pair of piftols, and fome other fmall articles. At Medina, the capital of Woolli, he was hofpitably received, and proceeded to the kingdom of Bondou, where the foveieign compelled him to furrender his coat, but neverthelefs gave him five drams of gold duft and plenty of provifions. From the capital of Bondou he travelled through Kajaaga, which is bounded on the North by the Senegal river, where the French formerly had a fmall factory. The king commanded that he fhould be brought before him; but Mr. Park, who had been cautioned to avoid him, declined the interview, and efcaped with the lofs of about half his goods and apparel. Hence he was conducted to Kaffon, under the protection of the nephew of the king of that diftrict, where he was treated with great kindnefs and hofpitality, but detained fome weeks an account of the extreme curiofity of the natives to behold an European. Hence he proceeded ftill further eaftward to Kemmoo, a large and populous town, fince deftroyed, but at that tlme the metropolis of an extenfive kingdom called Kaarta. The king of this place, who received our traveller with great kindnefs, was at that time at war with the neighbouring nation of Bambarra, to the eaftward, through which the Joliba or Niger river flows. Unfortunately for Mr. Park, it was the opinion of the fovereign of Kaarta, that he could not with fafety pafs into Bambarra immediately from his dominions; in confequence of which he advifed him to fhape his courfe to the northward into the territory of the Moors, called Ludamar, on the border of the Great Defert; through which territory he might continue his route eafterly, and enter Bambarra on the northern fide. By complying with thefe inftructions, Mr. Park entered the frontier town of the Moors, called Jarra, about a degree to the northward of Kemmoo, near which he paffed through the village of Simbing, whence the laft difpatch of Major Houghton written with pencil was received.

Thus far our traveller had continued his journey to the eaftward declining to the north, through fix degrees of longitude with about a degree and a half of northing, the town of Jarra being placed in the map in about 15° 5′ north latitude. The territory through which he paffed was very generally clothed with native woods, and prefented to the eye an appearance of great uniformity. In his progrefs eaftward the country rofe into hills, and the foil became various, but was every where fertile in fuch places as had been cleared. Bondou in particular is a land abounding with black cattle, fheep, goats, and poultry, with an excellent breed of horfes, though the ufual beaft of burthen in all the Negro territories is the afs. Animal labour is no where applied to agricultural purpofes. The land is cultivated by flaves, and affords plenty of rice and Indian corn. The Pagans make an intoxicating liquor from honey. The woods furnifh a fmall fpecies of antelope, of which the venifon is highly efteemed. Among wild animals in thefe countries, the moft common are the hyena, the panther, and the elephant. The latter is often deftroyed for the fake of its teeth, but they have not yet tamed it for the fervice of man.

Befides the grains proper to tropical climates, the inhabitants cultivate in confiderable quantities, ground nuts, yams, and pompions. They likewife raife cotton and indigo, and

 have

have fufficient fkill to convert thefe materials into tolerably fine cloth of a rich blue colour, and they make good foap from a mixture of ground nuts and a ley of wood afhes.

Their trade with the Whites is compofed of flaves, gold-duft, ivory, and bees-wax. Their inland traffic confifts chiefly of falt procured from the Moors, and warlike ftores obtained from the European traders on the Gambia river. Thefe articles are fold again to itinerant merchants called Slatees, who come down annually from diftant countries, fome of which are unknown even by name to the natives of the coaft, with flaves and a commodity called fhea-toulou, or tree-butter. This butter, in Mr. Park's opinion, befides the advantage of its keeping without falt the whole year, is whiter, firmer, and of a richer flavour than the beft butter he ever tafted made from cow's milk. The tree which affords it very much refembles the American oak; and the nut, from the kernel of which the butter is prepared by boiling it in water, has fomewhat the appearance of a Spanifh olive, and is enveloped in a fweet pulp under a thin green rind. The growth and preparation of this commodity are among the firft objects of African induftry in the Eaftern States, to which Mr. Park had accefs. The natives of the Gambia countries are alfo fupplied, in confiderable quantities, with fweet-fmelling gums and frankincenfe from Bondou.

The government in all thefe petty States, though monarchical, is no where abfolute. The chiefs form an ariftocracy, which greatly reftrains the powers of the Sovereign, and prevent him from declaring war or concluding peace without their confent. Every confiderable town is governed by a magiftrate, whofe office is hereditary, and who collects the duties and cuftoms from traders, which are paid in kind. The lower orders or bulk of the people are in a ftate of flavery or vaffalage to individual proprietors; but the power of the mafter, as well with regard to treatment, as the difpofal of the flave to a ftranger, is limited with regard to natives. Thefe indulgencies are not however extended to captives taken in war, or obtained in traffic.

To return to Mr. Park, whom we left at Jarra in the power of the Moors, a fet of the worft fanatics, who confider it as a meritorious act to deftroy a Chriftian. After a fortnight's waiting, permiffion arrived from Ali, the Moorifh chieftain or king of the country, for him to proceed in his journey to the eaftward. With much difficulty, danger and infult, he fucceeded in paffing through a diftrict of near two degrees in length, and was within two days journey of the frontier town of Bambarra, when he was carried back to the Moorifh camp by order of the chief. On his arrival he was thrown into confinement, in which he remained for eight or ten weeks expofed to daily infult, robbed of all his effects, in danger of perifhing from the frequent want of food and every other neceffary of life, with no other probable confequence to expect than ultimately to perifh by the caprice or fanaticifm of the barbarians around him. Here it was that he learned fome particulars of the death of Major Houghton, who was feduced into the Defert by the Moors, robbed of all his property, and died either for want of fuftenance, or by the violence of thofe who refufed to fupply that want. For the particulars of Mr. Park's adventures we muft wait till his work appears. He fucceeded in July 1796 in efcaping from his oppreffors. He was fortunate enough to procure his own horfe, faddle and bridle, a few articles of his apparel, and his pocket compafs, which he had concealed in the fand. The joy he experienced at his efcape foon fubfided into more anxious emotions. Alone in the woods of Africa, expofed to the forocity of wild beafts, and the dread of meeting again with men more ferocious

cious than thofe animals ; finking under the rage of hunger, and the ftill more intolerable torture of thirft, it was in vain that he chewed the bitter leaves of the trees, or climbed to look around him for a watering-place. A feafonable fhower however faved him from perifhing during the firft night ; and after a weary courfe without food or water for the greateft part of the day following, he had the good fortune to meet with relief among a few huts of Negro fhepherds. In this manner, and with no better dependance for fupport than the kindnefs of the moft wretched of human beings, he proceeded on the object of his miffion for fifteen days ; when, on the morning of the 16th, having been joined by fome Negroes who were travelling to the town of Sego, he had the inexpreffible fatisfaction of beholding the object of his wifhes, the long fought Niger glittering to the morning fun as broad as the Thames at Weftminfter, and flowing flowly from weft to eaft through the middle of a very extenfive town, which his fellow travellers told him was Sego, the capital of the great kingdom of Bambarra, which Major Rennell places in 14° 10′ North latitude, and 2° 26′ Weft longitude from Greenwich.

[*The remainder of this Abftract in our next.*]

A TABLE for reducing the Unities of the English Inch, Gallon, and Grain into Metres, Litres, and Grammes.

Eng. Measures	Inches in Metres.	Cubic Inches in Litres.	Ale Gallons of 282 Inches in Litres.	Wine Gallons of 231 Inches in Litres.	Grains in Grammes.	Eng. Measures	Inches in Metres.	Cubic Inches in Litres.	Ale Gallons of 282 Inches in Litres.	Wine Gallons of 231 Inches in Litres.	Grains in Grammes.
1	0.0254	0.0164	4.6168	3.7821	0.0435	51	1.2950	0.8349	235.46	192.89	2.2207
2	0.0508	0.0327	9.2336	7.5643	0.0871	52	1.3204	0.8513	240.07	196.67	2.2642
3	0.0762	0.0491	13·850	11.346	0.1306	53	1.3458	0.8677	244.69	200.45	2.3078
4	0.1016	0.0655	18.467	15.128	0.1742	54	1.3711	0.8840	249.31	204.24	2.3513
5	0·1270	0.0819	23.084	18.911	0.2177	55	1.3965	0.9004	253.92	208.02	2.3948
6	0.1523	0.0982	27·701	22.693	0.2613	56	1.4219	0.9168	258.54	211.80	2.4384
7	0.1777	0.1146	32.318	26.475	0.3048	57	1.4473	0.9332	263.16	215.58	2.4819
8	0.2031	0.1310	36.934	30.257	0.3483	58	1.4727	0.9495	267.77	219.36	2.5255
9	0.2285	0.1473	41.551	34.039	0.3919	59	1.4981	0.9659	272.39	223.15	2.5690
10	0.2539	0.1637	46.168	37.821	0.4354	60	1.5235	0.9823	277.01	226.93	2.6126
11	0.2793	0.1801	50.785	41.604	0.4790	61	1.5489	0.9986	281.63	230.71	2.6561
12	0.3047	0.1965	55.402	45.386	0.5225	62	1.5743	1.0150	286.24	234.49	2.6996
13	0.3301	0.2128	60.018	49.168	0.5661	63	1.5997	1.0314	290.86	238.28	2.7432
14	0.3555	0.2292	64.635	52.950	0·6096	64	1.6251	1.0478	295.48	242.06	2.7867
15	0.3809	0.2456	69.252	56·732	0.6531	65	1.6505	1.0641	300.09	245.84	2.8303
16	0.4063	0.2619	73.869	60.514	0.6967	66	1.6758	1.0805	304.71	249.62	2.8738
17	0.4317	0.2783	78.486	64.297	0.7402	67	1.7012	1.0969	309.33	253.40	2.9174
18	0.4570	0.2947	83.102	68.079	0.7838	68	1.7266	1.1132	313.94	357.19	2.9609
19	0.4824	0.3111	87.719	71.861	0.8273	69	1.7520	1.1296	318.56	260.97	3.0044
20	0.5078	0.3274	92.336	75.613	0.8709	70	1.7774	1.1460	323.18	264.75	3.0480
21	0.5332	0.3438	96.953	79.425	0·9144	71	1.8028	1.1623	327.79	268.53	3.0915
22	0.5586	0.3602	101.57	83.207	0.9579	72	1.8282	1.1787	332.41	272.31	3.1351
23	0.5840	0.3765	106.19	86.989	1.0015	73	1.8536	1.1951	337.03	276.10	3.1786
24	0.6094	0.3929	110.80	90.772	1.0150	74	1.8790	1.2115	341.64	279.88	3.2222
25	0.6348	0.4093	115.42	91.554	1.0886	75	1.9044	1.2278	346.26	283.66	3.2657
26	0.6602	0.4256	120.04	98.336	1.1321	76	1.9298	1.2442	350.88	287.44	3.3092
27	0.6856	0.4420	124.65	102.12	1.1757	77	1.9552	1.2606	355.49	291.23	3.3528
28	0.7110	0.4584	129.27	105.90	1.2192	78	1.9805	1.2769	360.11	295.01	3.3963
29	0.7364	9.4748	133.89	109.68	1.2627	79	2.0059	1.2933	364.73	298.79	3.4399
30	0.7617	0.4911	138.50	113.46	1.3063	80	2.0313	1.3097	369.34	302.57	3.4834
31	0.7871	0.5075	143.12	117.25	1.3498	81	2.0567	1.3261	273.96	306.35	3.5270
32	0.8125	0.5239	147.74	121.03	1.3934	82	2.0821	1.3424	378.58	310.14	3.5705
33	0.8379	0.5402	152.35	124.81	1.4369	83	2.1075	1.3588	383.19	313.92	3.6140
34	0.8633	0.5566	156.97	128.59	1.4804	84	2.1329	1.3752	387.81	317.70	3.6576
35	0.8887	0·5730	161.59	132.38	1.5240	85	2.1583	1.3915	392.43	321.48	3.7011
36	0.9141	0.5894	166.20	136.16	1.5675	86	2.1837	1.4079	397.05	325.26	3.7447
37	0.9395	0.6057	170.82	139.94	1.6111	87	2.2091	1.4243	401.66	329.05	3.7882
38	0.9649	0.6221	175.44	143.72	1.6546	88	2.2345	1.4407	406.28	332.83	3.8317
39	0.9903	0.6385	180.06	147.50	1.6982	89	2.2599	1.4570	410.90	336.61	3.8753
40	1.0157	0.6548	184.67	151.28	1.7417	90	2.2852	1.4734	415.51	340.39	3·9188
41	1.0411	0.6712	189.29	155.07	1.7852	91	2.3106	1.4898	420.13	344.18	3.9624
42	1.0664	0.6876	193.91	158.85	1.8288	92	2.3360	1.5061	424.75	347.96	4.0059
43	1.0918	0.7040	198.52	162·63	1.8723	93	2.3614	1.5225	429.36	351.74	4.0495
44	1.1172	0.7203	203.14	166.41	1.9159	94	2.3868	1.5389	433.98	355.52	4.0930
45	1.1426	0.7367	207.76	170.20	1.9594	95	2.4122	1.5553	438.60	359.30	4.1365
46	1.1680	0.7531	212.37	173.98	2.0030	96	2.4376	1.5716	443.21	363.09	4.1801
47	1.1934	0.7694	216.99	177.76	2.0465	97	2.4630	1.5880	447.83	366.87	4.2236
48	1.2188	0.7858	221.61	181.54	2.0900	98	2.4884	1.6044	452.45	370.65	4.2672
49	1.2442	0.8022	226.22	185.33	2.1336	99	2.5138	1.6207	457.06	374.43	4.3107
50	1.2696	0.8186	230.84	189.11	2.1771	100	2.5392	1.6371	461.68	378.21	4.3543

A 1 alfo in Decimals, at different Periods
fr Ioney inferred therefrom. To which is
a during the prefent Century, at fhorter
P

ndard and Value.)

Year of ou Lord.	eat.	Twelve Mifcellaneous Articles.	Meat.	Day Labour.	Mean of all.
		preciation of Money, according to the Price of			
105(	10	42			26
115(					
125(					
135(	00	56		75	77
145(					
155(	00	100	100	100	100
160(					
1625					
1650					
1675	216	239	166	188	210
1700					
1720					
1740	97	434	266	250	287
1760	203	492	400	275	342
1780					
1795	126	752	511	436	531

Mean Appreciation by Interpolation. A. D.	
1050	26
1100	34
1150	43
1200	51
1250	60
1300	68
1350	77
1400	83
1450	88
1500	94
1550	100
1600	144
1650	188
1675	210
1700	238
1720	257
1740	287
1750	314
1760	342
1770	384
1780	427
1790	496
1795	531
1800 nearly	562

viz. 100.

Befi *nicon Pretiofum*, 1ft and 2d edit. *Liber Garderobæ*, in
1299. oyal Houfehold, in divers Reigns, from Edw. III. to
King V England, from the Year 1000 to 1765, by Mr. Com-
brune, pied this Article from the Philofophical Tranfactions,
M.D.C

Apparatus for bleaching.

Fig. 1.

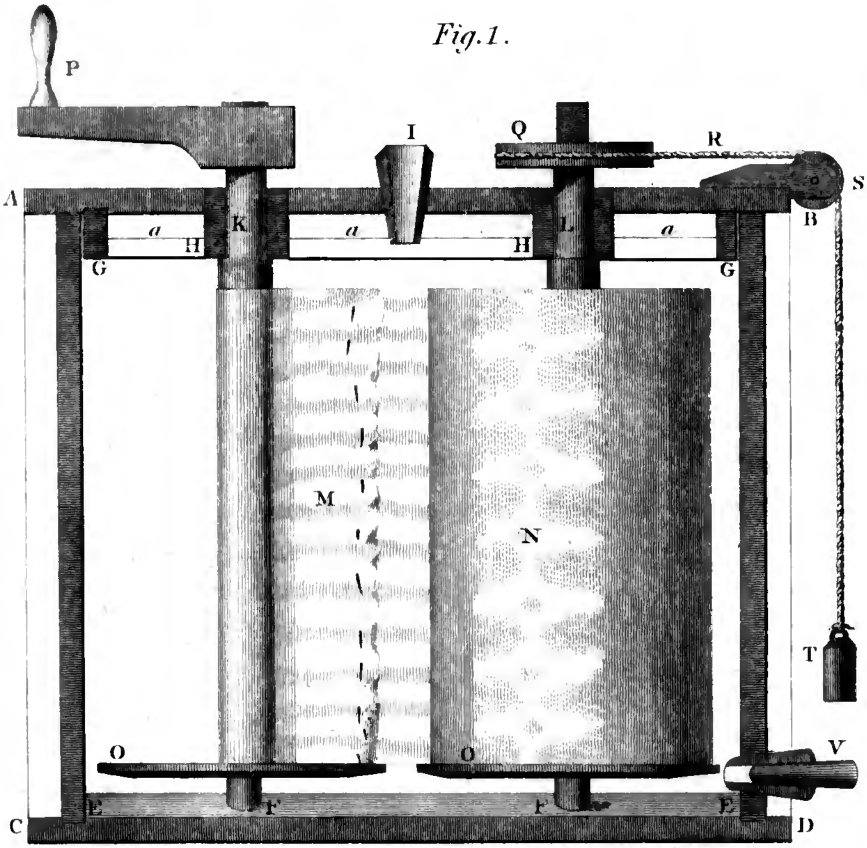
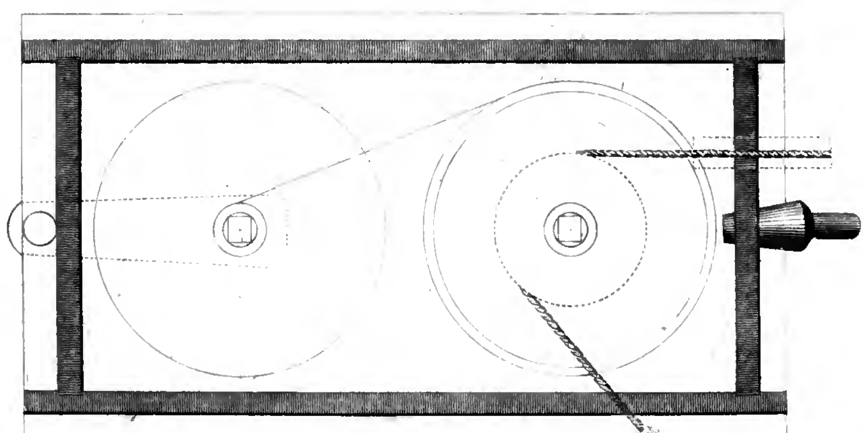


Fig. 2.



0 1 2 3 Feet.

Barlow sculp

An Artificial overflowing Well

Fig. 1.

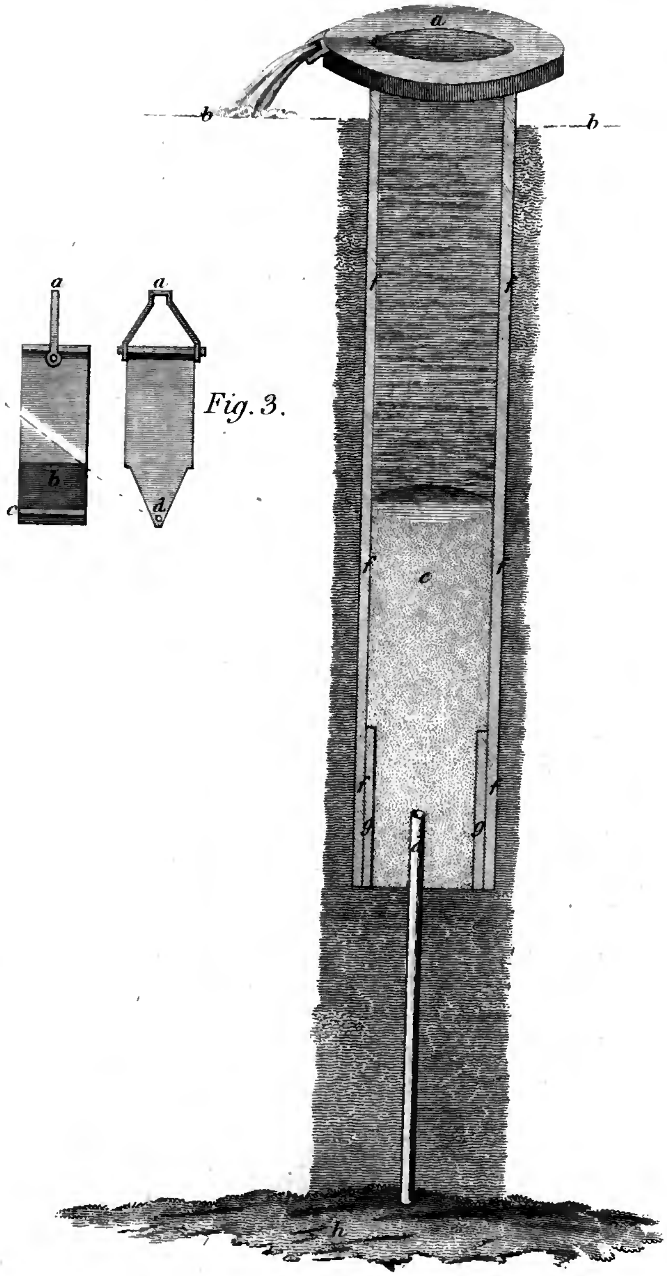


Fig. 2.

Fig. 3.

Barlow sculp.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

OCTOBER 1798.

ARTICLE I.

Curious Circumstances upon which the Vitreous or the Stony Character of Whinstone and Lava respectively depend; with other Facts. In an Account of Experiments made by Sir JAMES HALL, *Bart. F.R. and A.S.S. Edin.**

ON the 5th March, and 18th of June, Sir James Hall read to the Royal Society of Edinburgh an account of a series of experiments on whinstone and lava. These experiments, which relate to the theory of the earth published by the late Dr. Hutton, had been projected by Sir James Hall in 1790; at which time he read a paper to the society on the formation of granite and other unstratified substances. He had then begun a few experiments, but had been obliged by some circumstances to discontinue them. Having obtained, however, very promising appearances of success, they were again resumed, and carried into effect in the course of last winter †. In the execution of the experimental part, Sir James Hall acknowledges himself greatly indebted to the assistance of Dr. Kennedy.

The subject of the communication was suggested by some obvious and plausible objections which have lately been urged against Dr. Hutton's system. Whinstone or basaltes being considered in that system as having attained its present position in a state of igneous fusion, it has been alledged, as a sufficient refutation of this hypothesis, that whinstone, when made to undergo fusion in one of our furnaces, yields glass, a substance very different from the original stone; the formation of which cannot therefore be ascribed to the action of fire.

* Abstract, by favour of the Author, from his Paper which will appear in the Edin. Transf. vol. v.

† Sir James Hall showed the result of his first successful experiment at a meeting of the Society on the 5th of February, 1798.

This difficulty had formerly occurred to Sir James Hall; who had endeavoured to obviate it by stating, that the mass during slow refrigeration in the bowels of the earth had undergone a change similar to that of glass into Reaumur's porcelain; and that by crystallization it had lost the vitreous, and assumed the stony character. The truth of this explanation has now been amply confirmed by these experiments, which comprehend seven different species of whinstone. Each of the original substances was reduced, by fusion and subsequent rapid cooling, to a state of perfect glass. This glass, being again placed in the furnace, was subjected to a second fusion. The heat, being then reduced to a temperature generally about 28° of Wedgwood, was maintained stationary for some hours; when the crucible was either immediately removed, or allowed to cool with the furnace. The consequence was, that in every case the substance had lost the character of glass, and by crystallization had assumed in all respects that of an original whinstone.

It must be owned, that in most cases the new production did not exactly resemble the particular original from which it was formed, but some other original of the same class; owing to accidental varieties in the mode of refrigeration, and to chemical changes which unavoidably took place during the process. In the case, however, of the rock of Edinburgh Castle, and of that of the basaltic columns of Staffa, the artificial substances bear a complete resemblance to their originals, both in colour and texture.

Sir James Hall conceives, that the objection to Dr. Hutton's system, mentioned above, is thus completely obviated; since the stony character of whinstone is shown to be the natural consequence of the slow refrigeration, which, according to that system, must have taken place in the bowels of the earth, as the substance of whinstone passed from a liquid to a solid state.

Experiments were made with equal success on lava. This class of bodies is known to possess the crystalline and stony character, in common with whinstone, and to resemble it so much, that in many cases the two substances cannot be distinguished by their external appearance *. In others, however, certain circumstances occur by which they are characterized. But in consequence of fusion these distinguishing marks disappear; and the glass of a lava seems in no respect to differ from that of a whin. Sir James Hall therefore infers that the two substances are intrinsically the same, and ascribes their differences to the circumstances under which each class passed from a liquid to a solid state; the lavas having cooled in the open air, and the whins, according to Dr. Hutton, in the bowels of the earth. Six lavas, four of which † made part of undoubted currents of Ætna and Vesuvius, and broken from those currents by Sir James himself, were submitted to experiment. The glass produced by fusion and rapid cooling, yielded, when treated like the glass of whin, the same kind of crystallized, stony, or earthy masses, completely resembling an original whin or lava.

In the course of these experiments, the fusibilities of the various original substances, and of those artificially produced, were ascertained with much care by Wedgwood's pyrometer,

* These observations are applicable to lavas in general, comprehending all those of Ætna and Vesuvius; but do not strictly apply to some of the lavas of the Lipari Isles, and of Ischia, which possess some curious and interesting properties to be mentioned in a future communication. H.

† No. 8, 9, 11 and 12 of the table.

and

and are ſtated in the annexed table, which may be depended upon, in every example, to within two, or at moſt three degrees. The artificially cryſtallized ſubſtances have been denoted, at Dr. Hope's ſuggeſtion, by the name of *cryſtallite.*

The various fuſibilities ſtated in the table afford ſome concluſions of conſequence. The whins ſubmitted to trial are more refractory than the lavas; though their exceſs in this reſpect is not great, ſince the moſt fuſible of the former claſs equal the moſt refractory of the latter. The glaſſes are all incomparably more fuſible than the original ſtones. This laſt circumſtance has long been known as a fact; but Sir James propoſes to ſuggeſt the theory of it, and of all the peculiarities which occurred in thoſe experiments, in a ſecond communication which he means ſoon to lay before the Society.

It is obſervable, that the lava No. 12 is fuſible at 18; that is, it reſembles in this property the moſt fuſible glaſſes. And it is in fact a glaſs; for, being lifted in a ſoft ſtate from a flowing lava of Veſuvius by Sir James Hall, it quickly cooled, and has of conſequence aſſumed the vitreous character in every reſpect: for, beſides its eaſy fuſibility, it poſſeſſes the ſhining ſurface and fracture of glaſs. This ſubſtance, being treated like the artificial glaſſes of whin and lava, cryſtallized like theſe, and aſſumed the character of a ſtony lava, both in texture and in difficult fuſibility, ſince it ſoftened only at 35°. Here then is a proof beyond diſpute, that the ſtony character of a lava is occaſioned ſolely by ſlow cooling.

Although the internal ſtructure of lava was thus accounted for, yet Sir James was embarraſſed with the ſtate of its external ſurface; which, though cooled in contact with the open air, is ſeldom or ever vitreous, holding an intermediate ſtation between glaſs and ſtone;—but this difficulty was removed by a circumſtance which took place in the courſe of theſe experiments. It was found that a ſmall piece of glaſs of any of the lavas, or of ſeveral of the whins, being introduced into a muffle, the temperature of which was at any point between the 20th and the 22d degree of Wedgwood's ſcale, the glaſs became quite ſoft in the ſpace of one minute; but, being allowed to remain till the end of a ſecond minute, it was found to have become hard throughout in conſequence of a rapid cryſtallization, to have loſt its character of glaſs, and to have become by 12 or 14 degrees more infuſible, being unaffected by any heat under 30, though the glaſs had been fuſible at 18° or at 16. This accounted for the ſcoria on the ſurface of lavas; for the ſubſtance even at the ſurface, being in contact with the flowing ſtream and ſurrounded with heated air, could not cool with exceſſive rapidity; and the experiment ſhows, that ſhould any part of the maſs, in deſcending heat, employ more than one or two minutes in cooling from 22 to 20, it would infallibly loſe its vitreous character.

The internal phænomena of volcanos being thus explained by the properties of common fire, and the reſemblance, or identity, of many lavas and of whinſtone being thus eſtabliſhed, Sir James Hall conceives, that the powerful arguments advanced by Dr. Hutton, to prove the igneous origin of whinſtone and other mineral bodies, are very ſtrongly corroborated; ſince theſe experiments ſhow that theſe ſubſtances may have been formed by a ſimple extenſion of the ſame cauſes which continue at this day to agitate various parts of our globe.

Independently however of any alluſion to ſyſtem or to general theory, Sir James Hall flatters himſelf that theſe experiments may be of ſome importance by ſimplifying the hiſtory of volcanos; and above all by ſuperſeding ſome very extraordinary, and, he conceives,

Pp 2 unphiloſophical

unphilosophical opinions advanced with regard to volcanic heat, which has been stated as possessing very little intensity, and as acting by some occult and inconceivable influence, or with the help of some invisible agent, so as to produce liquidity without fusion. These suppositions, which have been maintained seriously by some of the most celebrated naturalists in Europe, have originated from the difficulty of accounting for the stony character of lavas, when compared with that of glass, which they assume in consequence of fusion in our furnaces. But now he hopes we may be relieved from the necessity of such violent efforts of imagination, since the phænomena have been fully accounted for by the simple though unnoticed principle of refrigeration, and have been repeated again and again with ease and certainty in a small chamber furnace.

TABLE.

The fusibilities, according to Wedgwood's pyrometer, stated in the following table, were ascertained by heating the substances in a muffle, in which they could be distinctly observed, while exposed to the action of the heat. A small piece of the substance to be examined being put into the muffle, a pyrometer-piece was placed close beside it, and the heat raised gradually. The substance was touched at intervals with a slender iron rod; and when found so soft as to yield easily to slight pressure, the pyrometer was withdrawn, and measured.

[All the whins except No. — were taken from their original rocks in the neighbourhood of Edinburgh.]

		Original substances softened.	Glass softened.	Crystallite softened.
No. 1.	Whin from a quarry on the Water of Leith,	40;45	17;15	33
No. 2.	Whin of the rock of Edinburgh Castle,	45	24;22	35
No. 3.	Whin of basaltic columns—Hill of Arthur's Seat,	55	18	35
No. 4.	Whin from the south side of Arthur's Seat, near Duddystone,	43	24	38
No. 5.	Stone of the nature of whin, found in large blocks in the bed of Water of Leith,	55	16	37
No. 6.	Whin of Salisbury Rock,	55	24	38;40
No. 7.	Whin of basaltic columns, Staffa,	38	—	—
No. 8.	Lava of Ætna, which destroyed part of Catania in 1669,	34	20	38
No. 9.	Lava near Piedmonte—Ætna,	32	18	36
No. 10.	Lava of La Motta di Catania—Ætna,	38;42	18	36
No. 11.	Lava of Torre del Greco—Vesuvius,	40	18	27;28
No. 12.	Lava lifted red hot from a flowing stream on Vesuvius, by Sir James Hall,	18	18	35
No. 13.	Lava from Iceland,	35	15	43

II.

On the Analysis of Pumice, which is found to contain Potash; and of Basaltes and Lava containing Soda. By Dr. KENNEDY *.

ON the 5th of February 1798, an analysis of pumice was communicated to the Royal Society of Edinburgh by Dr. Kennedy. By this analysis he shewed that the pumice contained potash as one of its component parts. The specimen analysed was of the common kind, having a fibrous texture and a silky lustre. By a heat of 60° of Wedgwood, it was converted into a kind of glassy enamel; and in a muffle, even at a heat of 35 or 40, was so far altered, that its fibrous texture could no longer be distinguished. Besides potash, it contained silex, argill, and a small quantity of iron; but no lime or magnesia.

Several other varieties of the common kinds of pumice used in the arts, were found as fusible as the specimen above mentioned, and gave the same kind of glassy enamel.

The result of the analysis, with respect to the earths and iron, corresponds very nearly with the result of Mr. Klaproth's, published in the 2d vol. of his Beiträge; only he did not find potash in the specimen he analysed; in which, however, there was probably some saline substance; for the same kind of pumice melted in the porcelain furnace of Berlin; in which a compound consisting only of silex, argill, and a minute portion of iron, would certainly not have melted. The heat of this furnace was 136 of Wedgwood.—See Klap. Beytr. vol. 2d, p. 88.

On the 6th of August Dr. Kennedy announced to the Society, that he had discovered mineral alkali in several varieties of basaltes or whinstone. He found by chemical analysis, that the alkali existed in these substances in a state of very intimate combination with their earthy bases; and that it was with difficulty separated, even by the strongest acids.

Dr. Kennedy also analysed a specimen of lava from Ætna, at the request of Sir James Hall, who thought it extremely probable that alkali would be found in lava, as well as in basaltes, on account of the great resemblance which these two substances have to each other, both in external appearance and in many chemical properties. The specimen was broken, by Sir James Hall and Dr. Hume, from the celebrated current of lava which in 1669 destroyed part of the town of Catania. It was found, like the basaltes, to contain soda.

One of the fixed alkalis, potash, has already been discovered in stony substances by the celebrated Mr. Klaproth, to whom the world is indebted for so many analyses performed with the greatest skill. The experiments above-mentioned show, that the other fixed alkali, soda, likewise exists in stony substances.

* Abstract, by favour of the Author. This paper will appear in the 5th vol. of the Edinburgh Transactions.

III.

Experiments and Obſervations on the Preparation, and ſome remarkable Properties of the Oxygenated Muriat of Pot-aſh. By Mr. THOMAS HOYLE, *jun.* *

HAVING an opportunity in preparing the oxygenated muriatic acid for the purpoſe of bleaching, by a ſmall extenſion of the apparatus, to prepare likewiſe the oxygenated muriat of potaſh, and to make experiments on that ſubſtance, I have been induced to digeſt the moſt material facts and obſervations which occurred, and to lay them before the Society: eſpecially as I do not find much on the ſubject in the writings of others, and as many have probably been deterred from the inveſtigation by the exorbitant price of the article, and by ſome apprehenſions of danger attending it.

A few experiments, which are not new, have been introduced, in order to bring under one point of view the principal chemical facts which relate to this ſalt. I have given in moſt caſes an exact account of the quantities of the different ingredients compoſing the mixtures; and as perſons not much accuſtomed to ſuch experiments may be inclined to repeat ſome of them, I would caution them *not to uſe greater quantities than are here ſpecified*, particularly where the terms violent detonation, exploſion, &c. are employed.

I would not by any means wiſh it to be underſtood that I have exhauſted the ſubject: many more experiments and much labour and aſſiduity are required, before the nature and uſes of ſo active a ſubſtance can be fully aſcertained.

I find it has been introduced into medicine with ſucceſs; and I hope its good effects in that reſpect will not be fruſtrated by the high price of the article, as it may be procured at a much cheaper rate than it is commonly charged.

I. *On the Preparation of the Salt, and its Solution in Water and the Acids.*

Finding that a quantity of gas eſcaped occaſionally from our apparatus for making the new bleaching liquor, more eſpecially when the fire was not properly managed, or when by any other means a greater quantity of gas was produced than the liquor could abſorb; I thought it would be uſeful to adapt to the large apparatus a ſmaller one, in which this ſuperfluous gas might be condenſed; as the eſcape of it was ſometimes diſagreeable to the workmen. This I did by filling an earthen-ware bottle with a ſtrong ſolution of potaſh in water (conſiſting of about three pounds of alkali to the gallon), which I found entirely relieved us from the diſagreeable ſmell we frequently experienced before, and at the ſame time yielded a conſiderable quantity of the oxygenated muriat. Though the production of the oxygenated muriat in this way be ſomewhat precarious, depending upon the management of the perſon who conducts the proceſs (it being the bleacher's intereſt to condenſe the whole of the gas in the liquor he wants for his buſineſs); yet I think, if the portion which commonly eſcapes were thus diſpoſed of, a conſiderable quantity of this ſalt might be made by bleachers with little additional expence, except what is incurred by the purchaſe of the alkali, and ſome more labour and attention. At ſome of my firſt trials, about two years ago, I found the gas which eſcaped from the materials of one diſtillation ſufficient to ſaturate two gallons of the alkaline ſolution, from which I procured about ſix ounces, and

* Mancheſter Memoirs, vol. v. part i.

fometimes more of the falt, after being purified by feveral cryftallizations. But having made fome alteration in the apparatus, I now find that the fame quantity of alkali may remain for three or four diftillations before fufficient gas be furnifhed to form the falt; except the perfon employed be remarkably inattentive to his duty. I confider this as a valuable improvement, the making of the falt being only a fecondary object. The falt was chiefly formed during the diftillation. The alkali became warm toward the latter end of the procefs, efpecially if the abforption of gas was very rapid, a quantity of caloric being difengaged. In this cafe, a confiderable part of the falt foon cryftallized, on the lixivium being fet in a cool place, and a great deal of gas appeared to efcape; which on one occafion I collected, and found that it precipitated lime from its folution in water, and extinguifhed flame: and therefore, though it had a flight fmell of the oxygenated muriatic acid gas, I believe that it confifted chiefly of carbonic acid, as the former occafions no precipitation of lime water, which the latter uniformly does. A glafs jar containing 32 ounce meafures of this gas, being left over water one night, was reduced to about one fourth its bulk. The gas that remained feemed to contain more oxygene than the air of the room; two meafures of it, with one of nitrous gas, gave 1.53, whilft an equal quantity of common air gave 1.9.

Before any of the falt appeared to be formed in the alkaline folution, I have conftantly obferved a quantity of earthy matter to be precipitated. This was carefully feparated from the falt, and, after being wafhed repeatedly in boiling water, was fuffered to dry; but not having examined it with fufficient minutenefs to fay what it is, I fhall content myfelf at prefent with ftating fome of its properties. It did not detonate with fulphur, and was totally or nearly infoluble in water. The fulphuric acid diffolved it, and gave evident figns of muriatic acid, which appeared to be flightly oxygenated. After being expofed to a red heat for half an hour, the above properties ftill appeared the fame, except with the fulphuric acid. I thought the gas that was difengaged had more of the fmell of fimple muriatic acid gas, though along with it a little of the oxygenated gas might be perceived. The muriatic acid did not appear to diffolve any of this fubftance, either before or after its calcination. With the nitrous acid a ftrong fmell of the oxygenated muriatic gas was produced. From a dram of this fubftance in an earthen retort expofed to a ftrong heat, about fix ounce-meafures of gas were produced, confifting of a mixture of carbonic and azotic gas, the latter of which was in the greateft quantity, forming by eftimate about three fourths of the whole.

The form of the cryftals that firft appeared in the folution of alkali were quadrangular plates: what were afterwards formed, when the lixivium became cool, were needle-like, as were thofe that were produced by fpontaneoufly evaporating the remainder of the ley: they appear to have the fame property of detonating as the firft. Thefe different forms of cryftals appeared on diffolving the falt in hot water, and, when cold, feparating the falt, and fuffering the water to evaporate fpontaneoufly.

I frequently obferved, that unlefs the alkali began to part with a confiderable portion of gas without the admiffion of any from the apparatus, little or none of the oxygenated muriat was procured; and that as this gas (which I have before obferved to be chiefly the carbonic acid) efcaped, the cryftallization took place, and increafed or diminifhed according to the evolution of that gas. This I found uniformly the cafe, whether mild or cauftic alkali

alkali was employed. A given quantity of the ftrong folution of potafh appeared to produce more of this falt than the fame quantity of a folution of pearlafh of the fame fpecific gravity.

The remaining lixivium, on evaporation, did not yield this falt, though a muriat of potafh was formed, that appeared to be confiderably oxygenated: fince, with the addition of the fulphuric or muriatic acid, it became a very powerful deftroyer of vegetable colours; it would not detonate with fulphur, or inflame combuftible fubftances, with acids; it was very foluble in water, much more fo than the muriat firft formed from the fame alkali.

I may here remark, that I think the French chemifts were right in calling the firft falt the hyperoxygenated muriat, as the falt laft mentioned is certainly oxygenated in fome degree: however, in the following experiments I fhall ufe the term oxygenated muriat, when fpeaking of the falt formed during the diftillation, and on cooling the lixivium after being faturated with the gas.

Experiment I. One part of the oxygenated muriat of potafh required about feventeen parts of water at the temperature of 60°, to diffolve it; whilft five parts of boiling water diffolved two of the falt. Repeated folutions did not appear to injure, but rather to increafe, its detonating property. The cryftals became much whiter; and a quantity of the earthy matter before mentioned was feparated at every frefh cryftallization.

Experiment II. A quantity of this falt was put into a bottle, and placed in a fituation much expofed to the light: after being kept there more than twelve months, it did not appear to have loft any part of its detonating property. This fact is contrary to Chaptal's affertion, that the mere impreffion of light is fufficient to decompofe it *.

Experiment III. Water faturated with this falt was expofed to the light for feveral months, without appearing to be at all changed. It was put into a bottle with a ground ftopper and tube, to which an apparatus was adapted to receive any gas that might come over; but no gas whatever was difengaged.

Experiment IV. Sixty grains of falt were fufed by the heat of a lamp in a bottle with a ground ftopper and tube. After having been kept in a fluid ftate for about half an hour, I found that it had loft two grains in weight, and that a fmall quantity of air was given out, which proved to be oxygenous by the teft of nitrous gas. The falt which had been melted would ftill detonate with fulphur, &c. The lofs of weight was, I am inclined to think, chiefly owing to the efcape of the water of cryftallization; for the falt when cool had loft its tranfparency.

Experiment V. From forty grains of the falt in an earthen retort, I procured by the application of heat about thirty-fix cubic inches of oxygenous gas; the evolution of which was very rapid, and commenced as foon as the retort became flightly red. Forty grains expofed in a crucible to a ftrong red heat appeared, from the mean of two experiments, to have loft about feventeen grains in weight. The remaining muriat, being afterwards thrown into the fulphuric acid, produced a very ftrong fmell of oxygenated muriatic acid; from which I inferred that the whole of the oxygen had not been expelled by the heat; whence the oxygenated muriat of potafh may, I think, be ftated to contain about half its weight of oxygen in a concrete ftate.

* Elements of Chemiftry, i. 250.

Experiment VI. Strong nitrous acid disengaged the oxygenated muriatic acid from this salt. During the solution of two or three grains of the oxygenated muriat in this acid, a grain or two of phosphorus was dropped into the glass containing the mixture; when a number of vivid flashes appeared in the liquor, darting forth at intervals for a considerable time. This is one of the most striking experiments I ever saw; but a little caution is necessary in performing it, the phosphorus being sometimes thrown out of the mixture *.

Experiment VII. The muriatic acid dissolved this salt, a great deal of the oxygenated acid being given out. A few grains of the salt added to an ounce of the acid rendered it a very powerful destroyer of vegetable colours. This mixture may probably be used with advantage in taking stains of ink, &c. out of linen or cotton.

Phosphorus added to this acid along with the salt did not produce the same effect as with the nitrous acid; no light appearing, as in the last experiment.

Experiment VIII. On putting a little of the salt into the sulphuric acid, a violent crackling, or a great number of small explosions, took place, and a very strong smell of nitrous gas was produced; the mixture at the same time assuming an orange colour, which disappeared after it had stood a short time. A very small piece of phosphorus having been dropped on about two grains of the salt (previously thrown into the acid), an explosion immediately took place, which blew out a great part of the mixture upon my hand; an accident that might have proved serious if I had not had water near me.

Experiment IX. Finding a great quantity of gas to be disengaged from this salt by the sulphuric acid, which had a very strong smell of nitrous gas, I put forty grains of the salt into a glass retort, and poured upon it nearly an equal weight of sulphuric acid diluted with water. With the heat of a lamp the gas began to come over very rapidly, and was received in a glass jar placed in a bason of water. A considerable portion of it appeared to be absorbed by the water, which acquired a yellowish colour. This colour disappeared on standing a few days, and a brown matter was deposited, which being carefully collected and dried weighed one grain, and appeared to be manganese; for a little of it, being put into the muriatic acid, so far oxygenated it that it would destroy the blue colour of a diluted solution of indigo in the sulphuric acid. The precipitate before mentioned, that was first produced in the alkali employed, did not appear to have this effect. The quantity of this sediment that I had an opportunity of collecting was so small, that I could not try many other experiments with it: indeed I did not always succeed in procuring it; for I found that, unless the disengagement of the gas was very rapid, but little of it could be obtained.

Experiment X. On two drams of the salt in a glass retort, I poured an equal weight of sulphuric acid diluted with a little water, and adapted the retort to Woulfe's apparatus. The heat of a lamp was applied; and presently the gas began to escape, and was absorbed by the water in a considerable quantity; to which it communicated a yellowish colour, and a liquid began to trickle down the neck of the retort into the receiver. This had continued but a short time before a violent explosion took place, which broke the retort and two of the receivers to pieces, together with several other glasses which were on the table. This was several times repeated, but with more caution than before; and I always found,

* This curious experiment was first noticed by J. Collier, and was communicated by him to the Society some time ago.

that when the mixture acquired a certain degree of heat an exploſion certainly took place, except the retort had a pretty wide neck, and the neck was ſimply introduced into a receiver with a conſiderable opening in it without any lute; or put into water, as in the laſt experiment: and even in this caſe I would not adviſe ſo much of the ſalt to be uſed at one time as is here mentioned. The ſmall quantity of acid I was able to collect in this way, by adapting a looſe receiver, appeared to be a weak muriatic acid ſlightly oxygenated; it was of a dilute purple colour, which diſappeared on its being expoſed a ſhort time to the light: a ſmall piece of iron dropped into it cauſed it to become tranſparent immediately.

It was a matter of much ſurpriſe to me, to find ſo ſtrong a ſmell of nitrous gas produced on decompoſing this ſalt with ſulphuric acid. Now, as nitrous gas conſiſts of azot and oxygen, ſuppoſing this to be nitrous gas (for I do not aſſert it to be ſo, though I ſhould think the ſmell in this inſtance an almoſt ſufficient criterion), whence comes the azot? At firſt I thought it might come from a decompoſition of the alkaline baſe of the ſalt; as ſome chemiſts have imagined the vegetable alkali to be compoſed of lime and azot: in that caſe, I expected the reſiduum would have been the ſulphat of lime; but I found it to be chiefly ſulphat of potaſh, with a little of the oxygenated muriat that remained undecompoſed along with it. At preſent I ſhall not hazard any opinion reſpecting the origin of this nitrous ſmell; but hope ſome experiments I am at preſent engaged with will, if I can find time to proſecute them, throw ſome light upon this ſubject.

II. *On the Detonation and Inflammation of combuſtible Subſtances with the Salt produced by Friction and the Acids.*

THE detonating properties of this ſalt were tried with various ſubſtances in the following experiments: the different mixtures were intimately combined by gently rubbing them in a ſtone-ware mortar: after this was done, one ſmart ſtroke acroſs the mixtures would cauſe the whole of ſome of them to explode at once, and others ſucceſſively by repeating the friction. The ſulphuric acid inflamed moſt of theſe mixtures of the ſalt with combuſtible ſubſtances: the nitrous acid alſo had the ſame effect with ſome of them.

Experiment I. *with Phoſphorus.* Half a grain of this ſubſtance rubbed with the ſame weight of the ſalt produced violent exploſion with flame. I apprehended it would be dangerous to uſe much greater quantities, as the phoſphorus is frequently thrown out with violence before it is conſumed. The ſulphuric acid inflamed this mixture, as I have before ſtated.

Experiment II. *with Charcoal.* Two grains of ſalt with one of charcoal intimately mixed, and perfectly dry, produced by a ſmart ſtroke a ſtrong flame without much report. The ſulphuric and nitrous acids inflamed this mixture, the latter with moſt rapidity.

Experiment III. *with Pit-coal.* A grain of dry pit-coal rubbed with the ſame quantity of the ſalt produced ſparks and ſome ſmall reports. With half the quantity of coal the reports were much louder.

The ſulphuric acid added to about twenty grains of the ſalt with ten of the coal, produced a bright red flame riſing up to a conſiderable height.

Experiment IV. *with Sulphur.* A grain of the ſalt rubbed with half a grain of ſulphur produced a very loud report, attended with flame and a ſtrong ſmell of ſulphureous acid. When the ſulphur was reduced to a quarter of a grain, the exploſion was not made at once

as

as before, but succeffively. When the proportion of fulphur was increafed to three fourths of a grain, it produced a very loud report, much the fame as the firft; and the whole appeared to explode at once. Equal parts of fulphur and the falt did not caufe fo ftrong reports as when a lefs quantity of fulphur was employed: this mixture exploded fucceffively. The fulphuric and nitrous acids inflamed it.

Experiment V. *with Sulphuret of Potafh.* One grain of the falt rubbed with the fame weight of this fubftance produced a very loud explofion with flame. With half a grain of the fulphuret I thought the report fully as violent. A little of thefe mixtures melted over the fire had not the effect of the fulminating powder made with nitre. It only emitted a flafh without any report, nor was I able to produce a fulminating mixture by varying the proportions of the falt, alkali and fulphur. The fulphuric or nitrous acids dropped on this mixture gave a very ftrong bright flame.

Experiment VI. *with Sulphuret of Mercury.* (*Cinnabar.*) Equal parts of this fubftance and of the falt detonated fucceffively by friction, a grain of each being ufed. A change of proportion appeared to weaken the detonating property of the mixture. The fulphuric acid inflamed this mixture, but not fo rapidly as in the laft experiment. The nitrous acid did not inflame it.

Experiment VII. *with Sulphuret of Arfenic.* (*Orpiment.*) A grain or two of the falt rubbed with an equal weight of this fubftance produced little more than a flafh; but a grain of the falt with half a grain of the fulphuret gave a ftrong report, though very little friction was ufed. Reducing the quantity of fulphuret to a quarter of a grain, the explofions were weak and fucceffive. A larger quantity of this mixture than is mentioned above makes a report which is very unpleafant, with confiderable flame. I was greatly furprifed, the firft time I made the experiment with two or three grains of the falt and a portion of the fulphuret, by their exploding in a moft violent manner, though a very flight friction had been ufed. The fulphuric or nitrous acids gave a very ftrong flame the moment they were dropped upon this mixture.

Experiment VIII. *with Cotton-wool.* A fmall quantity of very dry cotton-wool was rubbed with a little of the falt; no detonation took place. The wool was afterwards dropped into the fulphuric acid, and took fire immediately; but the nitrous acid would not inflame it.

Experiment IX. *with Loaf-fugar.* One grain of this fubftance rubbed with two of the falt gave a number of fucceffive reports. The fulphuric or nitrous acids dropped on this mixture inftantly produced a ftrong flame afcending to a confiderable height.

Experiment X. *with fixed and effential Oils.* A few drops of fpermaceti oil rubbed with a grain or two of the falt produced a number of loud reports. The fulphuric acid inflamed this mixture; the nitrous acid did not.

Olive oil, the effential oils of rofemary, juniper, cloves, carraway, anifeed, cinnamon, nutmeg, amber, mint, and effence of lemon, were rubbed with the falt: all of them detonated fucceffively, and fuch of the mixtures as were tried took fire with the fulphuric acid.

Experiment XI. *with Spirit of Turpentine.* A few drops of fpirit of turpentine rubbed with a little of the falt detonated in much the fame manner as the fubftances ufed in the laft experiment. The fulphuric acid dropped on this mixture produced a ftrong flame, with a cloud of very black fmoke.

Experiment XII. *with Camphor.* A little of this fubftance on being rubbed with a grain of the falt produced a number of fucceffive detonations. The fulphuric acid produced flame with fome explofions.

Experiment XIII. *with Rofin.* One part of this fubftance with two parts of the falt detonated fucceffively when well rubbed together. The fulphuric acid inflamed this mixture, but the nitrous acid did nor.

Experiment XIV. *with Gum-Arabic.* The detonations were very flight. It was mixed with twice its weight of the falt. The fulphuric acid fet fire to the mixture, but the nitrous acid would not.

Experiment XV. *with Pruffian Blue.* No detonations whatever were produced by friction, nor did the acids inflame a mixture of this fubftance with the falt*.

Experiment XVI. *with Indigo.* Half a grain of fine Spanifh indigo rubbed with a grain of the falt detonated fucceffively, like the mixture with rofin or gum. The fulphuric acid inflamed this mixture, but the nitrous acid did not.

Experiment XVII. *with Ether.* A few drops of ether on about two grains of the falt rubbed to a very fine powder produced no detonation by friction. The fulphuric or nitrous acids poured fuddenly upon it produced flame.

Experiment XVIII. *with Iron-filings.* Thefe alone rubbed with the falt produced no detonation by fimple friction; but two grains of the falt, one grain of iron-filings, and half a grain of fulphur, being well rubbed together, about a quarter of a grain of this mixture exploded violently with friction. The fulphuric acid added caufed a few fparks to appear; but the nitrous acid did not produce any. Varying the above proportions did not appear to improve the detonating property of the mixture.

Experiment XIX. *with Aurum Mufivum.* Equal parts of this fubftance and of the falt detonated ftrongly with flame, on being rubbed together in an iron mortar: a very flight friction was neceffary. The fulphuric acid gave a fmall flame, but with the nitrous I could not procure any.

From the foregoing experiments I think we may venture to conclude, that the oxygenated muriat of potafh is equally harmlefs as common nitre; except it be brought into an intimate union with fomething that has a greater affinity with one of its conftituent parts, than exifts between thofe parts when combined in the falt, and that fome combuftible fubftance be prefent: but its oxygen being fo eafily difengaged renders a little caution neceffary; and as the fulphuric or nitrous acids feem fo readily to inflame many of the mixtures, I would not advife any perfon to make more of them than is neceffary for immediate experiment. This precaution may prevent any unpleafant circumftance from accidental mixture with the acids, which appear to difengage a great part of the oxygen almoft inftantaneoufly.

I fhall not fay much about the theory of thefe detonations, none of the foregoing experiments having been fo carefully conducted as to determine accurately what changes took place; yet I think we may attempt to explain fome of them in the following manner: With phofphorus the oxygen feems to combine, and form phofphoreous acid gas, or phofphoric acid; with fulphur, the fulphureous acid gas, or fulphuric acid, according to the rapidity of

* Chaptal (Elements of Chemiftry, vol. ii. page 377,) fays, "Pruffian blue takes fire more eafily than fulphur, and detonates ftrongly with the oxygenated muriat of potafh. (Quære, Did he not make ufe of indigo here?)

the

the combuſtion ; with charcoal and other vegetable ſubſtances, the carbonic acid ; with ſulphuret of arſenic there may be ſulpureous acid gas and arſenic acid produced.

The ſudden production of gas ſtriking the ſurrounding air, is moſt probably the cauſe of the loud reports produced by friction, &c. agreeably to the concluſions of Berthollet ; and the muriatic acid may remain combined with the potaſh, and a portion of the combuſtible ſubſtance employed: but, when the ſulphuric or nitrous acids are uſed, the muriatic acid is certainly diſengaged.

Since the above experiments were made, I have found that a paper has been read before the National Inſtitute of Paris, On detonation by concuſſion, by Citizens Fourcroy and Vauquelin *. They there mention ſome of the mixtures I have deſcribed, and their inflammation with the ſulphuric acid. They likewiſe notice, that very loud reports and ſparks were produced, on a very ſmall quantity of different mixtures being ſtruck with a hammer on an anvil. This on trial I found to be the caſe; and a little cotton-wool well impregnated with the ſalt being ſtruck in that way immediately took fire. But, to get this to ſucceed, the ſalt and cotton ſhould be perfectly dry: this is a neceſſary precaution in all experiments on the detonating property of this ſalt by friction, &c. In the paper above alluded to it is ſtated, that ſugar, the gums, fixed and volatile oils, alcohol and ether, do not detonate or take fire by ſimple trituration: but the experiments I made ſeem not to agree with this aſſertion ; for all the above ſubſtances that I tried, except ether, detonated either more or leſs, on rubbing them briſkly in a ſtone-ware mortar; ſome of them required to be intimately mixed, as ſugar and gum ; but others produced very loud reports, as when fixed and eſſential oils were uſed.

IV.

Obſervations on the Natural Hiſtory of Guiana. In a Letter from WILLIAM LOCHEAD, *Eſq. F.R.S. Edin. to the Rev. Dr.* WALKER, *F.R.S. Edin. Regius Profeſſor of Natural Hiſtory in the Univerſity of Edinburgh* †.

DEAR SIR,

ALLOW me at preſent to trouble you with a few general obſervations on natural hiſtory, which I had an opportunity of making while on a botanical excurſion, with my friend Mr. Anderſon, to the Dutch colony of Demerary, Guiana is a country but little known in Europe, though its animals and vegetables have added conſiderably to the catalogue of natural productions. It is not, however, the organic kingdom which I mean at preſent to touch upon ; all I aim at is, to give you ſome idea of the face of the country, as leading to the knowledge of its formation and preſent ſtate. It is not a field for the mineralogiſt, as its interior is unexplored. But to the geologiſt, who wiſhes to trace revolutions of the lateſt date, it is not uninteresting to contemplate ſuch a recent and ſingular country as Guiana.

I need not inform you, that under Guiana is comprehended all the coaſt of South Ame-

* Annales de Chimie, tom. xxi. p. 235. Nicholſon's Chemical Journal, i. p. 169.

† Edinburgh Tranſactions, iv.

rica,

rica, from the Amazons to the Oroonoko; that it trends nearly N. W. and S. E.; that it is in general a very low and flat country, eſpecially the Dutch or weſternmoſt part of it; and that it is watered by ſeveral rivers and creeks, which riſe in a chain of mountains running nearly E. and W. and dividing Guiana from the inland parts of South America, which form the banks of the Amazons and its numerous branches.

Coaſt.—No coaſt can be more eaſy to make than that of Guiana. The changed colour of the water indicates ſoundings long before you make the land, and you may run on in ſeven fathoms before you can diſcover it from the deck. The bottom is at that diſtance a ſoft mud. All along the coaſt near Demerary, you have only about two fathoms at a good league from the ſhore; to leeward of Eſſequebo, it deepens ſtill more gradually. In ſtanding off or on five or ſix miles, you will hardly deepen or ſhallow the water as many feet. When a high ſea ſets in upon ſuch a coaſt, it is eaſy to conceive that at a very conſiderable diſtance from the land it muſt be affected by the bottom. The interval betwixt wave and wave becomes more diſtinct. As they roll on in ſucceſſion, the lower part is retarded, the upper ſurface accelerated: each billow of courſe becomes ſteeper and more abrupt, till at laſt it gradually ends in a breaker, when it has come to the depth of only a few feet. Theſe *rollers* as they are called are the dread of ſeamen, eſpecially betwixt Eſſequebo and Pomeroon, where the water is ſhallow, and the bearing of the coaſt very much north and ſouth, and expoſes it fully to the action of the trade winds. In ſmall craft, thoſe acquainted with navigation do not heſitate to run along the coaſt, even among the rollers themſelves; but veſſels drawing from eight to twelve feet water, eſpecially if the ſwell be heavy and it falls calm, can hardly get off. If anchor and cables fail, they drift on till they are faſt in the mud, and there they will continue ſometimes for weeks together before they go to pieces. The ſea water becomes exceedingly thick and muddy within a few leagues from the coaſt of Demerary, as much or more ſo than the Thames is at London. A ſtranger would naturally take this for the diſcharge of large flooded rivers after a rainy ſeaſon. By and by I ſhall explain the true cauſe of it.

On approaching the continent of South America, a change on the face of the ſky will ſtrike the attentive obſerver. The clouds become leſs diſtinct from each other, and the intervals between them leſs clear. They are blended into one another as it were, and ſuffuſed more generally over the atmoſphere. They appear to be ſurcharged with vapour, or to have a ſtronger diſpoſition to depoſit it.

There is a particular prevailing appearance of the heavens within the tropics, when you are at a diſtance from continents or very high iſlands, which has ſo often ſtruck me that I wonder it has not been taken notice of. I call it a *tropical ſky*, and thus deſcribe it. The clouds in fine weather are in a ſingle ſeries or ſtratum, ſailing away regularly with the trade-winds. They are ſmall, and diſtinctly ſeparated from each other. The intervals or ſky above them of a clear azure. The lower ſurface of the clouds is perfectly horizontal. As the temperature is commonly very equal over the ſea, the condenſation takes place every where at an equal height from the ſurface of the water. In the clouds that are over head you cannot indeed perceive this; but it becomes more and more viſible as the eye recedes from the zenith. The lower limb of each diſtant cloud appears perfectly level and well defined, brighter than the ſuperincumbent part. At a diſtance nothing is to be ſeen but theſe limbs cloſer and cloſer in gradation, one behind the other; and the whole horizon round

round reſembles the roof of a ſtage with an infinity of half dropt curtains as far as the eye can reach. In two voyages from Europe, I have met with this tropical ſky as far north as Cape Finiſterre. It came with a fair wind, which continued with us like a regular trade wind, accompanied with the ſame appearance of the clouds till we made the Weſt Indies. In running down the trade winds, every one has an opportunity of verifying this deſcription, and muſt be ſtruck with the beauties which this ſky preſents at the ſetting of the ſun. The inhabitants of the lower iſlands may alſo be well acquainted with it. In the higher ones, the attraction of the mountains ever forms ſets of clouds of other appearances, as being produced by other cauſes. With our preſent knowledge of meteorology, hardly any other cauſe can be aſſigned for the phenomena above mentioned, than the conſtant equal temperature that every where prevails on the intertropical ſeas. One analogous fact however may be mentioned; the exceeding ſmall range of the barometer in the torrid zone. Does the ſame cauſe regulate ſo exactly the height of the clouds, and maintain the uniform ſuſpenſion of the mercury? We might almoſt ſuſpect it did, were it not well known that the barometer varies as little upon continents, and in the vicinity of mountains in theſe regions as elſewhere, though the condenſation of vapours is in ſuch caſes much more irregular. Upon the continent you will frequently obſerve this tropical ſky alſo, eſpecially in fine ſettled weather; but much more commonly you will find the ſky there, and even before you make the coaſt, covered with heavy large dark clouds in ſome places, and in others at a greater height, the ſerene dappled ſky ſo often ſeen in Europe.

Winds.—The *trade wind* generally prevails all the day long, and on the ſea coaſt ſeldom fails even at night; but in leſs than fifty miles up the river it is a dead calm at night, and the breeze is not able to penetrate ſo far till towards noon. Still farther up we had whole days of a ſtark calm, and the heat very intenſe.

Dews, fogs and temperature.—The dews, following the law which they generally obſerve, are very heavy when and wherever there is but little wind, and the hotter the day and evening they fall the more copiouſly; they were of courſe more abundant up the river than near the ſea-coaſt. The exhalations in the day-time from a hot and miſty country covered with vegetables being very great, the condenſation occaſioned by the abſence of the ſun and the cold accompanying that condenſation are in proportion. Near the coaſt, the diurnal difference of temperature is but trifling, the conſtant trade-wind preſerving in the air nearly the ſame medium of heat as in the body of the ocean; but far up the river the range of the thermometer was very great. The heat of the day was intolerable. In the ſhade it was frequently above 90°. This, when there is no breeze, forces you into the woods for ſhelter. Towards evening it cools; during the night the cold increaſes, and is greateſt about five in the morning. The thermometer would then be from 72° to 74°. The body of the river being large enough to retain its heat, the evaporation goes on from its ſurface through the night, and is condenſed into thick fog, which hangs over it, and is ſeldom diſperſed before eight or ten next day. While the air was as above in the morning at 72°, the water along ſide gave 80° to 83°, and ſeldom roſe two degrees higher at noon. We had an opportunity of verifying an obſervation made by the few inhabitants who live far up the river Demerary, that, when it feels very cool in the morning before day-break, they are ſure of fine weather; when, on the contrary, it feels warm, they expect rain. They

ſleep,

ſleep in hammocks, and the houſes they have are pervious enough to the air; ſo they are ſenſib'y affected by any change in its temperature.

Seaſons.—As to the ſeaſons, it is not an eaſy matter, from the accounts given by the coloniſts, to aſcertain them exactly. All ſeem to agree, that ſince cultivation has been ſomewhat extended they are not ſo regular as before; that the dry ſeaſon encroaches on the rainy, and that during the latter they have often ſeveral dry days in ſucceſſion. The account given by Dr. Bancroft was the one generally allowed; that it is dry about the equinox, and rainy about the ſolſtices; that of conſequence they have two wet and two dry ſeaſons every year. We thought it difficult to reconcile this with the account given of the ſeaſons of other countries in ſimilar climates, and with what actually takes place in the Carib iſlands. I will give you my ideas on the ſubject. It is within the tropics a very general rule, that the vicinity of the ſun brings the rainy ſeaſon. To the northward of the line, therefore, this muſt be our ſummer months. It is another invariable law, that as in lunar influences, ſo in the change of ſeaſons produced by the ſun, ſome time is neceſſary after the maximum of the cauſe to produce the full effect. The higheſt tides are not till two or three days after the full and change. The greateſt heat of the day is two hours afternoon, and the hotteſt months in Europe are July and Auguſt, not June, when the ſun is higheſt. Among the Weſt India iſlands the full effect of the ſun's vicinity is ſtill later. I have found Auguſt, and more eſpecially September, to be the hotteſt months in the year, and they are accordingly the height of the rainy ſeaſon. It begins thus: No ſooner has the ſun come to the northward, and begun to be vertical among the iſlands in April and May, than his force is felt, the ſky is more diſturbed, the wind is more frequently from the ſouthward and in ſqualls, and now and then there are heavy ſhowers. In June the ſame effects continue, and increaſe in July, when the proper rainy ſeaſon may be ſaid to begin, and continues in force more or leſs till the middle of October. Auguſt and September, with part of July and October, when theſe effects are at their greateſt height, are ſtyled the hurricane months, and by the French *l'hivernage.* During them the full force of the great luminary which diſtributes light and life, however neceſſary, ſeems ſometimes too much for nature. She is oppreſſed and ſickens; her reſpiration is diſordered by intenſe heat; ſometimes calms, ſometimes heavy ſqualls; the agitated elements vent themſelves in lightning, with thunder and torrents of rain, or are ſometimes thrown out into thoſe horrid convulſions, hurricanes, which ſeem to threaten inſtant diſſolution. Guiana is happily free from theſe ſcourges of the Antilles. Their force has lately been partially felt at Tobago, which was thought beyond their reach. In Trinidad, the greateſt ſtorms they have hitherto experienced do not deſerve the name of hurricanes; and to the ſouthward on the main of America they are utterly unknown. The difference then between Guiana and the iſlands is this: In the former, the rainy ſeaſon ſets in earlier, as indeed the ſun is ſooner vertical. Their principal rains are in the end of April, in May, June and July. They are alſo ſooner over; for Auguſt, September and October, and I believe part of July, are commonly fair weather. But again November, in part December, January and February, reckoned dry months among the iſlands, are in Guiana a ſecond rainy ſeaſon. The cauſe of this I take to be as follows: North-eaſterly winds prettty ſtiff, cold and bleak comparatively in theſe climates, are frequently among the iſlands during the winter months.

months. They are well known by the name of Norths. They are often accompanied with rain; but it is not very heavy, nor thought of conſequence enough to give the denomination of a rainy ſeaſon. Theſe winds we know to reach as far as the coaſt of Guiana; and there I have reaſon to believe they are productive of more rain than in the iſlands. The face of a large continent, and its effects upon the atmoſphere, may very probably make them give up more of their humidity than they do among the Antilles, though at the ſame time their force and bleakneſs may not be ſo much felt. If this conjecture hits the truth, the following ought to be the corollaries, and are left to future obſervation:—In this rainy ſeaſon, when the ſun is near the ſouthern ſolſtice, their rains will be with pretty ſteady northerly breezes on the coaſt. They may be of longer continuance at a time, but they will not be ſo heavy as thoſe of ſummer, and they will be chiefly on the ſea-coaſt, and probably will not extend a great way up the country. It remains even a query with me, whether the rain that accompanies the norths among the iſlands, eſpecially thoſe moſt remote from the line, be not generally in a greater proportion than is commonly ſuppoſed.

Country.—I will now endeavour to give you ſome idea of the face of the country. Though, as is well known, Guiana is flat and ſwampy, yet it affords to the attentive eye an intereſting variety. The ſea-coaſt is little if at all raiſed above the level of high water, and is continued at this level for many miles inland. It is properly an immenſe woody ſwamp, never dry in the drieſt ſeaſon, covered with ſeveral feet of water in the wet. Next the ſhore, as far as the brackiſh water extends, it is covered with mangroves, which grow to a conſiderable height and form a thick ſhade. They are elevated on their branchy intermingled roots from the bare wet clay or mud, on which there is ſcarcely one herb or plant, but which ſeems to be all in motion from the prodigious number of crabs which make their holes in it. Further on, when the under water is freſh, you meet with a new ſet of vegetables, principally ſmall trees, which from their ſituation are obliged to adopt the habits of mangroves, having the bottom of their trunks ſupported three or four feet above ground by their ramified roots. Several climbing plants are mixed with them. Arunis in great variety and profuſion emerge from the water, or embrace the ſtems of the trees; and ſeveral broad-leafed plants of the hexandria and triandria claſſes aſſiſt the arunis in forming an herbage. In all this low part of Demerary there is not one tree of a large ſize, nor among them all above two or three ſpecies which can be applied to uſe as timber. Proceeding ſtill up the river, its banks are found generally to raiſe themſelves above the level of the water; and when you have gone up one tide (betwixt twenty and thirty miles), they are ſo high that there is no farther occaſion for dams to keep the plantations from being overflowed at high water, as below: canals or ditches are ſufficient to drain the land, which is ſtill perfectly flat. The trees are here different in ſpecies and larger in ſize than below, and the woods are much more practicable. As they are drier, the ground has acquired a regular ſort of ſurface, and there is neither that plexus of roots nor the ſame number of vines (the common name in the Weſt Indies for all climbing plants) to entangle thoſe who chooſe to traverſe them. The ſoil here is generally a ſtiff, cold, reddiſh clay, mixed a-top with a portion of vegetable mould.

The ſand-hills preſent to the admiring eye a ſcene very different from what it had been accuſtomed to below. The firſt you meet with upon the Demerary is upwards of thirty miles from the mouth of the river, and on the right hand aſcending, or on its weſtern ſhore.

There are of them further down in the country, but not cloſe to the river ſide. This one is the extremity of a ridge which extends to the weſtward ſeveral miles. As you aſcend the river you meet with many more of the ſame kind on both ſides, whoſe direction ſeems likewiſe to be eaſt and weſt, or nearly at right angles with the average courſe of the ſtream. They vary from 50 to 100, 150 or 200 feet of perpendicular height above the level of the river and the intervening flat country. Their breadth and extent vary ſometimes only a few hundred paces, ſometimes many miles. Their length is great; with ſome interruptions, I have reaſon to believe they are generally continued from one ſide of the colony to the other, only interſected in different places by the rivers and their branches. They conſiſt of a pure ſiliceous ſand, ſo white that it dazzles the eyes, commonly fine-grained and looſe, but not unfrequently mixed with little ſtrata of coarſer pebbles, moſtly quartz, and ſometimes concreted into a proper ſand-ſtone. In the laſt caſe, a black or reddiſh tinge is in many caſes communicated to it from clay, decayed vegetables, or other extraneous matter. There is no regular ſtratification to be found in it, more than what is common to all ſands the produce of depoſitions of different dates; and, as they are of different materials, thicker in one place, thinner in another, ſometimes horizontal but oftener inclined, and convex or concave according to circumſtances. We could meet with no appearance of ſhells or other marine productions, but in a few places pieces of broken vegetables buried in the ſand where it was concreted. They were black, as all the foſſil vegetables that I have ever ſeen in ſand-ſtone. Upon and by the ſides of the ſand-hills grows the moſt valuable timber of theſe colonies. The trees there are of a good ſize, and very clear of obſtructing underwoods or vines. The wallabba (parivoa grandiflora of Aublet); the ſipiri or green-heart (a new ſpecies of laurel); the coumarou or tonquæbean tree, coumarouna odorata of Aublet; the mora, valuable for boat timbers; and many others whoſe wood is equally hard and beautiful.

Continuing to aſcend the river, the ſand-hills become rather more frequent; but the intervals ſtill remain a perfect flat, though now ſeveral feet above the level of the ſtream, and the ſoil is ſtill a ſtiff clay. Hitherto the river is deep all over, generally from two to five fathoms; the bottom is mud or clay, and the ſhores on either ſide at low water covered with ooze. About 130 miles up, however, or juſt before it begins to ſhallow, the bottom is covered with banks of a hard white or brown ſand. It was a problem for ſome time, whence all this ſand originated in ſuch a country. It was ſoon ſolved. Leaving here the veſſel that had hitherto carried us, we proceeded in a canoe; and at about 160 or 170 miles diſtance from the mouth of the river we met with the firſt proper hills of ſolid materials. The neareſt to us was a rock of granite projecting into the ſtream, whoſe direction it gave a change to at this place; and it ſerved for a landing place to the higheſt piece of cleared land upon the river next to the poſt-holders. It was part of a low ridge of the ſame ſtone which croſſed the country probably to Berbia or beyond it, and was ſucceeded by many other ſeries of hills more inland, and, as far as we could examine them, of the ſame materials. The granite was both of the red and the gray kinds, but chiefly of the latter. A number of ſeams or dikes croſſed it here and there in all directions, not diſtinctly ſeparate, but firmly united to the reſt, making as it were but one body with it, and conſiſting of the ſame materials differently modified. Their component parts were generally ſmaller; they were more compact and cloſer in the texture than what ſurrounded them; and where they had

had been equally expoſed to the action of the weather, they appeared to have borne it much better than the ſurrounding granite. The origin of the ſand was now accounted for. This ſtone, in ſome caſes exceedingly firm and durable, is in others very liable to decay; and the waſh of theſe enormous chains of hills was able to furniſh abundance of ſuch ſand as we had met with below. The granite afforded many varieties; indeed, every ſhade from large and diſtinct grained to that whoſe component parts of feltſpar, ſchorl, or quartz, were ſo ſmall as to reſemble pretty compact compound lavas, or ſome of our mixed whin-ſtones in Scotland. All theſe varieties would be found at no great diſtance from each other. I brought ſome ſpecimens from Tiger's Berg, a hill about 500 feet perpendicular height, which have every appearance of having undergone the action of fire. They reſemble half vitrified ſcoriæ, and would be taken for them, but that they were actually broken off from the granite, and diſcover all its parts in the fracture. The ſummit of this hill is irregular, with ſeveral pits and holes among the rocks. A little higher than it, and I ſuppoſe nearly about 200 miles from the ſea, you meet what are called *the falls*. They are only five or ſix rapids within the ſpace of a mile or two, formed by ledges of very cloſe-grained gray granite that run acroſs the river. There are breaks in each of them, through which the dextrous Indians are able in their light canoes to paſs up at any ſeaſon, even the drieſt; and when the river is ſwelled by the rains they become totally obliterated. Two days journey or two and a half above this is the great fall, where the ſtream comes over the face of a rock, as we are informed, twenty feet high.

Savannahs.—Savannahs, ever ſince the diſcovery of America, have been known to occupy large ſpaces in the ſouthern parts of that continent. They are to be met with abundantly in Guiana, and are of two kinds very diſtinct from each other, the *wet* and the *dry*. Of the former many are extenſive as the eye can reach—immenſe verdant plains occupying the whole face of a country, with or without a few ſtraggling inſulated patches of wood. In the dry ſeaſon they appear meadows of long graſs or reeds, and are ſeldom practicable for any diſtance, for the bottom is very rarely dry. In the wet ſeaſon they are all one entire plain of water, over the ſurface of which the graſs ſtill riſes, but which may be every where navigated in the courialls or canoes. Towards the end of the drought the Indians ſet fire to them. The young growth which ſucceeds attracts the deer; and the native, on the return of the half deucalion days, purſues them in his little bark acroſs their former plains. The ſoil upon theſe ſavannahs can neither be very deep nor very good; yet water may be always commanded, and labour and induſtry might convert theſe deſerts into rice fields. It is a queſtion whether the days of ſlavery will ever ſee that event. The culture of this uſeful vegetable, which in the eaſt has for ages been the ſtanding food for millions, brings too moderate a return, at leaſt in an infant colony, for the rapacious agricultural ſyſtem of the Weſt Indies.

The dry ſavannahs are neither ſo frequent nor ſo extenſive, yet we have paſſed through ſome of them ſeveral leagues in circumference. They are formed along the flats on the top of the ſand ridges, and covered by a very thin coat of verdure. They reſemble exactly enough ſome of the bare moors in Scotland. Many beautiful plants of the claſs gynandria are their chief ornaments, as is alſo the orchis, which grows in ſimilar ſituations with you. Some melaſtomas and more rhexias ſupply the place, and bear ſomewhat of the habit of the ericæ; for your ſedums and ſaxifrages is the little ſauvageſia; and, in hollows of the

ſame

fame favannahs where moifture prevails, what I never could have expected to fee within five degrees of the line, and not more than 50 or 100 feet above the level of the fea, the drofera lifts its humble head from a bed of the fphagnum paluftre.

[*To be continued.*]

V.

On the Sugar Maple. By Citizen TESSIER *.

SUGAR is one of the moft common products of the vegetable kingdom, and is found ready formed in a great number of fpecies. It is obtained from the wild cherry-tree, (merifier), the poplar, the birch, the nut-tree, the pods of gleditzia, maize, the afclepias fyriaca, &c.; but the tree which next to the fugar-cane affords this product in the greateft abundance and beft quality is the fugar maple (acer faccharinum).

Many fpecies of maple grow naturally in France, Germany, Switzerland, and England. The acer opalus is found particularly in Italy, and that which Linnæus diftinguifhes by the name of acer tataricum in Afia. But America is the country of moft of the fugar maples. It was from Quebec that Mr. Sarafin forwarded this tree to the *Jardin des Plantes*. Father Charlevoix, at the diftance of a league and a half from Quebec, was regaled, to ufe his own expreffion, with the faccharine juice of maple. Kalm, at the poft of Three Rivers, between Montreal and Quebec, faw the procefs of making fugar with the fap of the maple. The acer faccharinum is there fo common that it is ufed for fuel.

A climate in which the winters are long and fevere is beft adapted to the fugar maple. I do not know whether it is found in a more northerly latitude than that of Canada. In that country, though fituated in the latitude of about 44°, the cold lafts longer, and is more intenfe than in France, on account of the enormous maffes of water, the woods and the mountains. Towards the fouth, the maple becomes very rare, fo that few are feen beyond the lower part of Louifiana. Kalm has remarked, that they grow to a lefs height in the fouthern parts of the United States than in Canada, and that they do not grow in New Jerfey and Pennfylvania, except on the fides of the Blue Mountains, and the fteep banks of rivers expofed to the north. And even in this expofition they do not obtain more than one third or a quarter of the height they acquire in Canada. The intelligent Dupratz, author of the Hiftory of Louifiana, confirms what Kalm has advanced. The fpecies of maple which we poffefs in France do not thrive excepting in places where the cold is of long duration. Kalm affures us, that being in the neighbourhood of Chefter, a fmall town on the Delaware in Pennfylvania, he faw red maples on a marfhy foil accompanied by the alder—an obfervation which points out the foil in which this tree ought to be planted. The fugar maple is alfo found in the State of Vermont in Kentucky, in the country beyond the Ohio, oppofite to Pennfylvania and Virginia. According to the author of the American Geography, fugar is made from it in thefe countries; but it does not feem to be an object of great intereft, as the author of that geography fimply mentions the fact.

* Inferted in the Annales d'Agriculture, and thence copied into La Decade Philofophique, &c. No. 9, an vi. from which laft work the prefent tranflation is made.

Citizen Michau, nurferyman at Charles Town, has a confiderable quantity near the banks of the Ohio in the ftate of Virginia, beyond the Apalachian mountains. They were planted in a good foil. In this country the maples have fuch a growth, that, if the author of the American Geography may be credited, fycamores are to be met with forty-four feet in circumference *.

The Canadians have long been accuftomed to ufe the juice of the maple as a refrefhing beverage. When it iffues out of the tree it is clear, whitifh, and of a cool faccharine tafte. By expofure to the fun in fummer, it is converted into good vinegar. It is obtained by boring the trunk of the tree, taking care to direct the inftrument upwards. The blade of a knife, or a piece of thin wood in the form of a ruler, is inferted to conduct the fluid to a veffel placed beneath for its reception: without this precaution, it would flow down the bark of the tree and be loft. Mr. Gaultier obferves, that the perforation muft be made into the proper ligneous circles, and that the faccharine juice is not to be obtained by making incifions in the middle bark or the liber, or at leaft that the quantity obtained will be very fmall. At the commencement of the thaws the fap flows abundantly for about three weeks; after which it thickens and entirely ftops. The maples afford more fap the greater the quantity of fnow has been, and the more rigorous the winter. The moft favourable period is when the fnow begins to melt, and the cold weather ftill continues. The flow is confiderable in the fpring, when the thaws are great and decided. The colder the nights, the greater the quantity of fap which flows on the following day. It feldom flows during the night, unlefs the weather be mild. If thefe obfervations had been made in all parts where there are fugar maples, they would afford reafon to conclude, that the regular alternation of great cold during the night and very perceptible heat during the day-time, which takes place in the northern parts of America, contributes to elaborate the fap of the maple and render it fweet.

The juice of the maple is collected earlier or later according to the country. In the vicinity of Quebec, Three Rivers, and Montreal, it lafts from the middle of March to the middle of May, when the fun begins to have power; but near Lake Champlain it is collected from the middle of February to the early part of March. In this country the thaws commence about the end of January.

If we give credit to Mr. Gaultier, the French taught the favages to extract fugar from the fap of the maple; but if we depend on Kalm, the favages knew this art before the Europeans had difcovered America, and the latter people have only followed the practice of the favages. Whatever may be the value of thefe two unfupported affertions, it is certain, that in order to extract this fugar the liquor is boiled over the fire, taking care to ftir and fkim it until it has obtained a very thick confiftence. If it be kept too long over the fire, it acquires a tafte of honey like melaffes. This fugar becomes fpontaneoufly purified. It is fometimes clarified with whites of eggs before it is fufficiently boiled, and after the clarification the ebullition is continued: when the boiling is fufficient, the fugar is poured into a veffel which gives it its form. In Canada, wherever the maples abound,

* This appears to me to be greatly exaggerated; the largeft American fycamores, according to the report of perfons who have meafured them, being no more than from twenty to twenty-four feet round. *Note of the Author.*

they

they make this fugar, as, in France, cherry brandy is made in fuch places as abound with cherries.

The firft juice which is extracted from the maples in the fpring is fweeter than that which flows at the end of the feafon, when it has the tafte which in Canada is called the tafte of the fap. This laft, from which the fugar is more difficultly extracted, is kept in the ftate of fyrup. It contains more fugar in proportion as the weather is colder. Trees of the fame age do not afford the fame quantity either of fap or of fugar, neither do the quantities of fugar in like quantities of fap agree. That of the old maples, which is lefs abundant, is more faccharine. The fap of maples which grow in a mountainous and ftony foil is more faccharine than that of fuch trees as grow in low humid foils. This obfervation holds good with regard to all vegetable productions.

A tree of three or four feet in circumference may afford from thirty to fixty pints of liquor, and fometimes more; and a pound of fugar is about the quantity obtained from fixteen pints, that is to fay, nearly three pounds of fugar per tree. Care muft be taken to make the openings on the fame fide every year, in order that the tree may not die. The fouth or fouth-weft is the proper fide. Mr. Gaultier obtained in a quarter of an hour a pint of this faccharine fluid from a perforation three inches deep, made on this fide of a tree four feet in circumference; at the fame time that a perforation in the fame tree on the north and north-eaft afforded him only a chopine or half pint in the fame time. If the perforations be multiplied the tree becomes exhaufted, and its old age is accelerated. It feems to me that it would be advifable to leave the tree untouched every fecond year, in the fame manner as good economifts fuffer their mulberry trees to repofe at like periods.

Father Charlevoix caufed a refiner at Orleans to make a trial to refine the maple fugar. He found fome difficulties in the attempt; but with attention thefe difficulties have been furmounted, for it is at prefent refined in America. This fugar, in the ftate we ufually receive it from that country, may be kept a long time without alteration, as I have feen in a piece in the collection of Citizen Juffieu.

Gaultier and Kalm affirm, that the maple fugar of the favages of Canada is mixed with flour, whether to render it more nutritious or to augment the quantity; but this kind of fophiftication, if the maple fugar fhould become an object of commercial intereft, would be eafily difcovered. Such fugar is whiter than other famples not fo adulterated.

The fugar of the maple is employed by the inhabitants of the remote parts of Canada, for the fame ufes as the fugar of the cane with us, becaufe they are poor; and this fugar, though lefs pleafing to the fight and tafte, cofts them nothing but the trouble of tapping the trees and evaporating the fluid. This fugar is more difficultly foluble in water; and its fweetening quality, if I may ufe this expreffion after Kalm, is to that of the fugar-cane as one to two. The richer inhabitants of towns which by commerce have intercourfe with the colonies where the fugar-cane is cultivated, prefer for their ordinary confumption the fugar of this laft plant, and ufe that of the maple as an agreeable medicament. It is particularly recommended for coughs and colds, and its ufe is even prefcribed for diforders of the lungs.

It is eftimated that between twelve and fifteen thoufand pounds weight of maple fugar is annually made in Canada. It is the product of four or five thoufand trees. From this fact, fuppofing an arpent of land of a hundred perches, at eighteen feet the perch, planted with maples in full growth, each affording fixty pints of liquor, the product would be fix hundred and fixty-fix pounds of unrefined fugar. For the arpent may fupport two hundred and twenty-two trees, placing the ftems at twelve feet diftance from each other.

The maples begin to afford a certain quantity of fugar at the age of 18 years, and ceafe to afford any at 60 or 70 years. In America, thofe trees are reckoned large which rife above 60 feet. There are fome which are two feet in diameter. The wood of certain fpecies is hard, and of a clofe texture. If they be tapped too foon, there is reafon to fear that it may injure the growth, and render the product of fugar inconfiderable. It is therefore of advantage to determine the period of life at which the tapping ought to be commenced. And on this head it has been afcertained, that the beft and moft abundant produce is obtained from trees of the middle fize, and that fcarcely any is afforded by maples which are large and old.

The fugar maple has not hitherto been cultivated in France, but as an object of curiofity. Some enlightened cultivators, who endeavour to direct all their purfuits to objects of utility, have entertained the hope that fome advantage might be derived from this tree. There are a fufficient number of thefe trees at prefent in France, to afford the means of determining whether they produce fugar in our climate, and in what quantity; and this decifion is neceffary to be had before the cultivation in the large way ought to be propofed. For it is poffible that they may afford a very faccharine liquid in America, and much lefs in Europe, as happens with the liquid amber; which, according to Kalm, affords much balfam in New Mexico and South Carolina; but produce lefs in Virginia, and none at all in Penfylvania and New York.

The difference which exifts between the cold of the nights in the American countries and thofe of Europe under the fame degrees of latitude, the heat of the fun being equal, muft neceffarily influence the progrefs of vegetation.

In the mean time, until we fhall acquire the neceffary information, if it fhould be thought fit to increafe the number of thefe trees, the following is the method of proceeding:—The fureft and moft ufeful is to fet the feeds. They may be found in the plantations of M. Duhamel, M. Les Malefherbes, and feveral other curious gardeners. It is obferved, that the acer faccharinum brought from America is not raifed but with difficulty, either becaufe it is not tranfplanted at the proper time, or becaufe it has failed of receiving proper care during the paffage. It would perhaps be neceffary to fend exprefsly to America an intelligent gardener, or to give inftructions in the country to an attentive perfon to fend the beft feed to France, and even the plant, in order to accelerate our progrefs. The acer faccharinum never grows from flips; but it grafts very well on the fycamore. Its refemblance to the plane maple of Europe, which is fuch that the two trees are confounded, feems to fhew that it would be moft convenient to graft upon this tree; but a gardener has feveral times attempted this in vain. An opaque matter which lies between the wood and the bark oppofes the union. The maple negundo, the only one which is multiplied by flips, is likewife propagated by feed and by layers. The jafper-maple is grafted with the greateft fuccefs on the fycamore. It rifes with a better ftem when grafted in this manner; but care muft be taken to graft very low. Ripe feeds of the red maple have not yet been obtained; but it may be raifed from American feed, and in this manner

the

the two individuals may be obtained. The acer tomentofum, laftly, is one of thofe which is moft eafily multiplied in our climates.

Though it is faid that fome fpecies of maple may grow in light ftoney foils, they profper better in good ground. The foil of the mountains cannot always be confidered as bad. The red maple delights in the plains and valleys, and all the fpecies are injured by a ftrong fun. They fucceed well if fheltered to the fouth by mountains or woods, as I conclude from the ftate of the fugar maples of Malefherbes and Rambouillet.

The advantages which would refult from cultivating the fugar maples in Europe, and particularly in France, are relative to the quantity of fugar they may afford. When it fhall be proved that a great quantity, or at leaft feveral pounds per tree, can be obtained, lands which are even fufceptible of cultivation will be devoted to this object. Some fpecies will require to be placed on the flopes of mountains, where the fnow and cold weather prevail for a long time. Others will be planted in low fituations on the borders of meadow lands, lakes, rivers, and grounds approaching to marfh lands, fuch as thofe where the alder delights to grow. Such plantations, which may contribute to increafe the production of fugar without any other care than that of tapping the trees and concentrating the liquor, will be of great utility in fupplying our habitual want of this article. Befides which, the fugar maple prefents to turners, mufical inftrument makers, inlayers, the workers in ftaining wood, and gunfmiths, a valuable material, which in fome of the fpecies is veined and marbled. It is proper, therefore, to encourage the growth of this tree.

In addition to the facts and obfervations contained in the foregoing memoir, which, if conclufive with refpect to France, would probably be much more fo with regard to the northern diftricts of our ifland; it may be added, that Dr. Benjamin Rufh of Philadelphia communicated an excellent paper on the fame fubject to the American Philofophical Society, which was publifhed in their Tranfactions, vol. iii. in the year 1793. The abftract of his paper is as follows:

The acer faccharinum of Linné, or fugar maple tree, grows in great quantities in the weftern countries of all the middle ftates of the American Union. It is as tall as the oak, and from two to three feet in diameter; puts forth a white bloffom in the fpring before any appearance of leaves: its fmall branches afford fuftenance for cattle, and its afhes afford a large quantity of excellent pot-afh. Twenty years are required for it to attain its full growth. Tapping does not injure it; but, on the contrary, it affords more fyrup, and of a better quality, the oftener it is tapped. A fingle tree has not only furvived, but flourifhed after tapping for forty years. Five or fix pounds of fugar are ufually afforded by the fap of one tree—though there are inftances of the quantity exceeding twenty pounds. The fugar is feparated from the fap either by freezing, by fpontaneous evaporation, or by boiling. The latter method is the moft ufed. Dr. Rufh defcribes the procefs, which is fimple, and practifed without any difficulty by the farmers.

From frequent trials of this fugar, it does not appear to be in any refpect inferior to that of the Weft Indies. It is prepared at a time of the year when neither infect nor the pollen of plants exifts to vitiate it, as is the cafe with common fugar. From calculations

tions grounded on exifting facts, it is afcertained that America is now capable of producing a furplus of one eighth more than its own confumption; that is, on the whole, about 135,000,000 pounds, which in the country may be valued at 15 pounds weight for one dollar. Dr. Rufh mentions many other benefits his country may derive from this invaluable tree, and concludes his paper with an account of fome of the advantages of fugar to mankind; not merely, as commonly confidered to be a luxury, but as an excellent wholefome and nourifhing article of food. Annexed alfo is an extract from the report of the committee of the Britifh privy council on the fubject of the African flave trade, containing Mr. Botham's ftatement of the mode of cultivating a fugar plantation at Batavia *.

VI.

The Progrefs of Mechanical Difcovery, exemplified in an Account of a Machine for cutting Files. (*W. N.*†)

THE folly and confequent diftrefs of purfuing experiments in chemiftry, for the fole purpofe of commercial advantage, has been repeatedly obferved both by public writers and in private life. The obfcurity which attends the proceffes of this art, the imperfections of theory, and the feductions of hope, have united to lead men in purfuit of medicines of uncommon powers, and agents which fhould convert the cheaper metals into gold and filver. It is a fubject of no wonder, to thofe who have not fuffered their mental habits to be vitiated by thefe feductive analogies, that difficulties and difappointment fhould attend the life of a man thus employed. But mechanics have, in general, been more favourably regarded. A number of fimple and admirably ufeful effects are produced by the operation of machines. We daily fee improvements produced by means eafily underftood. The mechanic who endeavours to ftrike into a new path, finds he can reafon from what has been done before him, and ufually begins his work with a conviction that the refults he is defirous of obtaining will infallibly happen. Hence it is that a prodigious number of new fchemes find their way into books; on which both the author and the reader fet a high value, and of which the futility is difcerned only by a few practical men. Some of my readers have fuppofed this fource of information to be much more productive than it really is. A very flight enquiry concerning new machines and inventions, whether they have been carried into effect, and whether they have fuperfeded the old methods of operation, will immediately ftrike out of the lift of valuable articles not lefs than nine tenths of the objects to which the public attention is folicited. And if it be afferted that the defcription of fuch abortive projects might be of ufe to afford hints to fpeculators, I muft take the liberty to obferve, that it is a moft ferious thing to engage in a new invention, and a no lefs ferious duty in the editor of a public work to

* To avoid the imputation of plagiarifm, I muft take notice that the two laft paragraphs appeared in an anonymous publication in the year 1794, but were written by myfelf. N.

† As the obfervation on the wrapper, *that all papers without name or fignature are written by the Editor,* has been overlooked by fome readers, who have made enquiry refpecting the authors of fuch papers, he has thought it expedient in future to add his initials.

be well aſſured of the value of what he recommends or ſuffers to recommend itſelf to his readers. From views of this kind, it has appeared to me, that I ſhould do ſome ſervice to an active ſet of men, ſome of whom have effectually ſerved this country, if I were conciſely to point out the courſe of mechanical invention, in order that thoſe individuals only may be induced to engage in it, who poſſeſs the acquiſitions and means to do it with ſome effect.

We will therefore ſuppoſe a very acute theoriſt, who is not himſelf a workman, nor in the habit of ſuperintending the practical execution of machinery, to have conceived the notion of ſome new combination of the mechanical powers to produce a determinate effect; and for the ſake of perſpicuity, let us take the example of a machine to cut files*. His firſt conception will be very ſimple or abſtracted. He knows that the notches in a file are cut with a chizel driven by the blow of a hammer, by a man whoſe hands are employed in applying theſe inſtruments, while his foot is exerted in holding the file on an anvil by means of a ſtrap. Hence he concludes, that it muſt be a very eaſy operation to fix the chizel in a machine, and cauſe it to riſe and fall by a lever, while a tilting hammer of the proper ſize and figure gives the blow. But, as his attention becomes fixed, other demands ariſe, and the ſubject expands before him. The file muſt be ſupported upon a bed or maſs of iron, of wood, of lead, or other material:—it muſt be fixed either by ſcrews or wedges, or weights, or ſome other effectual and ready contrivance:—and the file itſelf, or elſe the chizel, with its apparatus for ſtriking, muſt be moved through equal determinate ſpaces during the interval between ſtroke and ſtroke, which may be done either by a ratchet wheel or other eſcapement, or by a ſcrew. He muſt examine all theſe objects, and his ſtock of means in detail; fix upon ſuch methods as he conceives to be moſt deſerving of preference; combine, organize, and arrange the whole in his mind; for which purpoſe ſolitude, darkneſs, and no ſmall degree of mental effort, will be required:—and when this proceſs is conſiderably advanced, he muſt have recourſe to his drawing board. Meaſured plans and ſections will then ſhew many things which his imagination before diſregarded. New arrangements to be made, and unforeſeen difficulties to be overcome, will infallibly preſent themſelves. The firſt conception, or what the world calls the invention, required an infinitely ſmall portion of the ability he muſt now exert. We will ſuppoſe, however, that he has completed his drawings. Still he poſſeſſes the form of a machine only; but whether it ſhall anſwer his purpoſe, depends on his knowledge of his materials. Stone, wood, braſs, lead, iron forged or caſt, and ſteel in all its various modifications, are before him; the general proceſſes of the workſhop by which firmneſs, truth, and accuracy, are alone to be obtained; and thoſe methods of treatment, chemical as well as mechanical, which the ſeveral articles demand:—theſe and numberleſs other practical objects call for that ſkill and attention, which may either lead to ſucceſs, or, by their deficiency, expoſe him to the ignorance or obſtinacy of his workmen. If he ſhould find his powers deficient under a proſpect ſo arduous—if he

* Tranſactions of the American Philoſ. Society, vol. ii. or Repertory, v. 184. The file is fixed on a bed of lead, and a chizel fixed at the end of a lever, is ſtruck down with a hammer. This lever riſes again of itſelf by means of a ſpring, and during its riſe it moves a ratchet wheel, connected with the ſupport of the bed; which conſequently it ſhifts together with the file after every ſtroke.

cannot

cannot fubmit to the fevere difcipline of feeing his plans reverfed, and his hopes repeatedly deferred—if unfuccefsful experiment fhould produce anguifh without affording inftruction, what will then remain for him to do?—Will he embitter his life by directing his inceffant efforts, his powers and refources, to a fafcinating object, in which his difficulties daily increafe; or, will he make that ftrong exertion of candour and fortitude, which will lead him to abandon it at once?

Thefe are the inevitable ftages of operation, through which every inventor in mechanics muft pafs. To the mere habit of viewing objects in new lights, the habit which leads to the outline of invention, he muft add the power of difpofing his notions in the form of an individual engine or inftrument; and he muft himfelf become a workman, capable of difcerning the means by which his ideas may become realized in the proper materials. It may perhaps feem as if I had felected an inftance of difficulty, and indulged my imagination in a fketch of obftacles feldom likely to be met with. This, however, is far from being the cafe. Nothing feems more fimple and eafy at firft fight, than to make an engine to cut notches in a piece of fteel; and a very ingenious perfon, in the work above referred to, has accordingly given an accurate defign of an engine for that purpofe, which no doubt he thinks muft fucceed. But manufacturers well know the value of fuch an engine, and have long ago attempted to make it by that and various other methods without fuccefs. That engine in particular, promifing as it appears, is utterly incapable of working, for feveral reafons, fcarcely to be difcovered but by practical men, but which cannot with fufficient brevity be here detailed. And with regard to general obftacles in the detail of inventions, I am fo far from magnifying them, that I am warranted by much experience, as well on my own behalf, as that of others whofe plans and operations have come before me, to affirm, that no mechanical invention really new was ever brought to its complete or perfect ftate, at fo fmall a charge as three times the coft of the finifhed engine, exclufive of the incalculable labour of the contriver.

VII.

The Dutch Procefs for making the Blue diftinguifhed by the Name of Turnfol.*

LICHEN, Archil, or in cafe this laft cannot be obtained, the greater mofs of the oak, is dried, cleaned, and pulverized in a mill, refembling the oil mill, and then fifted through a brafs wire fieve, the interftices of which do not exceed one millimetre in width (1-250th of an inch). The fifted powder is then thrown into a trough, and mixed with an alkali called vedas, which is nothing elfe but the cendres gravelées in powder. The proportion is one part by weight of the alkali, to two parts of the pulverized vegetable. This mixture is moiftened with a fmall quantity of human urine; the urine of other animals does not contain a fufficient quantity of ammoniac. The mixture ferments, and is kept moift by fucceffive additions of urine. As foon as the materials have become red, they are transferred into another trough, where they are again moiftened with urine, and ftirred to renew the fermentation. Some days afterwards the pafte acquires a blue colour, in which ftate it is carefully mixed with one third of excellent pot-afh well powdered; and

* From the Journal du Commerce, copied in the Decade Philof. &c. No. 57.

with this new mixture certain trays are filled, which are one metre ($39\frac{1}{3}$ inches) deep, and eight decimetres ($31\frac{1}{2}$ inches) wide. When the fermentation which takes place for the third time has given the paste a considerably deep blue colour, chalk or powdered marble is added, and the whole is well and perfectly mixed. This last addition is made, not to improve the quality of the blue, but to add weight. It is merely an affair of profit. The blue thus prepared is put into iron moulds 32 centimetres long and 22 square at the end ($1\frac{1}{4}$ inch by $\frac{8}{10}$ of an inch). The moulded pieces are then placed upon deal planks, in well-aired lofts, to dry; after which they are packed in casks for sale.

The Hollanders made a secret of this process: and in order to mislead, they have published, that the blue was made with rags coloured by the plant turnsol; whence it has obtained its denomination *. We may derive much profit by carrying this discovery into practice.

VIII.

Experiments and Remarks on certain Ranges of Colours hitherto unobserved, which are producted by the relative Position of plain Glasses with regard to each other. (W. N.)

THOSE ranges of colours which are afforded by the reflection and transmission of light through thin transparent plates, have been an object of much attention to philosophers, ever since the experiments made by Robert Hooke and Sir Isaac Newton. The latter of these philosophers endeavoured to generalise the facts by a statement, which, because in part hypothetical, has been treated with contempt by some eminent men, though it has been referred to in most disquisitions on these phenomena. When a convex lens is applied to a plain glass, it is well known that coloured circles surround the place of contact; and as these circles are found to vary in their dimensions, the nearer the glasses lie with respect to each other, whether by means of the curvature or of pressure, it has been concluded that the effects are caused or governed by the distances of the surfaces. From various considerations Sir Isaac was led to conclude, that the rays of light are themselves possessed of a property, by which, in certain equidistant points of their length, they are disposed to enter transparent bodies, and in certain other points, intermediate between these, they are disposed to be reflected. From this assumption he deduced, that if a ray of light passed through the first surface of a medium, it would be either transmitted or reflected at the second surface, accordingly as the distance might happen to coincide with a point of transmission or reflection. According to this doctrine, it must follow that the interval will govern the effect, not in consequence of its own magnitude, but of the precise number of measures, or, as he calls them, fits of transmission or reflection it may contain.

Thus, for example, if we suppose a ray of indefinite length to be divided into equal parts, and imagine certain marks of division to subsist; if the ray pass, through the first surface of a medium exactly at one of these marks, it will be transmitted through any surface, however distant, which shall be remote from the first, either nearly or accurately some precise number of parts; because the fits of transmission are respectively at those points or marks: and

* English writers have used this denomination. But the dry-salters, or dealers in drugs, distinguish these pastils by the name of litmus.

on-

on the other hand, it will be reflected from any furface whofe diftance is meafured by fome number of whole parts, together with half a part; becaufe the fits of reflection lie half-way between the divifions. In this ftatement I have merely enunciated what are fuppofed to be the facts, and have not attended to his fuppofition of an etherial elaftic fluid, pervading all fpace and thrown into undulations more rapid than the velocity of light itfelf. Neither can I at prefent enter into any difcuffion concerning its probable value. My prefent object is to relate a few experiments, which, at the fame time that they fhew the exiftence of thefe fits at very confiderable intervals, appear to prove that the reflection and tranfmiffion of light in the fame medium is governed by other circumftances as well as by the diftance between the confines or furfaces. Thefe experiments lead to many fpeculations and extended paths of enquiry. It has long been my intention to multiply and repeat them, in order to difcover the laws upon which they depend: but other avocations have already delayed the accomplifhment of this purpofe for more than feven years; for which reafon, I hope that my defire to fee the object purfued by others may operate as an apology for the imperfect ftate of the refearch, of which I here prefent the commencement.

The experiments of the Abbé Mazeas on colours produced by applying flat plates of glafs to each other, are related at full length in Prieftley's Hiftory of Light and Colours, p. 499. This philofopher found, that rubbing the glaffes together caufed the colours to appear at the fame time that adhefion took place between them, and Mufchenbroeck found that lenfes of long focus do not afford colours after having been laid by for a length of time unlefs they be wafhed and wiped. One of the moft remarkable circumftances in the experiments of Mazeas was, that the colours of his flat plates are moveable by a very flight increafe of temperature; which produces an effect fimilar to what would have arifen from removing the glaffes further afunder, or diminifhing the preffure which might have been applied to them. Beccaria was, I believe, the firft who obferved that thefe colours may be produced by fuperinducing an electric charge on the external furfaces of two plates, which are by this means made to adhere. But in all the obfervations I have met with, whether made by Prieftley in his Hiftory, or by any other writer, thefe powers have been fuppofed to operate by increafing or diminifhing the diftance between the furfaces. The following facts will fhew that this conclufion requires to be modified.

In the year 1791 I cut a plate of very clear glafs into portions, which were intended to be ufed to defend a veffel of quickfilver from the agitation of the air. Its colour, when viewed edgewife, was a very light green, and it had been carefully ground by an optician, with its fides truly parallel to each other. The thicknefs was twelve hundredths of an inch. A piece 3,2 inches long and 2,4 wide was laid upon another larger piece, both having been previoufly wiped. The refult was, that faint colours appeared in rows about fix or feven in number. Preffure appeared to alter thefe very little in pofition; but it produced other more vivid colours, which were much more moveable, and croffed the former without affecting them. The fcarcely moveable ranges were very little difturbed by the heat of the finger applied againft the undermoft glafs; though this application was fufficient to produce a very great alteration in the figure of the other ranges. When the upper glafs was lightly placed upon the lower, the faint and fcarcely moveable ranges appeared alone, and the glaffes had very little adhefion; fo that the uppermoft flided about on the larger plate beneath, without feeming to carry the ranges with it, but arrived at other ranges, which

which from their immobility ſeemed as if attached to the lower glaſs. Theſe ſcarcely moveable colours, however, were quickly moved, and brought nearer to each other by raiſing one edge of the glaſs with the hand; and they continued viſible when the glaſs was lifted as high as one-fortieth or thirtieth of an inch, by ſlipping a knife under its edge. When a plate of metal was placed beneath the lower glaſs, and electricity (which from the diſpoſition of the machine happened at that time to be negative) was communicated to the upper plate, the vivid colours appeared, and the adheſion of the glaſſes was increaſed; but the ſcarcely moveable ranges remained little if at all affected. When the glaſſes were taken from the machine towards the window, the vivid colours ſlowly receded as the electricity was diſperſed, and in this ſtate they were very advantageouſly and evidently ſeen croſſing the other leſs moveable ranges.

Theſe facts appeared at that time to lead to no other concluſion than that the moveable ranges had been diſturbed by ſome other circumſtance beſides that of the mere diſtance of the plates: for, if the diſtance had indeed been varied, it ſeems reaſonable to ſuppoſe that both ſets of ranges would have been affected.

Soon afterwards, upon making ſome obſervations with the artificial horizon, and a ſextant conſtructed by Troughton, I obſerved a ſeries of colours in the horizon-glaſs, when the poſition of zero was to be aſcertained. They appeared both in the ſilvered and the clear part. The poſition of the glaſſes, when the colours were ſeen, was very nearly parallel as to the vertical ſituation of the planes; but it admitted of the index being moved through nearly forty minutes before they diſappeared. Theſe glaſſes, namely the index-glaſs, and the horizon-glaſs, were $3\frac{1}{2}$ inches aſunder.

This laſt fact appears to juſtify the inference of Newton, who conſidered the colours of thick and thin plates as depending on the ſame cauſe; which doctrine was applied by Dr. Pemberton, to account for the numerous ranges of colours ſometimes obſerved beneath the common rainbow *.

As one of the greateſt difficulties in Newton's doctrine ſeems to have been that the fits of reflection and tranſmiſſion are ſuppoſed to extend to vaſt diſtances, it ſeemed deſirable to repeat this experiment with as great an interval between the glaſſes as could conveniently be had. With this view I placed one plate of glaſs on the ſurface of a veſſel of mercury, and held another in my hand parallel to it, in ſuch a poſition that the reflected light from the firſt paſſed through the ſecond glaſs to my eye. The white clouded ſky and the chimney of an oppoſite houſe were ſeen by reflection in both glaſſes, and it was eaſy to move the upper glaſs till both images of the chimney coincided. In this poſition the glaſſes would have been parallel if the object had been indefinitely diſtant; but in the preſent caſe, the paralleliſm could not be obtained but by a ſlight elevation of the moveable image. By this diſpoſition the colours were made to appear when the plates were four feet aſunder, and I have no doubt but that the ſame would have happened at much greater diſtances if the imperfect method of adjuſting the paralleliſm could have been applied.

The ſame effect, as might naturally be expected, took place when the eye was ſo placed as to receive the reflected light from the lower glaſs, after it had been tranſmitted through the upper.

* Philoſ. Tranſ. abridged, vol. vii. or Prieſtley's Optics, p. 590.

In

In addition to these observations, it may be remarked, that I have not observed these colours in such plates of common looking-glass as I have tried; that they do not appear in a small pocket sextant which I possess; that the disposition to exhibit these colours seems to be increased by wiping or friction, and also, as I think, by a continuance of the action which is necessary to produce them, or cause them to run along the surface of the glasses.

IX.

*Some Account of the Country and Climate of the North-western Lakes of America. By Major C. Swan, Paymaster to the Western Army**.

D'Etroit, Oct. 10, 1797.

* * * * * * * * * * * * *
* * * * * * * * * * * * *

"This country is yet new, and almost in a state of nature, like its inhabitants. It is true the soil is extremely rich and fertile; and it is to a superabundant burden of vegetation, and a flat surface for hundreds of miles together producing much stagnant water, that we may attribute the unwholsomeness of the climate, which is almost certain to affect the inhabitants with bilious complaints every fall †.

* * * * * * * * * * * *
* * * * * * * * * * * *

"Gen. Wilkinson arrived here in June this year, and, after making some prompt arrangements for the garrison, proposed a voyage to Michilimackinac, and invited me to accompany him; and on the 4th day of August we embarked in a sloop of about 70 tons burthen. We had a safe and pleasant trip, not only to Michilimackinac, but even into Lake Superior; and returned to this place on the 4th of last month, highly gratified indeed.

"We first left this place, and traversed Lake Sinclair, a handsome circular lake about twenty-five miles across. We then proceeded up the river of that name, which is broad and very handsome, for about forty miles, to a rapid at the entrance of Lake Huron; traversed this immense beautiful lake, three hundred miles long; and arrived on the 15th of August at the streight which unites it to Lake Michigan. This streight is broad; and the Isle de Bois Blanc, or White Wood Island, Round Island, and Michilimackinac Island form a cluster in the middle of the streight, and afford a romantic and majestic landscape from the sea. The Isle de Bois Blanc is eleven miles and a half long, and from two to three wide, lying parallel to the two coasts of the streight, but nearest to the south-side. Round Island

* From the Medical Repository, printed at New York, vol. i. p. 526. It is introduced by the following Note of the American Editor:

Note. The following Article consists of Extracts from a Letter of Major Swan to Capt. Frye, commandant of the garrison at Governor's Island, who was so obliging as to communicate them to one of the Editors; and they are now made public with the consent of the Author. These Extracts are briefly made from a Journal kept by Major Swan at the time; and are inserted for the threefold purpose of illustrating the table of thermometrical observations which follow, of communicating a short but authentic view of a part of the north-western territory, and of inviting further information.

† These remarks have particular reference to the neighbourhood of D'Etroit.

is about three miles in circumference, and lies at the upper or fouth-weft end of De Bois Blanc. The ifland of Michilimackinac is circular, and lies between the upper end of De Bois Blanc and the north-weftern coaft of the ftreight; having a channel of about one mile and a half between it and De Bois Blanc, and a channel of nine miles between it and the north-weftern coaft of the ftreight. It meafures feven miles and three quarters in circumference, and is nearly circular. On the fouth-fide of this ifland there is a fmall bafon of a fegment of a circle, ferving as an excellent harbour for veffels of any burthen, and for canoes. Around this bafon the village is built, having two ftreets of nearly a quarter of a mile in length, a Roman chapel, and containing eighty-nine houfes and ftores; fome of them fpacious and handfome, with white lime plaftering in front, which fhews to great advantage from the fea. At one end, and in the rear of the town, is an elegant government-houfe of immenfe fize, and finifhed in great tafte. It is in the form of ‖⊽‖; one ftory high, the rooms fifteen feet and a half in the clear. It has a fpacious garden in front, laid out with tafte, and extending from the houfe on a gentle declivity to the water's edge. There are two natural limpid fprings in the rear of the houfe, and a very lively grove of fugar-trees called the Park. Suitable out-houfes, ftables and offices are added; and it is enriched on three fides with beautiful diftant profpects. Twenty rods from the rear there is a fudden and almoft perpendicular afcent of about a hundred feet of rock, upon the top of which ftands the fort, built of ftone and lime, with towers, baftions, &c. occupied by our troops, and commanded by Major Burbeck. About half a mile from the fort, in the rear, there is an eminence which I eftimate to be about two hundred and fifty feet from the furface of the water. This fpot commands an extenfive and fublime view of the adjacent country. The fort, the village, the neighbouring iflands and channels feem proftrated at your feet; while to the fouth-weft you look into the immenfity of Lake Michigan, which lofes itfelf in the fouthern hemifphere; and to the north-weft the great Lake Huron lies expanded to the bounds of the horizon. It was a beautiful morning when I had this view.

" This celebrated ftreight is the only key to the immenfe lucrative fkin trade now folely carried on by Britifh fubjects from Montreal, with the nations of Indians, called the Sauteurs or Chipewas, Sioux, Reynards, &c. who inhabit the water-courfes that fall into the Miffiffippi between the Illinois and the falls of St. Anthony. Canoes are loaded and fitted out by thefe traders every year from Michilimackinac. They commonly fet out in July, and return in June, July or Auguft the year following, to Michilimackinac, from whence they ftarted. Here they are again met by the Montreal canoes with frefh goods, exchange loading, and each return from whence they came. The Montreal canoes penetrate to Michilimackinac by way of Grand River; which, with the exception of a fmall portage, conveys them to the northern point of Lake Huron; and return by the fame route. Thofe from Michilimackinac penetrate the interior or Indian country by way of Green Bay, an arm of Lake Michigan, thence through Fox River into the Miffiffippi and its tributary ftreams; and return alfo to Michilimackinac by the fame route.

" On the 22d of Auguft we left Michilimackinac, and on the 23d anchored in the ftreight of St. Jofeph, which leads to Lake Superior. At this place nature has difplayed very handfomely again. The mouth of the ftreight is about thirty miles wide, but fo ftrewed over with innumerable fmall circular iflands that it is difficult to obtain a view in any direction of more than fix or eight miles. Indians have fometimes been loft among these iſlands for weeks together. They extend into Lake Huron, and continue along the north-weſt coaſt of the lake for an hundred and eighty miles, and are called by the ſavages the Meneto or Devil's iſlands. From the entrance of the ſtreight at a place called the Detour, it is nine miles to the new Britiſh garriſon built on the point of the iſland of St. Joſeph, commonly called the Carraboo iſland. This is the largeſt in the ſtreight; being about twenty-five miles long, and from ten to three broad.

"On the 23d of Auguſt we left the veſſel, embarked in three canoes, aſcended the ſtreight in what is called the Canoe channel, and encamped at Muſkito Point.

"The 24th, at one o'clock P. M. we arrived at the falls of St. Marie, called *le Saut de St. Marie*. Theſe falls are about three-quarters of a mile long and half a mile wide; the rapid not violent; and the perpendicular of the whole fall about thirty feet. There is a ſmall kind of village on the United States ſide containing ſundry large warehouſes, and a few decent dwelling-houſes occupied by the Agents of the Canada North-weſt Trading Company. There is not a clear white woman in the place.

* * * * "The 25th it rained * * * * * * * * "On the 26th we ſet off in two bark canoes from the upper end of the portage for Lake Superior. * * *. At one o'clock P. M. we entered Lake Superior; looked fairly into it; drank of its waters; ate our dinner; and put about with a fine fair wind. We reached the falls again at four o'clock in the afternoon; placed experienced guides with ſtrong paddles in the bow and ſtern of each canoe; hoiſted the fifteen ſtripes; and launched into the boſom of the cataract. In a moment we were ſafe in the baſon at the bottom of the falls!

"We embarked early on the 27th. Having a ſtrong current and fair wind we deſcended in the ſhip channel, and reached the veſſel at Carraboo iſland, at nine o'clock in the evening.

"The 28th we put to ſea again; and on the 4th of September at ſun-down reached this place.

"I incloſe to you herewith degrees of heat which were aſcertained by regular obſervation with Fahrenheit's thermometer every day; by which you will perceive that the temperature of the Lakes differs widely from that of the Atlantic country."

On comparing the Table of Obſervations which is annexed with obſervations made on the ſame days in this city, the difference will appear ſo remarkable that the reader may ſuſpect ſome error in the inſtrument made uſe of by Major Swan: and ſuch were my ſuſpicions. But croſſing the Eaſt river to Governor's Iſland in company with that gentleman on the 4th of March 1798, he obſerved, that the wind which we then felt, and which was very briſk, reſembled, in point of temperature, that which he experienced on Lake Huron on the 14th and 15th of Auguſt 1797; and added, that on the ſame evening a froſt affected the gardens at Michilimackinac ſo ſeverely as to deſtroy the greater part of the vegetables.

TABLE

OF THE

Degrees of Heat obferved on Fahrenheit's Thermometer, from Auguft 4, to September 4, 1797.

1797.	6 o'Clock A. M.	12 o'Clock M.	7 o'Clock P. M.	Average.	Remarks, where.
Auguft 4	66	63½	59	62¼	Lake Sinclair.
5	59	66	63½	62¼	Ditto.
6	55½	65	66	62	River Sinclair.
7	60	67	67	64	Ditto.
8	59	61	61	60	Ditto.
9	57	62	52	57	Ditto.
10	58	62	61	60	Ditto.
11	63	59½	60	60	Lake Huron.
12	57	60	61	59	Ditto.
13	55	55½	55	55	Ditto.
14	50	49	47	48	Ditto.
15	45	66	54	55	Michilimackinac.
16	52	70	53	58	Ditto.
17	54	69	60	61	Ditto.
18	52	67	62	60	Ditto.
19	54	64	56	58	Ditto.
20	53	64	58	58	Ditto.
21	62	63	63	62	Ditto.
22	54	61	59	58	Streights of St. Jofeph.
23	57	65	60½	62	Ditto.
24	50	63	60½	57	Ditto.
25	46	53	49	49	Falls of St. Marie.
26	46	49	50	48	Lake Superior.
27	50	56	49	51	Streights of St. Jofeph.
28	40	50	46	45	Lake Huron.
29	51	54	51	54	Ditto.
30	49	57	53	53	Ditto.
31	50	56	49	51	Ditto.
Sept. 1	48	57	47	50	Ditto.
2	51	59	50	53	Ditto.
3	49	58	50	52	River Sinclair.
4	48	56	49	51	Lake Sinclair.

X.

An Essay on the Art of conveying Secret and Swift Intelligence. By RICHARD LOVELL EDGEWORTH, *Esq.* F. R. S. *and* M. R. I. A.*.

MODELS of the French Telegraph have been so often exhibited, and the machine itself is so well known, that it is not necessary to describe it minutely in this place. It is sufficient to say, that it consists of a tall pole, with three moveable arms, which may be seen at a considerable distance through telescopes; these arms may be set in as many different positions as are requisite, to express all the letters of the alphabet:—By a successive combination of letters shewn in this manner, words and sentences are formed, and intelligence communicated. No doubt can be made of the utility of this machine, as it has been applied to the most important purposes. It is obviously liable to mistakes, from the number of changes requisite for each word, and from the velocity with which it must be moved to convey intelligence with any tolerable expedition.

The name, however, which is well chosen, has become so familiar, that I shall with a slight alteration adopt it for the apparatus which I am going to describe. *Telegraph* is a proper name for a machine, which describes at a distance. *Telelograph*, or contractedly *Tellograph*, is a proper name for a machine that describes *words* at a distance.

Dr. Hooke, to whom every mechanic philosopher must recur, has written an essay upon the subject of conveying swift intelligence, in which he proposes to use large wooden letters in succession. The siege of Vienna turned his attention to the business. This method is more cumbrous than the French Telegraph, but far less liable to error.

I tried it before I had seen Hooke's work in the year 1767, in London; and I could distinctly read letters illuminated with lamps in Hampstead church-yard, from the house of Mr. Elers in Great Russel Street, Bloomsbury, to whom I refer for the date and circumstance.—To him and to Mr. E. Delaval, F.R.S. to Mr. Perrot of Harehatch, and to Mr. Woulfe the chemist, I refer for the precedency which I claim in this invention. In that year I invented the idea of my present tellograph, proposing to make use of windmill sails, instead of the hands or pointers which I now employ. Mr. Perrot was so good as to accompany me more than once to a hill near his house, to observe with a telescope a windmill at Nettlebed; which places are, I think, sixteen miles asunder. My intention at that time was, to contrive not only a swift but an unsuspected mode of intelligence: By

* Transactions of the R. Irish Academy, vi. 125. The first part of this paper consists of near thirty pages of very interesting historical detail, respecting the art of conveying intelligence by sounds and signals. It was practised by Theseus, in the Argonautic expedition; by Agamemnon, at the siege of Troy; and by Mardonius, in the time of Xerxes. It is frequently mentioned in Thucydides; it was used by Tamerlane; by the Moors in Spain; and by the Welch in Britain; by the Irish; and by the Chinese, on their famous wall of fifteen hundred miles, by which they separated themselves from Tartary. I have omitted this part on account of its length.

means of common windmills this might have been effected, before an account of the French Telegraph was made public*.

My machinery consists of four triangular pointers or hands, each of which points like the hands of a clock to different situations, in the circles which they describe.

It is easy to distinguish whether a hand moving vertically, points perpendicularly downwards or upwards, horizontally to the right or left, or to any of the four intermediate situations.

The eye can easily perceive the eight different positions in which one of the pointers is represented, plate xiii. fig. 1. by turning the eye to the circle A.

A similar circle may be imagined round each of the pointers, by which the numbers which they are intended to express, may be discovered with much facility.

Of these eight positions, seven only are employed to denote figures; the upright position of the hand or pointer being reserved to represent O or Zero. The figures thus denoted, refer to a vocabulary in which all the words are numbered. Of the four pointers, plate I. that which appears to the left hand of the observer, represents thousands, the others hundreds, tens and units in succession, as in common numeration.

† In the annexed plate the four large pointers stand at 2774, which in the common arrangement of my vocabulary, denotes the Royal Irish Academy. For permanent stations, which may be seen clearly with tolerable glasses at twenty miles distance, stone or wooden pillars sixteen or twenty feet high must be solidly erected: on the top of these a moveable circle, or platform, turns horizontally upon a centre: on this platform an axis moves vertically, and carries the arm or pointer along with it. Eight handles turn the pointers,

* Since this paper was written I received a letter from Mr. Perrot, which has been seen by the President, containing the following passage.

"I perfectly recollect having several conversations with you in 1767, on the subject of a speedy and secret "conveyance of intelligence: I recollect our going up the hills, to see how far and how distinctly the arms "(and the position of them) of Nettlebed windmill were to be discovered with ease. As to the experiments "from Highgate to London, by means of lamps, I was not present at the time; but I remember your mentioning the circumstance to me, I believe in the same year. All these particulars were brought very strongly to "my memory, when the French a few years ago conveyed intelligence by signals; and I then thought and "declared, that the merit of the invention undoubtedly belonged to you.---I am very glad that I have it in "my power to send you this confirmation, because I imagine there is no other person now living who can wit-"ness your observations in Berkshire."

† I insert, Plate xiii. fig. 2. a line described by telegraphs as an example.—It is the first line of the following verses, written on the prospect of corresponding between England and Ireland by the Telegraph:

Hark from Basaltic rocks and giant walls,
To Britain's shores the glad Hibernia calls;
Her voice no longer waits retarding tides,
The meeting coasts no more the sea divides.
Quick, at the voice of fortune, or of fame,
Kindles from shore to shore the patriot flame;
Hov'ring in air, each kindred genius smiles,
And binds with closer bands the sister isles.

The numbers are, 2645, 2331, 573, 1113, 244, 2411, 6336.

which

which are fixed in their different positions by a catch or alidad. By means of the platform, the pointer may be turned to any part of the compass; and as one side of it is painted black and the other white, either side may be employed, as the colour of the clouds, or the situation of the place, may require.

Besides these permanent machines, of which dimensions and a description are subjoined in plate xiii. I make use of portable machines, (which may be detached like tentacula from the main body in hazy weather,) consisting of pointers ten or twelve feet high, and of a light triangular stand, which can be easily fastened with tent pegs to the ground: these may be lodged in any house near the place where they are used, or in times of danger may be carried back to the permanent stations every night.

In managing a correspondence by these machines, it is necessary to have certain signals established: nor are these signals merely arbitrary; it is absolutely necessary that they should be made by the two external or by the two internal pointers, else they could not be repeated by the intermediate stations without confusion; because, in the middle stations, that pointer which represents thousands when conveying a message eastward for instance, must, when answer is returned in an opposite direction, represent units; the same change will take place between the pointers that denote hundreds and tens.

Certain hours of the day must be appointed for ordinary communication. Suppose ten o'clock in the morning, and five in the afternoon in summer. Every communication begins from the capital. If no intelligence is required to be conveyed from thence, the word BEGIN is sent to the county station, which may then proceed or dismiss the meeting.

When any communication is to be commenced, the pointers that denote thousands and units are whirled round, till the same is done at the corresponding station. When this signal has been answered, the person who gave it proceeds to send his intelligence. As soon as he begins, the pointer of hundreds at the opposite station is turned to two, and kept in that position till the word is made out from the vocabulary; it is then turned up to O or ZERO. The person who is speaking, when he perceives by this signal that he is understood, turns all the machines to NOUGHT, which is always to be done at the conclusion of every word.

When all his machines are in this position, his correspondent again turns his pointer belonging to the place of hundreds to two, where it is to remain till he receives another word, and so on till all that is meant to be said is finished. To denote that his communication is finished *—THOUSANDS and UNITS are to be vibrated backwards and forwards with the point downwards like a pendulum, till the same is done at the opposite station.

If any interruption takes place on either side, from a cloud, a shower, or any accident, it is pointed out by vibrating THOUSANDS and UNITS with their points upwards; which signal must be repeated from the opposite station. Whoever has made the signal of interruption, must make a signal of recommencement, when he is ready to proceed, by vibrating HUNDREDS and TENS, with their points upwards; when this is answered (but not before,)

* I use the words Thousands and Units here, and in the rest of this description, for the pointers or machines that stand in the numerical place of thousands and units.

the

the bufinefs may proceed. It fhould be obferved in general, that every fignal fhould be anfwered.

It requires fome fteadinefs to abide by thefe fignals; but if they are patiently adhered to, the fuccefs that they enfure will foon convince the operator of their utility. Without them every thing would be in confufion; by their interpofition, perfpicuity and order are perfectly enfured.

In my firft experiments the impatience of friends who were prefent, was fometimes fo great as to make it very difficult to adhere to previous arrangements; but a very little practice (I mean the practice of five or fix days) reduced the routine of communication to as much facility as could be defired, fo that a word (or a fentence, if contained in the vocabulary) could be fent in twenty feconds.

Any perfon who has the flighteft tafte for fcience or literature muft be ftruck when he fees inftantaneous interpretation of fignals which are made at the diftance of fifteen or twenty miles, and when he perceives the power which is obtained of tranfmitting thought with fuch aftonifhing rapidity.

I fhall not enter into a detail of the fignals which are neceffary for intermediate ftations; it would take up fome time to explain them, and they will readily occur from what has been faid already.

What I have hitherto defcribed relates to a large and permanent eftablifhment *; for the management of which one man is required at each pointer, one at the telefcope, and another at the vocabulary; but for ordinary purpofes, a fingle pointer, with one man to work it, and another at the telefcope, with a fmaller vocabulary, are fufficient. With this reduced apparatus we can with eafe fpeak at the rate of one word per minute to a great diftance, as the time loft by intermediate ftations is but fmall.

The vocabulary correfponding with the numbers denoted by this machinery, is compofed of a large book with mahogany covers framed, to prevent them from warping. Its fize is forty-feven inches by twenty-one; it confifts of forty-nine double pages, that is to fay, each fheet is folded in the middle, where it opens from one page.

The book is divided into feven parts, confifting each of feven pages, by thin flips of mahogany, which ferve to open it eafily at each of thefe divifions. Every one of thefe feven divifions contains feven pages, and each page contains forty-nine words.

No more than forty-nine words are contained in a page, becaufe the numbers of 8 and 9, and Zero, are omitted. This omiffion arifes from the ftructure of the machinery, which points only to feven numbers, referving O for a point of reft, at which point the hands indicate nothing. In every hundred, therefore, only forty-nine numbers are ufed; and in every thoufand only feven hundred is counted. Each divifion of the book feparated by the mahogany rulers, contains all the efficient numbers in feven hundred. Each of thefe rulers projects (Plate xiii. fig. 3.) beyond the fides of the pages, and each is numbered in fucceffion from one to feven; and they are fo placed below one another, as to permit the numbers on all fides of them to be feen at once, as in plate xiii.

When any number of thoufands is pointed out, it can by means of thefe rulers be im-

* The houfe belonging to this eftablifhment might be made tenable againft a mob, or mufquetry, at a fmall expence, by port flankers of elm or afh adapted occafionally to the windows. See plate xiii. fig. 4.

mediately

mediately felected; the feries of feven pages, which one of thefe rulers opens, is cut, like the alphabet of a ledger, at the edge in feven divifions. By thefe means the page containing the hundred which is wanted, is inftantly found. In the page thus found the tens, from ten to feventy inclufive, are divided from each other fo as to be inftantly diftinguifhable, and the units under each divifion are in like manner eafily felected.

Plate III. is a fpecimen of the firft page of the vocabulary; and though it is but one-fifth of the real fize, it is fufficiently diftinct. It is divided into eight claffes; all the claffes are numbered downwards feriatim from 1 to 77, omitting cyphers or zero, and eights and nines. When once the clafs required is afcertained, any number on the page can be found immediately:—As for inftance, the reader will eafily felect clafs iv. number 36, or clafs vii. number 77, and fo of the reft.

Nothing remains to be explained but the manner in which the clafs in each page is pointed out by the machinery. For this purpofe, before the pointers are turned to any fet of figures, the pointer that reprefents thoufands is turned to the clafs that is wanted; as foon as the correfpondent anfwers this fignal, THOUSANDS is returned to O, and inftantly all the pointers are moved to the places which denote the figures required for any word or fentence.

When the clafs is thus afcertained, an index, which flides on the mahogany cover of the book, is fet to the column belonging to this clafs; the number of thoufands is then opened by the ruler, as foon as it is read off by the telefcope; the number of hundreds is opened by the pages when they are cut away, and the number of tens and units is feen on the page. As the pointers are moved in fucceffion from thoufands to units, the different divifions of the book can be opened as faft as the pointers are moved. The order of this book might be reverfed with apparent advantage, by dividing the book into claffes by the mahogany rulers, &c. But I prefer, for reafons which it would be tedious to infift upon, the arrangement which I have followed.

As fecrefy is an object of the greateft confequence, I fhall endeavour to point out in a few words the fuperiority of this mode of communication over any alphabetical arrangement, not only in point of expedition, but of concealment.

Although the common alphabet may be varied at pleafure, and any arbitrary figns may be employed to convey the powers of each letter, yet, by certain rules, any of thefe arrangements may be deciphered. Whoever fees the movements of the French telegraph (I mean of that which is commonly known as fuch) may unfold the intelligence which it conveys, by merely marking down the changes which he fees, and putting them into the hands of a decipherer. The rules for deciphering depend upon the ufual arrangements of letters. In our language a fingle letter muft be A or I. The proportions which exift between words of one, two, three, and any greater number of letters, are claffed in catalogues; and from thefe the monofyllables of any cipher are eafily obtained, and from the letters of thefe monofyllables the letters of longer words are difcovered. By fimilar rules, fome of which are very ingenious, and which depend upon the general philofophy of language, any alphabetical cipher may be eafily unfolded. But thefe rules, except a very few of them, are ufelefs, when we employ ciphers which denote entire words. Here the moft obvious means of difcovery may be avoided, by omitting thofe common words which occur fo frequently in every language, the, and, that, to, &c. But, fuppofing that from its frequent recurrence any particular word fhould be difcovered, no progrefs can be made from

thefe

thefe data. The cipher of each word is an ifolated fact, which leads to nothing farther. Suppofe the knowledge of any particular vocabulary fhould fall into hands for which it was not intended, a flight change in the numeration, without any actual change of the figures, would entirely prevent difcovery:—For inftance, if the Lord Lieutenant wifhed to fend orders to the Commander in Chief; if he made ufe of the numbers written in the vocabulary in one day, he might, after previous communication, employ a different numeration by ordering that 1 (for inftance) fhould be added to every figure. If clafs ii. number 3664, ftood in the vocabulary for gunpowder by the addition which I have propofed, the number would ftand clafs iii. number 4775, which might mean a crocodile, or Tippoo Saib, or any thing foreign to the real word. By fimilar provifions any number of feparate correfpondents might carry on a mutual intercourfe, without interfering with one another.

In the courfe of twelve months I tried a great number of experiments, and carried on a great number of converfations with the tellograph; of all thefe a regular journal has been kept, containing what was unfuccefsful, as well as what fucceeded. If fuch journals were kept in the profecution of philofophical purfuits, they would pay for the trouble of keeping them by the accuracy of the experience which they enfure.

I fhall not at prefent enter into any detail of my Nocturnal Tellograph. Its velocity far exceeds what can be done by day, as in clear weather ftations at fifty miles diftance may be plainly diftinguifhed.

When this paper was firft prefented to the Academy, I had determined to try an experiment acrofs the Channel from Donaghadee to Port-Patrick. I was ambitious of being the firft perfon who fhould connect the iflands more clofely, by facilitating their mutual Intercourfe. Public bufinefs prevented me from going to the fea-fide at the time I had intended, and from carrying on a feries of converfations by day and night between the two kingdoms; but Mr. Lovell Edgeworth, my fon, had the fatisfaction of fending four meffages acrofs the Channel at four o'clock P. M. on the 24th of Auguft 1795, and of receiving immediate anfwers before a vaft concourfe of people. The machines by which this communication was made, were thirty feet high, and fifteen feet at the bafe. A child of four years old could turn them. Mifty weather prevented them from being feen; but when the weather cleared up, a pointer of twelve feet high could have been plainly diftinguifhed acrofs the Channel.

Though I have beftowed much attention and labour upon this fubject, I do not pretend to fay that the means of tellographic communication, which I have invented, are the beft that can be devifed. Imitations without end may be attempted; pointers of various fhapes and materials may be employed; real improvements will alfo probably be made; and, perhaps, new principles may be adopted. The varieties of art are infinite, and none but perfons of narrow underftanding, who feel a want of refources in their own invention, are jealous of competition and difpofed to monopolize difcoveries. The thing itfelf muft fooner or later prevail, for utility convinces and governs mankind; and however inattention or timidity may for a time impede its progrefs, I will venture to predict, that it will at fome future period be generally practifed not only in thefe iflands, but that it will in time become a means of communication between the moft diftant parts of the world, wherever arts and fciences have civilized mankind.

TABLE.

SPECIMEN OF THE VOCABULARY
BELONGING TO
MR. EDGEWORTH's TELLOGRAPH.

Common Words. Claſs 0.	Words leſs common. Claſs 1.	Technical Terms, c. n. m. Claſs 2.	Perſons. Claſs 3.
1.	1.	1.	1.
11 A	11 Abaſe	11 Aback	11 Abbot
12 Ab	12 Abate	12 Abacus	12 Ackland
13 Ac	13 Abbey	13 Abaft	13 Acton
14 Ad	14 Abbeſs	14 Abatis	14 Acheſon
15 Ae	15 Abbot	15 Abdomen	15 Adams
16 Af	16 Abdicate	16 Abductor	16 Adamſon
17 Ag	17 Abed, Abet	17 Abeal	17 Adair
2.	2.	2.	2.
21 Ah	21 Abide	21 Aberration	21 Adolphus
22 Ai	22 Abjure	22 Abeyance	22 Addington
23 Ak	23 Ablative	23 Ablution	23 Ahmuty
24 Al	24 Able-bodied	24 Abortion	24 Aikin
25 Am	25 Aboliſh	25 Abreaſt	25 Alcock
26 An	26 Abomination	26 Abrogation	26 Aldridge
27 Ap	27 Abortive	27 Abſceſs	27 Allot
3.	3.	3.	3.
31 Aq	31 Above-all	31 Abſcis	31 Alley
32 Ar	32 Above-board	32 Abſcenthium	32 Allett
33 As	33 Above-mentioned	33 Abſorbent	33 Allen
34 At	34 Abridge	34 Abſorption	34 Alder
35 Av	35 Abridgement	35 Abſtergent	35 Alexander
36 Au	36 Abruptly	36 Acacia	36 Amyatt
37 Aw	37 Abſentee	37 Academic	37 Ambroſe
4.	4.	4.	4.
41 Ay	41 Abſolve	41 Acantha	41 Anderſon
42 Ax	42 Abſolution	42 Accretion	42 Andre
43 Az	43 Abſorb	43 Aceſcent	43 Andrews
44 Abandon	44 Abſtract	44 Acetous	44 Angel
45 Abuſe	45 Abſtruſe	45 Achromatic	45 Anger
46 Abhor	46 Abſurdly	46 Acids	46 Anneſly
47 Abject	47 Abyſs	47 Acidity	47 Anneſdale
5.	5.	5.	5.
51 Ability	51 Academy	51 Acme	51 Antrim
52 Able	52 Accelerate	52 Aconite	52 Anſon
53 Above	53 Accent	53 Acouſtics	53 Anſtruther
54 Abound	54 Acceſs	54 Acroſtic	54 Antonie
55 About	55 Acceſſary	55 Adamant	55 Anthony
56 Abundance	56 Acceſſible	56 Adder	56 Alfred
57 Abroad	57 Acclamation	57 Adder's-tongue	57 Alphonſus
6.	6.	6.	6.
61 Abrupt	61 Accommodate	61 Adductor	61 Amadeus
62 Abſent	62 Accomplice	62 Adelphi	62 Anne
63 Abſence	63 Accoſt	63 Ades	63 Anſolm
64 Abſolute	64 Accountant	64 Adit	64 Appleby
65 Abſtain	65 Account-book	65 Adjutant	65 Apſley
66 Abſurd	66 Accretion	66 Adnata	66 Archer
67 Abſtain	67 Accrue	67 Adonis	67 Auſtin
7.	7.	7.	7.
71 Abuſe	71 Accumulate	71 Adoſculation	71 Archdale
72 Accede	72 Accuſative	72 Adracanth	72 Arran, Ld.
73 Accept	73 Ace	73 Adrift	73 Archdall
74 Acceptable	74 Achieve	74 Advance-foſſe	74 Aſhe
75 Accident	75 Acquiſition	75 Advance-guard	75 Atkinſon
76 Accompany	76 Acquittance	76 Advancement	76 Aylward
77 Accompliſh	77 Acre	77 Advertiſement	77 Ayre

Officers. Clafs 4.	Places. Clafs 5.	Navy and Merchant Ships. Clafs 6.	Phrafes and Sentences. Clafs 7.
1.	1.	1.	1.
11 Academy of Infcriptions	11 Abbeville	11 Atlas	11 Attend to-day at A. M.
12 Academy of B. L. Paris	12 Aberdeen	12 Ajax	12 ——— at P. M.
13 Academy	13 Abergavenny	13 Albion	13 —— to-morrow at A. M.
14 Account Office	14 Abington	14 Africa	14 ——— at P. M.
15 Admiralty	15 Abydos	15 Audacious	15 —— to-night at ——
16 Agent to the —	16 Abyffinia	16 Agamemnon	16 —— to-morrow night at —
17 Admiral	17 Acadia	17 America	17 —— on Monday at A. M.
2.	2.	2.	2.
21 Adjutant	21 Acaniboo	21 Anfon	21 ——— at P. M.
22 Alderman of Briftol	22 Acapulca	22 Alcide	22 —— on Tuefday at A. M.
23 Alderman of Cork	23 Acam	23 Alexander	23 ——— at P. M.
24 Alderman of ——	24 Aida	24 Alfred	24 —— on Wednefday at A. M.
25 Archdeacon of —	25 Adrianople	25 Arrogant	25 ——— at P. M.
26 — Ardagh —	26 Ætna	26 Afia	26 —— on Thurfday at A. M.
27 — Ardfert —	27 Africa	27 Ardent	27 ——— at P. M.
3.	3.	3.	3.
31 — Armagh	31 Agincourt	31 Achilles	31 —— on Friday at A. M.
32 — Acowry	32 Aix la Chapelle	32 Adamant	32 ——— at P. M.
33 — Aghadoe	33 Albany	33 Affiftance	33 —— on Saturday at A. M.
34 — Clogher	34 Alcantara	34 Acteon	34 ——— at P. M.
35 — Clonfert	35 Aleppo	35 Argo	35 Alarming Intelligence is received from —
36 — Cloyne	36 Alexandria	36 Artois	
37 — Connor	37 Algiers	37 Affurance	37 Acquaint the Commiffioner's Officers at —
4.	4.	4.	4.
41 — Dublin	41 Alicant	41 Arethufa	41 — principal Magiftrates at —
42 Archbifhop of —	42 Alps	42 Æolus	42 — the High Sheriff at —
43 — Dublin	43 Alface	43 Active	43 — the Secretary of War
44 — Armagh	44 Antrim	44 Alarm	44 ——— of State
45 — Cafhel	45 Aylefbury	45 Amazon	45 Agreeable to the Orders of his Majefty
46 — Cork	46 Ayrfhire	46 Ambufcade	
47 — Canterbury	47 All Saints	47 Amphion	47 — of the Lord Lieutenant
5.	5.	5	5.
51 — Tuam	51 Alnwick	51 Apollo	51 — of Government
52 — York	52 Aloft.	52 Aftrea	52 — of the Commanding Officers at —
53 Admiral of	53 Alresford	53 Alcmene	53 — of the Magiftrate of —
54 — the Fleet	54 Aldborough.	54 Andromache	54 Agreeable to your Orders
55 — the White	55 Alençon	55 Albemarle	55 All is well
56 — ditto 1	56 Andes	56 Aurora	56 Alter your Telegraphs to Black
57 — ditto 2	57 Anglefey	57 Amphitrite	57 ——— to White
6.	6.	6.	6.
61 — ditto 3	61 Angola	61 Ariadne	61 Admit no Strangers
62 — ditto 4	62 Anjou	62 Alfred	62 Admiralty has iffued Orders.
63 — ditto 5	63 Anhalt	63 Atalanta	63 — has received Intelligence
64 — ditto 6	64 Antioch	64 Ariel	64 Arms been found hidden at —
65 — ditto 7	65 Antwerp	65 Allegiance	65 Army approaches in Number
66 — ditto 8	66 Archangel	66 Albany	66 An armed Mob at —
67 — ditto 9	67 Argenton	67 Alderney	67 Art. of Capitulation agreed to,
7.	7.	7.	7.
71 — ditto 10	71 Atherftone	71 Alert	71 Affiftance is required at —
72 — ditto 11	72 Ardee	72 Alligator	72 Appearances are againft —
73 Admiral of	73 Arklow	73 Avenger	73 Anfwer my laft to-morrow
74 — the Blue	74 Armagh	74 Ætna	74 Arrived fince my laft at —
75 — ditto 1	75 Athenry	75 Alecto	75 Arrived News from E. Indies
76 — ditto 2	76 Athlow	76 Aquillone	76 Arrived Mails from —
77 — ditto 3	77 Athy	77 Argus	77 Agreeable Intelligence is received

SUPPLEMENT.

SINCE the Royal Irish Academy did me the honour to accept of my essay on the Telegraph, I have made material improvements in its construction, which I think it my duty to communicate.

In September 1796, the Lord Lieutenant ordered me to prepare Telegraphs for an experiment before his Excellency. In consequence I constructed four new Telegraphs.—I had found that the large machines thirty feet high, with which my sons talked, in September 1794, across the channel between Ireland and Scotland, were liable to accidents in stormy weather: my first consideration therefore was to contrive some means of furling their canvas when they were not in in use; and, from the rigging of ships, it was obvious that cordage was for this purpose preferable to inflexible braces of wood. I therefore adopted the following construction:

A. (fig. 4. Plate XIV.) a hollow axle-tree made in separate pieces hooped together in the form of a double truncated cone, on the middle of which is fastened a wheel of wood (b, fig. 4) with eight notches cut out (a, fig. 4) to receive eight ribs (r. r. r. r. r. r. r. r fig. 1 and 2.) These ribs turning on a strong iron ring, shut up like the ribs of an umbrella, and are raised and adjusted by cords passing through eight holes in the flanches or shoulders (F fig. 1. 4.) These flanches and those at (f. fig. 2 and 4) serve to keep the machine in its place upon the stands which support it (fig. 2); the cords are strained and fastened like the cords of a tent (c. c. &c. fig. 2.)

Where permanent buildings are not required, supports for these machines may be constructed in the following manner. Two stands, each of them made of two pieces of wood simply bolted together, as (fig. 3), must be erected, and held steady by means of cords (c. c.) fastened to common tent pegs, as in (fig. 2 P. P. P.) When the machines are large, small piles should be used instead of pegs, and running tackle (t.) should be used both for the cords of the pointers, and the stands. A number of minute circumstances should be attended to in the construction and use of these machines; but I do not think it proper to detail them to this Academy; they should appear in a different place *.

Besides rendering the Telegraph safe against storms, and more easily manageable, I found by experience that one machine could be made to perform the same effect as four, with but little loss of time; what took up four minutes with four pointers, can be conveyed in five minutes by one. I have also found that, by answering each signal or number shown at every station, all possibility of mistake is avoided.

I believe that, in other establishments of this sort, it has been found that thick and foggy weather has occasioned more interruptions than were expected. With my Telegraphs, I

* Formerly, in France, every engineer who conducted any public work, was obliged to lodge in a public office exact drawings, with minute descriptions, of every part of and process of his operations. Numberless small improvements in workmanship and tools were preserved by these means, and by degrees were collected into publications of general circulation.

I was required to deliver drawings of all the machinery I employed in the work carried on at Lyons in 1772, for turning the course of the Rhone:

But, in the transactions of a literary Society, such details would be tedious and improper.

 have

have good reafon to affert, that there do not commonly occur above eight or ten days in the year when intelligence might not be conveyed by land.

If eight men were pofted at each permanent ftation, at the diftance of eighteen or twenty Englifh miles afunder, with machines of twenty-five feet high, in hazy weather they might detach two men with portable Telegraphs, to the diftance of about fix miles from each ftation, who, with eight foot Telegraphs, could keep up a regular communication.

The portable Telegraph refembles that which I have defcribed; it differs only in two circumftances: for convenience, as it is fmall, and does not oppofe much furface to the wind, it may be diftended with ribs of wood inftead of cords.

The portable Telegraph which my fon had the honour of fhowing to his Royal Highnefs the Duke of York in Kenfington gardens, in October laft, was furnifhed with filken cords, on purpofe to fhew how my larger Telegraphs were conftructed; but it was intended merely for reconnoitring near an army, and was only fix feet high.

In the effay which the Academy has already received, I faid that imitations without end might be made of my Telegraph. Every index or pointer that moves circularly, dividing an imaginary circle into parts, and denoting figures or figns that correfpond with a vocabulary, is founded on the fame principle as mine. The French have laid afide their former clumfy apparatus, and have conftructed a Telegraph on thefe principles; and the Admiralty in England have, as I am informed, very lately done the fame.

The firft pointers I employed in 1767 were windmill fails. I then tried indexes of the fhape (fig. 5.) Fig. 8. A pointer, like a fword-cutler's fign, was recommended to me by a member of the Academy, as a fecond or additional hand to move on the fame centre as the principal hand. But a triangle, whofe bafe is equal to half its fide, is, of all the figures I have tried, the moft diftinct.

The Night Telegraph remains ftill to be defcribed; its ufes are perhaps more extenfive than thofe of the Telegraphs I have already publifhed; and I propofe to make it the fubject of another paper upon a future occafion.

The art of conveying fwift and fecret intelligence is not one of thofe inventions which attracts attention only by its novelty; on the contrary, I am convinced that it will be thought more valuable, the longer it has been fubmitted to the teft of time and experience.

SCIENTIFIC NEWS, AND ACCOUNT OF BOOKS.

COPENHAGEN, *July* 12, 1798.

THE Royal Society has propofed the following Prize Queftions. The Prize for each is a Gold Medal, value one hundred rixdollars (22l. 10s.)

I. *Hiftory.* What nations difcovered America before the Norwegians, and performed voyages by fea to this part of the globe? How far did the Norwegians extend their difcoveries in America, particularly to the fouthward? What conclufions may be deduced on thefe points, either from decifive reafons or fimple conjectures, from the writings and monuments which ftill fubfift, fuch as forts, buildings, languages and traditions?

II. *Mathematics.* To find the function of all the quantities, which ferve conjointly to determine the magnitude of the calorific effect of every combuftible material in common ufe;

ufe; fuch as wood, turf, and pitcoal, whatever may be their peculiar characters in other refpects. The required equation muft be determined for at leaft four different cafes:

Cafe 1. Where the wood, turf, or coal is burned in a furnace to heat a certain volume of air, as for example, that of a chamber.

Cafe 2. Where the heat is employed in producing the ftate of ebullition, in a fluid proper to boil certain fubftances plunged in it.

Cafe 3. Where the heat is employed to harden foft matters, fuch as clay for tiles.

Cafe 4. And where certain hard or confiftant matters, fuch as metals, are required to be liquified by heat in a furnace or forge. Each of thefe equations grounded on various experiments muft be found and eftablifhed analytically, fo that it may be poffible to determine with precifion the ratio of the calorific effect, and confequently the degree of utility in economical applications of every kind of wood, turf, or pitcoal.

III. *Natural Philofophy.*—To find by experiment the greateft degree of heat which water in the ftate of vapour can communicate to other bodies; and to anfwer the queftion, whether that part of the water in Papin's digefter, which is not converted into fteam, can acquire a more elevated temperature than 212° of Fahrenheit.

IV. *Philofophy.*—What are the moft remarkable degrees, through which practical philofophy has paffed from the time in which it was firft treated fyftematically to the prefent time.

The memoirs in anfwer to thefe queftions are required to be written in Latin, Danifh, or German, and fent before the end of June 1799, to the Secretary of the Society, Profeffor Abilgaard.

BERLIN,

The Phyfical Society at Berlin has announced the following Queftion, for a Prize of 20 Holland Ducats. (9l. 5s.)

ADMITTING that Electricity is a neceffary Agent for the formation of Hail, are there any grounds for concluding, that the Electrical Cloud can be rendered incapable of generating Hail, as Lightning is prevented by Conductors? What are the means to be adopted for this purpofe, and the facts or obfervations in general upon the fubject which deferve notice?

Proceedings of the Affociation for promoting the Difcovery in the interior Parts of Africa, &c.

Abftract of Mr. Park's Travels. [Continued from page 283.]

INFORMATION of a confiderable river flowing through the centre of Africa, between the latitude of 15° and 20° north, had been received at very early periods from different quarters, which at different times was fuppofed to be part of the Senegal and of the Gambia. Further enquiries, however, though they confirm the ancient accounts, fhewed that this river was not only of greater magnitude than either the Senegal or Gambia, but flowed in a contrary direction, running not to the weftward into the Atlantic, but from weft to eaft, to regions unknown. The Moors defcribed it by the name of Nil il Abeed, or the River of Slaves: the Negroes beftowed upon it the appellation of Joliba, or the Great Waters. Something, however, of doubt ftill remained, particularly with regard to its

eaftern

eastern direction, which was not received by geographers without difficulty and hesitation. Mr. Park's testimony is decisive, not only that the two names denote the same stream, but also that it flows from west to east, which he ascertained by a perilous ambulation of some hundred miles along its bank. All question respecting the source, the existence, and the direction of this great river is therefore obviated, but its termination still remains unknown.

The city of Sego, at which Mr. Park now arrived, consists of four divisions or quarters, two on each side of the water. The houses are built of clay with flat roofs: some of them have two stories, and many are white-washed. Moorish Mosques are seen in every quarter. These objects, with the numerous boats on the river, a crowded population, and the cultivated state of the surrounding country, formed altogether a prospect of civilization and magnificence, which our traveller little expected to find in the bosom of Africa. From the best enquiries he could make, he had reason to believe that Sego contained thirty thousand inhabitants.

The boats on the Niger are of a singular construction, each being formed of the trunk of two large trees, hollowed out and joined together endways, the junction being exactly across the middle of the boat. They are, therefore, very long and narrow, and have neither decks nor masts. Mr. Park proceeded to the ferry, in order to cross to the King's residence, which was on the other side; but before he could obtain a passage, the King had sent to enquire concerning the object of his journey. Mr. Park gave the best answer he could, adding, that he had been robbed of all he possessed, and implored the King's bounty and protection. The messenger told him to go to a distant village, which he pointed out, and wait for the King's further orders. He found the inhabitants either afraid or unwilling to give him lodging or entertainment, and having turned his horse loose, he sought shelter from a storm of thunder and rain under a tree. At length as night approached, that kindness and humanity inherent in the female sex, to which he had often been indebted on former occasions, came to his relief on the present. A poor Negro woman returning from the labours of the field, observed that he was wet, weary, and dejected; and taking up his saddle and bridle told him to follow her. She led him to her cottage, lighted up a lamp, procured him an excellent supper of fish, and plenty of corn for his horse; after which, she spread a mat upon the floor, and said he might remain there for the night. For this well-timed bounty our traveller presented her with two of the four brass buttons which remained on his waistcoat.

Mr. Park adds other particulars concerning his benefactress, which heighten the picture. He relates, that the good woman having performed the rites of hospitality towards himself, called in the female part of the family, and made them spin cotton for a great part of the night. They lightened their labours by songs: one of which must have been composed extempore, for our traveller was himself the subject of it; and the air was, in his opinion, the sweetest and most plaintive he ever heard. The words, as may be expected, were simple; and may be literally translated as follows: "The winds roared, and the rain fell. The poor white "man faint and weary, came and sat under our tree. He has no mother to bring him milk, "no wife to grind his corn."—Chorus—"Let us pity the white man, no mother has he, "&c. &c." Simple as these words are, they are natural and affecting; and contain a curious allusion to the state of manners in savage life, in which the women perform all the domestic duties.

He

He continued all the next day in the village without receiving any orders from the king, and found himfelf the object of univerfal obfervation and enquiry. He foon heard enough to convince him that the fufpicions of the Moors and flave-traders refiding in Sego were very inimical to him; and that many confultations had been held with the king concerning his reception and difpofal. On the third day however the meffenger arrived with a prefent of five thoufand kowries, to enable him to purchafe provifions in his journey from Sego, from the vicinage of which he was commanded to depart immediately. From various circumftances, it feemed probable that the king would have admitted Mr. Park into his prefence, if he could have protected him againft the malice and fanaticifm of the Moors, and if the ftory of Mr. Park had been more comprehenfible by men, who could not conceive how the defire of extending knowledge could have induced him to travel through fuch an extent of country, without other views of a more objectionable nature. From other enquiries it was alfo rendered certain, that our traveller would have expofed himfelf to certain deftruction from the Moors, if he had ventured to proceed much further to the weftward without the protection of fome leading man among them, which he had no means of procuring. Notwithftanding thefe difcouraging circumftances, he determined to endeavour to penetrate further along the banks of the Niger. The firft town of note at which he arrived after leaving Sego, was called Kabba. This town is fituated in the midft of a beautiful and highly cultivated country, bearing a greater refemblance to the centre of England than Mr. Park could have fuppofed to have been in the middle of Africa, and the feafon was that of the fhea harveft, or gathering in the fruit which produces the fhea-toulou or tree-butter, the great abundance of which in this quarter was aftonifhing. The growth and preparation of this commodity feem to be among the firft objects of African induftry, in this and the neighbouring ftates, and it conftitutes a main article of their inland commerce. From Kabba, Mr. Park and his guide proceeded to Sanfanding, where notwithftanding his endeavours to avoid the notice of the Moors, he underwent their infults for a confiderable time.

Leaving this place early in the morning, he proceeded to a town called Nyara, and thence to Modiboo, a delightful place on the banks of the river, which is here very broad and enlivened with many fmall and verdant iflands, all of them ftocked with cattle, and crowded with villages. Here he was again compelled to fet off abruptly for fear of the Moors. The limit of his expedition to the weftward was Silla, a large town on the fouthern fide of the Niger, in 1° 24′ Weft of Greenwich, and latitude 14° 48′ North, where, from the aggregate of unfavourable circumftances, it became abfolutely neceffary for him to return.

The town of Silla, whence Mr. Park began his journey homeward, is within two fhort days journey of Jenné, which is fituated on an ifland in the river. At the diftance of two days more, the river empties itfelf into a confiderable lake called Dibbie, (or the dark lake) concerning the extent of which, all the information which could be obtained was, that, in croffing it from weft to eaft, the canoes lofe fight of land one whole day. From this lake the water iffues in many different ftreams, which terminate in two large branches, one whereof flows towards the north-eaft, the other to the eaft; but they join again at Kabra, one day's journey to the fouthward of Tombuctoo, and the port, or fhipping place, of that city. The tract of land which the two ftreams encircle, is called Jinbala, and is inhabited by negroes; and the whole diftance by land from Jenné to Tombuctoo, is twelve days journey.

From Kabra, at the diftance of eleven days journey down the ftream, the river paffes to

the

the ſouthward of Houſſa, which is two days journey diſtant from the river; and ſo far our information ſeems to be authentic. Of the further progreſs of this great river and its final exit—whether it be the ſame which paſſes by Kaſſina *—whether (as ancient charts ſeem to indicate) it ſpreads into one or more inland lakes; or, at an immenſe diſtance, intermixes with the waters of the Egyptian Nile—theſe are queſtions which future diſcovery can alone reſolve. On each of theſe points enquiry of the natives was not neglected, but ſatisfactory and certain information could not be obtained.

Such is the intelligence that was collected by Mr. Park concerning the courſe of the Niger from its leaving Sego, where he firſt diſcovered it. Of the chief towns of Jenné, Tombuctoo, and Houſſa, ſituated on its banks, the laſt was ſaid to be the moſt conſiderable, and the leaſt of them of far greater magnitude than Sego. But there is a place between Jenné and Tombuctoo deſerving notice, as containing a very conſiderable pottery: it is called Downie; and the earthen ware, which Mr. Park frequently met with, appeared to be of extraordinary good conſiſtency, but not glazed. He was told that caravans frequently arrive both at Tombuctoo and Houſſa, from the countries on the Mediterranean, travelling acroſs the Deſert, by the way of Fezzan, with European goods and other merchandize. By one of theſe the news was conveyed to the centre of Africa of the capture, by the French, of the Mediterranean convoy, in October 1795. Mr. Park received this information from a Moor who had come from Fezzan.

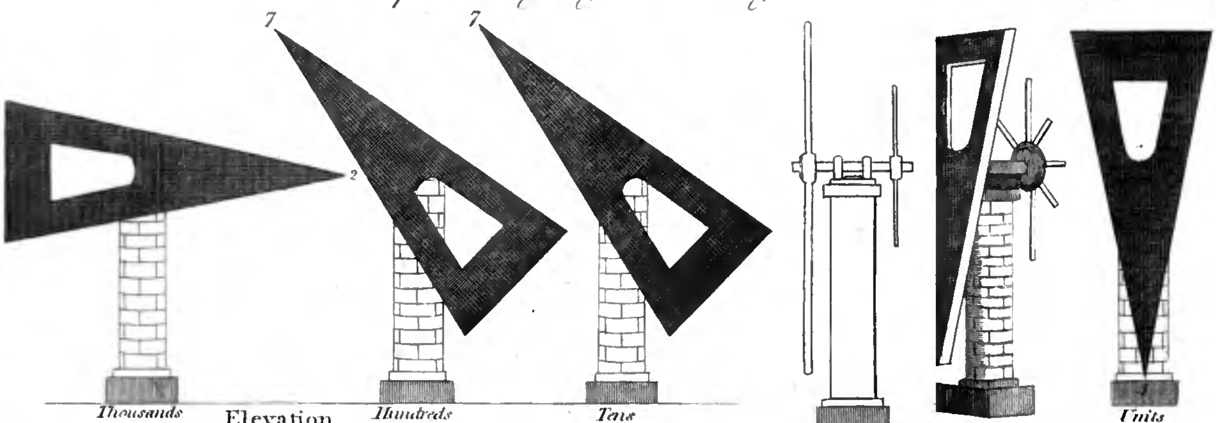
On his return back, Mr. Park learned that the ſovereign of the country had given orders to ſeize him, for which reaſon he avoided the town of Sego. His courſe, in aſcending the river, was to the ſouth-weſt; and his ſubſiſtence, in travelling through the territories of the benevolent Negroes, was in a great meaſure afforded him by the Dooty, or chief man of the towns through which he paſſed, it being, highly to the credit of the African police, part of the duty of this officer to provide food for the neceſſitous traveller. On many occaſions he offered to pay for what he received out of the kowries, which had been preſented him by the king, and his offer was ſometimes accepted and ſometimes refuſed. On other occaſions he rewarded his hoſt in a manner which, from its ſingularity, deſerves to be noticed. Among the various impoſtures practiſed by the Moors towards the poor Negroes, they frequently ſell them ſcraps of paper with an Arabic inſcription (commonly a paſſage from the Koran) which are called *ſaphies* or charms. With one of theſe about his perſon, the fond poſſeſſor conceives himſelf invulnerable: neither the lurking ſerpent nor the prowling tiger is any longer the object of his dread.

In the circumſtances to which Mr. Park was reduced, he had the good fortune to diſcover that the negro natives aſcribed to him the power of granting ſaphies of more than Arabic virtue. "If a Moor's ſaphie is good (ſays the Dooty of Sanfanding), a white man's muſt needs be better;" and Park, at his requeſt, gave him one poſſeſſed of all the virtues he could concentrate, for it contained the Lord's Prayer. The pen with which it was written, was made of reed; a little charcoal and gum-water made very tolerable ink, and a thin board anſwered the purpoſe of paper. In his journey weſtward, this merchandize turned to extraordinary good account; and it is ſurely needleſs (ſays Mr. Edwards) for Mr. Park to frame any apology for having availed himſelf of ſuch a reſource in his ſituation.

* Erroneouſly ſpelt in the former memoirs, Caſhna.

[*To be continued.*]

Art of conveying Intelligence.



Elevation. Section. Perspective.

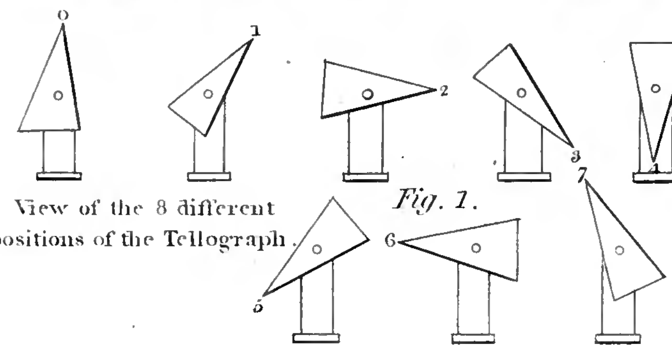
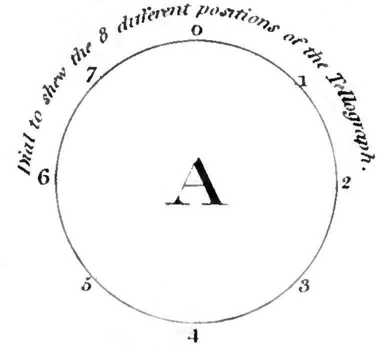
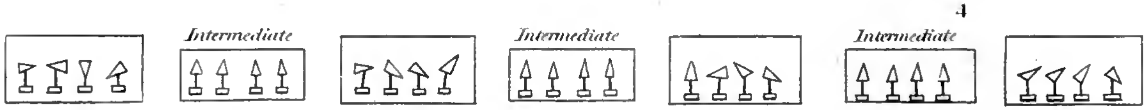


Fig. 1.

View of the 8 different positions of the Tellograph.





Example of a line of Six words described by the Tellograph.

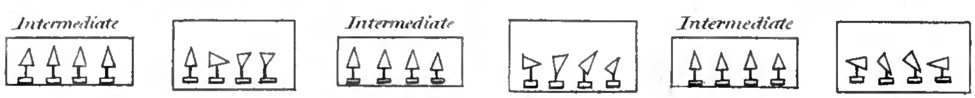
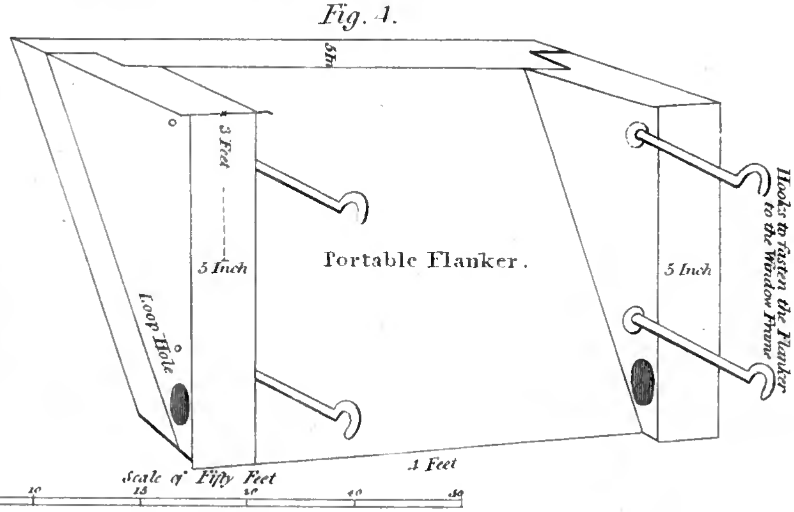


Fig. 3.

Fig. 4.



Barlow sculp

Portable Tellograph.

Philos. Journal Vol. II. Pl. XIV. facing p. 334.

Fig. 1. Front Elevation.

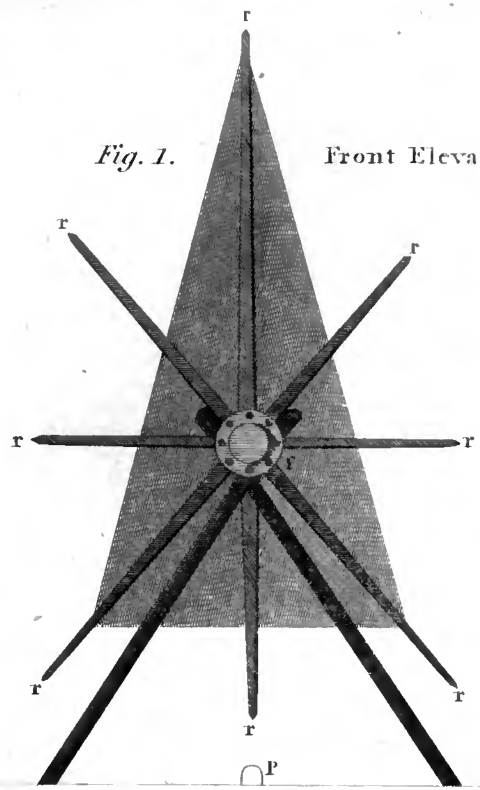


Fig. 2. Profile.

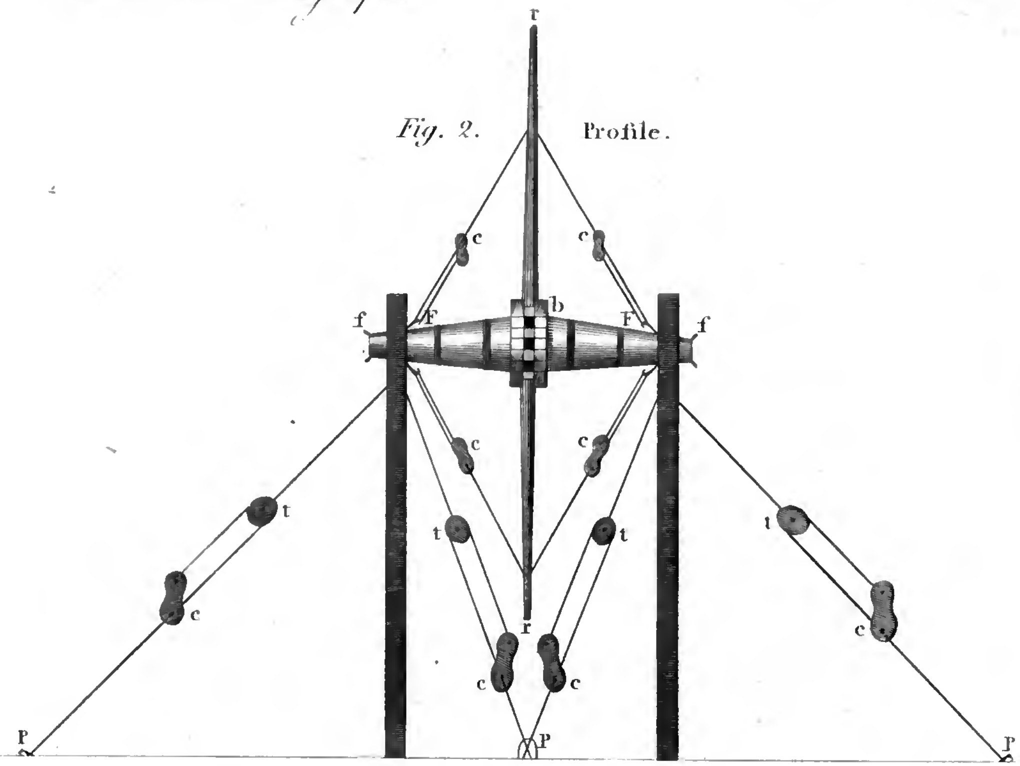


Fig. 3. Stand.

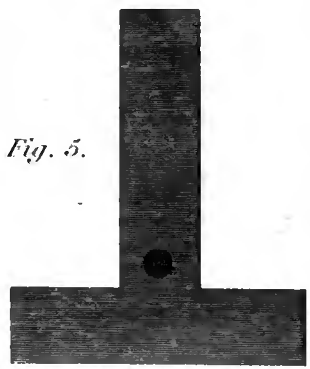


Fig. 5.

Index of a different Form.

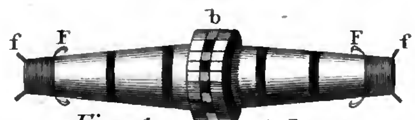


Fig. 4. Axletree.

Fig. 6.  Holder.

Fig. 7.  Flanch.

Fig. 8.

Index of a different Form.

Barlow sculp.

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

NOVEMBER 1798.

ARTICLE I.

An Account of three different Kinds of Timber-Trees, which are likely to prove a great Acquisition to this Kingdom, both in point of Profit and as Trees for Ornament and Shade. By CHARLES WHITE, *Esq. F.R.S.* *

IN making a collection of fuch hardy trees and fhrubs as would grow and even flourifh in the open air, at Sale, in the county of Chefter, I foon obferved that there were three foreft trees, of different genera, which grew much fafter than the others in the fame foil and fituation, viz. the Black American Birch with broad leaves, the Athenian Poplar, and the Iron Oak with prickly cups.

The Broad leaved American Black Birch, Betula nigra Linn. Spec. Plant. 1394, is defcribed by Mr. Aiton in his Hortus Kewenfis: *B. foliis rhombeo-ovatis, duplicato-ferratis, acutis, fubtus pubefcentibus, bafi integris; ftrobilorum fquamis villofis; laciniis linearibus, æqualibus.* It is a native of Virginia and Canada, and was firft introduced into England (where it grows in the greateft luxuriancy, and perfects its feeds) by Peter Collinfon, Efq. in the year 1736. There is no doubt, therefore, that it will foon become very plentiful and cheap. It is very defirable in pleafure-grounds, as it is the firft foreft tree in the fpring which prefents us with its leaves; thefe are of a light and lively green. Its bark, which is white, makes at all times a beautiful variety when intermixed with other trees. It is faid to be the moft ufeful timber tree in North America for building both of houfes and boats, and will grow faft in any foil or fituation, whether wet or dry.

* Manchefter Memoirs, vol. v. part 1.

Miller, ſpeaking of trees of this deſcription, ſays "that they may be propagated by ſeeds, in the ſame manner as the common birch-tree, and are equally hardy. Some of the trees now begin to produce their catkins in England, ſo that we may hope to have plenty of their ſeeds of our own growth, for at preſent we are ſupplied with them from America. As theſe grow more vigorouſly than the common ſort, and thrive on the moſt barren ground, they may be cultivated to great advantage in England; for their wood is much eſteemed in Canada, where the trees grow to a large ſize: and they are by no means an unſightly tree in parks; for their ſtems are ſtraight, the bark ſmooth, and their leaves are much larger than thoſe of the common birch; ſo may be planted in ſuch places where few other trees will thrive."

Mr. Hanbury ſays: "The black Virginian birch, being of foreign growth, is propagated for wilderneſs and ornamental plantations; but as it begins now to become pretty common, it is to be hoped it will ſoon make a figure among our foreſt trees, it being equally hardy with our common birch, and will arrive at a much greater magnitude. This ſpecies will grow to be upwards of ſixty feet in height. The branches are ſpotted, and more ſparingly ſet on the trees than the common ſorts. The leaves are broader, grow on long footſtalks, and add a dignity to the appearance of the tree; and as it is naturally of upright and ſwift growth, and arrives at ſo great a magnitude in few years, prudence will direct us to let it have a ſhare among our foreſt trees, to plant them for ſtandards in open places, as well as to let them join with other trees of their own growth, in plantations more immediately deſigned for relaxation and pleaſure." I planted one of theſe trees nineteen years ago, and it is now forty-five feet ſix inches in height, and three feet ſeven inches in the girth.

The Athenian Poplar Tree, Populus (Græca) foliis cordatis, glabris, baſi glanduloſis, remote crenatis; petiolis compreſſis; ramis teretibus. The Athenian poplar is a native of the iſlands of the Archipelago, and was firſt cultivated in England by Hugh duke of Northumberland in the year 1779. Perhaps there is no deciduous tree ſo beautiful, or ſo proper for pleaſure-grounds intended for ornament and ſhade, as this poplar; having a fine upright ſtem; the branches well diſpoſed; the bark ſmooth, and of a ſilvery hue, reſembling ſatin-wood. The leaves, which are of a light green, are produced very early in the ſpring, and are retained on the tree longer than on any deciduous tree in this country, not falling off till late in the autumn: they are never blighted nor infeſted with inſects, nor does it loſe a leaf during the whole ſummer. Though the poplar is generally termed an aquatic, this will grow in any ſoil or ſituation; and is of quicker growth in dry upland than any tree we are acquainted with in this climate, though not quite of ſuch quick growth as the Huntingdonſhire willow in rich moiſt meadow land. In ſuch a ſituation, I have fallen a Huntingdonſhire willow*, from which I made a ſtaircaſe when it was only of nineteen years growth, from a cutting. The Athenian poplar is propagated with the greateſt advantage by ſuckers and layers; but it is with great difficulty raiſed from cuttings or truncheons. The common way of raiſing them, amongſt the nurſerymen, is, by engrafting them on ſome other poplar: but the trees thus raiſed are of little value, being very ſlow in their growth; and it is owing to this circumſtance, perhaps, that their real

* I cannot find that this ſpecies of willow has been deſcribed by any botanical writer; but it is well known among the nurſerymen by this name.

worth

worth has not before been difcovered. About twelve years ago I purchafed two plants of this poplar, from two different nurferymen in London, at one guinea each; one of them was grafted upon a different kind of poplar, the other was upon its own roots. I placed them near together, in a dry fituation, in a light foil, underneath which was a ftratum of gravel. The grafted one made very little progrefs; I therefore converted it into a ftool, and raifed feveral plants from it. The other, which is upon its own roots, has made a rapid progrefs, being at leaft fifty-one feet high, and two feet nine inches in the girth. It produces annually a great number of fuckers, with which I have fupplied many of my friends*.

The third is *The Iron, Wainfcot,* or *Turkey Oak,* fo called by Mr. Luccomb. I have long been in doubt what fpecies of oak this really was; but one of mine having borne fome acorns this year, has afcertained it to be a variety of the *quercus cerris;* and it appears to me to be either a *nondefcript,* or what Mr. Aiton, in his Hortus Kewenfis, calls *frondofa: foliis ovato-oblongis, leviter finuatis, planiufculis:* commonly, Turkey oak tree. It is what Mr. Luccomb generally grafts his Luccomb oaks upon; and the plants certainly grow fafter when grafted upon this oak than upon any other. About twenty years ago, in making a collection of oaks, I received feveral from Mr. Luccomb, both of the iron and the Luccomb oak; but I foon found that the iron oak overgrew all the others, and was equally ornamental as the Englifh oak. From a branch which I have fawed off, the wood appears to be as hard and as ponderous as the Englifh oak.

The following is an account of the fize and age of fome Iron, Luccomb, and Englifh Oaks, growing in my collection at Sale:

	Height.		Girth.	
	Feet.	Inches.	Feet.	Inches.
An iron oak, 20 years old — ——	36	0	3	3
Another, of the fame age — ——	37	0	3	0
A Luccomb oak, of the fame age, grafted on an Englifh oak — ——	32	2	2	5
An Englifh oak, of the fame age ——	28	0	2	6
Another, 40 years old — ——	39	0	2	10
Another, 56 years old — ——	54	0	3	4

The following is a Copy of a Letter from Mr. Luccomb to Mr. Babington, dated Newbridge, Exeter, September 17, 1795:

"All I can fay of them (the iron oaks) is, that my father had a few of them as a prefent from William Ball, Efq. of Manhead-houfe (now lord Lefburne's, near Chudleigh, Devonfhire), about fifty years fince, by the name of the iron or wainfcot oak, which Mr. Ball received from Turkey by one of his own fhips trading there. They are the fame fort which you have noticed at Hillerfdon, as my father fold fome of them to Mr. Creroy about forty years fince. They have, as you obferve, a very jagged leaf, and the cup of the acorn is rough like a bur. They are not evergreen."

* There is another poplar of very fwift growth, which makes a very handfome tree, and will flourifh in any fituation or foil. It is the *Populus cordifolia canadenfis,* or Berry-bearing Poplar, as it is commonly called. This tree will grow freely from cuttings.

The following is a Letter I received from my worthy friend the Rev. Thomas Gisborne, author of several useful publications :

" Dear Sir,

" Yoxall Lodge, Oct. 20, 1795.

" I have this evening received a letter from my brother-in-law, Mr. Babington, respecting the measurements of the iron oaks at Hillersdon, near Cullompton, where he now is : I subjoin what he says on the subject, and have pleasure in finding the result so honourable to the tree which you recommend.

" I am, Sir, &c.

" Thomas Gisborne.

" ———To-day I have measured some of the oaks about three feet and a half from the ground; and give you the result, which I thought would be fair and satisfactory, in the following way :——

" No. of English oaks.			Circumference. Feet.	Inches.
8	———	———	31	1
9	———	———	30	1
9	———	———	32	8
26		26)	93	10
	Average circumference		3	7½

" No. of iron oaks.			Circumference. Feet.	Inches.
6	———	———	31	6
6	———	———	32	10
6	———	———	31	9
18		18)	96	1
	Average circumference		5	4

" As circles are as the squares of their circumferences, pieces of the buts, at this height, a foot long, would be to each other as 1877 : 4096. Now, supposing the iron oaks to carry their buts as much higher than the others as their substance below would lead us to expect (and they seem, in fact, to do this or more), there must be four or five times as much wood in them as there is in the English oaks. An old labourer here informs me, that all were planted at the same time, between forty and fifty years since. They stand in rows, ten feet asunder, and the trees are twenty feet from each other in each row. I measured such trees as first presented themselves, with the exception of one or two which seemed unhealthy. They are on a steep bank and a gravelly soil. The trunks of the iron oaks are covered with a lighter moss, on the whole, than fixes itself on the English oak ; but make a fine appearance. I measured many outside trees, and observed that the iron oak seemed to have as great a superiority over the other, in this situation, as it has when surrounded by neighbours. As to the height, the iron oaks very generally out-top the others, and are the master trees ; but you know that in a plantation a slender tree will often be nearly as tall as its sturdy neighbour.

" P. S.

“ P. S. On looking over the oaks again, I think the iron oaks carry up the thickneſs of their buts a good deal better, *cæteris paribus*, than the others ; and therefore they have five or ſix times the quantity of wood in them.

“ There are but two beeches, and they are both outſide trees, and therefore larger than they otherwiſe would be. The circumference of the two was 12 feet 4 inches ; average, 6 feet 2 inches.

“ Spaniſh cheſnuts, No.			Circumference. Feet.	Inches.
6	——	——	31	3
8	——	——	34	3
4	——	——	24	3
18	——	18)	89	9
	Average circumference		4	11$\frac{3}{4}$ nearly.”

N. B. Mr. Babington ſays, “ that there are gates and pales on the premiſes at Hillerſdon, which have been made of the iron oak ; and that, as far as he can judge, the wood appears as hard and as tough as that of the common oak.”

It has always been conſidered, that when men have planted oak, they have not planted for themſelves, or for their children, but for diſtant poſterity ; and even *they* could never be repaid where land bore any annual value : and to the planter himſelf little pleaſure could ariſe from trees of ſuch very ſlow growth. But the ſame perſon who plants the iron oak may poſſibly live to reap ſome little profit as well as pleaſure ; and it is not at all unreaſonable to ſuppoſe his immediate ſucceſſor may ſee it arrive to ſome degree of perfection. From what I have ſeen of the wood of this oak, and from the account given by Mr. Babington of the gates and pales made with it, there is great reaſon to ſuppoſe it will be equally uſeful as the Engliſh oak for any purpoſe whatever.

The general decreaſe of timber in this iſland, the many waſte lands unemployed, and the bill now propoſed to be brought into Parliament by that great friend to agriculture Sir John Sinclair, will be my apology for troubling the Society with this paper ; for the planter ought certainly to be furniſhed with every advantage, and every poſſible inducement ſhould be held out to him for promoting ſo uſeful and ſo national a work.

Explanation of Plate XV. fig. 3.

A.a. a.a. Leaf, acorn, and prickly cup of the Iron, Wainſcot, or Turkey Oak.

B.B. b.b. Leaf, acorn, and prickly cup of the *Quercus Cerris of Linnæus, fol. oblongis, lyrato-pinnatifidis ; laciniis tranſverſis, acutis, ſubtus ſubtomentoſis ; calyce hiſpido ; glande minore.* Small-acorned Spaniſh Oak with prickly cups.

II.

Abſtract of a Memoir of M. PROUST *on the Tanning Principle*. By Citizen* DESCOTILS.

A SOLUTION of muriate of tin, at any degree of oxydation whatever, being poured into a decoction of nut-gall, forms an abundant yellowiſh precipitate. After having di-

* Annales de Chimie, xxv. 225.

luted

luted this mixture with a quantity of water sufficient to separate the last portions of precipitate which the acids may hold in solution, it is filtered, and the liquor contains the acids of galls, muriatic acid, and muriate of tin. The precipitate is a combination of the tanning principle, and the oxyde of tin. It cannot contain the gallic acid, because this acid is not precipitated by tin. The solutions of lead, on the contrary, precipitate the gallic acid, as well as the tanning principle; and on this account it was that Scheele could not separate the acid of galls from the principle of astringency.

In order to obtain the acid of galls, the tin must first be separated by sulphurated hydrogenous gas. The sulphurated oxyde falls down in the form of a brown powder. After having left the fluid exposed to the sun, with a covering of paper, in order that the excess of sulphurated hydrogen gas may be dissipated or entirely decomposed, it is to be filtered, and then evaporated, in a vessel of silver, to the requisite point for the crystallization of the gallic acid by cooling. The crystals being then thrown on a filter must be washed with a small quantity of cold water. The remaining fluid is to be treated by evaporation, and cooling in the same manner, in order to deprive it as much as possible of gallic acid. At the end of the operation, it is necessary to use capsules of glass, because the muriatic acid becomes concentrated. The earths which some chemists have pretended to exist in the decoction of nut-galls ought to be found in this residue. Mr. Proust met with none; but he proposes to examine the residue with more particular attention to this point.

To separate the tanning principle from its combination with the oxyde of tin, this last powder is diffused in a quantity of water, through which a current of sulphurated hydrogenous gas is passed. The sulphurated oxyde falls down, and the pure tanning principle at the same time becomes dissolved in the water. After filtration, the solution is evaporated in a silver bason, in which it acquires the deep colour of a decoction of nut-galls, and its peculiar smell, which is one of the distinctive characters of the astringent principle. Its taste is exceedingly acerb, and rather bitter, without being disagreeable. It lathers like soap-water, without feeling unctuous to the touch. It becomes turbid by cooling, and lets fall a light brown powder, which is dissolved again by heat. It resembles the last decoctions of nut-galls, which do not become mouldy, and are thought by Mr. Proust to contain the tanning principle nearly in a state of purity.

The substance which remains after its evaporation is dry, brown, friable, breaks with a vitreous fracture, like aloes, and does not attract the humidity of the air. Its taste is very rough and harsh: it is totally soluble in hot water, and still more readily in alcohol.

All the acids precipitate the tanning principle from its aqueous solution, by uniting with it. As this fact belongs to the analysis of galls, which Mr. Proust has made by means of the sulphuric and muriatic acids, he promises to treat of it again in the second part of his work.

The aqueous solution of the tanning principle, poured into a solution of glue, immediately converts it into a magma, which possesses the elastic properties of the gluten of wheat. This substance, when left to dry, contracts in its dimensions, and, when perfectly dry, has the appearance of a brown mass, vitreous in its fracture, not capable of putrefaction, perfectly insoluble in water, yielding little to alcohol, and capable of resuming its elastic property by softening it in hot water. It is, in a word, says the author, the combination discovered by Seguin, the preserving principle of tanned leather. It is also obtained with the solution of the nut-gall.

Albuminous

Albuminous liquors are precipitated by the tanning folution, but the refult is not a magma fufceptible of the fame concentration.

The green fulphate of iron is no more altered by the tanning principle than by the acid of galls *.

The red fulphate, on the contrary, is precipitated in a fomewhat tarnifhed blue depofition, which is abundant, coarfe, and of a black colour when dry. If the precipitate which this fulphate affords with the gallic acid be compared with this, it will be feen that they greatly differ. The former is of an extreme fubtlety, and remains long fufpended in water: it is perfectly black, &c.

The gallate of iron is foluble in acids; the tannate of iron is decompofed by thofe falts. It abandons its iron, and the tanning part falls down.

If the red fulphate be poured, rather in excefs, into a folution of the tanning principle, the difengaged fulphuric acid re-diffolves the precipitate, and affords a black fluid in its greateft intenfity, which is blue when much diluted. To feparate the precipitate, without depriving it of the tanning principle, it is neceffary that the excefs of acid in the fluid fhould be gradually faturated by pot-afh. With a little attention, the operator may fucceed in rendering the liquor colourlefs, without affecting the fuperabundant portion of fulphate; or otherwife, if a fmall portion of oxyde of iron has been thrown down along with the black precipitate, which may be known by the ruft which is formed upon the filter, a few drops of acid will re diffolve it.

But it is a remarkable fact, that all the red fulphate remaining in the fluid is brought back to the ftate of green fulphate. The quantity of oxygen which conftitutes the difference between the one and the other, is feized by a portion of the tanning principle. The latter thus oxyded, and by that means rendered incapable of precipitating the red fulphate, remains in folution. This oxydation of the tanning principle is directly and eafily obtained by the oxygenated muriatic acid. The folution of the tanning principle, inftead of becoming clearer, acquires a deeper colour, and lofes all its peculiar characters in its tranfition to a new ftate, which Mr. Prouft has not yet examined†.

The gallic acid undergoes the fame alterations by the oxygenated muriatic acid, and, like the folution of the tanning matter, becomes incapable of precipitating the red fulphate. This is the effect which age produces upon ink. The gallic acid is deftroyed, the red oxyde remains, and may eafily be rendered blue by the method of Blagden.

As the aftringent juices cannot afford indeftructible inks, we muft therefore return to the method of the antients, who ufed coal for this purpofe. Perhaps, fays Mr. Prouft, there is no fubftance fo proper as Spanifh chalk. This ftone, which is neither bituminous nor ampelite, is found with the amianthus, and is compofed of alumine with fix or feven per cent. of pure charcoal.

The fame phenomenon of the oxydation of the blackening principle at the expence of the red fulphate, is alfo obferved in black dyes made with fumac and this metallic falt. Mr. Prouft had occafion to obferve it in a bath of this kind, which no longer added to the black colour of the piece goods. The liquor was greenifh; the red fulphate, or the oxygenated muriatic

* On this fubject, fee the interefting paper of this author, in our Journal, I. 453. N.

† See the obfervations of Seguin, Philof. Journal, I. 275.

acid,

acid, immediately formed black. Hence he concluded that the gallic acid, or the aſtringent principle and the green ſulphate, exiſted together in the bath. The red ſulphate which was poured in, combined with the vegetable principle; the oxygenated muriatic acid, by converting the green ſulphate into red, gave it the facility of combining and affording black with the ſame principle.

From theſe facts it follows, that, in the proceſs of dyeing black, a portion of the blackening principle, whether it be the tanning matter or the acid of galls, is deſtroyed by oxydation; that a certain period arrives at which the bath can no longer afford the black dye, unleſs, by expoſure to the air, the iron can reſume the quantity of oxygen neceſſary to bring it to the ſtate of red oxyde; and laſtly, that dyers would conſiderably accelerate their work by uſing the red ſulphate. It would only be neceſſary in that caſe to provide againſt the exceſs of ſulphuric acid, which always is found in the ſulphate of iron when it paſſes from green to red. With regard to the uſe of logwood, verdigris, &c. Mr. Prouſt is convinced that theſe ſubſtances are not neceſſary, and that the moſt beautiful blacks may be obtained by aſtringents alone with iron.

III.

Notice of a Memoir of Citizen GUYTON, upon the Tables of the Compoſition of Salts, and the Means of verifying the Proportions indicated by thoſe Tables.*

THE little agreement between the tables of Bergman, Wenzel, and Kirwan ſhews all the difficulty of determining with exactneſs the proportions of the component parts of ſalts. A method of verifying ſuch tables would therefore be uſeful to the progreſs of ſcience. The following appears ſo ſimple and appropriate, that it is aſtoniſhing it has not yet been attempted. It conſiſts in a compariſon of the reſults of experiment and computation, with regard to the agreement of the very perceptible effect ariſing from the exceſs or deficiency of one of the ſubſtances after mutual decompoſition.

For example, according to Bergman:

Sulphate of Barytes contains	Acid	13.
	Barytes	84.
Sulphate of Soda contains	Acid	28.
	Soda	16.
Muriate of Barytes contains	Acid	23,57
	Barytes	76,43
Muriate of Soda contains	Acid	52.
	Soda	42.

Neglecting the water, which, though of ſome conſequence in the effect, is of none in the reſult; we ſee that, in the caſe of an exchange of baſes between two ſalts, the reſult of the mixture muſt either be neutral, or poſſeſs an exceſs of acid, or an exceſs of the baſe; and that, by rendering the decompoſition of one of the ſalts complete, we ought to obtain the ſame reſult from calculation as that which ſhall be afforded by experiment.

* Read to the National Inſtitute of France, the 16th Prairial, in the year V. This notice is tranſlated from the Annales de Chimie, xxv. 292.

Now,

Now, if by the fulphate of foda we decompofe a quantity of muriate of barytes containing, according to the above table, 25,906 of muriatic acid, there ought to remain difengaged, or in excefs, 16,710;—neverthelefs the mixture remains neuter.

The fluid remains alfo neuter, if we ceafe to add the folution of fulphate of foda when the precipitate no longer falls down. The computation fhews, that in a hundred parts of the mixture of thefe two falts, in which the muriatic acid would be reprefented by 20, there ought to remain 12 unfaturated, or in excefs.

Let us apply the fame method of verification to the proportions determined by Kirwan in the new tables in the fecond edition of his Differtation.

It is known, that an exchange of bafes takes place between fulphate of foda and muriate of magnefia. The computation eftablifhed on the proportions indicated by the tables fo rigoroufly affumed from the real acids, always fhews a notable excefs of acid, either muriatic or fulphuric, accordingly as the quantity of one or the other of thefe falts is increafed in the mixture.

The doubts entertained by certain chemifts on the refpective decompofition of thefe two falts, have induced me to apply the fame calculation to the inverfe operation, that is to fay, by mixing the folutions of fulphate of magnefia and muriate of foda; and in this cafe alfo the refults do not agree.

Laftly, the method has been tried on a cafe of affinity, in which the decompofition immediately fhews itfelf by unequivocal figns; that is to fay, in the mixture of a folution of fulphate of potafh and nitrate of lime.

According to the laft data of Kirwan, the proportions of the component parts of thefe two falts, and the two others which ought to be formed, are as follows:

Sulphate of Potafh	Acid	100
	Potafh	108,7
Sulphate of Lime	Acid	100
	Lime	80,6
Nitrate of Potafh	Acid	100
	Potafh	83,33
Nitrate of Lime	Acid	100
	Lime	34,4

It is eafy to form the mixture of a quantity of fulphate of potafh, of which the acid fhall be reprefented by the number 100, together with fo much of the nitrate of lime that there fhall be more than fufficient to caufe the whole of the fulphuric acid to pafs into a new combination. It is evident that, to effect this, 80,6 of lime will be required, which will therefore be difengaged from 234,4 of nitric acid; a quantity which would require for its faturation 195,32 of potafh. But it finds only 10,87. The quantity of nitric acid remaining without any bafe, or in excefs, muft confequently be 64,87.

Such is the refult of calculation; but experiment pronounces otherwife. The fluid diluted or concentrated, and even carried to cryftallization, did not at any period prefent the leaft trace of difengaged acid.

The author declares in conclufion, that his intention is not to criticife experiments made with no lefs care than fagacity; but to offer to chemifts a problem which is interefting in many refpects, theoretical, practical, and even pharmaceutical; a problem of which the folution is no longer to be fought in the amendment of fuch errors as are unavoidable in manipulations of great delicacy; but which also leads us to consider the consequences of the affinities in saline mixtures under a new point of view, in order to discover the cause of a result so little agreeing with all the observations which have yet been made upon their composition*.

IV.

On Pasigraphy; or, the Art of Writing which shall be intelligible to all Nations†.

CITIZEN Memieux has promised to assist us with a pasigraphy, or universal writing, by means of twelve characters which may be learned in twelve hours. This extreme simplicity is entitled to the utmost attention of philosophers, and seems to indicate a very superior mind in the author. We must nevertheless enquire, what may be the nature of these characters; for their form is of no great consequence. Are they alphabetical? Or, are they a kind of hieroglyphic, each expressing an idea, like the characters of the Chinese writing?

If the question relate to an alphabet, the secret is certainly not uncommon; but, if the characters be hieroglyphical, it will be difficult to comprehend how twelve primitive ideas and their representative signs can be sufficient for every combination. In either case, there is a new language to be learned, which, it is expected, the various nations will adopt. A Portuguese receives the information that a certain character, a cross for example, is to be made use of to denote a ship; the same information is communicated to an Indian; and, when these two men meet, if they trace a cross upon their tablets, they will both comprehend that a ship is meant,—and so of the rest. Without proceeding so far from the ordinary practice, I might say both to the Portuguese and the Indian, Write the word navis, and you will equally understand each other; or, in short, I might command all the people on the face of the earth to learn Latin, or any other common language, and then acquaint them, that this qualification would enable them to understand each other.

In this manner it is, that all the nations who have adopted the Arabian cyphers can readily communicate all their ideas of numbers and their combinations, notwithstanding the difference of their languages. When a native of France sees a German write 234, both have a perception of the same object, and understand each other perfectly, though the one in oral speech would say *deux cents trente quatre*, and the other *zwey hundert vier und dreisig*. Let the German continue to write numbers, and to perform the most complicated operations, the Frenchman will follow and comprehend them without difficulty. The algebraists of all nations understand at first sight the processes written by nations possessing other idioms. The same thing happens with regard to the signs of pharmacy, the ancient chemistry, astronomy, and music. Let a number of Italians, English, French, and Germans, be collected

* At the end of this paper, the author does justice, in a note, to M. Richter, who, in his Neuern Gegenstande der Chymie, &c. makes the same observation, to which he was led by the great difference between the proportions indicated in the tables already known, and those he had determined by a new method, which he calls stachiometrique. I hope to procure this work soon. N.

† From the Spectateur du Nord, Mai 1798. The signature V****** is annexed to this paper. I have omitted the two first paragraphs, which form no essential part of the subject, and are written in a style of levity which can scarcely accord with the rest of this excellent paper.—Reference to two Memoirs on the same subject, with a few remarks, is made in our Journal, II. 189—191. N.

together

together in an orcheſtra, perfectly unacquainted with each other, let the parts of the ſame ſymphony be diſtributed among them, and they will, by performing the muſical compoſition, afford a conviction, that, in ſpite of the difference of language, the muſical paſigraphy repreſents the ſame ideas to them all.—But in theſe ſeveral caſes of arithmetic, algebra, muſic, &c. it is evident, that nothing more has happened, than that all the profeſſors have been previouſly induced to learn one common language. This conſideration cauſes the miracle to vaniſh.

As an univerſal language, the paſigraphy is therefore a mere pretenſion: but there is another point of view under which it may be conſidered; as a philoſophical language, better conſtructed than all our ancient languages, which are the products of chance, caprice, and frequently of ignorance. There is no doubt but a profound metaphyſician, meditating at leiſure in his cloſet, and modelling all his expreſſions from the type of the original ideas, might ſucceed in forming a language more ſimple, and in every reſpect ſuperior to that which we ſpeak. This would be the language which we might then adviſe all nations to adopt, if they would conſent to abandon their mother-tongue and acquire a language common to all men: but this language, perfect as it might be, would not, in its own nature, be an univerſal language. We ſhould always ſpeak erroneouſly, if we were to aſſert, that the Portugueſe and the Indian, each writing his own language but with other characters, might underſtand each other. On the contrary, we ought to ſay, that if both acquire the philoſophical language, or paſigraphy, they will underſtand each other.

The whole edifice of this pretended diſcovery is therefore reduced to the propoſal of a new language more ſimple, more accurately eſtabliſhed upon the generation of our ideas, and which, on that account, is better adapted than any other to become the univerſal language, admitting it to be poſſible to introduce ſuch a general medium.

But this propoſition is not at all new, and cannot therefore be called a diſcovery. It has already been made by ſeveral men of the firſt rank in point of ability, who have acquired nothing more than the name of ingenious inventors, without prevailing on other perſons to ſecond their efforts.

A good hiſtory of paſigraphy would not be an uſeleſs work; neither would it be very eaſy to write ſuch a treatiſe. The materials are ſcattered through a multitude of ſcarce books not eaſy to be collected.

In this hiſtory we ſhould ſee what efforts have formerly been employed to confine human language within general and determinate rules; what principles have already been laid down, and the progreſs which this art has made in a few philoſophical minds. We ſhould judge to what extent the modern paſigraph has availed himſelf of, or departed from, the ideas of his predeceſſors. We can only preſent a few unconnected portions of this hiſtory*.

It is known, that lord Bacon of Verulam comprehended nearly the whole of human knowledge; that he diſcerned almoſt every thing which was wanting in the entire ſyſtem, and foreſaw moſt of the diſcoveries which have ſince been made. He laid the foundations of an Encyclopædia, and was very near diſcovering various important philoſophical reſults

* In the *Genius der Zeit*, i. 108, for the preſent year, an article is found, to which recourſe has been had in drawing up the preſent memoir. Note of the Author.

of experiments, such as the weight of the air, &c. If we open his book on the progress of the sciences, we shall find the notion of a pasigraphy in the chapter entitled The Instrument of Discourse. "It is possible to invent such signs," says he, "for the communication of our thoughts, that people of different languages may, by this means, understand each other; and that each may read immediately, in his own language, a book which shall be written in another." But Bacon did not think of confining this to twelve characters: on the contrary, he requires a great number, at least as many as the number of radical words; on which head he quotes the example of the Chinese; "and although," adds he, "our alphabet may appear more commodious than this method of writing, the thing itself is nevertheless well deserving of attention. The problem relates to the signs by which thoughts may be rendered current; and as money may be struck of other materials as well as gold and silver, it is possible likewise to discover other signs of things as well as letters and words *."

Des Cartes, in his third letter to father Mersennus, discusses the invention of a Frenchman, whom he does not name, but who, by means of a certain language and an artificial writing, pretended to understand all the different idioms. He remarks on this subject, that it would be very possible to compose a short and convenient grammar, with general signs, which should render all foreign languages intelligible. Here are already two works on the pasigraphic art; but we shall proceed to exhibit circumstances of a more positive nature.

In the year 1661, John Joachim Becher published a Latin folio, the title of which was "Characters for the Univeral Knowledge of Languages: a Steganographic Invention hitherto unheard of" †. This unheard of invention was no other thing than what is now announced to us under the name of Pasigraphy; it is a method of making one's self understood by all foreigners by writing in one's own language, and also of comprehending what they write in theirs. It was truly at that time a thing unheard of; for Becher, being the first who had given a complete treatise on this arr, may be considered as the inventor. He begins his work by a set of very delicate and highly interesting observations upon general grammar, and the fundamental relations of all languages with regard to each other. He gives a learned comparative table of the relations and harmony of the Latin, the Greek, the Hebrew, the Arabian, the Sclavonian, the French, and the German. This work cannot be too highly esteemed, and assuredly was not unknown to the author of the work *Du monde primitif.* A Latin dictionary then follows, in which every word corresponds with one or more Arabic numeral figures arbitrarily taken. Every number is assumed as distinctive, or denoting the same word in all languages; and consequently nothing more is required than to compose a dictionary for each, similar to that which he has given for the Latin. There is likewise a table of declensions and conjugations, which presents certain determinate numbers for all the cases, moods, tenses, or persons. By means of this general disposition, when a Frenchman is desirous of writing to a German the following phrase, *La guerre est un grand mal* (war is a great evil), he seeks in his index, *guerre, être, grand, mal*; and he writes the correspondent numbers,

13, 33, 67, 68.

* I have translated from my original, instead of recurring to the book of chancellor Bacon, which I do not possess The passages marked as quotation, are not therefore the words of Bacon, but his sense. N.

† The author has not given the Latin title of this work. I have therefore translated his French title word for word. N.

The

The sentence might be understood by these four characteristic numbers; but to leave no room for ambiguity, he says, *Guerre* is the nominative case, and finds, as the characteristic of the nominative, the Arabic figure 1. *Est* is the third person singular of the indicative mood, present tense, of which the characteristic is 15. To *grand*, and to *mal*, belong likewise the figure 1, for the nominative case; he will therefore write

13.1 | 33.15 | 67.1 | 68.1 |

where the numbers are separated by small vertical bars to prevent confusion. It may easily be conceived how, by the inverse method, the German will find in his tables the words denoted by the cyphers, which will form *Der krieg ist ein grosses uebel.*

This invention of Becher, which is the same thing nearly with regard to language, as algebra is to arithmetic, could not have cost him any great effort of the imagination; and it evidently reduces itself, as I have observed with regard to all pasigraphy, to the learning a new language, or having a dictionary at hand for use. It is besides possessed of considerable simplicity, and even a few hours practice will render it easy. If any reader should be curious to see more applications of this kind, he may have recourse to the Latin work of Sturmius, *Essais d'Expériences curieuses* *.

In the same year George Dalgaru, an Englishman, published at London, a work of which the prolix title is sufficient to shew its object. It is, "The Art of Signs, or an Universal Character and Philosophical Language, by Means of which, Men of the most different Idioms may, in the Space of two Weeks, learn to communicate, whether by Word of Mouth or by Writing, all their Thoughts, as clearly as in their Mother-tongue. Besides which, young Persons may therein learn the Principles of Philosophy, and the Practice of true Logic, more speedily and more readily than in the ordinary philosophic Writings." The book of Dalgaru is written in Latin †. Becman accuses him of extreme pedantry. I do not know whether this book is common in England, but it is very scarce on the Continent. His characters likewise were cyphers.

Joachim Frischius, professor at the Gymnasium at Riga, was employed on a similar attempt, namely, to introduce a natural, rational, and universal language, of which some sheets printed at Thorn in 1681 may give an idea. The death of the author interrupted his labours. He purposed to call his new language Ludovicean, in honour of Louis XIV. under whose patronage he pursued his labours; a prince who extended his generosity to the learned of all countries.

The curious in researches of this nature may also find a project of this kind in a folio volume published at Rome in 1665, by the celebrated mathematician Athanasius Kircher, the title of which is, "A New and Universal Polygraphia, deduced from the Art of Combination ‡;" and by means of which, says Morhoff (Polyhistor. l. ii. c. 5.), he who understands one single language only may correspond in writing with all the nations of the earth.

* I suppose this work to be the *Collegium experimentale, sive curiosum, &c. Joannis Christophori Sturmii*, 4to. Norimberg, 1701. N.

† I know nothing of this author, nor his work. The title in the text is translated verbatim from the French. N.

‡ Neither Moreri nor any other biographer at present within my reach has given the title of this work of the voluminous Kircher. N.

It

It would be perhaps unjust to pass in silence the little-known work of father Besnier, Jesuit, who in a book entitled *La Réunion des Langues, ou l'Art de les apprendre toutes par une seule*, that is, The Union of Languages, or Art of learning all Languages by one alone, printed at Paris in 1674, has given several intimations which lead directly to pasigraphy.

The most remarkable work of all which has been written on this subject is, perhaps, that for which we are indebted to bishop Wilkins, the brother-in-law of Cromwell. It is entitled, An Essay towards a Real Character and a Philosophical Language, London 1668. It is divided into four parts. I. Considerations on the various languages, their defects and imperfections, from which a philosophical language ought to be exempt. II. Philosophical enquiries respecting all the things and notions to which proper names ought to be assigned. III. The organic science of native grammar considered as the necessary means of representing simple ideas in discourse. IV. The application of the general rules to every character and language. Examples, &c. This concise outline sufficiently shews the importance of the work *.

In his appendix, the author explains the utility of a method of writing without alphabetic characters, by means of signs, which are to be used to denote all the principal ideas, the relative attributes being designated by small strokes added at right, acute, or obtuse angles, to the right or left, &c. Of principal or chief ideas he admits but forty, under which he ranges all the others, by that means forming a kind of categories. His new language is calculated to afford great facility of comprehension, and new openings to the reasoning processes of science. A learner might make more progress in it in a month, than in the Latin in several years †.

After so many attempts more or less philosophical, and of different degrees of perfection, with others probably of which we know nothing, we must not overlook the efforts of the celebrated Leibnitz for the introduction of a pasigraphy. His History and Developement of a Characteristic Universal ‡ Language is in every library. Leibnitz exhibited, and with reason, his universal characteristic as the art of inventing and judging. He was convinced that an alphabet might be formed, and of this alphabet such words as would afford a language capable of giving mathematical precision to all the sciences. " Men may thus acquire," says he, " as it were a new organ, which would add energy to their moral faculties, as the microscopic lens increases the power of the eye. The compass is not more highly

* Wilkins's Essay towards a Real Character and a Philosophical Language is a thin folio of 454 pages. A dictionary of English words, referred to their places in the forty tables, was printed at the same time (1668), and of the same size. These books are not very scarce. They contain a treasure of information with regard to the objects comprehended in the scheme for a philosophical language; but neither this, nor the character itself, as the bishop has left them, appears to be enough completed to attract the attention of the world by their facility. Dr. Robert Hooke, whose prodigious abilities give a sanction to whatever he approved, did actually learn it, and published some valuable philosophical information on an engraved plate in this universal character, with a view to excite others to acquire the means of perusal. N.

† The Chinese writing, which is a very complicated pasigraphy, has given two learned men the idea of forming one upon a more simple plan. The first is Caramuel, in his *Apparat Philosophique*, page 128; and the other Andrew Muller Griessenhag, in his *Clé Chinoise*. The latter promised to teach to women and children, in the course of a few days, a kind of writing by which all the several languages should be rendered intelligible to them. Note of the Author.

‡ *Historia et commentatio linguæ charactericæ universalis, quæ simul sit ars inveniendi et judicandi.* Oeuvres Philos. Lat. et Françoises de Leibnitz, données par M. Raspe.

valuable

valuable to the navigator, than this philoſophical language would be to him who embarks on the ſea of reaſon and experiment, which is now ſo full of danger."

We muſt not forget the ingenious method of the abbé de l'Epée, who, by means of the ſame identical geſtures, dictated to his deaf and dumb pupils certain diſcourſes, which they wrote with equal readineſs in four languages. Nothing can more aſſuredly reſemble a true paſigraphy than theſe geſtures.

The abbé de Condillac, a ſubtle metaphyſician, particularly with regard to human language, has not neglected ſigns and characters. In his Art of Thinking (*Art de penſer*), in his Art of Writing (*Art d'écrire*), and more particularly in his Logic, he has ſhewn the advantages of a philoſophical language, which ſhould proceed perfectly in the order of the ideas, and of which the ſigns ſhould be the moſt ſimple and analytical. He expreſsly reduces the ſtudy of all the ſciences to that of a well conſtructed language, and quotes, as an example, algebra in the mathematical ſciences.

Strengthened by ſuch aſſiſtance, ſupported by the reſearches of ſo many predeceſſors, it muſt no doubt have been eaſy for Citizen Memieux to have conceived and executed the idea of a paſigraphy. To judge how far he is an inventor, to diſtinguiſh what excluſively belongs to him, it is neceſſary to examine and perpetually compare his work with all thoſe we have pointed out, and no doubt with many others which have eſcaped our notice.

Will this new author be more fortunate than Becher, Kircher, Wilkins, and Leibnitz? Will he eaſily perſuade the nations to learn the new language he propoſes? Of this we may ſtill reaſonably entertain ſome doubt. However this may be, it is to be hoped that a paſigraphy will hereafter be eſtabliſhed. In thoſe ages of peace, leiſure, and union, of which the proſpect is held out to us by M. Kant, men will have no object of more importance than to give perfection to their language, and to liſten to the voice of philoſophers, who ſhall propoſe to them a better ſyſtem of ſpeech. If it ſhould not be admitted in the lower claſſes of ſociety, it will at leaſt become the learned language of all the earth. On this ſubject I will venture to ground a comfortable hope, which tends to encourage the mind under its aſtoniſhment at the dreadful deluge of books which at preſent inundates the world.—Thoſe books only will be tranſlated into the learned language which deſerve the trouble: the moſt ſalutary ſcrutiny will take place; and the reſt will go to heat the baths. Philoſophers, literary men, authors of every deſcription, be aſſiduous, compoſe works of value, and be aſſured that your writings and your names will eſcape oblivion by means of the paſigraphic invention. In the mean time, till the arrival of this great epocha, one of the languages at preſent in uſe, one of the moſt imperfect in many reſpects, becomes every day more extended in its uſe * * * * *

V.

Obſervations on the Natural Hiſtory of Guiana. In a Letter from WILLIAM LOCHEAD, *Eſq. F.R.S. Edin. to the Rev. Dr.* WALKER, *F.R.S. Edin. Regius Profeſſor of Natural Hiſtory in the Univerſity of Edinburgh.*

[Concluded from Page 304, Vol. II.]

BESIDES theſe two kinds, there are alſo what we may denominate half ſavannahs, formed upon the tops of ſand-hills, higher and more irregular than in the caſe of thoſe juſt

deſcribed.

deſcribed. Some of theſe are alſo very extenſive. Few herbaceous vegetables are to be met with upon them. Broad ſpaces of arid ſand are interſected by clumps of ſhrubbery. Nothing grows to the height of a tree; but a particular ſet of plants, different from thoſe in other parts of the country, find ſubſiſtence enough to riſe to fifteen or thirty feet. How Nature, after all her efforts, ſhould have failed to induce a ſoil upon theſe is ſurpriſing. It appears chiefly owing to the great poroſity of the ſand, which every where admits the decayed vegetable matter deſtined for that purpoſe to be carried down through it, and filtered off by the rain. Even thoſe ſand-hills which are covered by tall trees, ſtill ſhew proofs of this. The trifling layer of mould formed upon them is exceedingly thin. When cleared, they are very barren; and when you dig in them to a great depth, you ſtill find ſmall portions of black vegetable earth diſperſed among the ſand. What corroborates the above ſuppoſition is the appearance of the ſprings. Abundance of theſe are found guſhing out copiouſly round the verges of the hills; and notwithſtanding the extreme whiteneſs and purity of the ſand from whence they flow, there is not one in an hundred whoſe waters are limpid. They come out not muddy, but of a browniſh colour, very much like the water which runs from peat-moſſes, and they are certainly tinged by the ſame cauſe. The rotten leaves of trees, and other decayed parts of vegetables on the hills, inſtead of being collected on the ſurface to form ſoil, are waſhed down into the ſand ſtrata by every rain; ſo that the reſervoirs of the ſprings, and the water which proceeds from them, are always coloured with theſe ſubſtances. There follows a corollary alſo from this general principle, and, when compared with facts, I believe it will hold good: The more the ſand is concreted into ſtone in any of the hills, the more and better will be the ſoil upon them. Where clay in ſmall beds, or in a certain proportion, is mixed with the ſand, the vegetable mould will likewiſe be better retained.

Rivers.—I will next give you what general obſervations I have been able to make upon the rivers and creeks of this part of America. The courſe of nearly all thoſe of Guiana is from ſouth to north. They originate in a chain of hills running eaſt and weſt, which ſeparates Guiana from the country of the Amazons, and likewiſe gives riſe, on its ſouth ſide, to the numerous branches which fall into that river. The Demerary is a conſiderable ſtream, equal if not ſuperior to the Thames; yet it is by no means among the largeſt of them. The Eſſequebo is five times larger at its mouth, forming a whole Archipelago of iſlands; but its ſtream ſoon divides, and, on account of rocks, ſhallows, and rapids, none of its branches are navigable ſo high up as the former. Moſt of the particulars I am now to give you, muſt be underſtood as applying to the Demerary. The bar, if it may be ſo called, is common to this with many other rivers which diſcharge themſelves into a ſhallow ſea; but ſtill with circumſtances in the preſent caſe which diſtinguiſh it from others where the bottom is not mud but ſand. It does not run like a ſingle narrow ridge, acroſs or nearly acroſs the mouth of the river; but it is of great extent, and is properly a continuation of the mud-bank which runs all along the coaſt. To the eaſt and weſt, and for two miles or more in the offing, you have ten or twelve feet water, with the utmoſt uniformity; and ſtanding in with the mouth of the river open, you neither deepen nor ſhallow till you enter it, when you find two, three, four, and five fathom; and it continues to average that depth for a long way, ſo that any veſſel which can enter, may, for draught of water, proceed up the river for 100 miles or more.

The

The mouth of the Essequebo, from the sand-hills and rocks being very near it, is exceedingly different. Three large islands present themselves in a breast, and divide its entrance into four channels. The length of these islands is, with the current, south and north; and from the tail or north end of each of them, as also from the banks of the main, or either side, run out sand-banks to a good distance. They are perfectly firm, quick in very few spots, and the body of them is above the level of low water. On the outside of them, you have the continuation of the mud-banks and shallow water, as above, only that the entrance of these channels is still shallower than that of the Demerary. The stream of this river runs very brown and muddy, and the sea is stained with it for some leagues off. A stranger naturally imputes this to the washings of a large flat country, or the stirring up of the muddy bottom by the tides. The latter may in part be a cause, though I believe it contributes to it but very little, and the former in a state of uncultivation, none at all. On ascending forty miles or so, you find the water clear again, or rather of a darkish hue; and so it continues above that. I was at first at a loss how to account for this; but, from a number of circumstances, was soon led to conclude that the thickness and light brown colour of the water near the mouth of the river, and on the coast, were almost entirely the effect of cultivation. Numberless ditches and canals have been opened by the inhabitants, which are receiving or discharging water every tide; and each particular piece on a plantation is every way intersected with open little drains, which communicate with these ditches. In digging and hoeing this clayey soil, much of it is suspended in the water, and carried off by the current of the tides. Nothing can be more certain, than that all up the river, and in all the creeks which discharge themselves into it, the colour of the water is constantly clear or blackish, even in the rainy seasons when it is swollen. On considering these circumstances, I have been led to this general conclusion, which is submitted to the proof of observation in different parts of the world. The reddish brown colour so common in freshes of rivers in Europe, and we may add every where, is almost entirely the effect of cultivation; and the natural colour of rivers, even in the highest and longest continued floods, where all the country is still in woods or pastures, is ever that of a dark brown, or blackish, pretty much like that of the streams which rise among peat-mosses, but rather more diluted. It is comparatively very clear, and deposits but a trifling sediment. The other is thick and opaque, and its sediment copious. Thus is man, in his little workings, made in a small degree one of the engineers of Nature. We cannot doubt that entire strata will owe to him their existence, accumulated in a series of ages at the bottom of the sea, and destined in future revolutions to act a more distinguished part. It may be curious, too, to consider the differences that may be expected betwixt the strata formed by these different depositions, which may be supposed between them to have been the origin of most of the clays upon our globe. Clay, earth, or loam, stirred up by the labourer, gives rise to the one: minutely decayed parts of vegetables form the body of the other.

It must also be observed, that clearing the ground along the coast, by cutting down trees, and opening ditches for the discharge of water, has exposed the land very much to the washing of the sea. The roots of the mangroves formed a plexus able to resist its force, and the former equal and very slow deepening of the water prevented its making a strong impression on any place. The discharge from the ditches at low water cut out channels in the mud, and left the sides of these channels more exposed to the returning waves, which here beat continually upon a lee ſhore. We find therefore on the coaſt, that the ſea has made here and there conſiderable encroachments, which generally begin on the weſt ſide of the canals or ditches, as being the moſt acted upon by the waves. The mouth of the Demerary itſelf furniſhes us with a ſtrong inſtance. That river is now nearly twice as wide as it was when the country firſt began to be cleared, the ſea and the ſtream together having ſince that ſwept away a large portion of land from the weſtern ſhore.

Creeks.—A number of creeks fall into the Demerary on both ſides, but ſo ſmall that they bear no proportion to the ſize of the river. You can hardly diſtinguiſh their mouths in the woods which overhang the banks. They are ſo narrow, that it is difficult to run a ſmall boat in them; yet you will find in them throughout from two and a half to four fathom water, and they run winding ſo far back that it will take five, ſix, eight hours, or more, to carry you up to their heads, where they terminate in ſmall ſtreams from among the ſand-hills. The banks of the creeks, at their mouths, are of the ſame height as thoſe of the river cloſe, from five perhaps to twelve feet above the water in the dry ſeaſon. As you aſcend the creek, you might naturally expect to find them riſe. It is, however, the very reverſe; they become gradually lower and lower, till at laſt all round them is a ſwamp: and the trees on each ſide, in like manner, become ſmaller and ſmaller, and of different ſpecies from what they were. It is now, in ſhort, exactly a mangrove ſwamp, with this difference, that the water is quite freſh, the vegetables are not the ſame, and there are abundance of arunis and other low herbaceous plants. A little higher up, you loſe the wood altogether, and find yourſelf in a beautiful deep canal, winding through a ſpacious wet ſavannah, which is ſometimes many leagues in circumference. The firſt time we went up one of theſe creeks (called Camouni), I was ſurpriſed at this appearance, and thought it muſt be a mere local circumſtance peculiar to it. We found afterwards the ſame in one or two more inſtances, and were ſatisfied upon enquiry that it is common to them all. It was natural to look for an explanation of this phenomenon, and I ſoon found it in one of thoſe laws which probably extend to all rivers ſubject to frequent inundations. It has been obſerved in particular of the Ganges*, that the banks of that river are higher than the adjacent lands at a diſtance from the ſtream, owing no doubt to the annual depoſitions of mud, &c. during the ſwell of the river. Apply the ſame rule to the Demerary, and the difficulty will be ſolved. The wet ſavannahs behind, and the ſwampy woods around them, are the body of the low country, at its natural level ſcarcely a foot or two above the ſea. Whatever additional height the land has in the vicinity of the river, from the time you have aſcended about twenty miles or ſo, is all acquired. It has riſen from the ſediment of the river during the rainy ſeaſon, when the country is overflowed, ſo as that all the lower part of it is under water. This depoſition muſt be always more copious in proportion as it is nearer the ſtream, where additional quantities are always brought, and where it is kept in motion both by the current and the tide.

Every thing we afterwards ſaw confirmed this theory, and nothing more directly than the canals which run out at right angles from the river. Some of theſe extend four miles inward, and they prove to a demonſtration that the land becomes lower and lower the farther you recede from the river. The maps of the colonies confirm it; for in all of them

* Account of the Ganges, &c. Phil. Tranſ. 1781, by Major Rennell.

the

the main body of the low land of Guiana is laid down as ſavannah; and the woody country, which a ſtranger or ſuperficial obſerver would ſuppoſe to be the whole, or much the greater part of it, is in fact only a border on the ſides of the rivers and of the ſea, but of conſiderable breadth, more or leſs in proportion to the ſize of the adjoining river, or, which is generally the ſame thing, to the acquired height and extent of the ſoil on either bank. It followed as a conſequence, and as far as we had opportunities of obſerving found it to be the caſe, that the low land was ſomewhat higher, and continued ſo farther down about the Eſſequebo, than the Demerary; the woods, conſequently, were of greater extent. We found, beſides, in the ſoil adjoining the Eſſequebo, at leaſt upon the eaſt ſide, a mixture of ſand. The river is full of ſand-banks; and it appears, that the finer parts of even this leſs ſuſpenſible ſubſtance are raiſed by the floods, and carried among the adjacent woods, to be depoſited with the mud. The Mahayka, a ſmall river or creek, which falls into the ſea about twenty or thirty miles to the eaſtward of the Demerary, though it runs a long way up the country, and ſpreads into many branches, has but a very narrow and often interrupted border of wood upon its banks; it runs through an immenſe ſavannah, and ſo do its branches, with little or no wood till they approach the ſand-hills. The Deltas of the river of Oronooko, and its numerous mouths, make a figure even in the map of the world. It is to be regretted that its noble ſtream has been ſo long hid from ſcience. What I learned in Trinidad, from a gentleman who had ſailed from its mouth to the Anguſturas, about 300 miles up, confirms and illuſtrates, in the fulleſt manner, the above general rule. The weſtern mouths of it, oppoſite Trinidad, are navigable only for launches drawing ſix or ſeven feet water. At and oppoſite them the bottom is ſhallow and muddy, and the coaſt a low mangrove ſwamp, reſembling in all reſpects that of Guiana. You muſt aſcend thoſe branches ſeveral days before you reach the main ſtream; and in doing ſo you find the ſame phenomena as in aſcending the Demerary, but in a ſtill greater degree. At firſt you have the mangrove, or ſome ſimilar ſwamp, and behind it, on both ſides, for about twenty leagues, the land, if you can call it ſo, hardly emerging from the water. Afterwards the ground appears; and, as you go up, riſes ſtill higher and higher on the banks above the common level of the ſtream. The trees become in the ſame manner of different ſpecies, and much taller than they were below. The channel in which you are, from being wide grows narrower by degrees. It is from about one and a half to three-fourths of a mile broad near the entrance, and when it joins the main ſtream is not more than about 200 yards. It has then acquired a conſiderable depth, and the banks may be about 20 feet high. Along the main ſtream of the river, or Boca de Naſios, the gradual riſe, and other circumſtances attending it, are quite ſimilar. All this height of the bank, I can make no doubt, is entirely acquired ground, formed by the ſediment of the floods, greater near the ſtreams than at a diſtance from them; and though I have no knowledge of the nature of the land in the Deltas and their vicinity, I would not heſitate to ſay, that great part of the interior body of each iſland, and moſt probably of the main, on either ſide where it is low country, conſiſts of nothing elſe than wet ſavannnahs.

Floods.—Before we leave the rivers, it may be proper to take notice of their floods. In no inſtance of a large river does the univerſal law within the tropics fail, that they annually overflow their banks for a certain ſeaſon. What was a prodigy in the Nile during the infancy of ſcience, is now a well-known phenomenon to every inhabitant of a continent in the torrid zone. From the ſituation of the river Amazons, it amounts to a certainty, that the Demerary, Eſſequebo, and other rivers of Guiana, cannot originate very far up in the continent of South America. This is confirmed by what I could learn of the riſe and duration of the floods of theſe two rivers. Enquiring about them at the plantations below is to little purpoſe, for there the floods are hardly diſcernible; but by the poſt-holder, and the ſettlers fartheſt up, I was informed that they are there ſenſible enough, and that, independent of all partial ſwells from accidental rains, the Demerary generally roſe every year in the month of June, and continued high through July and part of Auguſt. The riſe there upon the whole might be about twelve feet; it is ſufficient to lay the level parts of the country under water, and to render the woods that cover them in ſeveral places paſſable in canoes. We could have wiſhed for more exact information. This, however, was ſufficient to prove that the rivers did not riſe very far inland, elſe the floods would have been later in the year; but at the ſame time that they were of extent enough to follow the rule of all conſiderable intertropical rivers, ſo as to have a flood in the rainy ſeaſon, that is, in the months when the ſun is upon the ſame ſide of the line on which they have their origin and courſe.

The great Oronooko, I have been informed, begins to riſe a little in May: it continues increaſing through the ſummer months, and the inundation is at its height in September. At that time, as far up as the Anguſturas, the riſe is above forty feet perpendicular above the low water mark. It diminiſhes as you deſcend, till about the mouth, where it is only a very few feet.

Tides are of the utmoſt conſequence to the inhabitants of the coaſt of Guiana. They enable them to drain a country which otherwiſe would never have been cleared, and they aſcertain their journeys, which are made by water up and down the rivers, and even along the coaſt. At the mouth of the Demerary it is high water at about half paſt five, at new and full moon. The riſe in ſpring tides, a little way up, is twelve feet or more above low water mark. The tide runs very rapidly near the mouth of the river, ſeldom leſs than four or five miles in the hour. It continues to run with force for a long way up, and was ſufficient without wind to carry us up or down at 150 miles from the mouth. Above that it becomes feebler; and for a conſiderable diſtance below the Rapids, though there is a ſenſible riſe and fall of two or three feet, yet even in the dry ſeaſon the current is conſtantly down, only more gentle during the riſe or flood; and there alſo the continuance of the riſe is very ſhort, not more than two or three hours.

Some obſervations upon the ſoil of the different parts of the country may be the ſubject of a future communication. I will only add, at preſent, what I think has more than conjectural foundation; viz. that this moſt recent of countries, together with the large additional parts ſtill forming on its coaſt, appear to be the productions of two of the greateſt rivers on the globe, the Amazons and the Oronooko. If you caſt your eye upon the map, you will obſerve, from Cayenne to the bottom of the Gulph of Paria, this immenſe tract of ſwamp, formed by the ſediment of theſe rivers, and a ſimilar tract of ſhallow muddy coaſt, which their continued operation will one day elevate. The ſediment of the Amazons is carried down thus to leeward (the weſtward), by the conſtant currents which ſet along from the ſouthward and the coaſt of Brazil. That of the Oronooko

nooko is detained, and allowed to ſettle near its mouths, by the oppoſite iſlands of Trinidad, and ſtill more by the mountains on the main, which are only ſeparated from that iſland by the Bocos del Drago. The coaſt of Guiana has remained as it were the great eddy or reſting-place for the waſhings of great part of South America for ages; and its own comparatively ſmall ſtreams have but modified here and there the grand depoſit.

W. LOCHHEAD.

VI.

On the ſuppoſed Revival of Inſects after long Immerſion in Wine or other intoxicating Liquor. By Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR, Kendal, Oct. 7, 1798.

TO attack the opinions of any man is a diſagreeable taſk, eſpecially if ſuch opinions have been favoured by perſons of the firſt reputation in their reſpective purſuits. The force of the preceding reflection has embarraſſed me not a little in my preſent attempt, which preſumes to controvert a notion relative to the nature of inſects, ſupported by the authority of Dr. Franklin. This acute and induſtrious philoſopher maintains, that flies drowned in wine will revive, days, or even months, after their immerſion, upon being expoſed again to the air and ſun. The Doctor does not profeſs himſelf to be the author of the opinion, but ſuppoſes he ſaw it confirmed by an incident which he witneſſed in London, where diſcovering two flies in a veſſel that was employed to decant a bottle of Madeira wine that had been brought from Maryland, and concluding, perhaps too haſtily, that the flies were imported from America in the bottle, he expoſed them to the ſun: one of the two revived in a little time, and flew away; but the other could not be reſtored to life by this artleſs method of reſuſcitation. After ſtating the fact, the Doctor proceeds to entertain his readers, according to his cuſtom, with ſome lively reflections, which would have been not leſs important than they are amuſing, had the premiſes been well founded; but I am perſuaded that a more careful repetition of the experiment would have determined this ingenious obſerver to relinquiſh a notion which his high name has made current with the phyſiologiſts of the preſent day.

In order to obtain that information and certainty reſpecting the ſubject which cannot be had from caſual obſervations, I have repeated the experiment on a number of inſects, drowned for the purpoſe in wine and other intoxicating liquors; an account of which trials is related in the ſequel of this letter.

Experiment 1. Two large blue fleſh flies (*muſcæ vomitoriæ*), which had been immerſed in a phial of red wine, with a view to this experiment, on the 12th of July 1793, were expoſed again to the ſun and air, on a piece of black ſilk, in a window, on the 20th of Auguſt the year following. In this ſituation they remained two days without ſhewing any ſigns of returning life; on the contrary, they were found dry and ſhrivelled, though their bodies appeared plump and in high preſervation when taken out of the bottle.

Experiment 2. I repeated the preceding experiment on a number of flies, making uſe of Madeira and other kinds of wine, as well as brandy and beer; the time of immerſion being

being varied from two hours to three or even four days; but the ſame want of ſucceſs attended all theſe trials; for not one fly which had been rendered torpid by intoxication could be reſtored to life.

Experiment 3. Flies which were taken out of the wine two or three minutes after they ceaſed to ſhew indications of life, recovered, not only when expoſed to the ſun, but alſo when placed in a temperature kept high by means of the human breath for the ſpace of ſix or eight minutes.

The two firſt experiments taken in conjunction with the laſt ſeem to inſinuate, that the doctor was deceived in ſuppoſing his flies to have been imported from America; for the eagerneſs with which theſe inſects repair to veſſels containing ſpirituous and fermented liquors, makes it much more probable, that they were attracted by the ſmell of the wine into the funnel, from which they were extricated in a little time, but not before one of the two had been too long immerſed to recover; and unleſs ſome circumſtances of importance have been overlooked in the preceding attempts, flies made inſenſible by vinous ſpirit are ſubjected to the ſame law of ſuſpended animation which determines the fate of animals drowned in water.

I will even venture to advance a more deciſive propoſition on this head, in pronouncing alkohol highly pernicious to the living principle in inſects, which it deſtroys with certainty, but not with equal expedition in every kind; for, if the effects of this fluid on theſe diminutive animals be compared with the injuries they experience in the air-pump, intoxication will appear to kill them in a much ſhorter time than the abſence of oxygen. Dr. Derham found, that ſeveral inſects, which he ſpecifies, revived on the re-admiſſion of the air, after remaining torpid, in ſome inſtances, as long as 16 hours in an exhauſted receiver. (See Phyſico-Theology, chap. 1. note F.) But life ſeems to be extinguiſhed in a much ſhorter period by ſpirituous liquors.

The foregoing experiments being confined to common flies, or thoſe with one pair of wings, I thought it not improper to extend the enquiry to a greater variety of inſects, which was done accordingly in the courſe of the laſt ſummer. But theſe trials diſcovered nothing remarkable, except the power of the Nut Weevil *(Curculio Nucum)* to reſiſt the deſtructive effects of alkohol *. This conſtitutional ſingularity has been noticed prior to the preſent eſſay; but unleſs my experiments were made under ſome unfavourable circumſtances, the property in queſtion appears to be exaggerated.

Experiment 4. I immerſed ſeveral maggots taken out of hazel-nuts in brandy: theſe were afterwards incloſed in freſh nuts opened for their reception, and placed in a temperature varying from 70° to 80°. The maggots, which had been confined in ſpirit for a period not exceeding 17 hours, revived; but when the time of immerſion was prolonged to three, or even two days, every attempt to reſtore them proved fruitleſs.

Having now ſtated the moſt ſtriking facts of my experiments fully, I will cloſe the ſubject by forming the reſt into a table, expreſſing the number of inſects of each kind that have been killed by immerſion in wine, brandy, and beer. Thoſe marked with an aſteriſk, being ſuch as delight in the ſhade, were ſhut up in perforated chip boxes after immerſion, and placed in a warm temperature.

* The maggot of the filbert will remain torpid, but not deſtroyed, in a phial of brandy, though bottled up for many weeks. (Good on the Diſeaſes of Priſons, p. 174.)

Name.

Name.	Wine.	Brandy.	Beer.
Scarabæus fimetarius, - - -	—	1	—
Curculiones Nucum *, - - -	—	18	—
Grylli domeftici *, - - -	4	2	1
Erucæ Phalenarum, - - -	—	20	—
Panorpæ communes, - - -	—	2	—
Vefpa vulgaris, - - - -	—	1	—
Apes mellificæ, - - - -	2	1	—
Tipula oleracea, - - - -	—	1	—
Mufcæ vomitoriæ, - - -	2	—	—
Mufcæ carnariæ, - - - -	4	2	1
Mufcæ domefticæ, - - -	7	5	—
Tabani decutientes, - - -	6	4	3
Afilus crabroniformis, - - -	—	1	—
Araneæ domefticæ *, - - -	4	2	6
Onifci Afelli *, - - - -	3	2	3
Scolopendræ forficatæ *, - -	1	1	2
	33	63	16

JOHN GOUGH.

VII.

Various Notices refpecting the Practice of the Arts in Turkey.—Jeweller's Foil.—Glue, or Maftic, for Stones and Metals.—Cafting of Malleable Iron.—Filtration by Afcent.—Butter preferved without Salt.—Extemporaneous Yeaft *.

THE Armenian jewellers fet precious ftones, particularly diamonds, to much advantage, with a foil, which, under rofes or half brilliants, is remarkably beautiful, and is not fubject to tarnifh. Their method is as follows: An agate is cut, and highly polifhed, of the fhape defired; in a block of lead is formed a cavity of about its own fize; over this is placed a bit of tin, of the thicknefs of ftrong brown paper, fcraped bright. The agate is then placed on the tin over the cavity, and ftruck with a mallet. The beautiful polifh the tin receives is fcarcely to be imagined. This is in general kept a fecret, and fuch foils fell for half and three quarters of a dollar each.

The jewellers, who are moftly Armenians, have a curious method of ornamenting watch-cafes and fimilar things, with diamonds and other ftones, by fimply glueing them on.

The ftone is fet in filver or gold, and the lower part of the metal made flat, or to correfpond with the part to which it is to be fixed; it is then warmed gently, and the glue applied, which is fo very ftrong that the parts never feparate.

This glue, which may be applied to many purpofes, as it will ftrongly join bits of glafs or polifhed fteel, is thus made:

Diffolve five or fix bits of maftic as large as peas, in as much fpirit of wine as will fuffice to render it liquid; in another veffel diffolve as much ifinglafs (which has been

* From *Eaton's Survey of the Turkifh Empire,* octavo. London 1798.

previoufly

previously soaken in water till it is swollen and soft) in French brandy or rum, as will make two ounces by measure of strong glue, and add two small bits of gum galbanum or ammoniacum, which must be rubbed or ground till they are dissolved; then mix the whole with a sufficient heat; keep it in a phial stopt, and when it is to be used set it in hot water.

A remarkable instance occurred to my knowledge of an individual fact which might have been of the utmost use to society, but which, owing to the state of knowledge and government in Turkey, was wholly lost to the world. An Arabian at Constantinople had discovered the secret of casting iron, which, when it came out of the mould, was as malleable as hammered iron. Some of his fabrication was accidentally shewn to Mr. de Gaffron, the Prussian chargé d'affaires, and Mr. Franzaroli (men of mineralogical science), who were struck with the fact, and immediately instituted an enquiry for its author. This man, whose art in Christendom would have insured him a splendid fortune, had died poor and unknown, and his secret had perished with him! His utensils were found, and several pieces of his casting, all perfectly malleable. Mr. Franzaroli analysed them, and found that there was no admixture of any other metal. Mr. de Gaffron has since been made superintendant of the iron manufactory at Spandau, where he has in vain attempted to discover the process of the Arabian.

I have seen practised a method of filtering water by ascension, which is much superior to our filtering stones or other methods by descent, in which in time particles of the stone or the finer sand make a passage along with the water.

They make two wells from five to ten feet, or any depth, at a small distance, which have a communication at bottom. The separation must be of clay well beaten, or of other substances impervious to water. The two wells are then filled with sand and gravel. The opening of that into which the water to be filtered is to run, must be somewhat higher than that into which the water is to ascend; and this must not have sand quite up to its brim, that there may be room for the filtered water, or it may, by a spout, run into a vessel placed for that purpose. The greater the difference is between the height of the two wells, the faster the water will filter; but the less it is the better, provided a sufficient quantity of water be supplied by it.

This may be practised in a cask, tub, jar, or other vessel. The water may be conveyed to the bottom by a pipe, the lower end having a spunge in it, or the pipe may be filled with coarse sand.

It is evident that all such particles, which by their gravity are carried down by filtration by descent, will not rise with the water in filtration by ascension. This might be practised on board ships at little expence.

The butter which is mostly used in Constantinople comes from the Crim and the Kuban. They do not salt it, but melt it in large copper pans over a very slow fire, and scum off what rises; it will then preserve sweet a long time if the butter was fresh when it was melted. We preserve butter mostly by salting. I have had butter which, when fresh, was melted and scummed in the Tartan manner, and then salted in our manner, which kept two years good

good and fine tasted. Washing does not so effectually free butter from the curd and butter-milk, which it is necessary to do in order to preserve it, as boiling or melting; when then salt is added to prevent the pure butyrous part from growing rancid, we certainly have the best process for preserving butter. The melting or boiling, if done with care, does not discolour or injure the taste.

The preservation of yeast having been a subject of much research in this country, the following particulars may perhaps deserve attention:—On the coast of Persia my bread was made, in the English manner, of good wheat flower, and with the yeast generally used there. It is thus prepared: Take a small tea-cup or wine-glass full of split or bruised pease, pour on it a pint of boiling water, and set the whole in a vessel all night on the hearth, or any other warm place; the water will have a froth on its top next morning, and will be good yeast. In this cold climate, especially at a cold season, it should stand longer to ferment, perhaps twenty-four or forty-eight hours. The above quantity made me as much bread as two sixpenny loaves, the quality of which was very good and very light.

VIII.

Observation of the Passage of a Comet over the Disk of the Sun. By Citizen DANGOS.

ON the 18th of January 1798, Dangos observed a black, round, and well terminated body, which crossed the disk of the sun. The time of its passage lasted 20 minutes. He thinks it could be nothing but a comet. He recollects having observed a similar phenomenon in 1784.

Lalande remarks, that Mercury and Venus have been well observed crossing the solar disk in the form of black spots; but that comets had never been in that situation *.

* This notice, which is taken from the Journal de Physique for February 1798, leaves much to be desired. What might be the diameter of the spot; whether its course was direct or retrograde, with its inclination to the ecliptic, or to the solar axis; and particularly the chord it described; are objects of enquiry, concerning which we shall probably learn more in future, when a fuller account shall appear. If we admit that it was a comet, and, by way of obtaining a rough notion or guess of its distance, we suppose it to have described a whole diameter of the sun, we shall, from the time and the angular space, deduce, that it was about seventy times nearer the sun than our planet. This, on the supposition of its being near the perihelion, which however is mere supposition, would rank it among those comets which approach the nearest to that luminary. (See a Table of the elements of a considerable number of comets in Pingre's Cometographie, which is copied into Hutton's Dictionary, article Comet.)

If we consider how very seldom the inferior planets, Venus and Mercury, cross the sun, notwithstanding their short periods, and the little inclination of their orbits, it will be less surprising that the comets, though very numerous, should not often be found together with the earth in the line of their nodes; or that an unforeseen event, of such short duration, should scarcely ever meet the eye of the astronomer. N.

IX.

*Analysis of the Aqua-marine or Beryl; and the Discovery of a new Earth in that Stone. Read before the French National Institute 26 Pluviose, in the Year VI. (Feb. 14. 1798.) By Citizen VAUQUELIN *.*

Section I. Introduction.

THE analysis of minerals is one of those operations which are usually considered to be of little importance, and are submitted, by chemists of the first order, as unworthy of their care, to the manipulation of their pupils.

I am well aware that the greater number of analyses afford results of little importance, which do not repay the labour and the time bestowed in obtaining them. I am likewise aware that they do not offer so brilliant a prospect, nor promise to afford results of so general a nature, as the plan of operation which has been formed with regard to some of the most important points in chemistry. But I am not, from these reasons, of opinion, that this class of processes, which has likewise its difficulties, and requires, for its successful conduct, a certain series of reasoning, and particularly an exact knowledge of the bodies described;—I am not of opinion that it is so little entitled to engage the attention of philosophical chemists. For they must recollect, that it has afforded them the solid foundation of their theories, and new objects for the exercise of their abilities.

From a disregard of this kind it was that Bergmann, whose active mind could not submit to the details of experiment, has committed so many faults, by trusting his operations to young pupils, who had not acquired the habit of distinguishing new substances from those which were already known.

The analysis of the beryl, already made by Bindheim, will be a proof of what I here advance. It is composed, according to him, of silex 64, alumine 27, lime 8, and iron 2.

Citizen Hauy having found a perfect agreement between the structure, hardness, and weight of the beryl and the emerald, engaged me, some months ago, to compare these two stones by chemical means also, in order to know whether they were composed of the same principles in similar proportions.

The most interesting circumstance to the Institute in this result being a new earth, which I have discovered in the beryl, I shall pass slightly over the other objects, and dwell more particularly on its distinctive properties.

Section II. The Method of Analysis.

Experiment 1. One hundred parts of beryl reduced to fine powder were fused with 300 parts of caustic potash; the mass, after cooling, was diffused in water, and treated with the muriatic acid: by this means the solution was completed.

The muriatic solution was evaporated to dryness; towards the end of the evaporation the fluid assumed the form of a jelly: the dried matter was then diffused in a large quantity of water. Part of the matter was dissolved; but a white, granulated transparent powder remained. This substance, collected on a filter, washed with much water, and dried by ignition, weighed 69 parts. It had all the properties of silex.

* Annales de Chimie, xxvi. 155.

Experiment

Experiment 2. The fluid separated from the silex was precipitated by the carbonate of potash of commerce; the precipitate collected and drained was treated with a solution of caustic potash. The greatest part of the matter was dissolved; but there remained a certain quantity of earth which was not taken up. This being separated, washed, and dried by ignition, was of a brown greyish colour; it weighed nine parts. In these nine parts it is that our new earth is contained. We shall speak of it again in the subsequent part of this Memoir.

Experiment 3. The alkaline solution of the foregoing experiment was super-saturated with muriatic acid, until a perfect solution took place, and this was again precipitated by the carbonate of potash of commerce: the deposition, washed and dried by a red heat, weighed 21 parts.

This substance appeared to me at that time to be pure alumine. We shall see what conclusion ought to be made, after examining the properties of the new earth which I have announced.

Experiment 4. The nine parts in *Experiment* 2. remaining after the action of the potash, and in which I announced the existence of a new earth, were dissolved in the nitric acid; the solution was evaporated to dryness, and the residue again dissolved in water. The solution of this substance having assumed a reddish yellow colour, which indicated the presence of iron, a solution of the hydro-sulphuret of potash was mixed with it; a black voluminous precipitate was formed; the fluid was heated in order to favour the union of the parts, after which the fluid was decanted clear and colourless. The black precipitate by calcination became of a red brown colour, and weighed one part. When dissolved in the muriatic acid, and the solution evaporated to dryness, it afforded a beautiful blue when an atom of the matter was thrown into a solution of the Prussiate of potash: it was therefore the oxide of iron.

The fluid from which this oxide had been separated was again evaporated to dryness; and though at first it had no colour, it nevertheless became red towards the end of the process, and the residue preserved that colour. This residue having been left for 24 hours in a capsule, became reduced into a kind of gelatinous fluid, of a yellowish red colour: cold water poured thereon dissolved the whole mass; but the solution was red and turbid: by exposure on the heated sand-bath, red flocks were separated, and the fluid became as clear as water. These flocks, carefully separated, washed and dried, weighed half a part: it was also the oxide of iron; which, with the part before obtained by means of the hydro-sulphuret of potash, makes one part and a half of that substance.

Experiment 5. The earth being thus perfectly deprived of the oxide of iron, I separated it from the nitric acid by means of the common carbonate of potash; and I obtained 12 parts of a white earth, soft beneath the fingers, and soluble in acids with effervescence.

We see that this earth, in its separation from the nitric acid, did absorb four parts and a half of carbonic acid; since out of nine which were subjected to experiment, one and a half of the oxide of iron were obtained; which leaves 7,5 for the earth contained in the 12 parts of carbonate last precipitated.

Section III. Exposition of the Properties of the new Earth contained in the Beryl.

Experiment 1. The 12 parts of earth united with carbonic acid, as before mentioned, were put into sulphuric acid, which dissolved them completely with effervescence. The solution had a very saccharine taste at first, and astringent at last. The solution left till the following day afforded irregular crystals, very solid and saccharine like the solution which afforded them.

Experiment 2. These crystals were again dissolved in water; the solution mixed with sulphate of potash did not afford alum either immediately nor by evaporation, as happens in the alumine when combined with the sulphuric acid. Each of these salts crystallizes separately, without contracting any union.

I repeated this operation five times in succession, with different doses of sulphate of potash, without obtaining more success than at first. At last, to convince myself of the difference between this earth and alumine, I took equal quantities of the one and the other; and after having dissolved them in the sulphuric acid, I mixed them with like quantities of sulphate of potash. I constantly obtained octahedral alum with the alumine; but the earth of the beryl afforded only an irregular salt.

These differences first discovered between the two earths, induced me to seek for others, by comparing them in a greater number of points.

Section IV. Comparison of the Properties of the Earth of Beryl with those of Alumine.

Experiment 1. For this purpose, I separately dissolved equal quantities of alumine and of the earth of beryl in nitric acid, to perfect saturation.

The salt which arose from the combination of the earth of beryl with the nitric acid, did not appear susceptible of crystallization; it strongly retains moisture; by desiccation it becomes converted into a kind of ductile paste, which, when exposed to the air, powerfully attracts moisture. Its taste is at first very sweet, and afterwards astringent.

Experiment 2. The nitrate of alumine likewise crystallizes with considerable difficulty; but it does not attract moisture so strongly. Its taste is not saccharine, like that of the nitrate formed with the earth of beryl.

I made the following comparative essays of the solutions of these two salts, using equal quantities of each.

1. The nitrate of alumine, mixed with a solution of nut-galls in alcohol, afforded no precipitate. The fluid simply acquired a slight greenish colour, and lost somewhat of its transparence; however, at the expiration of some hours, the fluid, having been diluted with water, let fall a greyish precipitate.—2. The salt of the earth of beryl, mixed with the same re-agent, immediately afforded a deposition in flocks of a yellow brown colour.

3. The nitrate of alumine, mixed with the oxalate of potash, immediately afforded a precipitate in the form of very abundant white flocks, which subsided to the lower part of the vessel, and left the superior fluid perfectly clear.—4. The salt of the earth of beryl, with the same re-agent, did not afford the slightest appearance of a precipitate, even after several days.

5. The

5. The nitrate of alumine, mixed with tartrite of potafh, immediately formed a depofition in flocks, and the fuper-natant liquor became clear and colourlefs.—6. The falt of the earth of beryl, with the fame re-agent, did not produce any fign of precipitation after feveral days.

7. The nitrate of alumine, mixed with a folution of the phofphate of foda, afforded a gelatinous femi-tranfparent precipitate, which fubfided very flowly.—8. The falt of the earth of beryl alfo formed a precipitate with the fame re-agent; but it was lefs gelatinous, and lefs tranfparent, and it alfo fell down more fpeedily.

9. The nitrate of alumine, mixed with very pure Pruffiate of potafh, inftantly afforded a very abundant whitifh precipitate, which became green at the end of a few hours.—10. The falt of the earth of beryl, with the fame re-agent, afforded no precipitate, even after feveral days.

11. The nitrate of alumine, mixed with a faturated folution of potafh, afforded a gelatinous magma, which was femi-tranfparent, and foon became filled with numerous bubbles of gas, which raifed it to the upper part of the fluid.—12. The falt of the earth of beryl, mixed with the fame re-agent, afforded a precipitate in flocks, which was not filled with bubbles like the foregoing, and which fell to the bottom of the liquid.

13. The nitrate of alumine, mixed with a folution of cauftic potafh, at firft afforded a gelatinous depofition, which was afterwards taken up by the excefs of alkali.—14. The falt of the earth of beryl, treated with the fame re-agent, was affected in the fame manner, excepting only that a larger quantity of alkali was required for the fecond folution.

15. The nitrate of alumine, mixed with a folution of carbonate of ammoniac, formed a precipitate which was not re-diffolved by an excefs of alkali.—16. The falt of the earth of beryl, mixed with the fame re-agent, afforded a precipitate which was entirely re-diffolved by an excefs of alkali.

We fee by moft of thefe experiments, that the earth of beryl effentially differs from alumine, which however it refembles much more than any other earth, and with which it may even be eafily confounded in certain refpects.

But the experiment which induced me to fix my opinion irrevocably with refpect to this earth, was that which fhewed the comparative degree of attraction of the two earths for the fame acid.

To acquire this knowledge, I diffolved in the nitric acid twelve parts of very pure alumine, and evaporated to drynefs, in order to expel the furplus of acid which was in the fluid. The refidue being diffolved in water, I added to the folution ten parts of the earth of beryl, recently precipitated from its folvent, well wafhed, and ftill moift.

I added no more than ten parts of the earth of beryl, to precipitate the twelve parts of alumine, though I had afcertained, by other experiments, that a fomewhat greater quantity of the earth of beryl than of alumine was neceffary to faturate the fame quantity of acid; but I chofe rather that a fmall quantity of alumine fhould remain in folution, than that any portion of the earth of beryl fhould mix with the precipitate. When therefore the mixture had thus been made, I boiled the fluid for a quarter of an hour, then filtered it, and retained on the filter the earthy precipitate. After wafhing this, I combined it with the fulphuric acid, and evaporated to drynefs, in order to expel the excefs of acid; after which,

re-diffolving

re-dissolving it in water, I added a few drops of sulphate of pot-ash to the solution, and obtained octahedral crystals of alum.

Now it is evident that the earth of beryl has more affinity with the nitric acid than alumine has, and consequently that it is not the same earth. If the earth of beryl be not alumine, there is much greater reason to decide, that it is not one of the other known earths; for it differs much more from them than from alumine. I therefore consider this earth as a new substance, different from all those we are yet acquainted with. It is true, that it in some measure resembles alumine, namely, in its softness to the touch, its adhesion to the tongue, its levity, its solubility in pot-ash, and its precipitation from its solutions by ammoniac. But it differs from alumine in its other properties. Its combinations with acids have a very saccharine taste; it has a stronger affinity with those solvents; it does not afford alum with the sulphuric acid and potash; it is totally soluble in carbonate of ammoniac; and lastly, it is not, like alumine, precipitable from its solutions by the oxalate and the tartrite of pot-ash.

This earth being soluble in caustic pot-ash, like alumine, we can no longer trust to this simple character to ascertain the presence of the latter earth; for it may happen that the earth of beryl should be taken for alumine, or a mixture of both for one or the other of these pure earths. It will therefore be necessary, whenever an earth soluble in pot-ash is found, to endeavour to convert it into alum by the known methods. If it do not afford alum, it may be certainly concluded that it is not alumine. But it may possibly afford alum, and nevertheless contain the earth of beryl; a mother water will then remain, in which this last earth will be suspended.

To separate the small quantity of alum, which likewise remains in this mother water, it will be proper to decompose it by a solution of the carbonate of ammoniac added in excess; by this means alum will be entirely precipitated, and the earth of beryl will remain dissolved in the carbonate of ammoniac. This earth may afterwards be readily separated, by boiling the solution for a certain time. The heat will drive off the carbonate of ammoniac, and the earth will fall down in the form of a powder.

By comparing the results of the analysis of the beryl with those which Klaproth and myself obtained from that of the emerald, we might conclude, that these two stones are very different from each other; for I found that the emerald was composed of 64 of silex, 29 alumine, 2 lime, between 3 and 4 of the oxide of chrome *, and 1 or 2 of water; whereas the beryl is composed of 69 silex, 21 alumine, 8 of the peculiar earth, and $1\frac{1}{2}$ of the oxide of iron.

But since that time I have found that the emerald likewise contains this new earth; whence it follows, that the emerald and the beryl are one and the same substance, differing only in their colouring matter.

With regard to the proportion in which I have obtained this earth of the beryl, I do not give it as strictly accurate; for it is possible that part may have been dissolved at the same time as the alumine by the pot-ash †.

* This is the metallic acid discovered in the red-lead of Siberia, of which a short account was given in our Journal, II, 145. The memoir at length, with additional information, will appear in our next. N.

† Since the above was written, I have ascertained, that there was in fact a certain quantity of the earth of beryl dissolved by the pot-ash with the alumine; and that instead of 8 per cent, the beryl contains 16.

I have

I have not yet thought it proper to give a name to this earth. I fhall wait till its properties are better known; befides which, I fhould be glad to have the advice of my brother-chemifts on the fubject *.

In a fecond memoir, I fhall fpeedily give the moft complete account in my power, of its combinations with the acids and fome of the combuftible bodies †.

X.

Defcription of a new-invented detached Efcapement for Pocket Watches, &c. By Mr. John Prior ‡.

FIG. 1 and 2, Plate XV, reprefent the principal parts of the efcapement.

Fig. 1. A B, the pillar-plate; and A B E F, *fig.* 2, is a fection of the frame without pillars or potance. C, the cock fcrewed to the potance-plate at D. G, the balance, the lower part of which runs in the plate (but perhaps a potance will be more convenient for it in a pocket watch). H, the regulating fpring, pinned faft in the ftud at I. O, the friction wheel. N, its arbor, the higher pivot of which runs in the collet the balance is rivetted to, and the lower in a collet fcrewed faft on the balance-arbor at P; fo, when the balance turns round, the friction-wheel is taken along with it.

M, the intermittent lever-wheel. L, its arbor. R, its pinion, in the rim of which wheel are put an equal number of pins on each fide. The pins on one fide of the wheel are put exactly at the middle diftance of thofe on the other fide.

T, the intermittent lever, fcrewed faft to K, its arbor. Q Q are its banking fcrews.

The lever is all made of one folid piece of fteel. That end of it next the wheel is cut open wide enough to receive the rim, without touching either fide of it, as far as the pallets or inclined planes are; at the end of which it is cut wider, to admit the pins of the wheel. (See *fig.* 2, a fection of it as under L.)

The diftance from the centre of the balance to the extreme part of the lever, or end of the pallets, is divided in a right line into ten equal parts, and the pivot-hole of the friction-wheel is made at the firft tenth divifion from the centre of the balance. There are thirteen femi-diameters of the friction wheel from its centre to the end of the pallets.

When the balance is at reft, then the centre of the balance, the centre of the friction-wheel, and the centre of the lever, will be all in one right line. (See *fig.* 1.)

The lever is divided into three equal parts. The diftance from the end of the pallets to

* The moft characteriftic property of this earth, confirmed by the lateft experiments of our colleague, being, that it forms falts of a faccharine tafte, we propofe to call it Glucine, from γλυκυς fweet, γλυκυ fweet wine, γλυκαινω to render fweet. This denomination will be fignificant enough to affift the memory; it does not derive its etymology from a fenfe too ftrictly determined; neither does it prefent ideas falfely exclufive, like thofe names which might be taken from the name of the ftone which afforded the firft fpecimen of the new fubftance, the name of the firft village where it was met with, &c. &c. Thefe, we apprehend, are the true principles for the advancement of fcience, and facilitating its ftudy, by means of nomenclature. Note of the Editors of the Annales.

† Cit. Vauquelin has publifhed an appendix to this paper, on the general properties of this earth, which will appear in our next. N.

‡ Tranfactions of the Society for the Encouragement of Arts, 1798. A premium of thirty guineas was given to the inventor. N.

the

the centre of motion of the lever, one part; and from the centre of motion to the centre of the friction-wheel, when the balance is at rest, two parts: so that, whatever the velocity may be at the end of the pallets when in motion, that part of the lever, or the forked part of it, which is opposite the centre of motion of the friction-wheel, will be twice as much.

S S represents a superficial view of the pallets. The pallet below the pillar-plate is taken off from the other, otherwise the place of action could not be seen in the drawing.

In *fig.* 1, we have a view of the lever before the machine is wound up: a pin is half-way down the pallet; but when the pin impels the pallet, the lever moves, and gives motion to the balance, and the pin is disengaged from the pallet at the angle: that instant a pin, on the opposite side of the wheel, falls upon a circular part of the end of the other pallet, or upon that part where it and the lever are entirely at rest, until the balance makes its return, and the friction-wheel comes into the forked end of the lever. Then the friction-wheel impels the lever, while the balance runs over the space of about one hundred degrees. Then the pin drops off at the end of the pallets as before, and the balance is entirely left at liberty; at which time its velocity is so great, and its motion so easy, that it turns once round upon its pivots, and two hundred and forty degrees every vibration.

It is to be observed, that, in the ends of the fork, one part is turned up and the other down, in order they may not both go through one notch in the balance arbor; for, by having two different parts cut away, the fork is locked fast in every vibration, and is unlocked only by the friction-wheel in its return for another vibration.

Notwithstanding the balance makes one turn and two hundred and forty degrees every vibration around its axis, yet, when the balance is at rest, and the same power applied to it (which keeps it in motion as above), the balance will only move through the space of about fifteen degrees, which is only one fortieth part it keeps it up to when in motion.

It must be allowed, the less the wheels, or the power, have to do with the balance, the more accurate the time will be shewn *.

XI.

On Mr. CARTWRIGHT's Invention for rendering the Pistons of Steam Engines, Pumps, and other Hydraulic Apparatus tight by metallic Parts, without packing or leathering. (W. N.)

To MR. NICHOLSON.

SIR, Richmond, Oct. 10. 1798.

I HAVE lately been informed, that a new method of packing steam engines has been invented by a Mr. Cartwright, which is said to be particularly advantageous in saving friction and resisting an heavy column of water in pumps. I shall be very glad to see a description of the same in your Journal, with your opinion of its effects, &c.

Your constant Reader,

A. R.

* Artists will perceive that this is an improvement of the anchor scapement of Mudge, of which the inventor was not aware till he came to London. In this the pallets exactly resemble those of Graham's dead beat (Philos. Journal, II. 52.), and a tail acts against two planes in the axis of the balance. Pallets like those of Mr. Prior were adapted to a long pendulum by Mr. Crosthwaite of Dublin, in the year 1788, (Memoirs of the Irish Acad. vol. II.) and since that time also by other persons in London. N.

THE

THE contrivance to which my correfpondent refers, is part of a fteam engine, for which a patent has been taken out. It is generally underftood that in practice it is neceffary to apply the packing clofe round the pifton of a fteam engine, in fuch a manner as to make it act ftrongly againft the fides of the cylinder, which muft occafion a confiderable degree of friction: and when it has worked loofe, it may be concluded that fome lofs of force muft follow, from the efcape of elaftic fluid between the furfaces intended to be in contact. A fimilar obfervation may be applied to the ufual leathering of pumps, in which the friction is very great, and the refiftance to fevere preffure very far from being effectual. Early in the prefent century, a contrivance was made by a Mr. Hafkins to prevent friction, by fubftituting quickfilver inftead of leather; which, for feveral ftatical reafons, and alfo, as I fufpect, from fome of a chemical nature, could not be brought into general ufe. The reader may fee a very full defcription of this engine, in Defaguliers's Courfe of Experimental Philofophy, II. 491.

Mr. Cartwright's invention confifts in ufing folid maffes of metal inftead of the packing or leathering; which, by means of fprings, adapt themfelves to the variations of diameter in the cylinder, and, by their mode of application above each other, are expected to prevent that efcape of fluid which would elfe take place through the intervals between the feveral pieces. Imagine the pifton to confift of a circular plate of metal, nearly equal in diameter to the cylinder in which it is to move. It will make no difference in the general confideration of our fubject, whether this pifton have a valve in it or not. Upon the upper furface of this pifton are laid three or more pieces of metal, which all together compofe a flat circular ring; the ends of the feveral pieces nearly touching each other. A very moderate portion of mechanical knowledge will fuggeft the manner in which thefe pieces might be made to recede outwards, by means of fprings; fo as to occupy the circumference of a greater circle externally, than that to which they would nearly correfpond when regularly preffed inwards. If the pifton in this fituation be placed in its cylinder, it is evident, that the pieces compofing the ring will be forced againft the concavity; which, if of the proper curvature, they will fit, and prevent any fluid from paffing through, except at the interftices, where the ring is rendered incomplete by being divided into parts. To remedy this, a fecond ring is laid upon the firft, with its joints half-way between the joints of the former, in the fame manner as we every day obferve in courfes of brick-work. This fecond ring, being urged outwards like the firft, performs the fame function; but any fluid that may pafs downwards through the interftices of the upper ring, will be ftopped by the contiguous parts of the lower; and, on the contrary, whatever fluid may pafs upwards through the interftices of the lower ring will, for the fame reafon, be ftopped by the upper.

From this defcription it is fufficiently clear, that Mr. Cartwright is entitled to much praife for the ingenuity and acutenefs difplayed in his contrivance. The practical value of that contrivance, refpecting which I am requefted to ftate the facts, will probably be afcertained from the following confiderations:

In the ufual ftuffing or leathering, the elafticity of the organized matter made ufe of is fuppofed to act, in each individual part, fo far independently, that, if there be an irregularity in the cylinder by variation of its curvature or magnitude, this irregularity will be followed up and fitted by the elaftic material. But Mr. Cartwright's metallic packing poffeffing a determinate curvature, will fit only when the zone with which it is in contact poffeffes the

 fame

fame curvature. If his pifton be preffed into a fmaller cylinder than correfponds with the actual circle his pieces are adapted to form, thofe pieces will be preffed in, and will touch only at their extremities; fo that every joint will be immediately over a place where the oppofite ring does not in fact touch the cylinder: it will therefore, in this cafe, leak principally at the joints. And, on the contrary, if his pifton be preffed into a larger cylinder than correfponds with the circle of the rings, each portion of thofe rings will touch the cylinder in one point only. The moft favourable point will be at the half-way between the extremities of each piece. In this pofition the joints indeed will not leak, but every other part of the circumference will; and the places of the moft open paffage will be at thofe points of the circumference which are equidiftant between joint and joint. Thefe confiderations relate to perfect circles; but if we attend to fmaller irregularities, whether convexities or concavities in the cylinder, it appears evident, that the fegments of the rings being inflexible will ftill lefs effectually adapt themfelves to fuch imperfections.

Thefe objections, relating to the figure of the periphery of this metallic pifton, are of very ferious import: thofe which relate to its action are fcarcely lefs fo. The furfaces of the rings thus laid upon the pifton and upon each other, together with the furface of the piece which confines them from rifing, muft all be very well adapted to each other; and the number of fquare inches of this furface muft, in the nature of the contrivance, be confiderable enough to afford much friction. It may reafonably be doubted whether thefe pieces, in the rapidity of afcent and defcent, can obey the action of the fprings during the very fhort times in which the pieces are oppofed to the irregularities they are meant to remedy. But when a great preffure, fuch, for example, as the reaction of a column of 100 feet of water, comes to be exerted upon the face of this apparatus, the plates or pieces of thefe rings may be imagined to be confined in a vice. The preffure of fuch a column will amount to more than 40 pounds upon every fquare inch. Whence we may conclude, either that they would not move at all, or that the force of the fprings muft be fuch as greatly to load the work with friction, and damage the apparatus by fpeedy wear.

Laftly, it feems to be a queftion or doubt, which well deferves to be refolved by further experiment, whether, in any cafe of reiterated or long continued action, the fofter metals can be made to work in contact with each other, in the way of clofe fitting, with as little refiftance and wear as when an organifed fubftance containing oil or fat is interpofed.

When one individual fpeaks to the world concerning the works of another, the tranfaction is naturally accompanied with a fenfe of perfonality. As this fenfe ought not to lead men into unworthy actions, fo, on the other hand, it ought not to prevent their fulfilling any duty which may call upon them. Convinced that no vindication or apology is neceffary in defence of a fcientific examination of every object which is offered to the public acceptance, I have fpoken freely of a conftruction which, from its ingenuity, might be thought of greater value than it really is; but which, when carefully examined, appears to be inferior to the methods already in ufe.

XII *In-*

XII.

Information respecting the Zoonic Acid, discovered by BERTHOLLET.

THE fluid * obtained by diftillation from animal fubftances has been hitherto thought to contain no other principle than carbonate of ammoniac and an oil. Berthollet has afcertained that it contains an acid, to which he has given the name of zoonic acid. He has afcertained its prefence in the fluid obtained from the gluten of wheat, the yeaft of beer, bones, and woollen rags, diftilled for the preparation of the muriate of ammoniac; and he thinks himfelf authorized to confider it as the product of diftillation of all animal fubftances.

In order to feparate this acid, he mixes lime with the fluid afforded by this deftructive diftillation, after having feparated the oil. The mixture is then boiled or diftilled, to feparate the carbonate of ammoniac. When the odour ceafes to be penetrating, he filters and adds a fmall quantity of lime to the liquid, which he again boils till the odour of ammoniac has entirely difappeared. What remains is the zoonate of lime, which he again filters. To this he then adds the aqueous folution of carbonic acid, or otherwife he blows through a tube into the liquor, in order to precipitate any lime which might exift in the uncombined ftate. The zoonate of lime may then be ufed to produce other compounds by double affinity; or the pure zoonic acid may be had by the following procefs:

The well concentrated aqueous folution of zoonate of lime is to be mixed with phofphoric acid, in a tubulated retort, and expofed to diftillation. The zoonic acid is not very volatile, but requires a degree of heat nearly equal to that of boiling water to raife it. The fluid muft therefore be boiled; and if two fucceffive receivers be at the fame time adapted, it will not be driven into the fecond. Part of the acid feems to be deftroyed by the action of the heat; for the liquor becomes brown by the ebullition, and towards the end of the procefs black: whence it may be concluded that this acid contains carbone. Berthollet did not examine the other principles which are difengaged during the decompofition.

The zoonic acid has a fmell refembling meat which has been roafted; a procefs in which it is indeed formed. Its tafte is auftere, and, from the few experiments of Berthollet, no remarkable properties were exhibited. It ftrongly reddens paper tinged with turnfol, and effervefces with alkaline carbonates. It did not appear to him to afford cryftallizable falts with earths or alkaline bafes. It afforded a white precipitate in the aqueous folution of acetite of mercury, and in that of nitrate of lead; fo that it has a ftronger attraction to the oxydes of mercury and lead refpectively than the acetous and nitric acids. It does not act on the nitrate of filver but by double affinity. The precipitate which then falls down becomes brown in time, and therefore contains hydrogen. The zoonate of potafh calcined did not afford pruffiate of iron with a folution of that metal. A liquid, poffeffing all the characters of acidity, was feparated from flefh, which Berthollet had kept a long time in a ftate of putrefaction; but it was an ammoniacal falt with excefs of acid. This acid, combined with lime, appeared to him to refemble the zoonate of lime; but the quantity he had was too fmall to admit of its identity with the zoonic acid being accurately determined.

* Nearly in the words of Berthollet, in the Annales de Chimie, xxvi. 86.

XIII. *Hifto-*

XIII.

Hiftorical Notes concerning the Invention of the Air Pump with Metallic Valves; the Neceffity of Alkali to produce the cryftallized Salt called Alum; and the electrical Inftrument known by the name of the Revolving Doubler. (W. N.)

CITIZEN ADET, in vol. xxv. of the Annales de Chimie, p. 165, claims the invention of an air-pump for Cit. Ami Argand, at Paris, in the year 1776, of which that of Cuthbertfon is faid to be an imitation. Reference is made to the notes on the third volume of the *Leçons Elémentaires de Phyfique, de Sigaud de la Fond*, for a defcription.

I am happy in this opportunity of doing honour to a philofopher and mechanic, with whofe ability I am well acquainted. That tenacity with regard to the credit arifing from firft thoughts or inventions, and the partiality which leads men to exult in the nationality of difcovery, are eftimable qualities on the whole, becaufe they tend to the promotion of fcience; but they fometimes lead to infinuations of *mala fides* in cafes where the coincidences of reafoning have alone produced fimilar refults. On this occafion it feems proper to remark, that Mr. Cuthbertfon has candidly difplayed the fource from which he derived his information, and that, upon the whole, it appears highly probable, as well from the refpectable characters of the individuals, as from the general circumftances of the cafe, that this artift, as well as Paets van Trooftwyk, Dr. Rutherford, and Sir George Mackenzie, would have done juftice to the invention of Mr. Argand, if they had been acquainted with it *.

In the fame Annales, xxiii. 222, there is a claim on the part of the celebrated Chaptal refpecting the difcovery of the nature and triple compofition of alum, communicated to the Inftitute by Vauquelin †. To which this laft chemift has anfwered, in vol. xxv. p. 107, that he was unacquainted with the labours of Chaptal in that refpect, and had communicated his own memoir to the Inftitute a fortnight before Cit. Chaptal's memoir arrived; and, laftly, that the priority and merit of the difcovery belong to Cit. Defcroifilles, whofe refearches concerning the nature of alum were publifhed by Berthollet, in his Art of Dyeing, long before that time.

Another inftance in which I am in fome degree concerned, affords a curious example of the flownefs with which the improvements of philofophical apparatus are in fome cafes communicated. In the 22d number of the Bibliotheque Brittannique, there is an account of the doubler of electricity of *John Read.* The procefs of accumulating electricity ‡ by doubling, was invented by Lichtenberg and Klincock, and greatly improved by Bennett, who applied it to Volta's condenfer. In 1787, Dr. Darwin conftructed a machine for performing the procefs in part mechanically; and in 1788, I made and communicated to the Royal Society the Revolving Doubler by which the whole act is reduced to the fimple turning of a winch. Five years afterwards, namely in 1793, Mr. John Read publifhed his "Summary View of Spontaneous Electricity, &c." in the 4th chapter of which he

* On this fubject, fee our Journal, II. 28.

† Philof. Journal, I. 318.

‡ Philof. Journal, I. 396.

gives

gives a defcription of my inftrument in my own words, copied without acknowledgment from the Philofophical Tranfactions for 1788, but difguifed in a fmall degree by a new denomination in the title, and an unneceffary lengthening of the infulating parts, which before were upwards of ten times the length of the interval between plate and plate. It is evident, therefore, that of all the individuals who have written on this inftrument, the credit of the invention has, in 1798, been beftowed on the perfon who has the leaft claim to philofophical invention or candid narrative.

XIV.

Defcription of an Apparatus for faturating Pot-afh and Soda with Carbonic Acid. By Citizen WELTHER *.

THE intention of this apparatus is, to afford an uninterrupted contact between the alkali and the carbonic acid gas, and to proportion the difengagement of the gas to its fixation.

Fig. 1. *plate* XVI. reprefents the complete apparatus. It is compofed of four diftinct parts, A, B, C, D. The firft, A, ferves for the difengagement or production of the carbonic acid gas, whence it is diftributed into the apparatus. The fecond, B, connects all the feveral parts of the apparatus together. The third, C, is the refervoir for carbonic acid gas. The principal function of this part is to fupply gas to the alkali in proportion as the abforption takes place. And the fourth, D, contains the alkali which is to be faturated, in contact with the carbonic acid.

Each of thefe parts requires a particular defcription.

The part A, confifts of a bottle with two necks *a*, *b*, (the third, *c*, is not neceffary). This bottle contains fulphuric acid diluted with four parts of water, to the height *d*.—The neck *a*, receives a tube *e′ e′ e*, of which the upper part *e′ e′*, diverges like the bafe of a funnel, and the lower part *e*, which paffes through the cork and enters the bottle, is drawn out by the lamp, in fuch a manner, that the portion of tube which is in the ftopper has the fame internal diameter as the extremity *g* of the ftem *f*, reprefented *fig.* 2, A; and that the orifice *e* within the bottle, is only two millimetres in diameter ($\frac{1}{25}$ of an inch) †. Into the tube *e′ e′ e* enters the ftem or ftick of glafs *f*, reprefented *fig.* 2, A, the lower extremity of which is bound with flax, fo that it ferves as a ftopper in the contracted part of the tube, and does not permit its contents to flow into the bottle *a*, unlefs that ftopper be raifed.—The neck *b* contains a tube *h h h* bended into two angles, and forming a communication between A and B.

The part B, confifts of a bottle with five necks *a b f i h*, of which the plan is reprefented *fig.* 3, B. This bottle contains water as high as *l*.—The neck *h* receives the tube *h h h*,

* This apparatus was conftructed in the firft month of the third republican year. The defcription is tranflated from the Annales de Chimie, xxvii. 53.

† The tube *e′ e′ e* may be made out of a fmall matrafs with a long neck. The bottom may be taken away, leaving about half the body, which will form the part *e′ e′* of the tube; the neck will afford the part *a a*, and the extremity of the neck foftened and drawn out by the lamp to the above dimenfions, will afford the extremity *e*. W.

proceeding

proceeding from the bottle A.—The neck *b* contains a ſtrait tube *g g'*, whoſe extremity *g'* is plunged in the water to the depth of one or two centimetres (about $\frac{2}{3}$ of an inch.) It ſerves to ſhew the compreſſion to which the carbonic acid gas is ſubjected in the apparatus. —The neck *a* contains a ſtrait tube *a' a*, which allows a communication to be formed at pleaſure from the interior part of the bottle B, with the atmoſphere, by means of an apparatus of tubes ſhewn in *fig*. 4, B. The tube *a' a* has two ſtoppers at *m* and *n*: the ſtopper *m* enters the tube *m m*, encloſing the tube *a' a*; in the interval between theſe two tubes upon the ſtopper *m*, mercury is poured to the height *r* of twelve or fifteen millimetres (about $\frac{1}{2}$ an inch): the tube *a' a* is then covered by a third tube *d* cloſed above, which being immerſed at *p m* into the mercury, interrupts the communication of the atmoſphere with the orifice *a'* of the tube *a' a*, and conſequently cloſes the bottle B, when the ſtopper *n* is placed in the neck *a*.—The neck *i* contains a tube *i i*, compoſed like the former *a' a*; but of which the intermediate tube *o o o* twice recurved, eſtabliſhes a communication between the bottle B and the reſervoir C.—The laſt neck *f* contains a tube *f f f'*, twice recurved, which eſtabliſhes a communication between the bottles B and D.

The part C is compoſed of a tub *t*, in the ſide of which there is a hole at a ſmall diſtance above the bottom *a a*.—In this lateral hole is inſerted a perforated ſtopper, the interior orifice of which receives a tube *b l' b'' b*, bended into a right angle, of which the portion *b'* muſt be bended in ſuch a manner, that the angular part *b''* may touch the bottom *a a* of the tub, which renders it more firm in its poſition. In the external orifice of this cork, or ſtopper, there is ſcrewed a braſs cock *r*, of which the part *c* receives a tube *c c' c'' c*, bended at *c*, to riſe vertically, and from *c'* to *c''* to bind againſt the frame *d d*. This tube, at the height *d*, is compoſed like that of *a' a*, deſcribed *fig*. 4, B. It there receives the tube *o o o*, which affords a communication with B.—The pieces which are applied to the hole *b* of the tub, are luted; the inner with graver's wax, and the outer with fat lute.—In the tub *t*, containing water to the height *e e e*, there enters an inverted jar C, provided with a copper cock at *l* and *m*. This jar is ſuſpended at *g* by a ſtring, which paſſes over the pulleys *h h*, and ſupports at *i*, a weight K, ſomewhat leſs heavy than the jar itſelf, when entirely plunged in the water contained in the tub.—When the jar C is totally immerſed in the tub, it will reſt on two pieces of wood about two or three centimetres thick, which are fixed at *l l l* near the inner circumference of the bottom *a a* of the tub (ſee *fig*. 5, C, which repreſents the plan of the tub; and *fig*. 6, which repreſents the vertical ſection as far as A B), ſo that the baſe *l' l' l'* of the jar cannot touch the tube *b*.

The part D conſiſts of a bottle with three necks, *a*, *b*, *c*. The neck *a* receives a tube *a a' a*, proceeding to a ſmall bottle *d*, containing water as high as *e*.—The neck *b* receives the tube *f f f'* proceeding from the bottle B; this tube ought to poſſeſs a diameter of about two centimetres ($\frac{3}{4}$ of an inch) at its orifice *f'*.—The neck *c* contains a ſyphon *g g'*, communicating with the bottle *l*, of which the branch *g'* ought to be longer than *g*.—The bottle *l* has three necks, *h*, *i*, *k*.—It receives in its neck *i*, the ſyphon *g g'*; in the neck *k*, a tube *b b*, with one ſingle bend; and the neck *n* remains free. It is to be corked.

After the formation of the apparatus by uniting the four parts here deſcribed, each of them demands a particular preparation before the operation can be proceeded upon.

The tube *e' e' e* of the part A is to be filled with carbonate of lime mixed with water.—Through the neck *h* of the bottle *l*, in the part D, that bottle is to be filled with alkali. The neck

neck *b* is then to be ftopped with its cork, and by blowing through the tube K the alkali is to be forced through the fyphon *g' g* into the bottle D. The air contained in this laft bottle efcapes through the tube *a a' a*, and paffes through the water of the fmall bottle *d*. The fluid becomes nearly on a level in the bottles D and *l*, and the extremity of the tube *f f f'*, which connects the bottles B and D, is plunged in the alkali.—Proceeding then to the part *c*, the cock *m* of the inverted veffel is to be opened, while that of the tub *r* is kept fhut. By the excefs of weight of the jar beyond that of K it finks in the tub, and becomes filled with water, while the common air it contained efcapes at *g*.—As foon as the inverted veffel, being totally immerfed in the tub, repofes on the circular fegments *l l l*, the orifice of the upper tube of the tube *b* is found in the brafs receptacle *l*. It is neceffary, that the level of the water, which by the immerfion of the inverted veffel may have rifen from *e* to *e'*, fhould have the elevation *be'* of the tube *b* equal at leaft to two centimetres higher, in order that this tube may not be clofed by the water, which would prevent the gas from entering into the veffel C.—When this laft veffel is full of water, its cock *l* muft be fhut, and the cock *r* of the cafk muft be opened.

In this difpofition of the apparatus, the glafs ftem *f* of the part A is to be raifed, and carbonate of lime gradually introduced, which falling upon the fulphuric acid becomes decompofed, and lofes its carbonic acid. This laft poffeffing the elaftic ftate, paffes through the tube *b b b* into the bottle B. The tube *d* is then to be raifed, in order that the atmofpheric air contained in the bottles A and B may flow out; after which, the neck *a* of the bottle B is to be clofed, by replacing tube *d*. In this fituation the carbonic acid gas, which enters B, (meeting lefs refiftance in its paffage to the inverted veffel through the tubes *i i* and *o o o*, than to pafs into the part D by the tube *f f f'*, which is plunged in the alkali) raifes and fills the veffel *c*.—As foon as this event has happened, no more carbonate of lime is to be introduced into the bottle A, and the bottle D is filled with alkali by blowing through the tube K of the bottle *l*. The levels of the alkaline fluid are at the height *n* in the bottle D, and $m < n$ in the bottle *l*; and the fluid tending to its level, by means of the fyphon *g g'*, the furface *n* falls, and produces a vacuum in the bottle D.—The carbonic acid gas is then drawn into the bottle D by the tube *f f f'*; at the fame time that the water of the fmall bottle *d* (part D) rifes in the tube *a a' a*; but the height of this tube is fuch, that the carbonic acid gas can enter the bottle D before the water rifes to *a'* in the tube *a a' a*.—The two furfaces *n* and *m* acquire an equilibrium in the bottles D and *l*, and the carbonic acid gas, which occupies the upper part of the bottle D, combining with the pot-afh, a vacuum is formed, which is continually fupplied with new gas.—When the veffel C is nearly exhaufted of its gas, it muft be filled by a new difengagement.

N. B. As the carbonic acid gas is mixed with a fmall portion of atmofpheric air, this air, which is not abforbed by the pot-afh, accumulates in the bottle D, and may ftop the operation. It muft be driven out by blowing into the tube *b* of the bottle *l*, and filling the bottle D with alkali.

XV. *Abftract*

XV.

Abstract of a Memoir of KLAPROTH, *on a new Metal denominated Tellurium. Read at the Public Session of the Academy of Sciences at Berlin, January the 25th, 1798* *.

KLAPROTH, the chemist of Berlin, in the chemical analysis of the auriferous ore, known by the name of the white ore of gold (weiss golderz), aurum paradoxum, metallum vel aurum problematicum †, has discovered in that mineral, a metal absolutely different from all those which have hitherto been known, to which he has given the name of Tellurium, forming a kind of series or arrangement with the new metals discovered by him some time ago, and denominated Uranium and Titanium. Mr. Muller of Reichenstein, in the year 1782, had suspected the existence of a peculiar metallic substance in this mineral. Bergman, to whom he had forwarded a specimen of the ore, confirmed his suspicion; but on account of the small quantity upon which he operated, he did not think fit to decide, whether this fossil did actually contain a new metal, or whether it might not be antimony which he had mistaken for a new product. The numerous and ingenious experiments to which Klaproth has subjected a more considerable quantity of this ore, which was sent to him by Mr. Muller ‡, perfectly confirm the suspicions of that chemist, and of Bergman.

The Process for obtaining this Metal from its Ore.

1. A portion of the ore is gently heated, with six parts of muriatic acid; three parts of nitric acid are then to be added, and the mixture subjected to ebullition. A very considerable effervescence takes place, and the solution becomes complete.

2. The filtered solution is to be diluted with as much water as it can bear without becoming turbid, which quantity is very little. A solution of caustic pot-ash is then to be added, until the white precipitate, which is at first formed, shall disappear, and nothing but a brown deposition in flocks shall remain.

3. This last precipitate is a mixture of the oxides of gold and of iron, which may be separated by the usual methods.

4. To the alkaline solution (2), muriatic acid must be added, sufficient for the perfect saturation of the alkali; but not in excess. A white and very abundant precipitate is afforded, which, on the application of heat, falls to the bottom of the vessel in the form of a heavy powder. After washing and drying this precipitate, it is to be formed into a kind of paste, with a sufficient quantity of any fat oil; and this mass is introduced into a small glass retort, to which a receiver is loosely to be applied. In this disposition of the apparatus,

* This abstract was communicated on the part of the author, by M. Rose, à chemist of Berlin, and was translated and forwarded to the Phylomatic Society at Paris, by L. Hecht the younger. The French translation, which of course I must follow (as the original is unpublished), is inserted in the xxvth vol. of the Annales de Chimie, p. 273.

† This mineral is found in the mine called *Mariahilf*, in the *Fatzbay* mountains near *Zalethna* in *Transilvania*. See Emmerling's Elements of Mineralogy, II. 124. et seq. (or Kirwan, II. 324. N.)

‡ In the original the words are "M. de Reichenstein," which I suppose to be an oversight, and that I am correct in translating the words M. Muller de Reichenstein which occur a few lines before, on the supposition that M. Muller actually resides at Reichenstein, without deriving any titular name from that town. N.

apparatus, heat is gradually to be applied to ignition. In proportion as the oil becomes decompofed, brilliant metallic drops are obferved, fimilar to thofe in the diftillation of mercury, which line the upper part of the retort, and run down at intervals to the bottom of the veffel, immediately after which they are replaced by others. After the cooling, thefe metallic drops are found congealed, and adhering to the fides of the retort and the bottom of the veffel; and the reft of the metal, reduced in the form of a button with a brilliant furface, moft commonly prefenting a cryftallized face.

The effential Characters of this new Metal.

1. Its colour is white like tin, but inclining to a leaden grey. Its metallic fplendor is confiderable; its fracture lamellated (gerade blattrig). It is very brittle and friable, and by flow cooling it readily acquires a cryftallized furface.

2. Its fpecific gravity is 6,115.

3. It belongs to the clafs of the moft fufible metals.

4. Heated with the blow-pipe upon charcoal it burns with a flame confiderably brilliant, of a blue colour, but greenifh at the edges; it rifes totally in a grey whitifh fume, and emits a difagreeable fmell, which approaches that of radifhes. If the flame be withdrawn before the fmall portion fubjected to the heat is entirely volatilized, the remaining button preferves its fluid ftate for a long time, and, during the refrigeration, becomes covered with a radiated vegetation.

5. This metal amalgamates eafily with mercury.

6. With fulphur it forms a fulphuret of a leaden-grey colour, and radiated ftructure.

7. Its folution in the nitric acid is clear and colourlefs; when concentrated, it fpontaneoufly, in the courfe of time, affords fmall white and light cryftals in the form of needles, which poffefs the dendritic aggregation.

8. The new metal is likewife foluble in the nitro-muriatic acid: when a large quantity of water is added to a folution of this nature, the metal falls down in the ftate of oxide, in the form of a white powder, which in this ftate is foluble in the muriatic acid.

9. When a fmall quantity of this metal is mixed in the cold, with one hundred times its weight of concentrated fulphuric acid, in a clofed veffel, the fluid gradually affumes a beautiful crimfon red colour. By the addition of a fmall quantity of water, added drop by drop, the colour difappears, and the minute portion of metal which was diffolved falls down in the form of black flocks. Mere heat alfo deftroys this folution; it caufes the red colour to difappear, and difpofes the metal to feparate in the ftate of a white oxide.

10. When, on the contrary, the concentrated fulphuric acid is diluted with two or three parts of water, and a fmall quantity of nitric acid is added, this mixture diffolves a confiderable portion of the metal. The folution is clear and colourlefs, and is not decompofed by the mixture of a greater quantity of water.

11. All the pure alkalis precipitate from the acid folutions of this metal a white oxide, foluble in all the acids. The precipitate is entirely foluble by excefs of alkali. If a carbonate be ufed inftead of a pure alkali, the fame phenomenon takes place; with this difference, however, that by an excefs of the carbonate the precipitate which is formed is rediffolved in part only.

12. A very pure pruſſiate of potaſh occaſions no precipitate in the ſolutions of this metal. This is a remarkable exception to the general rule of metallic precipitations; which, however, it partakes with gold, platina, and antimony.

13. The alkaline ſulphurets, mixed with the acid ſolution, occaſion a brown or blackiſh precipitate, accordingly as the metal is combined with more or leſs of oxygen. It ſometimes happens that the colour of the precipitate perfectly reſembles kermes mineral, or the red ſulphurated oxide of antimony. When the ſulphuret of tellurium is expoſed on an ignited coal, the metal burns with a blue colour, together with the ſulphur.

14. The infuſion of the nut-gall, combined with the ſame ſolutions, affords a precipitate in flocks of an Iſabella colour.

15. Iron and zinc precipitate tellurium from its acid ſolutions in the metallic ſtate, under the form of ſmall black flocks, which reſume the metallic brilliancy when rubbed, and are fuſed into a metallic button upon an ignited coal.

16. Tin and antimony occaſion the ſame phenomenon with the acid ſolutions of the new metal. The precipitate formed by antimony proves, in the moſt ſtriking manner, that tellurium is not antimony diſguiſed, as had been ſuppoſed. The ſolution of tin in the muriatic acid, mixed with a ſolution of tellurium in the ſame acid, likewiſe produces a black metallic precipitate.

17. The oxides of tellurium, obtained from the acid ſolutions by alkalis, or from alkaline ſolutions by acids, are in either caſe reduced with a degree of rapidity approaching to detonation, by expoſure to heat upon charcoal. It burns, and is volatilized, as has already been mentioned.

18. By the application of heat, for a certain time, in a retort, this oxide of tellurium becomes fuſed, and, when cold, appears of a ſtraw colour, with a kind of radiated texture.

19. With the addition of any fat ſubſtance, the oxide of tellurium is perfectly reducible by the method before deſcribed.

The white ore of gold from *Futzbay*, aurum vel metallum problematicum, contains, in 1000 parts, tellurium in the metallic ſtate, 925,5; iron, 72,0; gold, 2,5.

The graphic gold of Offenbanya contains, in 100 parts, tellurium in the metallic ſtate, 60; gold, 30; ſilver, 10.

The mineral known by the name of the yellow ore of Nagyag contains, in 100 parts, metallic tellurium, 45,0; gold, 27,0; lead, 19,5; ſilver, 8,5; and of ſulphur a minute portion.

The mineral known by the name of the grey foliated ore of gold from Nagyag contains, in 100 parts, lead, 50; metallic tellurium, 33; gold, 8,5; ſulphur, 7,5; ſilver and copper, 1.

After the above was printed in the Annales de Chimie, the editors of that excellent work received a tranſlation of a manuſcript addreſſed by M. Klaproth to Cit. Van Mons, on their behalf. From this they extracted the following mineralogical and chemical details *:

The colour of the metallum paradoxum, of the mine of Fatzbay, is between the white of tin and the grey of lead: it has much metallic brilliancy. It is ſometimes in lumps, and then forms an aggregate of cryſtalline grains. Its texture is uſually fine or ſmall grained.

* Annales de Chimie, xxv. 327.

Its

Its matrix is compofed of quartz and marl. The fubject of the firft analyfis was taken from a piece of the compact variety detached in 1780.

The graphic gold of the mine Francifcus at Offenbanya, forming the fecond variety, is of the white colour of tin, partly inclining to the yellow of brafs: it is very brilliant, compofed of prifmatic cryftals, flat and compreffed, of which the mutual pofition affects the form of the characters of Turkifh writing, which has given rife to its empyrical name. It is ufually found between the greyifh blue argillaceous porphyry, bedded in grey quartz. The proportion of the conftituent parts of this ore vary much: the middle term has been taken.

The third variety, called the yellow ore of gold, is of a white filver colour inclining to the yellow of brafs, compact, and interfperfed with quartz and brown fpar. It is not known whether that fpecimen which prefents radiations of confiderable magnitude, and exhibits a lamellated texture and fracture, is of the fame fpecies: it is found in a mixture of quartz cryftals, and brown red fpar, and fometimes in the foliated ore.

The foliated ore, or grey foliated gold ore of Nagyag, differs, in its chemical and mineralogical characters, from the three foregoing, which are comprifed under the name of white gold ores; which has caufed it to be admitted, in the new fyftem of mineralogy, as a particular fpecies of the genus Gold. Its colour is a deep leaden grey, inclining to the iron black. It is feldom found compact, but moft commonly inferted in the form of fmall united leaves, and likewife in thin, oblong, hexahedral tables, partly accumulated in cavities. Its metallic fplendor is but moderate: its fracture moft commonly exhibits contorted leaves; it is fpeckled, and in fome fpecimens the leaves are flightly flexible. Its matrix is compofed of quartz mixed with reddifh manganefe, which it has penetrated in every direction.

Scopoli, Sage, and Ruprecht, attempted to analyfe this ore; but the difference of their refults rendered their experiments very uncertain. They had clearly afcertained the prefence of a fubftance volatile by heat; but they were deceived in taking it fometimes for arfenic, and in other inftances for antimony.

It is to M. Von Muller, at prefent director of the mines at Zalathna, that M. Klaproth afcribes the honour of having firft obferved this new metal (in the collection of Memoirs of the United Friends of Vienna, publifhed by Born). He expreffes his acknowledgment for the fpecimens he fent to him, which enabled him to prove its exiftence. He does not omit the obfervation, that Bergman, though he durft not decide concerning the true character of this metallic fubftance, did neverthelefs declare that it was not antimony.

We have feen, in the foregoing abftract, (p. 372, No. 3.) that the precipitate which is not re-diffolved by ammoniac is a mixture of gold and iron. There are, doubtlefs, feveral methods of feparating thefe two metals; but, perhaps, it may be fatisfactory to know the method which was ufed by M. Klaproth. He re-diffolved the whole in the nitro-muriatic acid, and precipitated the gold by a folution of the nitrate of mercury made without heat, which he poured gradually into the former folution, till the precipitate which fell down had changed its brown colour for white. The precipitate, being carefully collected, was reduced into pure gold.

"I fhall give," fays M. Klaproth, "in the third volume of my *Beytraege*, &c. the particulars of thefe analyfes of the gold ores of Tranfylvania which contain tellurium,

 after

after having once more repeated them, in order to determine the proportions with more certainty and precifion. The refearches of mineralogifts and chemifts will foon inform us whether tellurium is likewife met with in other places, or whether ature has exclufively appropria ted this metal to the gold mines of Tranfylvania.

SCIENTIFIC NEWS, AND ACCOUNT OF BOOKS.

American News.

IN confideration of the general utility that would refult from the citizens of the United States being enabled to procure, free from expence, an analyfis of any ores, or mineral fubftances, "The Chemical Society of Philadelphia," on the 20th of June 1797, paffed the following Refolution :

"Refolved,

"That a Committee of five Members be appointed, whofe bufinefs it fhall be to notify in the different papers of the United States, and by circular letters, that they will give an analyfis of all minerals which may be fent them."

In conformity to the above refolution, they have given notice that they will analyze any mineral which may be fent them, provided it be forwarded free of expence, and accompanied with an account of the place and fituation in which it was found.

Committee.

Thomas Smith, No. 19, North Fifth Street.
James Woodhoufe, No. 13, Cherry Street.
Samuel Cooper, No. 178, South Front Street.
Adam Seybert, No. 191, North Second Street.
John C. Otto, No. 37, North Fourth Street.

Profeffor Barton of Philadelphia, who has lately publifhed a fmall tract on the Vegetable Materia Medica of our country, is preparing for the prefs a work to be entitled "Strictures on the Arrangement of the Materia Medica, adopted by Dr. Darwin in his Zoonomia."

January 19, 1798. The *American Philofophical Society* held their annual election of officers on the firft Friday of this inftant, when the following were duly chofen :

President—The Hon. Thomas Jefferfon.

Vice-Prefidents—Nicholas Collin, D. D. Dr. Benjamin Rufh, and Dr. Cafpar Wiftar.

Treafurer—Mr. John Vaughan.

Secretaries—Samuel Magaw, D. D. Dr. Adam Seybert, Dr. J. C. James, and Mr. Samuel H. Smith.

Curators—Mr. Charles W. Peale, Dr. Benjamin S. Barton, and Mr. Robert Patterfon.

Clafs of Counfellors for three years—Mr. Jonathan B. Smith, Dr. William Currie, William Smith, D. D. and Mr. Jonathan Williams, two years from January 1798.

Aerostation.

AEROSTATION.—*Auguſt* 12, 1798.

BLANCHARD aſcended into the atmoſphere, for the forty-ſixth time, by means of an apparatus conſiſting of five balloons attached to a car, and a ſixth ſmall globe. He threw out a dog attached to a parachute, which deſcended gradually, but of whoſe fate no mention is made in the Moniteur, whence I extract this intelligence. He roſe at half paſt noon from Rouen, aſcended to the height of 2500 toiſes, and landed again at 55 minutes after two, near the village of Bazancourt, 12 leagues diſtant from the firſt place. We have no account of any remarkable obſervation made during this voyage, nor the reaſons why his apparatus was ſo complicated.

Cit. Garnerin, on the 28th of the ſame month, made his eleventh aſcenſion from Paris. His courſe for a conſiderable time was near the ground, during which he converſed with the people below. Theſe converſations ſhewed how much the earth reflected ſound; for all his words were repeated five or ſix times. He thought at firſt that it might be governed by ſome local circumſtances, which indeed is very probable with regard to the repetition. He deſcended ſeveral times to the ſame level, at diſtances of ten leagues aſunder, where he conſtantly obſerved the ſame effect. This great vibration of the air was not ſenſible to diſtances exceeding 150 or 200 toiſes. It decreaſes with the diſtance.

Does the vertical tranſmiſſion of ſound differ from that which is made in an horizontal direction? On this head, however, may be read the very entertaining account of David Frœdlichius, at the end of the 19th chapter of the firſt book of Varenius, who aſcended the higheſt eminences of Carpathas, near Keſmarkt, in Hungary. It is copied by Derham, in the firſt volume of his Phyſico-Theology.

This aëronaut was accompanied by a female Citizen, Henry. They roſe at 25 minutes after four, and deſcended at nine in the evening, at the gates of Chalons, forty leagues from Paris, and thirty-ſeven from the place whence they departed.

Eſſays Political, Economical, and Philoſophical, by Benjamin Count of Rumford. Eſſays VIII. and IX.

THESE two Eſſays have been before publiſhed in the Philoſophical Tranſactions; the one as long ago as the years 1786 and 1792, and the other in 1798. As this laſt is inſerted in our Journal, Vol. II. p. 106, it will be unneceſſary to inſert its contents in this place. Both will be highly acceptable to the poſſeſſors of the other Eſſays of the Count, to complete the collection of which theſe were wanted.

The contents of the eighth Eſſay are, Chap. I. An Account of the Inſtruments that were prepared for making the propoſed Experiments. A Thermometer conſtructed, whoſe bulb is ſurrounded by a Torricellian Vacuum. Heat is found to paſs in a Torricellian Vacuum with greater difficulty than in Air. Relative conducting Powers of a Torricellian Vacuum and of Air with regard to Heat, determined by Experiment. Relative conducting Powers of dry Air and of moiſt Air. Relative conducting Powers of Air of different degrees of Denſity. Relative conducting Powers of Mercury, Water, Air, and a Torricellian Vacuum.—Chap. II. The relative Warmth of various Subſtances uſed in making Artificial Clothing, determined by Experiment. Relative Warmth of Coverings of the ſame Thickneſs, nefs, and formed of the fame Subftance, but of different Denfities. Relative Warmth of Coverings formed of equal Quantities of the fame Subftance, difpofed in different ways. Experiments made with a view to determining how far the Power which certain Bodies poffefs of confining Heat depends on their Chemical Properties. Experiments with Charcoal, with Lamp-black, with Wood-afhes. Striking Experiments with Semen Lycopodii. All thefe Experiments indicate that the Air, which occupies the Interftices of Subftances ufed in forming Coverings for confining Heat, acts a very important part in that operation. Thofe Subftances appear to prevent the Air from conducting the Heat. An Enquiry concerning the Manner in which this is effected. This Enquiry leads to a decifive Experiment, from the refult of which it appears that Air is a perfect Non-conductor of Heat. Tnis Difcovery affords the means of explaining a Variety of interefting Phenomena in the Œconomy of Nature.

Traité de la Sphere et du Calendrier, par Rivard, 5me Edition, revue et augmentée par Jérôme Lalande, 1 vol. 8vo. avec Gravures, à Paris. A Treatife on the Sphere and the Calendar, by Rivard, revifed and augmented by Jerome Lalande, 1 vol. 8vo. with Plates.

THE modern advances in Aftronomy rendered it neceffary to make fome alterations in this Work, which poffeffes a high character for Perfpicuity and Accuracy. Cit. Lalande has corrected the Table of Latitudes and Longitudes, and added a Chapter on Time, befides making other Improvements.

Philofophy of Mineralogy. By Robert Townfon, LL.D. F.R.S. Edin. 8vo, 219 pages, with three Engravings.

THIS performance is the outline of a larger work formerly announced, and intended to have been accompanied by a collection of foffils, but which did not meet with the expected fupport. It confifts of twelve chapters. The three firft contain an introduction, with an account of the fimple elementary fubftances of which minerals are compofed, and the laws of aggregation and combination by which they are governed. Thefe are followed by an enumeration of compounds, according to Dr. Babington's excellent "Syftematic Arrangement," and four chapters refpectively treating upon Stratification, the Irregularities of the Earth's Surface, Veins, and Petrifactions. The Author then proceeds to confider the value and ufe of the external characters of Minerals ; and gives a very ample terminology, confifting of the appellations in Englifh, Latin, and German, with their correfpondent definitions, under the titles of Colour, Figure, Surface, Luftre, Texture, Structure, Fracture, and Fragments ; Tranfparency, Scratch, Score, and Soiling ; Cohefion, Adhefion, Sound, Feel, Coldnefs, Denfity, Smell, Tafte, and Friability : To which he adds the Habitudes, or Refults of Experiment. Two fubfequent chapters indicate the ufe of thefe terms in Claffification, Defcription, and Inveftigation ; with fhort Inftructions for collecting Specimens, and forming Cabinets. The concluding chapter contains a lift of near three hundred works on Mineralogy.

Proceedings

Proceedings of the Aſſociation for promoting the Diſcovery in the Interior Parts of Africa, &c.

Abſtract of Mr. Park's Travels. [Concluded from page 332.]

AFTER travelling upwards of a month, aſcending by the ſide of the Niger till it ceaſed to be navigable, he at length ſunk under his fatigues, and the difficulties of his enterpriſe; and at Kamalia, five hundred miles ſhort of any friendly country, on the Gambia, he fell into a ſevere and dangerous fit of ſickneſs. If in this ſituation had he been able to travel, great part of his way lay through a deſert. He had therefore no other reſource but to wait for the firſt caravan of ſlaves which might travel the ſame track. Such a one was expected to paſs through Kamalia at the end of three months, and the chief director reſided at the place. To him, therefore, Mr. Park applied; and for the value of one ſlave, to be paid on his ſafe arrival at the Gambia, this worthy negro, whoſe name was Karfa Taura, not only undertook to conduct him ſafe to Piſania, but offered him likewiſe the accommodation of his houſe until the time of the caravan's departure. Under this man's roof our traveller was confined to his mat (his only bed), by a ſevere and dangerous fever, for upwards of a month. Five months longer was he detained for the caravan. During this long interval, not a murmur eſcaped the lips of Karfa, nor of any of his wives, at the trouble and expence which their inmate brought upon them. To the kind attentions, the tender ſolicitude, the cheerful aſſiduity, and flowing hoſpitality, of theſe poor Pagans, Mr. Park declares that he is indebted, not only for his ſafe return to Great Britain, but alſo for the preſervation of his life; and he admits that he made his friend Karfa but an inadequate return (though the beſt in his power), by preſenting him, on their arrival at the Gambia, with double the ſum that he had originally promiſed.

During this long confinement of Mr. Park, he acquired much information reſpecting the trade in ſlaves and gold-duſt, the vegetable productions of Africa, the character of the natives, their agriculture and manufactures, their modes of living, manners, ſuperſtitions, wars, police, and government, which have never yet been competently deſcribed, and for which we muſt wait for the appearance of his work. In the mean time it may be remarked, that though the climate on the borders of the deſert is prodigiouſly hot, yet in the ſouthern diſtricts, which abound with wood and water, the climate improves, and in the mornings and evenings the air is ſerene, temperate and pleaſant. Some of the vegetable products have been noticed. To theſe may be added the Lotus, of ancient renown, affording a ſmall, yellow, farinaceous berry about the ſize of an olive, which being pounded in a wooden veſſel, and afterwards dried in the ſun, is made into excellent cakes reſembling the ſweeteſt gingerbread. Moſt of the edible roots of the Weſt Indies are likewiſe found here, together with indigo, cotton and tobacco; but neither the ſugar-cane, coffee, cacao, the pine-apple, nor a variety of other fruits, were ſeen by him, nor known to the natives. Uncultivated lands belong to the ſtate; but in other reſpects landed property is admitted, without, as it ſhould appear, any particular feodal or other limitations. Among their manufactures may be reckoned, an excellent beer made from corn; the fabrics of cotton cloth, which are dyed with indigo; the tanning of leather, which is ſtained both yellow and red; the ſmelting of iron, though imperfectly; and the caſting and working of gold.

From

From the conduct and cruelty of the wars between the petty and independent states of Africa, it appears that those who do not make slaves of their captives put them to death. With regard to religion, the Mahometans shew much zeal in teaching the Negro children to read, and avail themselves of this means to convert them from Paganism. Circumcision, which is notoriously more ancient and extensive in its prevalence than the religion of Mahomet, is practised also by the Negroes, who consider it rather as an operation of physical than religious necessity. The conviction of a future state of rewards and punishments was universally prevalent in every district visited by our traveller.

In the latter end of April 1797, the caravan being completed, and Mr. Park's health perfectly re-established, he set out from Kamalia, in company with seventy persons, of whom thirty-seven only were slaves for sale. On the 4th of June they fell in with the river Gambia; and in six days more, namely on the 10th, Mr. Park had the satisfaction to enter the house of Dr. Laidley, from which he had set out eighteen months before. On the 15th of the same month, he embarked in a slave-ship for America; which being driven by stress of weather into the island of Antigua, Mr. Park took his passage from thence in a vessel bound to Great Britain, and on the 25th of December arrived safely in London.

*** The respectable Writer of a Paper on the Secondary Foci of Lenses, is informed, that a bad sort of concave Reflectors has long since been constructed and sold in London by silvering one surface of a convex lens; and that it is probable these foci (which are easily determined from the curvatures and refractive powers) may have been neglected by optical writers, because the images are in every case more dilute and indistinct than such as are produced either by refraction, or reflection alone. That the effect has not been disregarded as a source of imperfection in lenses, may be seen by consulting our Journal, II. 233.

Mr. John Priors improved Escapement.

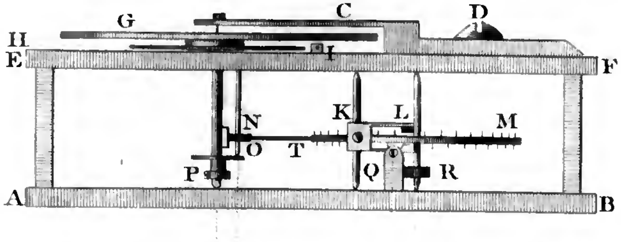


Fig. 2.

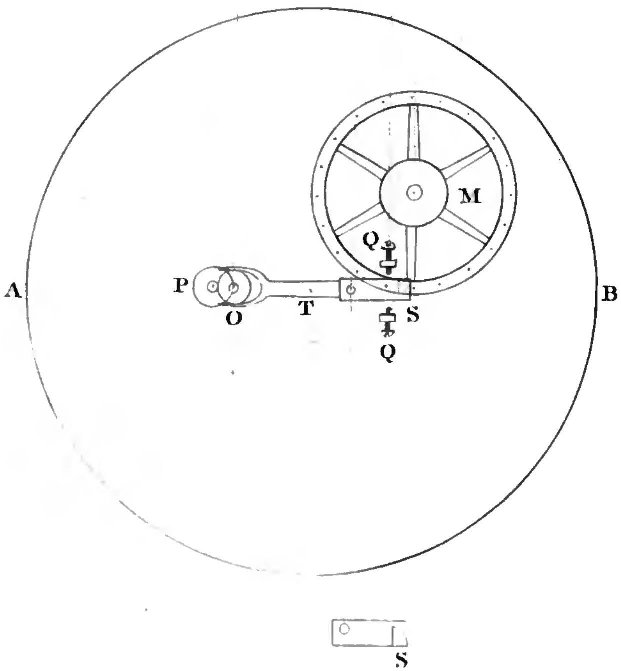


Fig. 1.

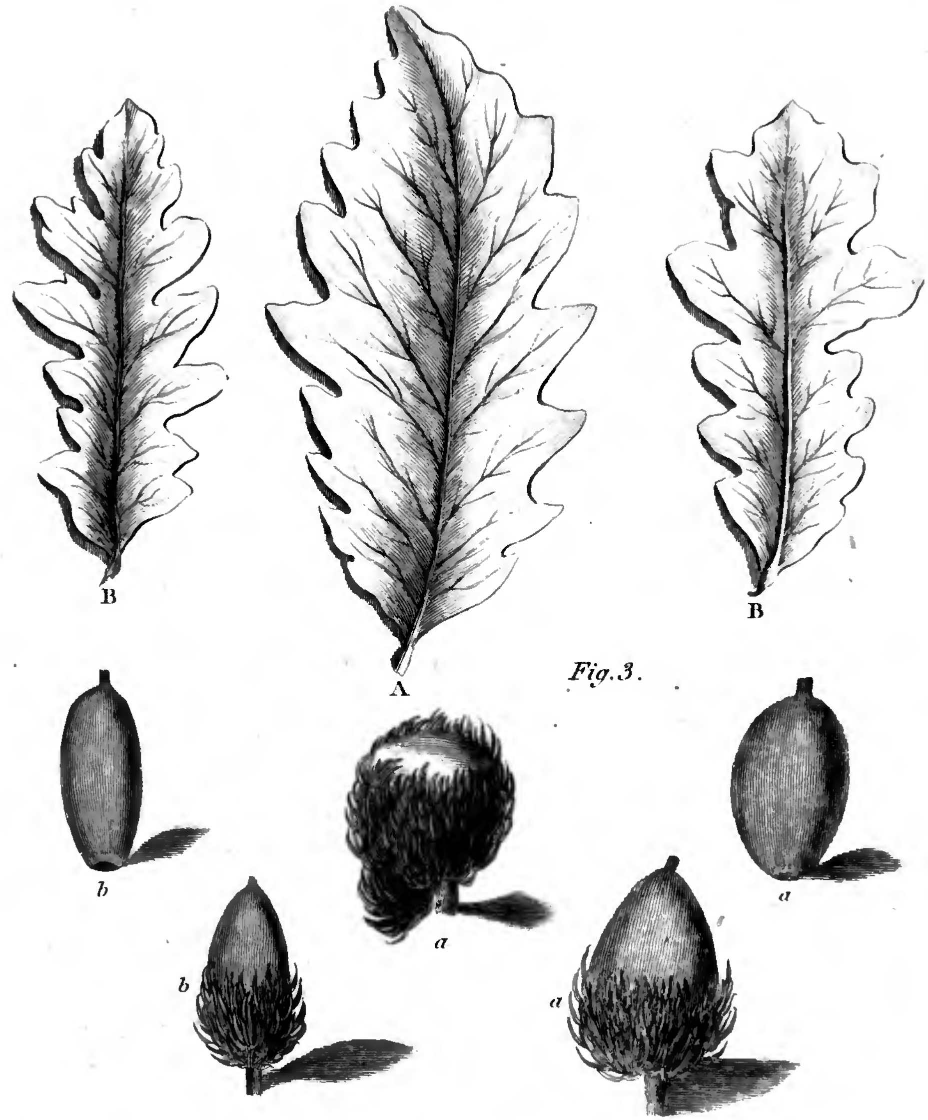
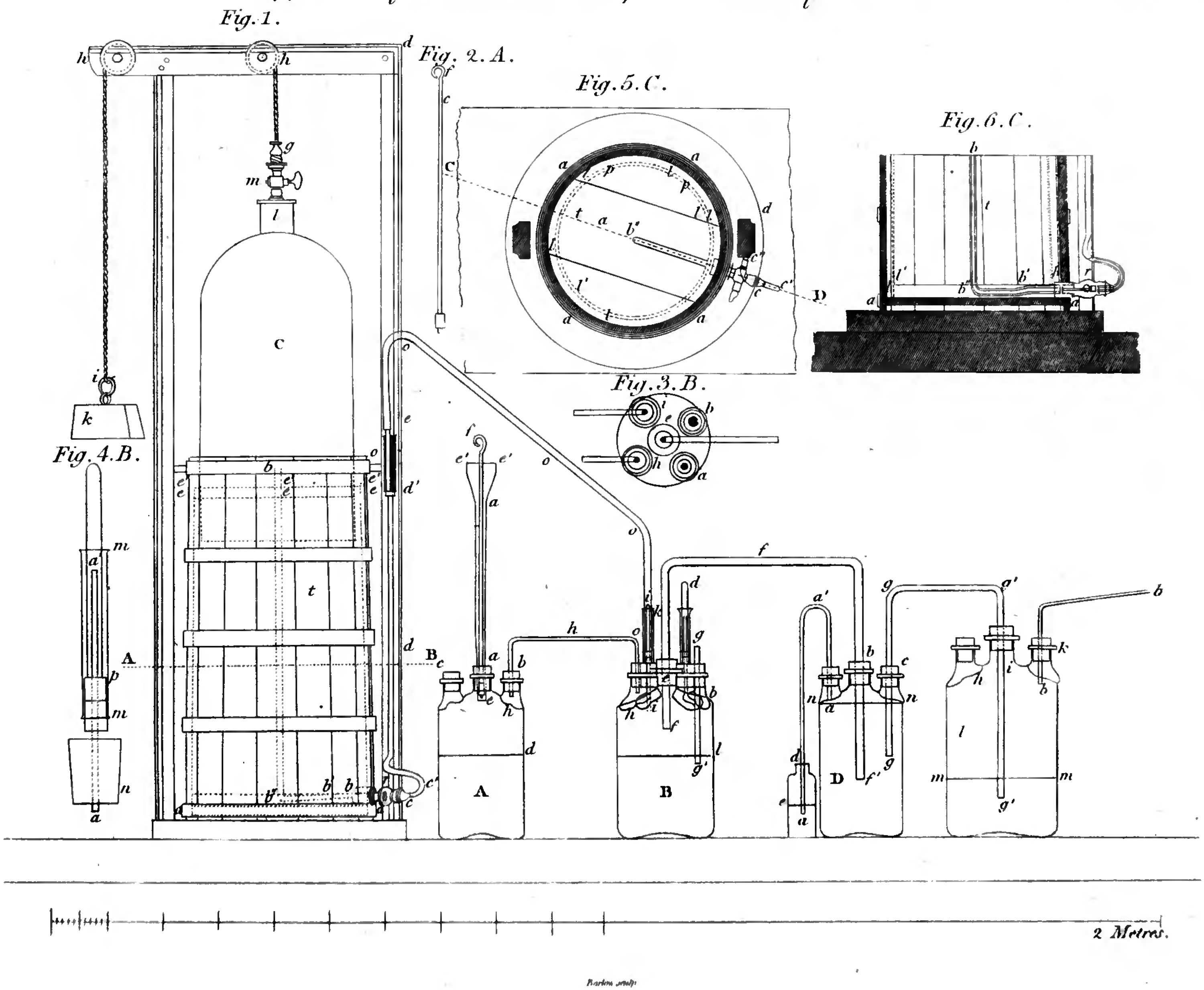


Fig. 3.

Barlow sculp.

Apparatus for the saturation of Carbonate of Potash.



A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

DECEMBER 1798.

ARTICLE I.

Memoir on the Climate of Ireland. By the Rev. WILLIAM HAMILTON, *of Favet, in the County of Donegal; late Fellow of Trinity College, Dublin; M.R.I.A. Corresponding Member of the Royal Society of Edinburgh, &c.**

IT is generally ſuppoſed that the ſeaſons in our iſland have ſuffered a conſiderable change, almoſt within the memory of the preſent generation. The winters of our climate are ſaid to have laid aſide their ancient horrors, and frequently to have aſſumed the mildneſs and vegetative powers of ſpring; while ſummer is repreſented as leſs favourable than heretofore, leſs genial in promoting vegetation, and leſs vigorous in forwarding the fruits of the earth to maturity.

It is indeed true, that in this inſtance popular opinion does not ſtand supported by the concurrent teſtimony of meteorological obſervations: there is no clear evidence derivable from them, that the preſent ſeaſons are materially different from former ones; and therefore philoſophers and meteorologiſts naturally aſcribe to the querulous diſpoſition of the farmer, the chill ſenſations of old age, or the predilection which every one feels for the cheerful days of childhood, the adoption of an opinion that ſeems ſo eaſily to flow from theſe ſources.

But let it be remembered, that the inſtruments of atmoſpherical obſervations do not extend to all the circumſtances which influence the crops of the farmer, or the ſenſations of the man. The thermometer may mark the general temperature of our climate as unchangeable; and the pluviometer may aſcertain its uſual moiſture; whilſt a clouded atmoſphere or a tempeſtuous wind ſhall mar the progreſſive maturity of harveſt, and ſhatter the languid frame of declining age.

* Iriſh Acad. VI.

Heat and cold and rains are, indeed, principals in the economy of feafons; but winds, clouds, vapours, and other circumftances rarely regiftered, often unperceived, are to be deemed at leaft ancillary in the extenfive fyftem; and may give plaufibility to popular fenfations and opinions, even without the aid of meteorological teftimony.

It is the purpofe of this paper to offer to the Academy fome obfervations relating to this interefting fubject; and to mark a few prominent events in the phenomena of our climate, which may add credibility to general report.

Of the Winds, and their Effects.

THE winds which moft ufually prevail in our latitudes blow from the weftward, for reafons unneceffary to be detailed here. Thefe winds are commonly mild in their temperature, and moift in their nature. They are from thefe properties extremely friendly to animal and vegetable life; and to them the great population of Ireland, and the uncommon fertility of its foil, may among natural caufes be afcribed.

But from whatever circumftances it has arifen, thefe winds have of late years fwept with uncommon violence over the furface of our ifland; fruftrating the ufual effects of their genial properties by the overbearing fury of their courfe; and, like Saturn, fometimes devouring the offspring to which themfelves had given birth.

Why thefe wefterly winds have ceafed to bear the character of zephyrs may admit of much curious and interefting inveftigation: at prefent I fhall be fatisfied with endeavouring to eftablifh the fact itfelf, by fuggefting to the Academy fome circumftances that feem to determine the matter with a very great degree of probability.

The effects of thefe winds are marked in vifible characters over the whole furface of the kingdom; but they are peculiarly diftinguifhable in the northern province of Ulfter; and chiefly in the extreme countries of that province, where a northerly latitude, joined to an expofed fituation on the coafts of the ocean, forms an apt ftation for obfervations, and exhibits as it were on a magnified fcale the degrees of the phenomena themfelves.

Three natural regifters of thefe effects have come within my obfervation; the trees of the country, the fands of the fea-coaft, and the tides of the ocean: of each of thefe I fhall make mention in its order.

Of the Trees.

IT is a fact extremely well eftablifhed, that the pine-tree, peculiarly that fpecies vulgarly denominated the Scotch fir, formerly grew on many of the mountains of this kingdom, and on parts of the northern and weftern coafts, exceedingly bare and open to ftorms. Vaft roots and noble trunks of this fpecies of pine have been feen and examined by me with attention, in fituations where human induftry cannot now rear a twig of the hardieft tree. On the higheft lands of the general furface of the kingdom, in the county of Weftmeath, amid the mountains of the county of Antrim, and on the naked coafts of Enifhowen and Roffes, in the county of Donegal, pine-trees have formerly arrived at an age of an hundred and twenty years, have grown to the fize of a yard in diameter, and furpaffed fifty feet in height.

There is great reafon to think that two centuries have hardly elapfed fince many of thefe trees grew in thofe fituations; and probable reafons might be adduced to limit the great period

riod of their deſtruction to the age of James the firſt of England. In thoſe reigns, rewards were held out for ſettling the kingdom, and clearing its ſurface of foreſts, which under favour of inceſſant wars and neglected tillage, during a period of eight centuries, had overſpread the face of the country*.

The harſh and furrowed bark of this pine has occurred to me in ſuch a perfect ſtate of preſervation as almoſt alone to determine its ſpecies†.

The cones have been found by me at a depth of many feet from the ſurface of the earth, in ſuch condition as almoſt to give hope of raiſing plants from their ſeed‡. Marks of the woodman's hatchet on their trunks; veſtiges of fire applied for their deſtruction; and pieces of charcoal into which many of them have been burnt §; paleings and ſmall encloſures found at the level where they have formerly grown ||.

Leathern ſhoes, wooden veſſels filled with butter and other light ſubſtances found at conſiderable depths in turf bogs**, and not likely to have deſcended through the matted texture of that ſubſtance, give additional teſtimony to the opinion that the exiſtence of theſe bogs, and of courſe that of the trees which they contain, is not of an extremely ancient date.

It is needleſs to recall the attention of the Academy to the difficulty of raiſing trees, at preſent, in many of thoſe ſituations where the ancient pine and oak of Ireland have within the period of human exiſtence flouriſhed with luxuriance.

The labours of the farmer, the reſources of wealth and information, the rewards of patriotic ſocieties, and even the liberal encouragement of the legiſlature itſelf, have in vain ſtruggled againſt the weſtern ſtorms during the latter part of the preſent century; and the planters of our age, wearied with combating the tempeſt, have generally found it neceſſary to fly from all elevated and expoſed ſituations, and to abandon the pleaſing idea of covering the nakedneſs of mountains, the ſterility of rocks, and the bleak uniformity of bogs, with the luxuriant foliage of the oak and the pine.

Of all the foreſt trees which in later times have been cultivated for general uſe, there is none higher in the eſtimation of our farmers than the aſh. It is a tree which buds late, but finally iſſues forth ſtrong and ſucculent ſhoots: ſecure by its deciduous nature from wintry blaſts, it is nevertheleſs extremely ſenſible to the efforts of ſummer ſtorms; and becomes

* "In this reign ipe-ſtaves was one of the ordinary exports of Ireland; ſo that a mighty trade was driven with them, and thouſands of trees were felled every year for this purpoſe. A multitude of iron-mills were erected; and it is incredible how much charcoal a ſingle iron-mill will conſume in one year. So that all the great woods which the maps ſhew us, on the mountains between Dundalk and Newry, are quite vaniſhed, except one tree cloſe to the highway, at the very top of the mountain, which, as it may be ſeen a great way off, therefore ſerveth travellers for a mark."——

"Yet there are ſtill great woods remaining in Dunnagall, in Tyrone, in Antrim, &c." See Nat. Hiſt. of Ireland, by Boates, Molleneux, and others, written about the middle of the laſt century.

† In Bracknahoevlin bog, county of Weſtmeath.

‡ In Lackbeg oog, near Rutland, county of Donegal.

§ Found in a bog in the liberties of the city of Londonderry.

|| In a bog near Surock, county of Weſtmeath; near Kilruſh, county of Clare; at Carniſk, near Rameltons, county of Donegal.

** Omitting other inſtances, two wooden veſſels containing butter were very lately found deep in a turf bog, in the Fews Mountains, near Ballymoire, the ſeat of Sir Walter Synnott. The veſſels were extremely inartificial, being little better than the hollow trunks of ſome large ſpecies of willow: the butter was inſipid, inodorous, colourleſs, ſomewhat reſembling unctuous white ſteatites in its touch and appearance; but its inflammability remained ſo perfect, as to admit of its being made into candles, to which uſe much of it was applied.

a faithful register of the winds of our climate, marking their most prevalent direction by the inclination of its boughs, and their violence, by the degree wherein its tender shoots or more mature branches are withered or blasted.

Half a century has scarce elapsed since this useful species of tree was generally planted, and grew luxuriantly in every part of Ireland: yet are there hardly any exposed places in the kingdom where its top branches do not now in one part or another exhibit the withered vestiges of commencing decay. Through many parts of Ulster it is a blasted tree; and in all unsheltered situations in the three northern counties of Antrim, Derry, and Donegal, the species seems fast verging toward annihilation*.

Attentive to each prognostic of the seasons, alive to every circumstance whereon the future subsistence of his numerous family in a populous country may depend, the farmer of the north sees these vestiges of the tempest with solicitude; and often taught of late to dread the effects of summer storms on his luxuriant crops of potatoes, he has become querulous from experience, and thinks he has some reason to complain that the seasons are less favourable to his hopes than formerly; that the pine-tree has not forsaken the mountains, nor the ash been blasted in his fields, without the influence of increasing tempests.

Of the Sands.

THE waste and dreary solitude of extended barren sands which almost every sea-coast exhibits, is generally passed over with rapidity, as useless to the philosopher from the incalculable fluctuation of its surface, and wearisome to the traveller by its disgusting uniformity.

Yet, in the midst of such a sterile scene, where nothing occurs to divert the path or distract the pensive mind of the traveller, if traces of human existence should suddenly become visible; if enclosures should appear, to mark the cheerful fire-side of some former villager, or the circuit of his little garden; if embattled walls or marble piers should start up amid the sands, suggesting ideas of ancient elegance and festivity; he must have a Stoic's mind, indeed, who will not venture to calculate causes, and feel interested in events, that come home so closely to the human heart.

On many parts of the coast of Ireland scenes such as these may be discovered.

At the entrance of the river Bannow, in the celebrated barony of Forth in the county of Wexford, vestiges of ruins, traced with difficulty amid the heaps of barren sand, serve to ascertain the site of a town, whose ancient respectability was marked by the royal charter, which endowed it with the privilege of sending representatives to the parliament of the nation, and whose opulence may be inferred from the streets which are registered in the revenue records of the last century †.

* In Lancashire, in England, between Garstang and Preston, the trees are also cut off by the westerly winds. See Newte's Tour, year 1791.

† "So late as the year 1626, Bannow is registered in the custom-house books of Wexford, as having four streets which paid quit-rent to the crown, viz. Lady-street, High-street, Weavers-street, Little-street, and some buildings surrounding the church. The only remains of Bannow which stand visible at this day (1786) are the walls of its church. There is not on or near the town but one poor solitary hut. The election for the representatives of the town is held on the walls of an old chimney adjoining to the church, which tumbled down piece-meal, and forms the council-table of that ancient and loyal corporation."

Private Letter of a Friend in the County of Wexford.

Amid

Amid the fands between Portrufh and Dunluce, in the county of Antrim, in the year 1783, the ruins of a village might be feen deferted by its inhabitants, who had been obliged to move further into the country.

In the year 1787, the peninfula of Hornhead, in the county of Donegal, contained veftiges of enclofures fo fmall and fo numerous as to mark the refidence of a confiderable number of human families, in a fpot which exhibited nothing but

——— " a defert, falt and bare,
" The haunt of feals and orcs, and fea-mews clang." MILTON.

Somewhat about a century ago, the peninfula of Roffgull, lying between the harbours of Sheep-haven and Mulroy, in the county of Donegal, was felected as the refidence of one of the noble families of Hamilton, titled Boyne. It is to be prefumed there was then but little apprehenfion, that the elegant edifice of that age fhould, after the fhort interval of an hundred years, ftand, like Tadmor of the Eaft, the folitary wonder of a furrounding defert.

For the age wherein it was built, and the ftyle of architecture of that day, the manfion of Rofeapenna may be called elegant. The approach was from a level green on the fhore, through a fucceffion of embattled courts and hanging terraces, rifing in order one above the other, and adorned with marble piers of no mean defign and workmanfhip.

The rear was ornamented with gardens laid out and planted in the fafhion of the laft century; and the parks and fields of the demefne feem to have been well divided and enclofed.

At prefent, every object in this place prefents to view peculiar characters of defolation. The gardens are totally denuded of trees and fhrubs, by the fury of the weftern winds: their walls, unable to fuftain the mafs of overbearing fands, have bent before the accumulated preffure, and, overthrown in numberlefs places, have given free paffage to this reftlefs enemy to all fertility. The courts, the flights of fteps, the terraces, are all involved in equal ruin, and their limits only difcoverable by tops of embattled walls vifible amid hills of fand.

The manfion itfelf, yielding to the unconquerable fury of the tempeft, approaches faft to deftruction: the freighted whirlwind, howling through every avenue and crevice, bears inceffantly along its drifted burthen, which has already filled the lower apartments of the building, and begins now to rife above the once elevated threfholds. Fields, fences, villages, involved in common defolation, are reduced to one undiftinguifhable fcene of fterile uniformity; and twelve hundred acres of land are faid thus to have been buried within a fhort period in irrecoverable ruin *.

Hence

* It would be teafing to dwell on a repetition of fimilar examples. I fhall juft mention two others: — In a fummer excurfion from College in the year 1787, paffing from Dunfanaghy to Rutland, along the weftern coaft of Donegal, I had great difficulty to difcover a houfe fituated between the river Guidore and the Roffes iflands, whither I had been directed to enquire for a guide; and after much fearch at length perceived its roof juft emerging from the fands. The owner told me that his houfe was not long built, and had at firft a confiderable tract of pafture ground between it and the fea-fhore; but that of late he was every year obliged with great labour to dig it out of the encroaching fands, and purpofed fhortly to remove it to the oppofite fhore of a lake, called Mullochdearg, which lay behind the houfe, in defpair of being able to maintain his prefent fituation.

A difputed.

Hence it appears, that interefting natural events, the confequence of ftorms, have occurred on our coafts in the latter part of the prefent century, which were not forefeen, nor even fufpected, at its commencement.

Of the Tides.

IT cannot be wondered at if thefe tempefts fhould have had confiderable effect on the tides of the ocean. The waters of that element, as long as they are unimpeded by extraneous caufes, obey the influence of the heavenly luminaries, and ebb and flow with degrees of quantity and regularity of periods which come within the reach of human calculation. But, when they are agitated by ftorms or other violent convulfions, their quantities and periods become altogether uncertain and incalculable.

Generally fpeaking, agitation of the ocean, from whatever caufe it may arife, produces increafe in the influx and reflux of its tides, as well as deviation from their calculated times; and where the movement of this extraneous influence coincides with the natural direction of the waters, the effects are vifibly diftinguifhable by the traces of inundation which attend their unufual progrefs.

As the tides of our coaft raifed in the Atlantic ocean flow in upon us from the weftward, a ftorm from that quarter invariably gives them an uncommon elevation in our harbours; and this accumulation of waters fometimes anticipates the tempeft itfelf, becoming the forerunner and prognoftic of its diftant commencement and approaching impetuofity.

Of late years thefe extraordinary influxes of the ocean have been much greater and more frequent than formerly. Every perfon on our coafts, whofe fituation has made the conftruction or prefervation of embankments againft them neceffary, knows, by painful experience, how much his labours have of late years increafed, and how impotent works formerly effectual are now found to be in repelling the increafing tides of the prefent day. Public roads encroached on; walls beaten down; ftrands lefs paffable than heretofore; meadow and tillage land oftener and more deeply inundated; all concur to prove increafing tides and frequency of ftorms on our coafts.

Thefe phenomena faithfully regiftered, extenfively and diftinctly delineated in natural characters, independent of every bias from human fyftem or prejudice, free from the uncertainty of cafual records or the locality of peculiar ftations for obfervation, feem directly to demonftrate an unufual and increafing violence in the winds of our climate during the prefent century. That thefe tempefts have chiefly borne upon us from the weftward is plain, from the fame general appearances: for, where local circumftances have not directly interfered in oppofition, the trees, fhrinking before the preffure from the ocean, have univerfally yielded to the weftern blaft. The fands have drifted, and the tides rufhed upon us from the fame quarter, evidently demonftrating the more frequent recurrence and fuperior potency of the Atlantic ftorms.

[To be continued.]

A difputed boundary in Favet, on the northern coaft of the fame country, between the tenants of one of my own glebes and the neighbouring peafants, is afcertained by a heap of iron fcoriæ in the midft of loofe and fhifting fands. Thirty or forty years ago, there ftood here the forge of the village; but no remains of it, or its fmith, are now difcoverable, except the cinders of the forge, and the rank weeds that fpring from a rich ftratum of earth, once the foil of his garden, and ftill vifible in a hillock of fand.

II. *Analyfis*

II.

Analysis of the Red Lead of Siberia; with Experiments on the new Metal it contains. By Citizen VAUQUELIN, *Inspector of Ores*, and Conservator of Chemical Products at the Mineralogical School †.*

In arctum coacta rerum naturæ majestas. PLIN.

SECTION I.—*Historical Facts.*

THE fossil known by the name of red lead was discovered in 1770, by M. Pallas, in the gold-mine of Beresof, near Ecatherineburg in Siberia, in the form of four-sided prisms, with or without pyramidal terminations, of a beautiful orange red, commonly fixed in a quartzose matrix, to which they so strongly adhere as not to be detached without difficulty.

All the specimens of this substance which are to be found in the several mineralogical cabinets in Europe were obtained from this gold-mine; which indicates, that it was formerly abundant; but it is said, that for some years past it has become very scarce, and that at present it is bought for its weight in gold, especially if pure and regularly formed. The specimens which do not possess the regular figure, or are broken into fragments, are appropriated to painting, in which art this substance is of high value for its beautiful orange yellow colour, its unchangeableness in the air, and the facility with which it can be levigated with oil. The following are the expressions of M. Pallas, who speaks of this mineral, in his Travels in the year 1770, under the article of the Gold-mine of Pischminskoi, tom. ii. page 235:—" A very remarkable red ore of lead is likewise worked at this place, which has not yet been found in any other mine of the empire, nor elsewhere. This ore of lead is weighty, of various colours; sometimes of a cinnabar red, and semi-transparent. It is fixed in long or short crystals in the clefts of quartz, and also in the bed of the mine, which is a sand-stone. It very frequently possesses, and every where when the space permits, the same thickness and prismatic form, with four plane faces and two extremities irregularly truncated. It is likewise found in small irregular contorted pyramids attached to quartz, and resembling small rubies. By reducing it to powder it affords a beautiful guhr of a deep yellow colour, which may be used in miniature painting. In all the essays of this lead ore in the laboratories of Ecatherineburg a small portion of silver has been obtained. It produces more than half its weight (valeur) in lead. Mr. Lehman could not ascertain whether this bley-spath did or did not contain silver, because his experiments were made on too small a scale to render that metal perceptible. It is difficult at present to procure the necessary quantity for trials in great, because the miners do not often work in the place where this ore is found, for want of air. In the mixed gangues of quartz in which this rare and curious mineral is formed, there are found small crystals pointed at each end, and of the colour of sulphur. They resemble native sulphur, and are considered as such by the miners; but they do not burn in the fire, nor decrepitate before the flame, like the ore of lead. This may, perhaps, be a metallic spar; but it is difficult to procure the quantity necessary

* Des mines.

† L'Ecole des Mines.—This Memoir is translated from the Journal des Mines, No. xxxiv. page 737.

ceſſary for examination. Theſe ſmall cryſtals are found on the ſand-ſtone as well as on the quartz.

"I can give no other detail reſpecting theſe minerals, which will not ſpeedily be exhauſted in this country, unleſs the veins ſhould extend themſelves to a great depth."

SECTION II.

Account of the Experiments formerly made on the Siberian Red Lead.

THE beautiful red colour, tranſparency, and cryſtalline figure, of the Siberian red lead ſoon induced mineralogiſts and chemiſts to make enquiries into its nature. The place of its diſcovery, its ſpecific gravity, and the lead ore which accompanies it, produced an immediate ſuſpicion of the preſence of that metal; but, as lead had never been found in poſſeſſion of the characteriſtic properties of this Siberian ore, they thought, with juſtice, that it was mineralized by ſome other ſubſtance; and Lehman, who firſt ſubjected it to chemical analyſis, aſſerted, in a Latin diſſertation printed at Peterſburgh in 1766 (I ſuppoſe 1786), that the mineraliſers were arſenic and ſulphur.

In 1789 Citizen Maquart undertook a long courſe of experiments, in which I had the advantage to participate, as he has been pleaſed to mention in the introduction to his work entitled *Eſſais de Mineralogie du Nord.* The object of theſe experiments was to determine the nature of the mineraliſer of red lead. We ſought in vain for the preſence of arſenic; but, by an error, ariſing from the ſtate of chemical knowledge at that time, we conſidered the red lead ore as a combination of ſuper-oxygenated lead, iron and alumine.

Since that time Bindheim affirmed, that he had found it to contain molybdic acid, iron, nickel, cobalt, and copper.

From the conſideration of theſe reſults, ſo diametrically oppoſite to each other, and under the encouraging conſideration of the immenſe progreſs of chemical ſcience ſince the renovation of its language and the rectification of its theory, and venturing likewiſe to place ſome dependance on the ſlight experience I have acquired in the art of eſſaying ſince I had the advantage of belonging to the eſtabliſhment of mines, I thought proper to ſubmit this ſubſtance to a new examination. My labours have not been without their recompenſe; and I hope to prove, in the following paragraphs, that all which has hitherto been aſſerted with regard to the mineraliſer of the Siberian red lead is entirely deſtitute of foundation; that it contains neither arſenic, as Lehman pretended; nor the molybdic acid, and the three or four metals, announced by Bindheim; nor iron nor clay, as Maquart and myſelf imagined; but a new metal, poſſeſſing properties entirely unlike thoſe of any other metal.

SECTION III.

The new Methods of Analyſis applied to the Red Lead of Siberia.

THE ſcience of chemiſtry is at preſent founded upon principles ſo certain, that, when the operator has reaſon to form a preſumption reſpecting the nature of a compound body, it is poſſible to diſcover, by mere induction, the means which are moſt likely to ſeparate its elements. Thus, from the ſuſpicion that an acid might exiſt in the compoſition of the red lead, I concluded that I might decompoſe it in the way of double affinity; and the reſult of my experiments confirms the indications of theory.

The

The first Method.

Experiment 1.—100 parts of this mineral reduced to a fine powder were mixed with 300 parts of saturated carbonate of potash; and this mixture, together with about 4000 parts of water, was boiled for an hour. I observed that, 1. as soon as the re-action of the principles began, a strong effervescence was produced, which lasted for a long time; 2. the orange colour of the lead became a brickdust red; 3. at a certain period the whole appeared to be in solution; 4. in proportion as the effervescence proceeded, a coarse powder of a dirty yellow colour was thrown down; 5, and lastly, the fluid assumed a very fine golden yellow colour.

As soon as the effervescence had entirely ceased, and the re-action appeared to have terminated, the fluid was filtered, and the metallic powder collected. After washing and drying, it was found to weigh only 78 parts; the potash had therefore taken up 22 parts.

Experiment 2.—Upon the 78 parts last mentioned I poured nitric acid diluted with 12 parts of water. A strong effervescence followed; the greatest part of the matter was dissolved; the fluid did not acquire any colour; and the undissolved residue consisted of a small quantity of lemon-coloured powder. I separated the fluid part from the residue by means of a syphon, washed the remaining powder several times, and added the waters to the former solvent. The residue, when dried, weighed only 14 parts; whence it follows, that the nitric acid had dissolved 64 parts.

Experiment 3.—I again mixed these 14 parts with 42 parts of carbonate of potash, and the requisite quantity of water. These being treated as before, afforded the same phenomena. After filtration of the liquid, it was added to the first solution. The residue, washed and dried, weighed 2 parts. It was red lead, and was neglected.

Experiment 4.—The two nitric solutions put together and evaporated afforded 92 parts of nitrate of lead, crystallised in octahedrons, perfectly white and transparent.

These 92 parts of nitrate of lead dissolved in water were precipitated by a solution of sulphate of soda. The product was 81 parts of sulphate of lead, which answer to 56,68 of metallic lead.

Experiment 5.—The alkaline solutions, which were of an orange-yellow colour, were put together. In the course of several days they deposited two parts of a yellow powder which did not contain lead. These solutions, evaporated till a pellicle was formed on the surface, afforded yellow crystals by cooling, among which was carbonate of potash not decomposed.

These crystals were dissolved in water, and the solution, together with the mother water, was mixed with the weak nitric acid till the carbonate of potash was saturated. The fluid had then a very deep orange-red colour. When mixed with a solution of muriate of tin recently prepared, it first assumed a brown colour, which afterwards became greenish. When mixed with a solution of the nitrate of lead, there was an immediate regeneration of the red lead. And, lastly, by spontaneous evaporation, it afforded crystals of a ruby red colour, mixed with crystals of nitrate of potash.

Ninety-eight parts of this mineral, decomposed as has been here related, having afforded 81 parts of sulphate of lead, 100 parts would have afforded 82,65, which are equivalent to 57,1 of metallic lead. Now, admitting, as is proved by experiment, that 100 parts of lead

 absorb,

abforb, in order to combine with acids, 12 parts of oxygen, the 57,1 of metallic lead muft contain in the red lead 6,86 of this principle; which leaves the weight of the mineralifing acid 36,4.

Experiment 6.—To verify by fynthefis the proportion of principles found by analyfis in the red lead, I diffolved 50 grains, or about 2,654 grammes, of metallic lead in nitric acid, which was divided into two equal parts. One of thefe was completely precipitated by the requifite quantity of the combination of the acid of red lead with potafh; and I obtained 43 grains, or about 2,282 grammes, of red lead as fine as the native mineral.

The other portion of the nitrate of lead, precipitated by cauftic potafh, afforded 28 grains of the white oxide of lead. So that by this fynthefis 100 parts of red lead would be compofed of 65,12 of oxide of lead, and 34,88 of acid. It gives, therefore, a difference of 1,72 lefs of acid than was deduced by the analyfis, in the mineralifing acid of the red lead; a difference which fhews as great a degree of accuracy as the chemical methods can afford.

SECTION IV.

The fecond Procefs for decompofing the Red Lead Ore.

ANOTHER method, of which the execution is no lefs eafy than the foregoing, confifts in pouring 100 parts of muriatic acid diluted with the fame quantity of water upon 100 parts of the red lead ore in powder, and occafionally agitating the mixture.

In this cafe the muriatic acid combines with the oxide of lead, and forms an infoluble falt, which falls down, while the acid of the red lead remains diffolved in the water which was before mixed with the muriatic acid. The fluid affumes a colour perfectly refembling that of the red lead before it was pulverifed. As foon as it is perceived that the muriatic acid exerts no further action on the red lead, and the precipitated muriate of lead ftill contains fome red particles, the fupernatant liquor is decanted, and a new quantity of muriatic acid, one third or one fourth of the original quantity, and diluted in the fame manner, is poured on the refidue.

The decompofition being complete, the fecond folution is decanted, and added to the firft; the refidue is wafhed with a fmall quantity of cold water, which is alfo added to the folutions before decanted off.

In this procefs the operator finds himfelf under the neceffity of obtaining his acid of red lead mixed with a certain quantity of muriatic acid, or of leaving a portion of the red lead undecompofed.

In fact, if, according to the known proportions of the component parts, no more of the muriatic acid were to be added than is neceffary for the faturation of the oxide, part of the red lead would efcape decompofition; for it appears that the acid of the red lead, at the time of its feparation, unites with a portion of the muriatic acid, which feems neceffary for its more ready folution in the water.

Of thefe two inevitable inconveniences I have therefore preferred that which leaves a fmall portion of the muriatic acid in the acid of red lead, becaufe it is eafy to feparate it.

For this purpofe the acid of the red lead, contaminated with muriatic acid, is to be diluted with a fmall quantity of water, and left at reft in a cool place for feveral days, in order that the fmall portion of muriate of lead which it may contain fhould cryftallize and fall down. The fluid is then to be filtered or drawn off by a fyphon; and to this muft be added,

added, a little at a time, the oxide of ſilver precipitated from its ſolution by lime water or by a cauſtic alkali, and well waſhed.

The oxide of ſilver unites by preference with the muriatic acid, and forms with it a white inſoluble ſalt, which falls to the bottom of the fluid. But great care muſt be taken not to add too great a quantity of the oxide of ſilver, becauſe it will unite likewiſe with the acid of the red lead, and form an inſoluble combination, which will mix with the muriate of ſilver. As ſoon, therefore, as it is obſerved that the oxide of ſilver aſſumes a purple red colour, it is a ſign that the muriatic acid is entirely ſaturated, and no more of the oxide muſt be added. It is better, however, to add too much than too little; for, in this caſe, the only danger is that of loſing a ſmall quantity of the acid of red lead; whereas, in the other, the inconvenience will be, that a portion of the muriatic acid will be left in combination with the metallic acid. When the acid of red lead, mixed with muriatic acid, is evaporated to dryneſs, a lilac-coloured powder is obtained, which becomes green by the contact of the air, and is a combination of the oxide of the new metal with the muriatic acid.

SECTION V.

The Nature and Properties of the Acid of Red Lead.

THE acid of red lead, prepared according to the foregoing inſtructions, is of an orange-red colour, with a ſharp metallic taſte. It is very ſoluble in water, and its ſolution evaporated by a gentle heat, or ſpontaneouſly by expoſure to the air, cryſtallizes in ſmall long priſms of a ruby-red colour.

Experiment 1.—Paper wetted with this acid, and expoſed for ſeveral days to the ſun's light, aſſumes a green colour, which does not change in obſcurity.

Experiment 2.—A plate of iron, of tin, or moſt of the other metals, immerſed in the ſolution of this acid, cauſes it to aſſume the ſame colour.

Experiment 3.—Ether or alcohol, boiled for a few inſtants with this ſubſtance, produces the ſame effect.

Experiment 4.—The muriatic acid, heated in a retort with this acid, whether ſolid or in ſolution, produces a lively efferveſcence. Much oxygenated muriatic acid is afforded, and the fluid aſſumes a beautiful deep green colour.

Experiment 5.—Theſe phenomena, which alſo take place when the ore of red lead is diſſolved in the muriatic acid by means of heat, having led me to preſume that the acid of red lead, by virtue of the great quantity of oxygen it contains, and the ſlight adherence it contracts with that principle, might favour the ſolution of gold in the muriatic acid, I put ſome leaves of that metal in a mixture of theſe two acids, and, by a ſlight ebullition, I actually obtained a complete ſolution of the gold.

The colour of this ſolution was green; it tinged the ſkin purple; and the ſolution of tin recently prepared occaſioned a very abundant precipitate of the ſame colour.

Experiment 6.—This acid, mixed with a ſolution of the hydro-ſulphuret of pot-aſh, is precipitated in greeniſh brown flocks.

Experiment 7.—The aqueous ſolution of the tanning principle precipitates it in flocks of a yellowiſh brown colour.

Experiment 8.—Heated by the blow-pipe upon charcoal, it boils, and leaves a green infusible substance.

Experiment 9.—When fused with the phosphoric glass, or with borax, it communicates to the vitreous globules a very fine emerald green colour.

Experiment 10.—Lastly, this acid combines with alkalis and earths, from which it disengages the carbonic acid with effervescence, and forms, with those substances, salts more or less coloured, of which the properties are described in the following section.

From the preceding results it evidently follows, that the mineraliser of the red lead ore is a true acid; that the radical of this acid is a peculiar metallic substance; for no other metallic acid yet known exhibits properties similar to the present. In fact, What metallic acid possesses a ruby colour; communicates to all its combinations red or yellow colours, more or less deep, or yields to the muriatic acid part of its oxygen, converting it into the oxygenated muriatic acid, while itself passes to the state of a green oxide, soluble in the muriatic acid? Lastly, What metallic acid is there, which forms with mercury a combination of a cinnabar red; with silver, a carmine red compound; with lead, an orange-yellow mineral; with the hydro-sulphuret of potash, an olive-green, &c.? If I am not deceived, there is none such. Therefore, notwithstanding the repugnance which I have to admit new simple bodies, a repugnance grounded on the numberless modifications which nature may give to bodies already known, and render them apparently new, I am nevertheless forced, by the great number of new characters possessed by this and by no other substance, to regard it as a metal naturally acidified, which resembles no other we are acquainted with.

This opinion will be still more confirmed by the experiments in the following paragraphs.

SECTION VI.

Combinations of the Acid of Red Lead with Alkalis, Earths, and Metallic Oxides.

THE small quantity of native red lead which I have hitherto possessed, has not allowed me to prepare any large masses of the salts which this acid is capable of forming with the alkaline, earthy, and metallic substances, in order to examine their properties with all the precision necessary to render them perfectly known. I shall confine myself, therefore, in this place, to exhibit their principal characters, such as colour, solubility in water, taste, their habitudes in the fire, &c. reserving myself to return to this object when circumstances shall be more favourable. It is well known, likewise, that a great interval of time frequently takes place between the period when the discovery of an uncommon substance has been made, and that in which all its properties are perfectly known, and that researches of this kind are usually indebted to time and opportunity for their ultimate degree of perfection.

Combination of the Acid of Red Lead with Barytes.

THE acid of red lead ore readily unites with barytes, with which it forms a salt of very sparing solubility; for, by pouring an aqueous solution of this acid into a solution of the earth, a precipitate is formed of a pale lemon-yellow colour. This salt, however, is not entirely insoluble; for the supernatant fluid still retains a slight yellow colour, though the two principles of the salt are mutually saturated. This pulverulent salt has no perceptible

taste;

taſte; it is decompoſed by the mineral acids; it gives out oxygen gas by heat, and the reſidue is an earthy maſs of a green colour.

Combination of the Acid of Red Lead with Lime.

LIME combines with the acid of red lead, and affords a ſalt which does not appear to be more ſoluble than the foregoing: for, by mixing lime-water with a ſolution of this acid, a depoſition of an orange-yellow colour is made, which is leſs abundant than with barytes; a conſequence which naturally follows, from the barytes being about twenty times more ſoluble in cold water than lime; in conſequence of which the precipitate formed with this laſt earth muſt be twenty times leſs in quantity. A large quantity of ſalt, of the ſame colour as the former, is alſo obtained by evaporation of the fluid. The ſalt formed by the combination of lime and the acid of red lead does not appear to differ from that of barytes, except in its being leſs ſoluble, and poſſeſſing different affinities and proportions of component parts.

Its habitudes with acids, and with heat, are the ſame as thoſe of the combination of barytes with the ſame acid.

(To be concluded in our next.)

III.

Information reſpecting the Earth of the Beryl; in Continuation of the firſt Memoir on the ſame Subject. By Citizen VAUQUELIN*.

I HAVE announced in my Memoir on the Earth of the Beryl, that this foſſil contains about 8 per cent. of that principle; but at the ſame time I obſerved, that I did not conſider this proportion as being very accurate, becauſe I preſumed that a certain quantity had been diſſolved by the potaſh employed to ſeparate the alumine.

I alſo announced that I had begun ſome trials, to aſcertain whether the alumine contained in the beryl was in fact contaminated by a mixture of this new earth. The reſult of theſe experiments, related in a few words, will form the ſubject of the preſent communication to the Inſtitute, together with an account of certain properties of this ſubſtance, of which I have ſince extracted a larger quantity.

Experiment 1. I put together the alumine which had been obtained from three docimaſtic quintals of beryl, which had been analyſed in the ſame number of ſeparate operations. The quantity was 63 grammes. I diſſolved them in ſulphuric acid, and, after having (bréveté) the ſolution, I ſubmitted it to evaporation to obtain the alum. The evaporation was continued until cryſtals were no longer afforded. The remaining mother water was very ſaccharine and thick.

Experiment 2. I mixed this mother water with a ſolution of carbonate of ammoniac more than ſufficient to ſaturate the acid. The mixture was repeatedly ſhaken during twenty-four hours. I perceived that the bulk of the precipitate thrown down by the firſt portions of the carbonate

* The firſt Memoir is inſerted in the preſent volume of our Journal, page 358. This continuation is tranſlated from the Annales de Chimie, xxvi. 170.

carbonate had very perceptibly decreafed. At the expiration of that time I filtered the fluid, to feparate the undiffolved part: this laft, after wafhing and ignition, weighed 5 grammes, and had all the properties of alumine.

Experiment 3.—I expofed the ammoniacal folution to the action of heat in a capfule of porcelain: as foon as the temperature was fufficiently elevated to drive off the carbonate of ammoniac, a large quantity of a white, granulated, and very voluminous earth was depofited. I continued the evaporation until all the ammoniacal falt was diffipated. I then threw the refidue upon a filter, and wafhed it with much water. This refidue, after drying by a gentle heat, was perfectly white, pulverulent, and foluble in acids, with a ftrong effervefcence. It weighed 42 grammes, which by a red heat were reduced to 25 grammes; whence it follows, that thefe 42 grammes contained 17 grammes of carbonic acid and water.

Confequently, the three quintals of beryl having afforded 24 grammes of the new earth which was not diffolved by potafh, and the alumine which thefe three quintals afforded having alfo, as has been juft obferved, 25 grammes of the fame fubftance, it is evident that each quintal of beryl contains 16,33. One hundred parts of beryl are therefore compofed of

69	parts filex,
16	earth of beryl,
13	alumine,
1	oxide of iron,
0,5	lime.
99,5	

Experiment 4.—I have obferved in my firft Memoir, that alumine diffolved in the nitric acid was precipitated by the earth of beryl. I was defirous of knowing whether the fame phenomenon would likewife take place with alum. I therefore diffolved one hundred parts of this falt in about fix hundred parts of hot water. The folution afforded cryftals of alum. Into this folution I put a certain quantity of the earth of beryl, recently precipitated from a folution by ammoniac, and well wafhed. The mixture was boiled for an hour. I foon perceived that the earth of beryl was taken up; and in proportion as the excefs of acid in the alum was faturated, there fell down a great quantity of earthy matter, in white flocks, in a very divided ftate. As foon as the decompofition appeared to me to be complete, I filtered the liquor, and collected the precipitated earth, which, when wafhed and diffolved in the fulphuric acid, afforded, with a fufficient quantity of fulphate of pot-afh, cryftals of alum perfectly octahedral.

The fluid from which the alumine had been feparated poffeffed a very faccharine tafte; and, when fubjected to evaporation, it did not afford alum. Hence we find, that the earth of beryl has a greater affinity than alumine with refpect to the fulphuric acid, as well as the nitric.

Experiment 5.—Being defirous of afcertaining ftill more effectually the difference which exifts between alumine and the earth of beryl, I diffolved ten parts of the former earth in the fulphuric acid; and after having added the neceffary quantity of fulphate of potafh, ash, I obtained, by several successive crystallizations, 90 parts of alum. I also dissolved 10 parts of the earth of beryl in the same acid, and added the same quantity of sulphate of pot-ash. I obtained only 50 parts of salt in small crystalline grains, of which I could not determine the figure. This salt is soluble in 7 or 8 parts of cold water, a quantity very insufficient to dissolve alum at the same temperature.

There cannot, therefore, remain any doubt concerning the particular nature of the earth contained in the beryl; which must henceforward be reckoned in the number of substances of this order, of which it will compose the eighth species.

It almost always happens in the sciences of observation, and even in the speculative sciences, that a body, a principle, or a property, formerly unknown, though it may often have been used, or even held in the hands, and referred to other simple species, may, when once discovered, be afterwards found in a great variety of situations, and be applied to many useful purposes.

Chemistry affords many recent examples of this truth. Klaproth had no sooner discovered the different substances with which he has enriched the science, but they were found in various other bodies; and if I may refer to my own processes, it will be seen, that after I had determined the characters of chrome, first found in the native red lead, I easily recognised it in the emerald and the ruby. The same has happened with regard to the earth of the beryl. I have likewise detected it in the emerald; in which, nevertheless, it was overlooked both by Klaproth and myself in our first analysis: so difficult it is to be aware of the presence of a new substance, particularly when it possesses some properties resembling those already known!

Though I have not yet determined with much accuracy the proportion in which this earth exists in the emerald, I think, nevertheless, that it is nearly the same as in the beryl; but I shall ascertain this point with more certainty in my second analysis. The emerald and the beryl are therefore two stones of the same nature, excepting the colouring part; and the sciences of crystallography and chemistry are here also perfectly consistent in their results.

A Table of the general Properties of the Earth of the Beryl.

1. It is white.
2. Insipid.
3. Insoluble in water.
4. Adhesive to the tongue.
5. Infusible.
6. Soluble in the fixed alkalis.
7. Insoluble in ammoniac.
8. Soluble in the carbonate of ammoniac.
9. Soluble in almost every one of the acids (except the carbonic and phosphoric acids), and forming salts of a saccharine taste.
10. Fusible with borax into a transparent glass.
11. Absorbs one-fourth of its weight of carbonic acid.
12. Decomposes the aluminous salts.
13. Is not precipitable by well-saturated hydro-sulphurets.

A Table of the specific Characters of the Earth of Beryl.

1. Its salts are saccharine, and slightly astringent.
2. It is very soluble in the sulphuric acid by excess.
3. It decomposes the aluminous salts.
4. It is soluble in the carbonate of ammoniac.
5. Is completely precipitated from its solutions by ammoniac.
6. Its affinity for the acids is intermediate between magnesia and alumine.

None of the known earths unite the six properties announced in this table.

I present to the Institute a certain quantity of this earth, and shall produce at one of its future sittings a series of combinations formed with this earth, extracted from a considerable quantity of beryl given to me by Citizen Patrin, whose zeal for the advancement of the sciences is well known to every one of their cultivators.

IV.

Observations on Electricity, Light, and Caloric, chiefly directed to the Results of Dr. PEARSON'S *Experiments on Electric Discharges through Water. By a Correspondent.*

To Mr. NICHOLSON.

SIR,

WHEN I received the 6th, 7th, and 8th Numbers of your Journal for 1797, in succession, I read Dr. Pearson's Experiments and Observations on the Gaz produced by passing Electric Discharges through Water *, with attention.

His experiments appeared to me to be well devised and conducted, and his conclusions fair and satisfactory: but his explanations of the manner in which those gazes were produced from water, and then re-converted into water by the same agent, the electric fluid, were by no means satisfactory.

That I might not be premature in my decisions against Dr. Pearson's principles or reasonings, I diverted my attention from the subject till now; but, upon the re-perusal of his papers, I still think his principles lax and indeterminate, and his reasonings, in part, unphilosophical, and to me apparently indefensible. I take the liberty, therefore, of laying the few following animadversions and objections before you:

The electric fluid, common fire, and light, those universal and general agents of nature, so far from being understood, appear to me to be not only confounded, but also to be so imperfectly considered, as to be the cause of endless confusion in every department of philosophy.

Oxygen gaz is said to be formed of oxygen and caloric:—what idea then are we to form of caloric?—Is it a simple, homogeneous principle, or is it a compound?—The general idea, or at least the common expression, seems to imply the idea of its being simply the calorific principle, and consequently uncompounded.

Oxygen may be converted into oxygen gaz by the electric fluid, or by light, as well as by fire. The electric fluid, then, imparts caloric to oxygen, and so does light.—What

* Philos. Journal I. 241. 299. 349.

ideas

ideas then are we to form of the electric fluid and of light?—Are they merely modifications of simple caloric, or are they compounds in which caloric forms a part?

If the electric fluid be a mode of existence of simple, homogeneous caloric, how are we to explain the fact *, that certain electric atmospheres repel each other, which atmospheres will attract other electric atmospheres that are also repulsive to each other?

It is incontrovertibly evident that electric atmospheres are of two distinct kinds; equally extensive, equally powerful, and mutually attractive to each other; although each is invariably repulsive to every atmosphere of the same kind as itself; and it is equally certain that they only produce fire, or take that form when they rush together, and cease to be electric. Instead, then, of supposing the electric fluid to be caloric, or partly formed of caloric, it appears to me to be more philosophical to suppose that it consists of two principles † in states of separation, which form caloric when they quit their electric states, and combine.

Is light a simple fluid ‡, or a modification of caloric?—If so, what ingenuity can explain how one homogeneous fluid, or simple fire, can, by passing through a prism, be separated into such variety of parts so permanently dissimilar to each other? and how happens it that light never produces fire but by evidently changing its mode of existence, as it ceases to be light when it takes the state and properties of fire?—This consideration again seems to lead to the conclusion that light consists of distinct and dissimilar principles, which, combining together, lose their properties as light, and constitute a fluid of very different properties and character, which we call fire, or caloric.

Nay, even if we advert to caloric itself, is it an uncompounded principle, or can any simple principle possibly perform the various parts assigned to it?—According to the present system of chemistry, caloric, homogeneous, simple caloric, destroys combinations which itself had formed; it attaches itself to particles of matter, and forms itself into repulsive spheres around them: and yet certain spheres of caloric in this state of repulsion will rapidly attract other spheres of the same caloric in similar states of repulsion! In short, caloric is hot or cold, attractive or repulsive, visible or invisible, just as occasion may serve; and, Proteus-like, it takes all shapes and forms:—we dread to meet it in Jove's thunderbolt, and court its influence in the cooling breeze!

* The facts are simply, that *bodies* attract and repel each other in certain states of electrization; but the existence of electric atmospheres, of one simple electric fluid, or of two, &c. are mere hypotheses which yet remain without proof. M. de Luc (Idées sur la Meteorologie) supposes electricity to consist of the matter of heat combined with another matter incapable of passing through glass, &c. and that the charge is produced by the condensation of this last with the transmission of heat, in the same manner as steam (water and heat) might be condensed on a pane of glass, and cause the evaporation of water, supposed to be placed on the opposite surface, by virtue of the transmitted heat. N.

† That light of every kind is emitted by the electric spark is easily seen with the prism. N.

‡ The great Leonard Euler, and others, have maintained that the sensations and effects of light are merely consequences of the vibrations of a rare and very elastic fluid. To this doctrine have been opposed the rectilinear motion of light, which does not flow into the lateral spaces, together with its reflection, refraction, and colours. Much complexity of vibration would indeed be required to account for these phenomena: but not more, perhaps, than necessarily results from the consideration of sound; its echo, which deviates little from the angle of reflection; and the harmony, melody, tone, &c. of its distinct cotemporaneous and successive impressions. N.

 Having

Having thus, in a general way, pointed out the confufion which obfcures and depreciates philofophy in confequence of employing words without ideas, and of admitting certain agents as fimple principles which probably are compounds, without deigning to confider whether they are fo or not, even though the moft palpable abfurdities attend the admiffion; permit me to make a few critical remarks upon the paper in queftion.

Dr. Pearfon explains the production of the gazes from water, by fuppofing that the denfe electric fire, at the moment of diffufion, interpofes betwixt the conftituent elements of water, and places them beyond the fphere of attraction for each other, when each ultimate particle of the oxygen and hydrogen unites with a determinate quantity of fire, and they form hydrogen gaz and oxygen gaz.

It appears, then, that electric fire forcibly deftroys the chemical union between oxygen and hydrogen; and that then a portion of it affumes the ftate of caloric, attaches itfelf to the particles of oxygen and hydrogen, and counteracts their chemical union, by keeping them diftant from each other in the ftate of gaz.

Dr. Pearfon then proceeds to explain in what manner thefe two gazes are made to recombine, and form water, by means of the electric fire or caloric. He firft points out the manner in which he fuppofes the caloric acts in thefe words:—"I conceive its agency to be merely diminifhing or deftroying the powers which counteract chemical union;" and then proceeds to apply his theory to the explanation of the production of water from hydrogen gaz, and oxygen gaz, by the agency of caloric, in thefe words:—"Accordingly, when an electric fpark, or the fmalleft particle of flame, or of an ignited fubftance, is applied to the gaz produced in the above procefs, or to the mixture of hydrogen and oxygen gaz, the ultimate particles of thefe gazes neareft to the flame are driven from it in all directions, as from a centre, by the interpofition of fire, or of caloric and light; fo that they are brought within the fphere of their chemical attraction for the ultimate particles of the gafes at a certain diftance from the centre of application of fire, which therefore unites," &c. Now, I muft take the liberty to fay, that I think this no explanation whatever. The conclufion appears to me to be drawn from premifes which neither warrant fuch conclufion, nor make it either probable or conceivable.

According to Dr. Pearfon's own theory and explanation, caloric counteracts the chemical union betwixt oxygen and hydrogen, and holds them in the ftate of gas; and, according to his own principles, he ought to have explained in what manner the caloric of an ignited body diminifhes, or deftroys, the fpheres of caloric which counteract their chemical union; inftead of which he only tells us, that caloric drives the ultimate particles of thofe gazes, neareft to it, in all directions, which, therefore, combine with other particles at a diftance; but gives no idea whatfoever of the manner in which that flame, or that caloric, flowing off from an ignited body, diminifhes or deftroys the caloric which is already attached to the particles of oxygen and hydrogen, and counteracts their chemical affinity or union. In fhort, if caloric actually is attached to the difunited particles of oxygen and hydrogen, and prevents their chemical union, nothing that Dr. Pearfon has faid tends, in the leaft, to fhow how the acceffion of ftill more caloric can either diminifh, or deftroy, the caloric already attached to them, in fufficient quantity to prevent their union.

Much might be faid upon a fubject like this; but, as I wifh not to take up too much of your time, I fhall not proceed further with my remarks at prefent. What I have already advanced

advanced is, I think, ſufficient to ſhew that Dr. Pearſon's explanation of the ſubject in queſtion is neither philoſophical nor admiſſible; and, for the credit of the doctrine which he eſpouſes, ſome more ſatisfactory explanation ſhould be given. With reſpect to Dr. Parr's theory, that light and fire repel each other, when diſengaged from matter, it appears to me too fanciful to be ſolid, and too inconſiſtent to be ſupported. If light and fire repel each other when diſengaged from matter, by its decompoſition, it is not an eaſy matter to ſhew how they, when in ſtates of freedom and conſequent repulſion, were brought together on the ſame ſubſtance at its formation: and, if oxygen and light repel each other, as he contends, why does light combine at once with the oxygen of nitric acid, and form oxygen gas, by merely placing the acid in its way? But this is not the hypotheſis I have undertaken to combat.

I offer this letter for your peruſal, and confeſs that I wiſh to ſee the ſubject impartially attended to. It is painful and humiliating to ſee how readily the moſt glaring abſurdities are overlooked, nay, even adopted as principles, and employed as indiſputable facts; and no further proof is neceſſary to point out the confuſion and abſurdity to which ſuch erroneous principles tend, than the reſult of Count Rumford's experiments and reaſonings on the production of heat by friction *; which is, that as heat thus produced by friction cannot be accounted for upon the adopted principles of chemiſtry, he ſeems inclinable to conclude, that it cannot be a material ſubſtance, and is moſt probably nothing but motion !!! Surely this is ſacrificing too much to hypotheſis, to give up the conviction of reaſon, and the teſtimony of the ſenſes, rather than ſuppoſe it poſſible that a favourite theory may be falſe. The production of heat by friction, is, no doubt, inexplicable upon the principles of M. Lavoiſier, which ought to excite the ſuſpicion of thoſe who have embraced them; for, if ſo ſimple a fact cannot be explained by thoſe principles, it is, at leaſt, probable, that they are not deſerving of that confidence which is ſo generally and implicitly placed in them. The production of heat, *ad libitum*, by friction or percuſſion between ſolid bodies, does, however, admit of an eaſy explanation, without being driven to the neceſſity of making caloric a non-entity, or heat a particular kind of motion †, propagated in ſuch a particular

* Philoſ. Journal, II. 106.

† The prodigious quantity of heat which follows from an apparently minute action, has always been conſidered as a ſtrong argument againſt the hypotheſis, that heat is mere motion; but, upon cloſe examination, it is found to apply to both theories. If a very minute portion of a large maſs of oxygen gas and carbon be *put into a ſtate of vibration* (or heated), and the combination of this firſt portion be ſucceeded by vibrations and combinations of the remaining parts of the maſs, we ſhall be led to inveſtigate the attractive and repulſive powers concerned in the phenomena, and ſhall in all probability find the taſk by no means eaſy. But will our difficulties be alleviated or removed by ſuppoſing the preſence of a third ſubſtance (caloric) in previous combination with the oxygen, which will certainly demand an exhibition of ſimilar powers to account for its tranſition? Whatever may be the ſuppoſed play of the affinities or powers *with caloric*, it ſeems probable that the mere theory might be conſtructed as well (I do not ſay better) *without it*.

It has been ſaid that latent motion (or heat) is an abſurdity, as in terms indeed it is; but this might be explained in a variety of ways. Caloric is abſorbed, or motion is accumulated, in certain proceſſes. Slight incidents, not commenſurate to the effect, ſet them free. He who ſets the catch of the pile-engine at liberty, or diſcharges a croſs-bow, or breaks the tail of Prince Rupert's drop, or communicates a ſpark to a powder-magazine, may be ſaid to give effect to latent motion, or rather to deſtroy the equilibrium of forces generated by former efforts, far ſuperior to that employed to ſet them at liberty.

 Though

particular kind of manner, that the motion of a fingle fpark of fire, directed into a powder-magazine, will not only give motion to the building itfelf, and its contents, but will alfo fhake a county!

Should this letter be thought unworthy of attention, or unfit for the public eye, I could wifh, however, that you would oblige me fo far, if convenient, as to tranfmit it to Dr. Pearfon. I have the higheft opinion of his chemical knowledge, as well as of the liberality of his principles; and though I have plainly pointed out fome parts of his writings which I think erroneous, I ftill hold him no lefs in eftimation. My only motive in making the objections and remarks which I have, is, if poffible, to arrive at truth.

I am, with grateful efteem for the valuable information which I have repeatedly received from your labours in the fields of fcience, Sir,

Your moft obedient fervant,

Nov. 16, 1798.

A conftant Reader of your valuable Publications.

Though I have ftated a few facts and obfervations, I by no means wifh to be thought a maintainer of any of the theories alluded to in the text or the notes.—None of them appear to me to be eftablifhed upon indubitable facts.

V.

An Inquiry concerning the Chemical Properties that have been attributed to Light. By BENJAMIN, *Count of Rumford, F. R. S. M. R. I. A.**

IN the fecond part of my feventh effay, (on the propagation of heat in fluids,) I have mentioned the reafons which had induced me to doubt of the exiftence of thofe chemical properties in light that have been attributed to it, and to conclude, that all thofe vifible changes produced in bodies by expofure to the action of the fun's rays, are effected, not by any chemical combination of the matter of light with fuch bodies, but merely by the heat which is generated, or excited, by the light that is abforbed by them.

As the decifion of this queftion is a matter of great importance to the advancement of fcience, and particularly to chemiftry, and as the fubject is in many refpects curious and interefting, it has often employed my thoughts in my leifure hours; and I have fpent much time in endeavouring to contrive experiments, from the unequivocal refults of which the truth might be made to appear. Though I have not been fo fuccefsful in thefe inveftigations as I could wifh, yet I cannot help flattering myfelf, that an account of the refults of fome of my late experiments will be thought fufficiently interefting to merit the attention of the Royal Society.

Having found that gold, or filver, might be melted by the heat (invifible to the fight) which exifts in the air, at the diftance of more than an inch above the point of the flame of a wax-candle, (fee my feventh effay, part II. page 350.†) I was curious to know what effect this heat would produce on the oxides of thofe metals.

Experiment No. 1. Having evaporated to drynefs a folution of fine gold in aqua regia, I diffolved the refiduum, in juft as much diftilled water as was neceffary in order that the fo-

* Philof. Tranf. 1798, p. 449. † Or Philof. Journal, II. 165.

lution

lution (which was of a beautiful yellow colour) might not be difpofed to cryftallize; and, wetting the middle of a piece of white taffeta riband, $1\frac{1}{2}$ inch wide, and about eight inches long, in this folution, I held the riband, with both my hands, ftretched horizontally over the clear bright flame of a wax candle; the under fide of the riband being kept at the diftance of about $1\frac{1}{2}$ inch above the point of the flame. The refult of this experiment was very ftriking. That part of the riband which was directly over the point of the flame, began almoft immediately to emit fteam in denfe clouds; and, in about 10 feconds, a circular fpot, about $\frac{3}{4}$ of an inch in diameter, having become nearly dry, a fpot of a very fine purple colour, approaching to crimfon, fuddenly made its appearance in the middle of it, and, fpreading rapidly on all fides, became, in one or two feconds more, nearly an inch in diameter.

By moving the riband, fo as to bring, in their turns, all the parts of it which had been wetted with the folution to be expofed to the action of the current of hot vapour that arofe from the burning candle, all thofe parts which had been fo wetted, were tinged with the fame beautiful purple colour.

This colour, which was uncommonly brilliant, paffed quite through the riband, and I found the ftain to be perfectly indelible. I endeavoured to wafh it out; but nothing I applied to it, and among other things I tried fuper-oxygenated marine acid, appeared in the fmalleft degree to diminifh its luftre. The hue was not uniform, but varied from a light crimfon to a very deep purple, approaching to a reddifh brown.

I fearched, but in vain, for traces of revived gold, in its reguline form and colour; but, though I could not perceive that the riband was gilded, it had all the appearance of being covered with a thin coating of the moft beautiful purple enamel, which, in the fun, had a degree of brilliancy that was fometimes quite dazzling.

Experiment No. 2. A piece of the riband which had been wetted with the aqueous folution of the oxide, was carefully dried in a dark clofet, and was then expofed, dry, over the flame of a burning wax candle. The part of the riband which had been wetted with the folution (and which on drying had acquired a faint yellow colour) was tinged of the fame bright purple colour as was produced in the laft-mentioned experiment, when the riband was expofed wet to the action of the heat*.

Experiment No. 3. A piece of the riband which had been wetted with the folution, and dried in the dark, was now wetted with diftilled water, and expofed wet to the action of the afcending current of hot vapour which arofe from the burning candle: the purple ftain was produced as before, which extended as far as the riband had been wetted with the folution, but no farther.

I afterwards varied this experiment in feveral ways, fometimes ufing paper, fometimes fine linen, and fometimes fine cotton cloths, inftead of the filk riband; but nearly the fame tinge was produced, whatever the fubftance was that was made to imbibe the aqueous folution of the metallic oxide.

Similar experiments, and with fimilar refults, were likewife made with pieces of riband,

* We fhall hereafter find reafon to conclude, that the fuccefs of this experiment, or the appearance of the purple tinge, was owing to the watery vapour which exifted in the hot current that afcended from the flame of the candle. R.

fine

fine linen, cotton, paper, &c. wetted in an aqueous folution of nitrate of filver ; with this difference, however, that the tinge produced by this metallic oxide, inftead of being of a deep purple, inclining to crimfon, was of a very dark orange colour, or rather of a yellowifh brown.

In order to difcover whether the purple tinge, in the experiments with the oxide of gold, was occafioned by the *heat* communicated by the afcending current of hot vapour, or by the *light* of the candle, I made the following experiment, the refult of which I conceive to have been decifive.

Experiment No. 4. A piece of riband was wetted with the aqueous folution of the oxide of gold, and held vertically by the fide of the clear flame of a burning wax candle, at the diftance of lefs than half an inch from the flame.

The riband was dried, but its colour was not in the fmalleft degree changed.

When it was held a few feconds within about $\frac{1}{8}$ of an inch of the flame, a tinge of a moft beautiful crimfon colour, in the form of a narrow vertical ftripe, was produced.

The heat which exifted at that diftance from the flame, *on the fide of it*, where this coloured ftripe was produced, was fufficiently intenfe, as I found by experiment, to melt very fine filver wire, flatted, fuch as is ufed in making filver lace.

The objects I had in view in the following experiments are too evident to require any particular explanation.

Experiment No. 5. Two like pieces of riband were wetted at the fame time in the folution, and fufpended, while wet, in two thin phials, A and B, of very tranfparent and colourlefs glafs ; the mouths of the phials being left open. Both thefe phials were placed in a window which fronted the fouth ; that diftinguifhed by the letter A being expofed naked to the direct rays of a bright fun ; while B was inclofed in a cylinder of pafteboard, painted black within and without, and clofed with a fit cover, and confequently remained in perfect darknefs.

In a very few minutes, the riband in the phial A began fenfibly to change its colour, and to take a purple hue ; and, at the end of five hours, it had acquired a deep crimfon tint throughout.

The phial B was expofed in the window, in its dark cylindrical cover, three days ; but there was not the fmalleft appearance of any change of colour in the filk.

Experiment No. 6. Two fmall parcels of *magnefia alba*, in an impalpable powder, (about half as much in each as could be made to lie on a fhilling,) were placed in heaps, in two china plates, A and B, and thoroughly moiftened with the before-mentioned aqueous folution of the oxide of gold. Both plates were placed in the fame window ; the moiftened earth in the plate A being expofed naked to the fun's rays ; while that in the plate B was exactly covered with a tea-cup, turned upfide down, which excluded all light.

The *magnefia alba* in the plate A, which was expofed to the ftrong light of the fun, began almoft immediately to change colour, taking a faint violet hue, which by degrees became more and more intenfe, and in a few hours ended in a deep purple ; while that in the plate B, which was kept in the dark, retained the yellowifh caft it had acquired from the folution, without the fmalleft appearance of change.

Experiment No. 7. A fmall parcel of *magnefia alba*, placed on a china plate, having been moiftened moiſtened with the aqueous ſolution of the oxide of gold, and thoroughly dried in a dark cloſet, was now expoſed, *in this dry ſtate*, to the action of the direct rays of a very bright ſun.

It had been expoſed to this ſtrong light above half an hour, before its colour began to be *ſenſibly changed*; and, at the end of three hours, it had acquired only a very faint violet hue.

Being now thoroughly wetted with diſtilled water, it changed colour very rapidly, and ſoon came to be of a deep purple tint, approaching to crimſon.

Experiment No. 8. A piece of white taffeta riband, which had been wetted with the ſolution, and thoroughly dried in the dark, was ſuſpended in a clean dry phial of very fine tranſparent glaſs; and the phial, being well ſtopped with a dry cork, was expoſed to the ſtrong light of a bright ſun.

After the riband had been expoſed, in this manner, to the action of the ſun's direct rays about half an hour, there were here and there ſome faint appearances of a change of its colour; but it ſhowed no diſpoſition to take that deep purple hue which the riband had always acquired, when expoſed to the light in the preceding experiments.

On taking the riband out of the phial, and wetting it thoroughly with diſtilled water, and expoſing it again, *while thus wetted*, to the ſun's rays, it almoſt inſtantly began to change colour, and ſoon became of a deep purple tint; but, though I examined the ſurface of the riband with the utmoſt care, and with a good lens, both during the experiment and after it, I could not perceive the ſmalleſt particle of *revived gold*, nor did I ſee any veſtige remaining that appeared to indicate that any had in fact been revived.

This experiment was repeated ſeveral times, and always with reſults which led me to conclude, (what indeed was reaſonable to expect,) that light has little effect in changing the colour of metallic oxides, *as long as they are in a ſtate of cryſtallization.*

The heat which is generated by the abſorption of the rays of light muſt neceſſarily, *at the moment of its generation* at leaſt, exiſt in almoſt infinitely ſmall ſpaces; and conſequently, it is only in bodies that are *inconceivably ſmall* that it can produce durable effects, in any degree indicative of its extreme intenſity.

Perhaps the particles of the oxide of gold diſſolved in water are of ſuch dimenſions; and it is very remarkable, that the colours produced, in ſome of my experiments on white ribands, by means of an aqueous ſolution of the oxide of gold, are preciſely the ſame as are produced from the oxide of that metal, by enamellers, in the intenſe heat of their furnaces.

As the colouring ſubſtance is the ſame, and as the colours produced are the ſame, why ſhould we not conclude that the effects are produced in both theſe caſes by the ſame means, that is to ſay, by the agency of heat? or, in other words, and to be more explicit, by expoſing the oxide in a certain temperature, at which it becomes diſpoſed to vitrify, or to undergo a change in regard to the quantity of oxygen with which it is combined?

But the reſults of the following experiments afford ſtill more ſatisfactory information, reſpecting the intenſity of the heat generated in all caſes where light is abſorbed, and the ſtriking effects which, under certain circumſtances, it is capable of producing.

The facility with which moſt of the metallic oxides are reduced, in the dry way, by means

means of charcoal, ſhows that, at a certain (high) temperature, oxygen is diſpoſed to quit thoſe metals, in order to form a chemical union with the charcoal, or at leaſt with ſome one of its conſtituent principles, if it be a compound ſubſtance; and hence I concluded, that gold might be revived, *in the moiſt way*, by means of charcoal, from a ſolution of its oxide in water, were it poſſible, under ſuch circumſtances, to communicate to the charcoal, and to the oxide, *at the ſame time*, a degree of heat ſufficient for that purpoſe.

To ſee if this might not be done by means of light, I made, or rather repeated, the following very intereſting experiment:

Experiment No. 9. Into a thin tube of very fine colourleſs glaſs, 10 inches long, and $\frac{6}{10}$ of an inch in diameter, cloſed hermetically at its lower end, I put as many pieces of charcoal, about the ſize of large peas, as filled the tube to the height of two inches; and, having poured on them as much of the aqueous ſolution of nitro-muriate of gold as nearly covered them, expoſed the tube, with its contents, to the action of the direct rays of a very bright ſun.

In leſs than half an hour, ſmall ſpecks of revived gold, in all its metallic ſplendour, began to make their appearance here and there on the ſurface of the charcoal; and, in ſix hours, the ſolution, which at firſt was of a bright yellow colour, became perfectly *colourleſs*, AND AS CLEAR AND TRANSPARENT AS THE PUREST WATER.

The ſurface of the charcoal was, in ſeveral places, nearly covered with ſmall particles of revived gold; and the inſide of the glaſs tube, in that part where it was in contact with the upper ſurface of the contained liquid, was moſt beautifully gilded.

This gilding of the tube was very ſplendid, when viewed by reflected light; but, when the tube was placed between the light and the eye, it appeared like a thin cloud, of a greeniſh blue colour, without the ſmalleſt appearance of any metallic ſplendour.

From the colour, and apparent denſity of this cloud, I was induced to conclude, that the gilding on the glaſs was leſs than *one millionth part of an inch* in thickneſs.

This intereſting experiment was repeated ſix times, and always with nearly the ſame reſult. The gold was completely revived in each of them, and the ſolution left perfectly colourleſs: in moſt of the experiments, however, the ſides of the glaſs were not gilded, all the revived gold remaining attached to the ſurface of the charcoal.

In two of theſe experiments, I made uſe of pieces of charcoal which had been previouſly boiled ſeveral hours in a large quantity of diſtilled water, and which were introduced *wet*, and *hot*, into the tube, and immediately covered by the ſolution, to prevent them from imbibing any air; and, in different experiments, the ſolution was uſed of different degrees of ſtrength.

I plainly perceived that the experiment ſucceeded beſt, that is to ſay, that the gold was *ſooneſt revived*, in thoſe caſes in which the ſolution was *moſt diluted:* one of the experiments, however, and which ſucceeded perfectly, was made with the ſolution ſo much condenſed, that it was nearly at the point at which it became diſpoſed to cryſtallize *.

* This agrees perfectly with the reſults of ſimilar experiments made by the ingenious and lively Mrs. FULHAME. (See her Eſſay on Combuſtion, page 124.)

It was on reading her book that I was induced to engage in theſe inveſtigations; and it was by her experiments that moſt of the foregoing experiments were ſuggeſted. R.

On examining, with a good microfcope, the particles of revived gold which remained attached to the furface of the charcoal, after it had been dried, I found them to confift of an infinite number of fmall fcales, feparated from each other; not very highly polifhed, but poffeffing the true metallic fplendour, and a very deep and rich gold colour.

The gold which attached itfelf to the infide of the glafs tube, was in the form of a ring, about $\frac{1}{10}$ of an inch wide, (badly defined however below,) and adhered to the glafs with fo much obftinacy, as not to be removed by rinfing out the tube a great number of times with water; it had, as has already been obferved, a very high polifh, when feen by reflected light.

Thofe who enter into the fpirit of thefe inveftigations, will eafily imagine how impatient I muft have been, after feeing the refults of thefe experiments, to find out whether gold could be revived from this aqueous folution of its oxide by means of charcoal, *without the affiftance of light*, and merely by fuch a degree of equal heat as could be given to it in the dark. To determine that important queftion, the following experiment was made.

Experiment No. 10.—A cylindrical glafs tube, $\frac{6}{10}$ of an inch in diameter, and 10 inches long, clofed hermetically at its lower end, and containing a quantity of a diluted aqueous folution of the oxide of gold, mixed with charcoal in broken pieces, about the fize of large peas, was put into a fit cylindrical tin cafe, which was nicely clofed with a fit cover; and the glafs tube, with its contents, fo fhut up in the dark, was expofed two hours, in the temperature of 210° of Fahrenheit's fcale.

On taking the glafs tube out of its tin cafe, I found the folution *perfectly colourlefs*, and the revived gold adhering to the furface of the charcoal.

On repeating the experiment, and ufing the folution nearly faturated with the oxide, the refult was precifely the fame; the folution being found perfectly colourlefs, and the revived gold adhering to the furface of the charcoal.

I own fairly, that the refults of thefe experiments were quite contrary to my expectations, and that I am not able to reconcile them with my hypothefis, refpecting the caufes of the reduction of the oxide, in the foregoing experiments; but, whatever may be the fate of this, or of any other hypothefis of mine, I hope and truft that I never fhall be fo weak as to feel pain at the difcovery of truth, however contrary it may be to my expectations; and ftill lefs, to feel a fecret wifh to fupprefs experiments, merely becaufe their refults militate againft my fpeculative opinions.

It is proper I fhould obferve, that the charcoal ufed in this laft-mentioned experiment had been boiled two hours in diftilled water; by which means its pores had been fo completely filled with that fluid, that the pieces of it that were ufed were fpecifically heavier than water, and funk in it to the bottom of the containing veffel.

(To be continued.)

VI.

Experiments and Obfervations on the Nature of Sugar, and of Vegetable Mucilage. By Mr. WILLIAM CRUICKSHANK, *Chemift to the Ordnance, &c.**

FROM the failure of thefe trials to convert mucilages into fomething refembling fugar, we began to fufpect that they were not fo fimple as had been generally fuppofed. With a view to throw fome light on this fubject, the following comparative experiments were made:

One ounce of powdered gum arabic was introduced into a coated glafs retort, to which a receiver with the pneumato-chemical apparatus was adapted: heat being gradually applied, there came over into the receiver 3 dr. 30 gr. of pyromucous acid, mixed with a little heavy empyreumatic oil. After the retort had been red-hot for fome time, it was removed, and the charry refiduum which it contained was found to weigh 1 dr. 46 gr. This had a greyifh colour, and burned very flowly; but when expofed to a ftrong heat in an open crucible, it left a whitifh powder, amounting to 10 grains, which was found to be lime mixed with a very fmall proportion of calcareous phofphate.

The pyromucous acid being fuper-faturated with lime, a ftrong fmell of ammonia was inftantly perceived; and a piece of paper dipped in muriatic acid being held over the veffel, copious white fumes were immediately produced. This circumftance fhews, that azote forms a conftituent part of the gum.

There were collected in the pneumato-chemical apparatus 273 oz. meafures of gas; of this 93 were carbonic acid, and the remaining 180, that fpecies of hydro-carbonate which is obtained by heat from moiftened charcoal.

Two meafures of this gas, well freed from carbonic acid, were mixed with $1\frac{1}{2}$ of very pure oxygen gas, and introduced in a ftrong glafs jar, filled with and inverted over mercury: when fired by the electric fhock, they occupied the fpace of one meafure only: lime water being admitted, the whole was abforbed, except a very fmall particle, which was found, from the nitrous teft, to be pure air. From a number of experiments we have found, that twelve meafures of oxygen gas, when united with carbon, produce ten of carbonic acid gas. Hence it follows, that the quantity of oxygen gas neceffary to the formation of carbonic acid gas muft, in this cafe, have been 1.1. meafures, or a little better; the remaining 4 muft therefore have been confumed in the production of water, and would be fufficient to faturate .8 of hydrogen, equal in weight to .048 of a grain nearly.

* Thefe Experiments, &c. are a continuation of the Refearch communicated in the firft volume of this Journal, page 337. In the fecond edition of Dr. Rollo's Treatife on Diabetes Mellitus in which that article is reprinted, and whence the above is taken, there are a few emendations in the fecond and third paragraphs, as follow: The fugar afforded $8\frac{3}{4}$ drams of acid, which required 150 grains of the alkaline folution to faturate it; the charry refidue was 5 drams; and the gas which efcaped $2\frac{1}{2}$ drams. The gum arabic afforded 7 dr. 40 gr. of acid, which required 118 grains of the folution of pot-afh to faturate it; the charry refidue was 3 dr. 45 gr.; and the gas which efcaped 5 drams. Whence the fugar yielded more pyromucous acid than the gum, in the proportion of 150 to 118.

The concluding paragraph on page 341, beginning, "*Indeed, when we reflect,*" &c. is cancelled, and the inveftigation is continued as in the text. The prefent article muft therefore be read as a continuation from vol. i. p. 341, of this Journal. N.

Now,

Now, an ounce meafure of carbonic acid gas weighs .864 of a grain, and this contains .24 of pure charcoal: hence the quantity of charcoal to hydrogen in this inflammable gas muft be as .24 to .048, or 5 to 1.

But one meafure of pure hydro-carbonate, fuch as may be obtained from the decompofition of camphor, by making its vapour pafs through a red-hot earthen tube, or from the diftillation of animal fubftances, opium, &c. requires two of pure air to faturate it; and the quantity of carbonic acid amounts to 1.45, which makes the proportion of carbon to hydrogen as 12 or 13 to 1. Hence thefe gafes differ materially, and ought not to be confounded: indeed, the difference between them is manifeft, from the manner in which they burn when mixed with common air, and brought into contact with an ignited body. Pure hydro-carbonate burns flowly, with a perfectly white flame, and never detonates: on the contrary, the compound inflammable gas juft-mentioned burns rapidly, with a reddifh blue flame, and more or lefs of a detonation. We thought it neceffary to mention this circumftance, as much confufion might be produced by applying the fame name to fubftances confiderably different. It is remarkable that æther, decompofed by heat, affords pure hydro-carbonate, whilft alcohol yields the mixed fpecies.

But to return:—An ounce of gum tragacanth was fubmitted in a coated glafs retort to a fimilar procefs, and the products collected were as follows:

	dr.	gr.
Charcoal remaining in the retort, - - -	1	45
Pyromucous acid, - - - - -	4	5
Carbonic acid gas, - - - -	78 oz. meaf.	
Hydro-carbonate, - - - -	91 ditto.	

The charcoal burned flowly with a peculiar phofphorefcent flame, and left a white refiduum of 12 grains, which was found to confift of lime mixed with a little calcareous phofphate.

The pyromucous acid being fuper-faturated with lime, the quantity of ammonia difengaged appeared to be confiderably greater than from that afforded by the gum arabic.

Having in this way difcovered lime in both fpecies of gum, we were anxious to know if this earth could be detected by reagents, without having recourfe to decompofition by heat. Accordingly, a quantity of fulphuric acid was dropped into a folution of gum arabic: after ftanding for fome hours, a number of needle-like cryftals were flowly depofited: thefe being feparated were diffolved in diftilled water; to this folution the oxalate of ammonia was added, when a copious precipitate of oxalate of lime immediately took place.

From thefe experiments, therefore, it is manifeft, that gums confift of oxygen, hydrogen, carbon, azote, and lime, with a little phofphoric acid.

An ounce of refined fugar was next introduced into a coated retort, and the pneumato-chemical apparatus applied, as in the former experiments. The products obtained were,

	oz.	dr.	gr.
Pyromucous acid, with a drop or two of empyreumatic oil,	0	4	30
Charcoal, - - - - - - -	0	2	0
Hydro-carbonate, - - - - -	119 oz. meaf.		
Carbonic acid, - - - - - -	41 ditto.		

The pyromucous acid being fuperfaturated with lime, not the leaft veftige of ammonia could be perceived. Sugar, therefore, does not contain azote; neither does it contain lime;

 for

for the charcoal, which was of a beautifully black colour, burned out completely when exposed to a strong red heat.

Being desirous of ascertaining more completely the difference between common sugar and the saccharine matter secreted by the breasts of animals, an ounce of the crystallized sugar of milk was distilled in an apparatus similar to that already described, and the products were found to be,

	oz.	dr.	gr.
Pyromucous acid, mixed with a very little empyreumatic oil,	0	6	0
Charcoal, with a little phosphate of lime, - -	0	1	0
Carbonic acid gas, - - - -	31 oz. meas.		
Hydro-carbonate, of the same nature with that obtained in the former experiments, - - -	103 ditto.		

The charcoal being burned in an open crucible, there remained about one grain, which appeared to be phosphate of lime chiefly.

The pyromucous acid was next supersaturated with lime; but the quantity of ammonia disengaged was so small that it could with difficulty be detected: there appeared, however, to be a very little.

Hence, then, it would seem that this animal sugar, contrary to what might be expected, contains hardly any azote.

It would appear also, that it contains less charcoal, and more oxygen, than common sugar.

In order to investigate still further the nature of these substances, we endeavoured to ascertain the quantity of oxalic basis or radical contained in each, or how much oxalic acid they would afford when treated with an equal proportion of the nitrous acid.

An ounce of sugar was added to six ounces of the concentrated nitrous acid, diluted with an equal bulk of water. When the action had in a great measure ceased, heat was applied, and the evaporation continued until the liquor was reduced to about an ounce by measure: after this had cooled, the crystals were separated by filtration, and the remaining fluid again evaporated, until the whole, when cold, shot into a mass of crystals, leaving only a few drops which refused to crystallize. These crystals being collected, and well dried on blotting paper, amounted to 4 dr. 20 gr. or a little better than half the weight of the sugar employed.

An ounce of gum arabic was next treated with an equal proportion of nitrous acid properly diluted: the quantity of crystals collected amounted to 3 dr. 36 gr.; but in this instance, the last crystals obtained were mixed with an insoluble white powder, which, being separated from the oxalic acid by the addition of distilled water, amounted to 6 gr. and was found to be oxalate of lime. The pure acid, therefore, did not exceed 3 dr. and a half. An equal quantity of gum tragacanth afforded, by the same process, 3 dr. 10 gr. of oxalic acid, and 10 gr. oxalate of lime.

An ounce of the sugar of milk was also treated in a similar manner, with six ounces of the concentrated nitrous acid properly diluted, and there were obtained in all 3 dr. 48 gr. These crystals, however, were mixed with a white powder, which, being but a little soluble in water, was readily separated: it amounted to 30 gr. and appeared to be the saccholactic acid of Scheele; hence the pure oxalic acid did not exceed 3 dr. 18 gr.

An

An ounce of honey yielded, by a ſimilar proceſs, 4 dr. 4 gr. of pure oxalic acid, but exhibited no ſigns of ſaccholactic acid.

The following Table will ſhew, at one view, the different products reſulting from deſtructive diſtillation, with their relative proportions, and likewiſe thoſe obtained by the nitrous acid.

Subſtances employed, 1 oz. of each.	Products obtained by Heat.				Products obtained by Nitrous Acid.	
	Charcoal, &c.	Pyromucous Acid, &c.	Carbonic Acid Gas.	Hydrocarbonate.	Oxalic Acid.	Other Subſtances.
	oz. dr. gr.	oz. dr. gr.	oz. meaſures.	oz. meaſures.	oz. dr. gr.	
Sugar, - - -	0 2 0	0 4 30	41	119	0 4 20	None.
Honey, - -					0 4 4	None.
Sugar of Milk,	0 1 0	0 6 0	31	103	0 3 18	30 grains of ſaccholactic acid.
Gum Arabic, -	0 1 36 and lime 10 0 1 46	0 3 30 with ſome ammonia.	93	180	0 3 30	Oxalate of lime, 6 grains.
Gum Tragacanth,	0 1 33 and lime 12 0 1 45	0 4 5 with ſome ammonia.	78	91	0 3 10	Oxalate of lime, 10 grains.

From theſe experiments it would appear, that ſugar conſiſts merely of carbon, hydrogen, and oxygen; and that gum differs from ſugar, not only in containing a leſs proportion of oxygen, but alſo by its combination with lime and azote; and that the ſugar of milk differs from both, as it contains the radical of the ſaccholactic acid: in other reſpects, however, it approaches very nearly to the nature of vegetable ſugar. Does the milk of carnivorous and graminivorous animals yield the ſame proportion of this acid? and is this ſugar itſelf always of the ſame nature?

From the well known facts reſpecting vinous fermentation, there is now reaſon to ſuppoſe, that no ſubſtances, but thoſe which conſiſt ſimply of carbon, hydrogen, and oxygen, are ſuſceptible of it, and that an union with a fourth changes the nature of the compound ſo much as to render this proceſs impracticable.

With a view to throw ſome light on this obſcure ſubject, the following experiments were made:

Auguſt 4, 1798.—An ounce of ſugar diſſolved in 5 ounces of water was digeſted with a little freſh ſlaked quicklime, in a moderate heat, for about 15 minutes; the ſolution was then filtered, and about 2 drachms, by meaſure, of good yeaſt added; the veſſel and mixture, weighing 17 oz. 2 dr. were introduced below a large glaſs bell.

There was placed cloſe to it, as a ſtandard, another veſſel, containing an ounce of ſugar diſſolved in 5 ounces of warm water, and to which an equal quantity of yeaſt was added. This veſſel and mixture, which weighed together 17 oz. 1 drachm, 20 grains, were likewiſe placed

placed under a glaſs bell. The temperature of the room during the whole of this experiment was rather high, and varied from 68° to 75° of Fahrenheit.

The laſt mixture began to ferment in 12 hours, and in 24 the proceſs ſeemed very briſk, much water now condenſing on the ſides of the bell. In five days the proceſs began to ſubſide; in ſeven, the yeaſt fell to the bottom, and the liquor became clear. The mixture, at this time, ſmelled ſtrongly of beer turned a little ſour, although it ſtill taſted ſweetiſh. It was ſuffered to remain until the 28th, when it was removed from the bell and weighed, and the loſs was found to exceed three drachms: it now taſted much like a mixture of ſtrong vinegar and honey. During the whole of this period, amounting to 24 days, the mixture, containing the ſugar digeſted with the lime, never ſhewed the leaſt appearance of fermentation, nor was there any moiſture condenſed on the ſides of the bell. The veſſel and mixture being now weighed, the loſs amounted to two drachms nearly; the liquor ſmelled very muſty, and had a rough aſtringent taſte, mixed with ſweetneſs, but was not in the leaſt acid. In this inſtance the yeaſt fell to the bottom very ſoon, the liquor continued more or leſs mouldy, and became, at laſt, a little mouldy at the top.

We next digeſted an ounce of ſugar, diſſolved in five ounces of water, with a little pot-aſh, and to the filtered ſolution added about two drachms of very good yeaſt. This mixture was expoſed in an open veſſel to a temperature of about 65°; and another veſſel, containing a ſolution of an equal quantity of ſugar, mixed with yeaſt, placed by it as a ſtandard.

The ſolution with the yeaſt alone began to ferment in 12 hours; but the other, containing the pot-aſh, ſhewed no ſymptoms of fermentation at the end of 12 days.

Being now in a great meaſure ſatisfied, that any fourth ſubſtance combined, although in ſmall quantity, with the three which form ſugar, would prevent fermentation, we next wiſhed to know if every compound, conſiſting of carbon, hydrogen, and oxygen, however differently theſe ſubſtances might be proportioned, were ſuſceptible of its proceſs. The ſugar of milk, from what has already been obſerved, evidently conſiſts of theſe three ſimple ſubſtances; but, from a number of its ſenſible properties, and the reſult of its analyſis by heat, as well as its containing the radical of the ſaccholactic acid, it is manifeſt that their combinations and proportions muſt be very different from thoſe in common ſugar.

We therefore mixed a ſolution of its ſaccharine matter with the uſual quantity of yeaſt, and expoſed it to a temperature ranging from 65° to 70°: in four days ſome degree of fermentation was perceptible, and in three days more, became briſk: at the end of fixteen, when its proceſs had apparently ceaſed, the liquor was examined: it now had the ſmell of cyder, but rather more of the flavour of apples; to the taſte it was very ſour, and, when added to the tincture of litmus, ſtrongly reddened it.

The acid thus procured either contained or conſiſted of common vinegar; for with the oxyde of lead it formed a ſweet ſaline maſs, compoſed evidently of ſlender priſmatic cryſtals which were not deliqueſcent.*

Hence it is manifeſt, that the ſugar of milk is at leaſt in ſome degree ſuſceptible of the vinous fermentation.

Having obſerved, in our attempts to convert gum into ſugar, that it ſeemed to run eaſily into the acid ſtate, we were anxious to know if any thing like fermentation preceded this

* See Scheele's Eſſays, page 274.

ſtate,

ftate, or if it was poffible to convert it into an acid, by mere expofure to the air, without the addition of fome fubftance containing much oxygen, as the nitrous or oxygenated muriatic acids. Accordingly a folution of gum arabic, mixed with a proper proportion of good yeaft, was introduced into an open veffel, and kept at a temperature ranging from 68° to 75° for twenty-fix days, but during this period nothing like fermentation was perceived : the mixture at laft emitted a very peculiar and offenfive fmell : the gum, however, ftill retained its natural tafte, and was not in the leaft four. In this cafe it fhould appear, that the azote, and lime, which in the gum are combined with carbon, hydrogen, and oxygen, prevented the vinous fermentation, and confequently the formation of any thing like vinegar.

It has been fuppofed, that a decoction of purely animal matter might undergo certain fpontaneous changes, and at laft become acid. In order to determine this point, about fixteen ounces of a ftrong decoction of beef were introduced into an open veffel, and kept at the temperature of about 68° or 70°. A few days after, an equal quantity of a fimilar decoction, mixed with an ounce of yeaft, was likewife expofed in an open veffel to air of the fame temperature.

At the end of five days, the decoction, without any mixture, began to fhew evident marks of putrefaction, but did not in the leaft tafte acid, nor had it ever fhewn any figns of vinous fermentation; in two days more it became extremely offenfive, accompanied with the production of ammonia.

The decoction with the yeaft did not fhew any evident figns of putrefaction until the feventh day, but there was nothing like fermentation perceived ; in two days more it became extremely putrid and offenfive, and was thrown away.

Hence it fhould appear, that neither vegetable nor animal mucilages are, when pure, in any degree fufceptible of the vinous fermentation: indeed, thefe are facts which have been fo generally admitted, that, had not a contrary opinion been lately advanced, we fhould have conceived the three laft experiments unneceffary.

The miftake has no doubt arifen from obferving the facility with which a decoction of a mixture of animal and vegetable matter runs into the acid ftate : but in this cafe the animal fubftance performs the part of yeaft only ; and it is in this way that diabetic urine fo readily ferments fpontaneoufly, and becomes vinegar.

From the preceding experiments we may draw the following conclufions :

1ft. That fugar confifts of carbon, hydrogen, and oxygen ; and may be confidered as a pure vegetable oxyde.

2d. That fugar of milk is compofed of the fame principles, but contains more oxygen and confiderably lefs charcoal.

3d. That gum differs from fugar in containing, befides carbon, hydrogen, and oxygen, both lime and azote.

4th. That vegetable farina cannot be converted into faccharine matter, without the joint action of oxygen and water ; the firft of which appears to be abforbed, and the laft decompofed, during this procefs.

5th. That when fugar is deprived of its oxygen, or combined with other fubftances, it lofes its characteriftic properties, and is no longer fufceptible of the vinous fermentation.

6th. That neither vegetable nor animal mucilages, in their pure ftate, are fufceptible of this procefs.

VII. *On*

VII.

On the Art of covering Wire Cloth with a transparent Varnish, as a Substitute for Horn; and on other Objects of public Utility. By ALEXIS ROCHON, *of the National Institute of France, &c.**

IN the progress of the present war, the marine store-houses of France were totally without the essential article of horns for lanthorns. It was impossible to substitute glass in the place of this article, on account of its brittleness, and the obvious danger which might result from that quality. In this situation of distress, the agents of the French government consulted Citizen Rochon, and directed him to make every experiment he could think of to discover a proper substitute. His attention was first directed to a memoir of the celebrated Poivre on the fabrication of lanthorns of horn by the Chinese. It is known that this industrious nation prefer horn to glass on account of its cheapness and toughness, and that they possess the art of welding this substance together with so much delicacy, that they make lanthorns of two feet diameter of astonishing transparency, and to all appearance of one single piece. It is also known that the Chinese use the horns of goats and sheep only, which they soften and split into laminæ by processes supposed to be unknown in Europe; or, perhaps, by employing a proportion of human labour and patience for that purpose which the European demand might be inadequate to repay. Citizen Rochon, who does not appear to be perfectly aware of the degree of accuracy with which the same art of splitting horn is practised in Europe, proposed, that the horns of beeves should be sawed into laminæ, and then scraped and polished; or, to which he gives the preference, that they should be laminated in boiling water.

While this active philosopher was employed at Brest in establishing a manufactory for laminating the horns of beeves, which he purposed to reduce into the state of a paste by means of pure alkali in the digester of Papin, it occurred to him, that he might supply the pressing wants of the navy by another expedient, which consisted in the application of a coating of glue upon wire cloth.

In this process, he at first tinned the iron wires of the sieve cloth he made use of, but afterwards found it more convenient, in every respect, to give it a slight coating of oil paint to preserve it from rust. The glue he made use of was afforded by boiling the clippings of parchment with the air-bladders and membranes of sea-fish; materials which he used, not from any notion that they were preferable to isinglass, but because they were the cheapest he could procure. He added the juice of garlick and cyder to his composition, in proportions which, I suppose, he did not measure, but which he found to communicate great tenacity and somewhat more of transparence than it would have possessed without them. Into this transparent and very pure glue or size he plunged his wire cloth, which came out with its interstices filled with the compound. It is requisite that the size should possess a determinate heat and consistence, concerning which, experience alone must guide the operator.

* Extracted from a memoir read to the National Institute of France the 21st Ventose, in the year VI. (March 11th, 1798), and inserted in the Journal de Physique for April 1798. The memoir contains various political and economical observations more particularly applicable to France, with general observations, which I have not thought it necessary either to transcribe or abridge; neither have I been solicitous to take the very words of my author in the parts I have abstracted. N.

When

When this prepared wire cloth is fixed in the lanthorn, it muft be defended from moifture by a coating of pure drying linfeed oil; but even in this ftate it is not fit to be expofed to the weather. The eafe with which thefe lanthorns are repaired in cafe of accident, by a flight coating of glue, is pointed out as a great advantage by the inventor, who likewife informs us that they were ufed in the expedition to Ireland as fignal lanthorns, though contrary to his wifhes. For this ufe he recommends the large plates of mica, which were then imported from Bofton. With the latter fubftance, enclofed between two pieces of very open wire cloth, he made certain fquares, 26 inches in length and 18 in width, for the light-houfe at Ufhant, which had been damaged by a flock of wild ducks, that flew through the windows and dafhed out the lights.

Citizen Rochon affirms, that lanthorns of wire cloth, prepared in his method, are much cheaper than thofe made of tin and horn; that they are very cheaply repaired, and afford a ftronger light.

He applied coarfe iron wire cloth to another ufe, which, he thinks, may be worth attention in future. He made the roof of one of his workfhops of this wire-cloth in order to avoid the danger of fire, and covered it with a flight coating of plaifter. He thinks that a compofition of lime and pounded fcales of iron would have been preferable. This coating ought not to be thicker than a flate; and he recommends that it fhould be penetrated with boiling whale oil, and painted with tar and ochre. Such a roof would afford no hold for the wind, and might, as he apprehends, be of confiderable ufe to defend buildings and fheds which require particular defence againft fire.

In the courfe of experiments made for the difcovery of a varnifh proper to defend his new lanthorns from moifture, Citizen Rochon did not employ refins or copal, which are always fomewhat friable, but a perfect folution of elaftic gum in drying linfeed oil. This varnifh or unguent having fixed the attention of Genouin, that learned chemift demanded whether it might not be practicable to ufe it in making bougies and other medical inftruments, which were alfo at that time very much wanted. From this fuggeftion, Rochon employed the Englifh machine for weaving whips to make the more confiftent part of the inftrument. He plunged this woven piece in a mixture of melted wax with a little ochre, then drew it through a wire plate to take off the fuperfluous wax, and render it perfectly fmooth; after which he applied the varnifh of elaftic gum, which completed the inftrument.

Sartori, ornamental painter at Breft, pointed out to our operator that fifh glue is preferable to parchment fize upon open wire cloth, becaufe it is more tranfparent and ftronger.

VIII.

On the Production of Nitric Acid by the Contact of Oxygen very much heated and the Air of the Atmofphere.*

PAUL, a celebrated artift at Geneva, having conftructed and improved the apparatus of Watt for the production of the gafes, was employed in obtaining by means of this apparatus the oxygen gas from the black oxide of manganefe, taking care not to clofe the apparatus till the manganefe was red-hot, in order to permit the efcape of the water and carbonic acid

* Communicated to Dr. Delametherie by J. L. Odier. Journal de Phyfique, iii. New Series, p. 464.

which this oxide always contains in its native state. While the gas was extricated in great abundance, one of the cocks of the tube through which the gas passed was accidentally opened, so that a portion of the gas, very hot and pure, was emitted into the atmosphere. At the same instant all the assistants were surprised by a manifest smell of nitric acid, and a slight fume was seen to rise from the place whence the gas had issued.

Professor Pictet, who was present, first took notice of the singularity and importance of this fact. It appears to prove, that when hot and very pure oxygen is brought into contact with the atmospheric air at the ordinary temperature, nitric acid is formed by the chemical combination of the two constituent principles of that acid; so that by causing pure and hot oxygen gas on the one hand, and atmospheric air on the other, to pass into a glass globe or any other close vessel, a great quantity of nitric acid gas would be obtained, which might be condensed and absorbed by water previously put into the vessel. Again, it is known that manganese has the property of absorbing the oxygen of the air, or of water, when deprived of its original portion by means of fire. It might therefore be practicable, by means of a determinate quantity of the black oxide of manganese, to obtain successively from the atmospheric air itself an unlimited quantity of nitric acid, which would besides have the advantage of being perfectly pure, if care was taken to purify the atmospheric air made use of in the operation*.

IX.

Analysis of the Chrysolite of the Jewellers, proving it to be Phosphate of Lime. By Citizen VAUQUELIN †.

WHEN I frequently heard from the mouth of Fourcroy, in his lectures during the last ten years, and repeated in my memoir on the new metal contained in native red lead, that, if the art of the chemist could be exercised upon the objects of natural history preserved in collections, discoveries would often be made of much utility to the advancement of that science; I did not expect that I should have so speedy an opportunity of evincing the truth of that observation to the Institute.

Naturalists have hitherto considered the chrysolite as a precious stone of the second order, and all writers have arranged it at the end of the gems properly so called. Citizen Lametherie, in his Manuel du Minéralogiste, placed it immediately after the emerald and the aqua marine; Citizen Sage, in his Chemical Analysis, has ranged it with the saphir;

* This fact appears to deserve the notice which the Author and the worthy Genevan Professor have bestowed upon it, and is certainly entitled to farther investigation. In order that the speculations at the end of the paper may be admitted or refuted, it appears necessary to determine, whether the elastic fluid which escaped was pure oxygen, or was contaminated with nitric acid. This might have been ascertained by examining the water over which the gas was collected. As the native oxide of manganese contains azote, which, as Fourcroy informs us, is driven over, for the most part, before the ignition; and as Milner and Cavendish have shewn that nitrous acid is formed by the combination of oxygen and azote at a red heat, there seems to be some ground for suspicion, that the nitrous acid in the case before us was formed by a remaining portion of azote in the manganese after the ignition took place. N.

† Read to the first class of the National Institute of France, in Brumaire, in the year VI. (Nov. 1797.) It is inserted in the Annales de Chimie, xxvi. 123. I have added the words in the title expressing the component parts of this stone. N.

and

and Wallerius has placed it between the emerald and the garnet. The account of this author is copied in the note below*.

Kirwan uses the term chrysolite to denote merely the peridot, which nevertheless differs from it greatly in the nature of its principles.

Achard of Berlin analysed a species of chrysolite in which he affirms that he found silex 0.15; alumine 0.64; lime 0.17; iron 0.1.

But the results of this analysis are so different from mine, that I strongly suspect he must have operated on a different stone from the true chrysolite. This suspicion is so much the more probable, as the name of chrysolite has been given to many different stones, particularly the peridot, the chryso-beryl, the olivine, and in general all stones which have a greenish yellow colour.

Citizen Launoy, in a journey he is at present performing in Spain to collect the objects of natural history, found in the hands of a dealer a considerable quantity of chrysolites, which he sent to Paris; and the Council of Mines having purchased part of them, directed me to submit them to analysis.

It was not long before I discovered that this fossil, which has all the external appearances of a stone, is not truly of that class; but that, on the contrary, it is a salt composed of an acid and a base which are both well known; namely, the phosphoric acid and lime.

As soon as I had obtained this first result I enquired of Citizen Hauy, whether he had compared the masses of chrysolite with those of the apatite or crystallised phosphate of lime. He answered, that he had not, but that he had among his papers the relative results of the primitive forms of both, and would immediately compare them. He found with pleasure that they did not differ in the least appreciable quantity; and this conclusion may even be drawn from the results of which he has given an abstract in his treatise published in the Journal des Mines. Citizen Hauy, therefore, discovered by geometry what I confirmed by chemical analysis; and this satisfactory agreement, between two sciences apparently so remote from each other, affords a proof of the truth and certainty of their respective principles.

I shall now proceed to relate the experiments by means of which I ascertained the nature of the principles of the chrysolite, and determined their proportions.

Experiment 1. Two hundred parts of chrysolite in crystals being subjected to the action of a strong heat for one hour, lost their yellow colour without undergoing any change of form or transparency. They then resembled rock crystal, and had lost only one two-hundredth part of their weight.

Experiment 2. One hundred parts of pulverised chrysolite were mixed with the same quantity of concentrated sulphuric acid, and about four hundred parts of distilled water; the mixture immediately became hot, and assumed the consistence of thick soup. Nearly the same quantity of water as before was added, and the whole was boiled for several hours in a matrass with a long neck. The mixture being then diluted with much water

* "Colore hæc gemma gramineo viridi flavo, seu aurantiorum, omnibus gemmis imo crystallo montano mollior, chalybe rasilis, calcinata colorum transparentiam perdit, albescens, pondere et parum diminuitur; certis circumstantiis per se liquabilis in vitrum opacum album; in eo etiam à reliquis gemmis distincta, quòd in momento fusionis eodem modo phosphorescat ut terra aluminaris, vel spathum gypsosum. Cum borace instar smaragdi in fusionibus se habet, quam tamen gravitate specificâ superat in proportione ad aquam ut 3.600, vel 3.700 : 1.000. Figura dicitur esse polygona seu quadrangularis; occurrit etiam filiciformis, rotundata, in Brasiliâ."

 was

was filtered; and the solid matter being collected, washed, and ignited in a silver crucible, weighed 116 parts. This matter, subjected to various proofs, exhibited all the characters of sulphate of lime.

The filtered liquor was evaporated to dryness in order to expel the sulphuric acid. The residue was ductile, and might be drawn out into threads as long as it preserved its heat; but it hardened by cooling like a kind of glass slightly opaque; it weighed 46 parts. These forty-six parts, dissolved in water and mixed with carbonate of ammoniac, formed an abundant precipitate which became still more abundant by heat. This precipitate separated by the filter, washed, and ignited in a silver crucible, weighed 11 parts. It was phosphate of lime not decomposed.

The fluid thus cleared of the phosphate of lime was evaporated to the consistence of a syrup. At the expiration of several days it afforded crystals in the form of a four-sided prism terminating in quadrangular pyramids, whose sides corresponded with those of the prism, and of a penetrating urinous taste. Heat decomposed this salt, ammoniac was driven off, and the acid remained in the state of a perfectly transparent glass. The glass, mixed with the powder of charcoal and strongly heated in a retort, very speedily afforded phosphorus.

It cannot be doubted, therefore, but that this pretended stone is a combination of lime and phosphoric acid; a true native phosphate of lime in crystals. Nevertheless, though I am convinced by experience of the accuracy of the result I have announced, I was desirous of making some other experiments in order that there might not be the slightest doubt upon the subject.

Experiment 3. One hundred parts of the same substance in powder were digested in muriatic acid diluted with two or three parts of water. The solution took place speedily without the least effervescence; it was clear and colourless.

This solution, evaporated nearly to dryness in order to dispel the excess of muriatic acid, was again diluted with water, and mixed with a solution of oxalic acid, till the precipitate which immediately followed was no longer produced. The filtered liquor left upon the paper a precipitate, which after washing and drying weighed 118 parts. These 118 parts of precipitate, strongly calcined in a crucible, assumed at first a black colour arising from the coal of the oxalic acid decomposed by the fire. This coal being burned off, there remained 54,28 parts of a white pulverulent substance, which was acrid, soluble in water, turned vegetable blue colours to a green, and, in a word, presented all the characters of pure lime. The fluid from which the oxalate of lime had been separated was evaporated to dryness, and assumed a black colour on account of an excess of oxalic acid which the heat had decomposed.

When the whole of this last acid appeared to have been entirely converted into carbon, the residue was dissolved in water, and the solution filtered in order to separate the carbon. The fluid part, saturated with carbonate of ammoniac, let fall a few light flocks of phosphate of lime which weighed one part. This fluid, subjected to evaporation, afforded a salt perfectly resembling that of the former experiment. It was decomposed by lime water, and was fused with the blow-pipe, with inflation, and emitted a smell of ammoniac and a yellow greenish light. The residue was a transparent glass.

Since, therefore, this experiment perfectly agrees with the former, not only with regard to the nature of the principles of the chrysolite, but also the proportions, as will immediately

diately be shewn; it would be useless to add to the number of experiments, which would convey no other information than is already known concerning the phosphate of lime.

In order to establish the proportions of phosphoric acid and lime in the chrysolite, it must be recollected, 1. that 100 parts of this substance afforded in the second experiment 116 parts of calcined sulphate of lime, which, according to Bergman, contained 48,84 of pure lime: 2. that there remained 11 parts of phosphate of lime not decomposed, which were capable of forming 14,33 of sulphate of lime, which added to the 116 give 130,33. Now if 116 contain 48,84 of lime, it is evident that 130,33 must contain 53,32. According to this experiment, therefore, 100 parts of chrysolite contain 53,32 of lime; and subtracting this from the 100, there will remain for the phosphoric acid 46,68. In the third experiment it is also seen, that 100 parts of chrysolite, dissolved in the muriatic acid, afforded by the oxalic acid 118 parts of oxalate of lime; and that these 118 parts left after calcination 54,28 parts of pure lime, which subtracted from 100, give 45,72 for the phosphoric acid.

We see, therefore, that the results of these two experiments do not differ so much as one hundredth part from each other, and that they perfectly agree with the component parts obtained by Klaproth in his analysis of the apatite, from which he obtained 55 parts of lime and 45 of phosphoric acid.

X.

Account of a singular Instance of Atmospherical Refraction. In a Letter from WILLIAM LATHAM, *Esq. F.R.S. and A.S. to the Rev. H.* WHITFIELD, *D.D. F.R.S. and A.S.**

DEAR SIR, Hastings, August 1, 1797.

ON Wednesday last, July 26, about five o'clock in the afternoon, whilst I was sitting in my dining-room at this place, which is situated upon the parade close to the sea-shore, nearly fronting the south, my attention was excited by a great number of people running down to the sea-side. Upon enquiring the reason, I was informed that the coast of France was plainly to be distinguished with the naked eye. I immediately went down to the shore, and was surprised to find that, even without the assistance of a telescope, I could very plainly see the cliffs on the opposite coast; which, at the nearest part, are between forty and fifty miles distant, and are not to be discerned, from that low situation, by the aid of the best glasses. They appeared to be only a few miles off, and seemed to extend for some leagues along the coast. I pursued my walk along the shore to the eastward, close to the water's edge, conversing with the sailors and fishermen upon the subject. They, at first, could not be persuaded of the reality of the appearance; but they soon became so thoroughly convinced, by the cliffs gradually appearing more elevated, and approaching nearer, as it were, that they pointed out, and named to me, the different places they had been accustomed to visit; such as, the Bay, the Old Head or Man, the Windmill, &c. at Boulogne; St. Vallery, and other places on the coast of Picardy; which they afterwards confirmed, when they viewed them through their telescopes. Their observations were, that the places appeared as near as if they were sailing, at a small distance, into the harbours.

* Philos. Trans. 1798, page 357.

Having

Having indulged my curioſity upon the ſhore for near an hour, during which the cliffs appeared to be at ſome times more bright and near, at others more faint and at a greater diſtance, but never out of ſight, I went upon the eaſtern cliff or hill, which is of a very conſiderable height, when a moſt beautiful ſcene preſented itſelf to my view; for I could at once ſee Dengeneſs, Dover cliffs, and the French coaſt, all along from Calais, Boulogne, &c. to St. Vallery; and, as ſome of the fiſhermen affirmed, as far to the weſtward even as Dieppe. By the teleſcope, the French fiſhing-boats were plainly to be ſeen at anchor; and the different colours of the land upon the heights, together with the buildings, were perfectly diſcernible. This curious phenomenon continued in the higheſt ſplendour till paſt eight o'clock (although a black cloud totally obſcured the face of the ſun for ſome time), when it gradually vaniſhed.

Now, Sir, as I was aſſured, from every enquiry I could poſſibly make, that ſo remarkable an inſtance of atmoſpherical refraction had never been witneſſed by the oldeſt inhabitant of Haſtings, nor by any of the numerous viſitors (it happening to be the day of the great annual fair, called Rock fair, which always attracts multitudes from the neighbouring places), I thought an account of it, however trifling, would be gratifying to you.

I ſhould obſerve, the day was extremely hot, as you will perceive by the ſubjoined rough journal of a ſmall thermometer, which was kept in the dining-room above mentioned. I had no barometer with me, but ſuppoſe the mercury muſt have been high, as that and the three preceding days were remarkably fine and clear. To the beſt of my recollection, it was high water at Haſtings about two o'clock P.M. Not a breath of wind was ſtirring the whole of the day; but the ſmall pennons at the maſt-heads of the fiſhing-boats in the harbour were, in the morning, at all points of the compaſs.

I am, &c.

WILLIAM LATHAM.

P. S. I forgot to mention, that I was a few days afterwards at Winchelſea, and at ſeveral places along the coaſt; where I was informed the above phenomenon had been equally viſible. I ſhould alſo have obſerved, that when I was upon the eaſtern hill, the cape of land called Dengeneſs, which extends nearly two miles into the ſea, and is about ſixteen miles diſtant from Haſtings, in a right line, appeared as if quite cloſe to it; as did the fiſhing-boats, and other veſſels, which were ſailing between the two places: they were likewiſe magnified to a great degree*.

* On this intereſting ſubject ſee Mr. Huddart's Obſervations on Horizontal Refractions, Philoſ. Journal, I. 145. Ellicot, on the Phenomenon of Looming, I. 152. The *Fata Morgana*, or Appearance of Figures in the Sea and Air, before Reggio, deſcribed, I. 225. Mudge, in the Philoſ. Tranſ. 1795, p. 586, 587. Smeaton's Account of Eddyſtone Light-Houſe, p. 191. Hutton's Dictionary, II. 352. Theſe appearances are much more frequent and general than has uſually been ſuppoſed. I have been credibly informed, that the *Fata Morgana* has been ſeen from Broad Stairs, in Kent; and that the elevation and inverſion of terreſtrial objects is commonly or frequently obſervable (in ſummer, I ſuppoſe), through a teleſcope, over level ground, if the eye of the obſerver be not much elevated. In the curious inſtance related above by Mr. Latham, it does not ſeem probable that the French coaſt was enlarged, but that it was only elevated by the refraction. At all events, the objects could ſcarcely have ſuffered any alteration of the horizontal angles or bearings. N.

STATE

STATE *of the* THERMOMETER *at HASTINGS, during the Month of* JULY 1797.

1797.	Therm.	Time.	Wind.	Weather.
July 1.	64	10 A. M.	SW	Windy. Fair.
2.	64	10	SW	Windy. Fair.
3.	62	10	SW	Rain. Windy.
4.	62	10	SW	Fair. Windy.
5.	61	10	SW	Rain. Windy.
6.	60	10	SW	Rain. Windy.
7.	61	10	W	Rain. Windy.
8.	62	10	NW	Fine.
	66	5 P. M.	NW	Fine.
9.	66	10 A. M.	SW	Fine.
10.	67	10	N afterw. SW	Fine.
11.	65	10	SW	Foggy all day.
12.	63	10	SW	Fine.
13.	72	10	SW	Fine.
14.	76	10	W	Fine.
	68	12	W	Fine.
15.	72	10	W	Fine.
16.	72	10	N	Fine.
	78	7 P. M.	E	Storm of Wind. Lightning.
17.	73	10 A. M.	W	Fine.
18.	70	10	W	Fine. Showers in the Night.
19.	67	10	WSW	Fine. Windy.
20.	67	10	SW	Rain. Windy.
21.	65	10	SW	Fine. Windy.
22.	61	10	S	Rain.
23.	65	10	S	Fine.
24.	66	10	S	Fine.
25.	66	10	SW	Fine.
26.	68	10	SW	Fine. Dead calm all day.
	76	5 P. M.	SW	Fine.
27.	72	10 A. M.	SW	Fine.
28.	70	10	S	Fine.
29.	72	10	E	Fine.
30.	70	10	SW	Rain.
31.	69	10	S	Fine. Windy.

XI.

An Account of Electrical Machines of considerable Power, in which Silk is used instead of Glass. W. N.

ELECTRICITY has been, in most practical cases, excited or collected by the friction of various substances against each other. In this process, which has not yet been explained by reference to simpler phenomena, it is a known condition, that one at least of the substances rubbed must be a non-conductor. Resin, lack, silk, baked wood, and above all glass, are the bodies which have hitherto been used. The durability and unchangeable nature of glass*, and its being very little if at all affected by the atmospherical variations, are undoubtedly the causes why it has obtained the preference. Its brittleness, and the great expence of large plates or cylinders, are certainly among those reasons why philosophical operators should be desirous of a substitute of less cost and danger.

Dr. Ingenhousz, the inventor of the plate machine, made a variety of experiments for this purpose. Pasteboard thoroughly dried and heated, and then soaked and varnished with a solution of amber in linseed oil, formed plates which were strongly electrified when rubbed with a cat's skin or hare's skin. He tried baked wood boiled in linseed oil, but with less success. A cylinder of strong silk velvet, formed by stretching that substance upon two circular wooden disks, was found to afford considerable electrical force when caused to revolve against a cushion covered with hare's skin†. And lastly, the same philosopher contrived a portable apparatus for charging a jar by means of a varnished silk ribband, exposed to the friction of a rubber attached to the external coating, while the opposite electricity of the silk was taken off by a metallic part communicating with the inside‡.

It was at the beginning of 1784 that M. Walckiers de St. Amand undertook to construct a machine, in which a piece of silk was made to revolve incessantly, and pass between two pair of rubbers. He made one of small dimensions, and afterwards a larger in which the silk was twenty-five feet in length, and five feet broad. In the following year a machine of the same kind was constructed by M. Rouland, professor and lecturer in natural philosophy in the university of Paris §. As no accounts of either of these have been published in this country ||, and the advantages and effects described by the authors appear

* Mr. C. Cuypers of Delft affirms that glass becomes harder, and fitter for electrical purposes, by long exposure to the warm air of a room; and Mr. Birch, of Essex-street, in a very extensive electrical practice, found that glass cylinders lose their power by long use, so as to become of no value; but he ascribes this change to the use of the aurum musivum on the cushion.

† Bakerian Lecture, Phil. Trans. 1779.

‡ Nouvelles Expériences, &c. sur la Physique, par J. Ingenhousz, F.R.S. &c. Paris, 1785.

§ Description des Machines electriques à taffetas, par M. Rouland, &c. Amsterdam and Paris 1785, octavo, 35 pages, with one plate, of which Pl. XVIII. is a copy. —The Report of the Parisian Academy on the Machine of M. Walckiers, dated 25th March 1784, was separately printed in 29 pages octavo, with a coloured plate. The Report is copied in M. Rouland's pamphlet.

|| Some time after the construction of these machines, Mr. Edward Nairne, of Cornhill, whose researches in this and other practical departments of science are well known, received an order to construct one of the same form, but found it impossible by any adjustment to prevent the silk from running totally to one or the other end of

appear to be confiderable, I have thought it of advantage to infert the defcription of the latter in this place.

A, B, Pl. XVIII. is a wooden table four feet and a half long, two feet nine inches wide, and fomewhat more than an inch and a half thick: its feet are 18 inches long. Upon this table are faftened by ftrong wooden fcrews, *a b c d*, two crofs pieces, each nine inches broad, which carry the uprights C, D, E, F, which laft are 27 inches in height. At about two-thirds or more of the height of thefe uprights, there are cut notches of an inch fquare each, in which the axes of the two cylinders G and H turn freely. Thefe axes are parallel to the table and to each other, and are kept in their place by clamps of wood fcrewed over them. The cylinders G and H are formed of light wood glued together, and covered at the ends by a circular piece, whofe rounded edges arife half an inch above the furface of the cylinders themfelves. Their diameter is eight inches; the axes are of box wood, and are lefs than an inch in diameter, having a fhoulder which prevents the ends of the cylinders from touching the uprights when turned round; and laftly, the cylinders are covered with ferge.

The handle is copper, its radius being fix inches long.

K, L, is a piece of taffety covered with oily and refinous matter, of the fame kind as is ufed in France in the conftruction of air-balloons, which, M. Rouland fays, renders the filk very electrical: the breadth of the filk is nearly one inch lefs than the length of the cylinders, and it is wrapped round them with its ends fewed together.

The whole breadth of the filk is taken hold of or pinched between two flattened tin tubes oppofite each other at M, and two of the fame kind at N: thefe are the rubbers, and may be made to prefs againft each other, more or lefs ftrongly, by means of fcrews. They are retained by ftrings of filk faftened to the four uprights of the machine. *v v* are two brafs chains hooked upon the rubbers, and communicating with the earth; *o p* and *q r* are four pieces of taffety, prepared in the fame manner as the principal piece, fewed in the direction of their length to the rubbers, and faftened to each other by their correfponding corners by means of threads of filk. The metallic tubes or rubbers are covered with cat's fkin.

S reprefents the conductor. It is a cylinder of brafs three inches in diameter, 36 inches in length, including the balls at the end, whofe diameters are four inches: one of thefe balls has a ring, *t*, above it, which ferves to form a communication between the conductor S and any other conductor.

The upper and lower parts of this cylindrical prime conductor are armed with two plates of brafs *y y*, whofe length is equal and correfpondent to the breadth of the taffety, which is 26 inches, and 132 inches or 11 feet long: the edges of the plates are about half an inch diftant from the filk, and ferve inftead of the metallic points that were ufed by M. Walckiers, but rejected by M. Rouland, becaufe they were apt to ftick into the filk and damage it.

of the cylinders. The inventors have given no inftructions to obviate this defect. I have no doubt but that they made their rollers gradually largeft in the middle. I have feen a machine for folding woollens, invented and made by Mr. Rehe, of Shoe-lane, in which this difficulty was removed in fome leading rollers, by making them in the form of two very acute fruftums of cones, joined at the middle by the larger bafe.—See Phil. Journal, I. 23.

 The

The conductor S is suspended by silk strings, fastened to the uprights of the machine by the hooks and rings *i i*: its situation is parallel to the cylinders G, H, and equidistant from each. The action of this machine is as follows: The cylinder H is moved rapidly on its axis by means of the handle, and the cylinder G moves of course in the same direction on the two extremities of its axis, provided the taffety K, L, be properly stretched. This tension is easily obtained; because the cross pieces to which the uprights C, D, and E, F, are fixed, may be moved nearer or further from each other, and fastened by means of the screws *a b* and *c d*, which pass through holes cut in the direction of the table.

The rotation of the cylinders necessarily producing a circulation of the taffety, it must consequently be rubbed in its passage between the tin tubes covered with cat's skin at M and N; and by this friction it obtains what is called the negative electricity, which is communicated from both parts of the silk to the common conductor S. But it may be made to electrify positively, by removing the rubbers to the middle of the silk, so that the prime conductor may communicate with them: or, if the two cushions be removed to half the distance between the revolving cylinders and the prime conductor, positive and negative electricity may be had at the same time, the rubbers being in a negative state, and the prime conductor in a positive state.

The advantages of a machine of this construction beyond those of glass are stated by the inventor to be, 1. It is not brittle in any part. 2. Its excitation is more steady, because it requires no amalgam. 3. Its dimensions have no limit.

The power of excitation in this way appears to have been very considerable. The facts are not related with so much detail as could be wished in the Report of the Academy; but it appears that the negative sparks from the conductor of Walckiers, which was five feet long, were from 15 to 17 inches in length, very loud and dense, and very painful to the hand; that pointed bodies emitted very sensible sparks to the conductor; and that a battery of 50 square feet was charged by 30 turns of the machine, which gives 19 feet of silk rubbed to charge one foot of glass *. In another instance, however, it is said, that a square foot was charged by one turn of the machine, which answered to $31\frac{1}{2}$ square feet of silk. It is not said whether the labour of turning was considerable or not.

M. Rouland made several trials to substitute plain silk instead of that which was varnished; and he also tried woollens and mixed cloth containing goat's hair; but none of these answered to his satisfaction.

XII.

Experimental Researches concerning the Principle of the lateral Communication of Motion in Fluids, applied to the Explanation of various Hydraulic Phenomena. By Citizen J. B. Venturi, Professor of Experimental Philosophy at Modena, Member of the Italian Society, of the Institute of Bologna, the Agrarian Society of Turin, &c.

(Continued from page 276, vol. ii.)

Proposition VI.

THE expence of fluid is less through cylindrical tubes than through conical tubes, which diverge from the commencement of the contracted vein, and have the same external diameter.

* See Philos. Journ. I. 87.

The

The general theory is the ſame for both theſe forms of tubes; but the loſs of living force is greater in the cylinder, and the effect of the communication of motion in theſe tubes cannot approach its maximum as in the cone. Let the tube A C N M, fig. 5. Pl. VIII. of the preſent volume, have the form of the contracted vein in A C F D; the cylindrical part G I N M has its diameter M N, greater than D F. By the reaſoning made uſe of in the preceding propoſition, it is proved, that the lateral communication of motion tends to produce a vacuum in the ſolid zone R O Y S X Q T Z. If the communication of motion in this tube were completely made, it would follow, that the preſſure of the atmoſphere would increaſe the velocity of the contracted vein in the ratio of DF^2 to MN^2.

But the form itſelf of the cylindrical pipe always deſtroys a notable part of the effect: for the fluid filaments A D, in turning through the curve D R, proceed briſkly to ſtrike the ſides of the tube G M at R, where they loſe part of their motion. In the ſpace D G R eddies, or circular whirls, are produced, as in a baſon which receives water by a channel. Theſe eddies are, to a certain extent, a failure of the effect, and retard the efflux of the ſtream. A much leſs increaſe of the expenditure takes place in the cylindrical tube than would anſwer to the ratio of DF^2 to MN^2.

Experiment XVIII.—A notion may be formed of theſe internal ſhocks and eddies in the cylindric tube, and their effects on the efflux of the fluid, if attention be paid to the following table of the expenditure through the different additional tubes in the horizontal poſition. All theſe tubes have the diameter of their two extremities = 18 lines; they were all provided with the conical tube of the form of the contracted vein at their inner extremity, excepting that of fig. 6. The charge was always 32.5 inches above the centre of the orifice.

Table of the Times employed in diſcharging Four Cubic Feet of Water through the different Adjutages.

Through the orifice in a thin plate, - - -	41″.
Through the ſimple tube of fig. 6. - - -	31″.
Through the tube of the form of fig. 5. - -	31″.
After having amended (adouci) the conical divergent part, D F I G, of the ſame tube, - - - - -	30″.
Through the tube fig. 9. - - - -	32″.5
Through the conical tube of the form fig. 10. - -	27″.5
Through the tube fig. 5. the portion G I N M being 23.5 lines in diameter, and 84 in length, the reſt as before, -	27″.

It may perhaps be demanded, whether, in the internal part of the ſimple cylindric tube K L V of fig. 6. there be the ſame augmentation of velocity, and the ſame contraction of the ſtream, as in the compound tube of fig. 5.? By reaſoning according to the principles we have eſtabliſhed, I think, 1. That in the ſection K L of fig. 6. there is the ſame increaſe of velocity as we have ſeen (Prop. II.) take place in the ſection A C of fig. 5. The direction of the fluid particles which paſs through theſe two ſections muſt be the ſame in both caſes, becauſe this direction can depend only on the impulſe received within the reſervoir, which is the ſame in both. 2. In fig. 6. the fluid particles, after having paſſed through the ſection K L, begin immediately to experience the effect of the lateral communication of motion. They muſt therefore deviate laterally through the curve L *x z*, before they arrive

rive at the place of contraction which they assume at D F, fig. 5. and which they likewise assume when the orifice is made in a thin plate. If we imagine a tube of glass y K, one extremity of which is applied at K, fig. 6. and the other extremity open in the interior part of the reservoir, it will be seen that the pressure of the atmosphere, which is exerted upon the coloured fluid T, must likewise act on the surface of the reservoir, and join the pressure of the fluid in the reservoir to press the water into the tube y K, as it presses the coloured liquor into T S. The pressure of the atmosphere must, in the same manner, augment the impulse of all the fluid particles which arrive at K L, and consequently must increase the expenditure.

Since the checks and eddies in an additional cylindric tube must always destroy a part of the active force of the fluid, it follows, that the fluid column issuing out of the tube can never acquire the whole velocity which is due to the actual charge, and is observed nearly entire in the orifices through a thin plate; and the diminution of velocity corresponds with the increase of the time beyond that indicated by the theory, as may be seen in the following

Experiment XIX. The orifice P fig. 1. being made through a thin plate, and the vertical height P M, being 54 inches, the distance M N of the jet was 81,5 inches. Having applied to the same orifice the cylindrical tube of fig. 5. and the perpendicular P M being let fall from the external orifice of the tube, the distance M N was found to be 69 inches. According to the theory, the expenditure of four cubical feet through this tube ought to have taken place in 26″,24, but it really employed 31″. And the proportions 31″ : 26″,24 = 81,5 : 69 nearly.

The same observation may be made on an experiment of Michelotti (tom. ii. pages 22 and 23). P M being 19,33 feet, and the water issuing through an orifice in a thin plate M N was 23,2 feet; it was no more than 20 when an additional cylindric tube was applied which had not even the proper length.

It is evident, that the theory of the lateral communication of motion must likewise apply in the same manner to descending and ascending tubes, whenever their form admits of this lateral communication. In descending tubes, we must add the increase of expenditure occasioned by this cause to that which is produced by the acceleration of gravity, and which we have estimated in Proposition IV. In ascending tubes, gravity acts in a contrary direction, and consequently its effect must be deducted from that of the lateral communication. Experiment VII. relates to ascending tubes. The following relate to other positions.

Experiment XX. The tube A B E E of fig. 11, Experiment XV. was applied in the place of the tube B C Q O, in fig. 7. The height of the water in the reservoir above the lower extremity of the tube was 41,5 inches. The four cubical feet of water were emitted in 22″.

I applied the same conical tube A B F E, fig. 11, to the orifice R, fig. 8, to form an ascending jet a little inclined from the perpendicular. The height of the water of the reservoir above the upper extremity of the tube was 23 inches. The expenditure of four cubical feet was made in 30″.

The time of the expenditure in Experiment XV. was 25″. And by comparing it with the present, we find nearly $\sqrt{41,5} : \sqrt{32,5} = 25'' : 22''$. And $\sqrt{23} : \sqrt{32,5} = 25'' : 30''$.

Expe-

Experiment XXI. The orifice R, fig. 8, was circular, and 4,5 lines in diameter; the charge was 31,7 inches, and the jet declined a little from the perpendicular. The orifice being through a thin plate, afforded a cubical foot of water in 161″. With an additional cylindrical tube of the fame diameter, and ten lines in length, the cubical foot of water was emitted in 121″.

Under a charge of 56 inches, the fame orifice afforded, through the vertical jet, a cubical foot in 123″ through the thin plate, and in 91″ with the fame additional tube.

Thefe two refults being combined, give for the expenditure of vertical jets a mean ratio, between the thin plate and the cylindrical adjutage, of 100 to 134, which is alfo the ratio between the horizontal jets.

Experiment XXII. I applied the glafs tube QRT, fig. 6, to the point S fig. 5. of the compound tube ACMN, the diftance BS being 24 lines. In this fituation the fluid T no longer rifes in the tube. This proves that the lateral tranflation of the fluid in the cylindrical tube is made very near the place where the vein is contracted, and that confequently DR muft brifkly ftrike the fide GM.

By this experiment we fee that the diftance BR to which the oblique filaments ftrike the fides of the tube, does not amount to 24 lines. Suppofing DO = 20 lines, the time which the particle D employs to pafs through the fpace DO in my experiments is lefs than 0″,01. Let us decompofe the curve-lined motion DR according to the lines DO, OR. Let us fuppofe the acceleration through OR to be uniform, and it will be found that this acceleration is at leaft five times as great as that of heavy bodies. If the lateral force through OR were fimply the mutual attraction of the particles of the water, this attraction in the particle D muft not only overcome the inertia of the particle itfelf, but likewife that of the other particles nearer the axis, which follow D in its deviation through DR, and imprefs upon them a much greater fum of acceleration than that of gravity. Now the force of attraction of one particle of water is not greater than the natural gravity of a thread of water of the length of one line at moft. The lateral communication of motion, which is the caufe of the acceleration through OR, is therefore much greater than could have been produced by the mutual attraction of the particles of water.

Propofition VII. By means of proper adjutages applied to a given cylindric tube, it is poffible to increafe the expenditure of water through that tube in the proportion of 24 to 10, the charge or height of the refervoir remaining the fame.

I fhall here give an account of the different precautions neceffary to be taken when the expenditure of water through a cylindrical tube of a given length is required to be the greateft poffible.

1. The inner extremity of the tube AD (fig. 13.) muft be fitted at AB with a conical piece of the form of the contracted vein*; this increafes the expenditure as 12,1 to 10. Every other form will afford lefs. If the diameter at A be too great, the contraction will be made beyond B, and the fection of the vein will be fmaller than the fection of the tube.

2. At the other extremity of the pipe BC apply a truncated conical tube CD, of which let the length be nearly nine times the diameter C, and its external diameter D muft be 1,8 C. This additional piece will increafe the expenditure as 24 to 12,1. (Experiment XVI.) By this means the quantity of water will be increafed by the two adjutages ABCD, in the proportion of 24 to 10.

* Boffut, Art. 509.

At

At Rome, the inhabitants purchafe the right of conveying water from the public refervoirs into their houfes. The law prohibits them from making the pipe of conveyance larger than the aperture granted them at the refervoir, as far as the diftance of fifty feet *. The legiflature was therefore aware, that an additional pipe of greater diameter than the orifice would increafe the expenditure; but it was not perceived that the law might be equally evaded by applying the conical fruftum C D beyond the fifty feet. From this fecond rule we learn, that it is not proper to make the flues of chimneys too large in the apartments; but that it will be fufficient if they be enlarged at their upper terminations, according to the form C D, fig. 13. This divergency of the upper part will carry off the fmoke very well, even when it is not practicable to afford chimneys of fufficient length to the upper apartments. The fame obfervation is applicable to chemical furnaces for ftrong fire.

3. The pipe B C ought to be ftraight, without elbows or curvatures. To the experiments which Boffut has made on this head† I fhall add the following.

[To be continued.]

ACCOUNTS OF BOOKS.

Philofophical Tranfactions of the Royal Society of London, for the Year 1798. Part II. Quarto. 593 pages, with a Lift of Prefents and Index, occupying 12 pages more, and 17 plates. Sold by Elmfly, London.

THIS Part contains the following papers: 10. A Difquifition of the Stability of Ships, by George Atwood, Efq.—11. Quelques Remarques d'Optique, principalement rélatives à la Reflexibilité des Rayons de la Lumiere. Par P. Prevoft, Profeffeur de Philofophie à Geneve, de l'Académie de Berlin, de la Société des Curieux de la Nature, et de la Société Royale d'Edimbourg. Communicated by Sir Charles Blagden, Knt. F. R. S.—12. An Account of the Orifice in the Retina of the Human Eye, difcovered by Profeffor Sœmmering: to which are added, Proofs of this Appearance being extended to the Eyes of other Animals. By Everard Home, Efq. F. R. S.—13. A Defcription of a very unufual Formation of the Human Heart. By Mr. James Wilfon, Surgeon. Communicated by Matthew Baillie, M. D. F. R. S.—14. Account of a fingular Inftance of Atmofpherical Refraction. In a Letter from William Latham, Efq. F. R. S. and A. S. to the Rev. Henry Whitfield, D. D. F. R. S. and A. S (See Philof. Journal, II. 417.)—15. Account of a Tumour found in the Subftance of the Human Placenta. By John Clarke, M. D. Communicated by the Right Hon. Sir Jofeph Banks, Bart. K. B. P. R. S.—16. On the Roots of Equations. By James Wood, B. D. Fellow of St. John's College, Cambridge. Communicated by the Rev. Nevil Mafkelyne, D. D. F. R. S. and Aftronomer Royal.—17. General Theorems, chiefly Porifms, in the higher Geometry. By Henry Brougham, jun. Efq. Communicated by Sir Charles Blagden, Knt. F. R. S.—18. Obfervations of the Diurnal Variation of the Magnetic Needle in the Ifland of St. Helena; with a Continuation of the Obfervations at Fort Marlborough in the Ifland of Sumatra. By John Macdonald, Efq. In a Letter to the Right Hon. Sir Jofeph Banks, Bart. K. B. P. R. S. —19. On the Corundum Stone, from Afia. By the Right Hon. Charles Greville, F. R. S. —20. An Enquiry concerning the Chemical Properties that have been attributed to Light. By Benjamin Count of Rumford, F. R. S. M. R. I. A. (See Philof. Journal II. 400.)—

* Fontin. de aquæduct. art. 205. 106 et 112.

† Art. 631 et feq.

21. Expe-

21. Experiments to determine the Denſity of the Earth. By H. Cavendiſh, Eſq. F. R. S. and A. S.—22. An improved Solution of a Problem in Phyſical Aſtronomy; by which ſwiftly converging Series are obtained, which are uſeful in computing the Perturbations of the Motions of the Earth, Mars, and Venus, by their mutual Attraction: To which is added an Appendix, containing an eaſy Method of obtaining the Sums of many ſlowly converging Series which ariſe in taking the Fluents of Binomial Surds, &c. By the Rev. John Hellins, F R. S. Vicar of Potter's Pury, in Northamptonſhire. In a Letter to the Rev. Nevil Maſkelyne, D. D. F. R S. and Aſtronomer Royal.—23. Account of a Subſtance found in a Clay-pit; and of the Effect of the Mere of Diſs upon various Subſtances immerſed in it By Mr. Benjamin Wiſeman, of Diſs, in Norfolk. Communicated by John Frere, Eſq. F. R. S. With an Analyſis of the Water of the ſaid Mere. By Charles Hatchett, Eſq. F. R. S. In a Letter to the Rt. Hon. Sir Joſeph Banks, Bart. K. B. P. R. S.—24. A Catalogue of Sanſcrita Manuſcripts, preſented to the Royal Society by Sir William and Lady Jones. By Charles Wilkins, Eſq. F. R. S.—Preſents received by the Royal Society from November 1797 to June 1798.—Index.

Caſes of the Diabetes Mellitus, with the Reſults of the Trials of certain Acids and other Subſtances in the Cure of the Lues Venerea. By John Rollo, M. D. Surgeon General, Royal Artillery. Second edition, with large additions. Octavo, 628 pages. London, Dilly, 1798.

An account of the firſt edition of this excellent treatiſe was given in the firſt volume of this Journal, page 285. The preſent edition is greatly enlarged; but the time of receiving it, and other circumſtances, prevent me from giving an abſtract of its contents at preſent. The chemical reader will ſee with pleaſure, in one of the articles of the preſent Number, that Mr. Cruickſhank has extended his reſearches into the nature of Sugar and Vegetable Mucilage. See page 406.

L'Art du Blanchement des Toiles, Fils et Cotons de tout Genre, &c. *In Engliſh.* The Art of Bleaching Piece Goods, Thread and Cottons, of every Deſcription; rendered more eaſy and general by means of the Oxygenated Muriatic Acid; with the Method of rendering Painted or Printed Goods perfectly White and Colourleſs. To which are added, the moſt certain Methods of Bleaching Silk and Wool; and the Diſcoveries made by the Author in the Art of Bleaching Paper. Illuſtrated with 9 large Plates in 4to. repreſenting all the Utenſils and different Manipulations of the Bleaching Proceſs: An elementary Work, compoſed for the Uſe of Manufacturers, Bleachers, Dyers, Caliico Printers, and Paper-Makers. By Pajot des Charmes, formerly Inſpector of Manufactures. 8vo. 202 pages. Sold by Dugour and Durand, Rue et Hôtel Serpente, à Paris.

This work appears to be of ſuch great practical utility that I have begun a tranſlation, which will appear in the courſe of the winter, with additional information, which I hope to procure, reſpecting the practice of the intelligent manufacturers of our own country.

Memoires et Obſervations de Chimie de Bertrand Pelletier, Docteur de Medecine, &c.; Or, Memoirs and Obſervations in Chemiſtry, by Bertrand Pelletier, Doctor of Medicine, &c. Collected and arranged by Charles Pelletier, and Sedellot the younger, M.D. &c. 2 vols. octavo, with 5 plates, and a portrait of the Author. Sold at Paris by Croulebois, Fuchs, Barrois, and the principal bookſellers.

The works of Pelletier are known and valued by every ſcientific chemiſt. Moſt of them have

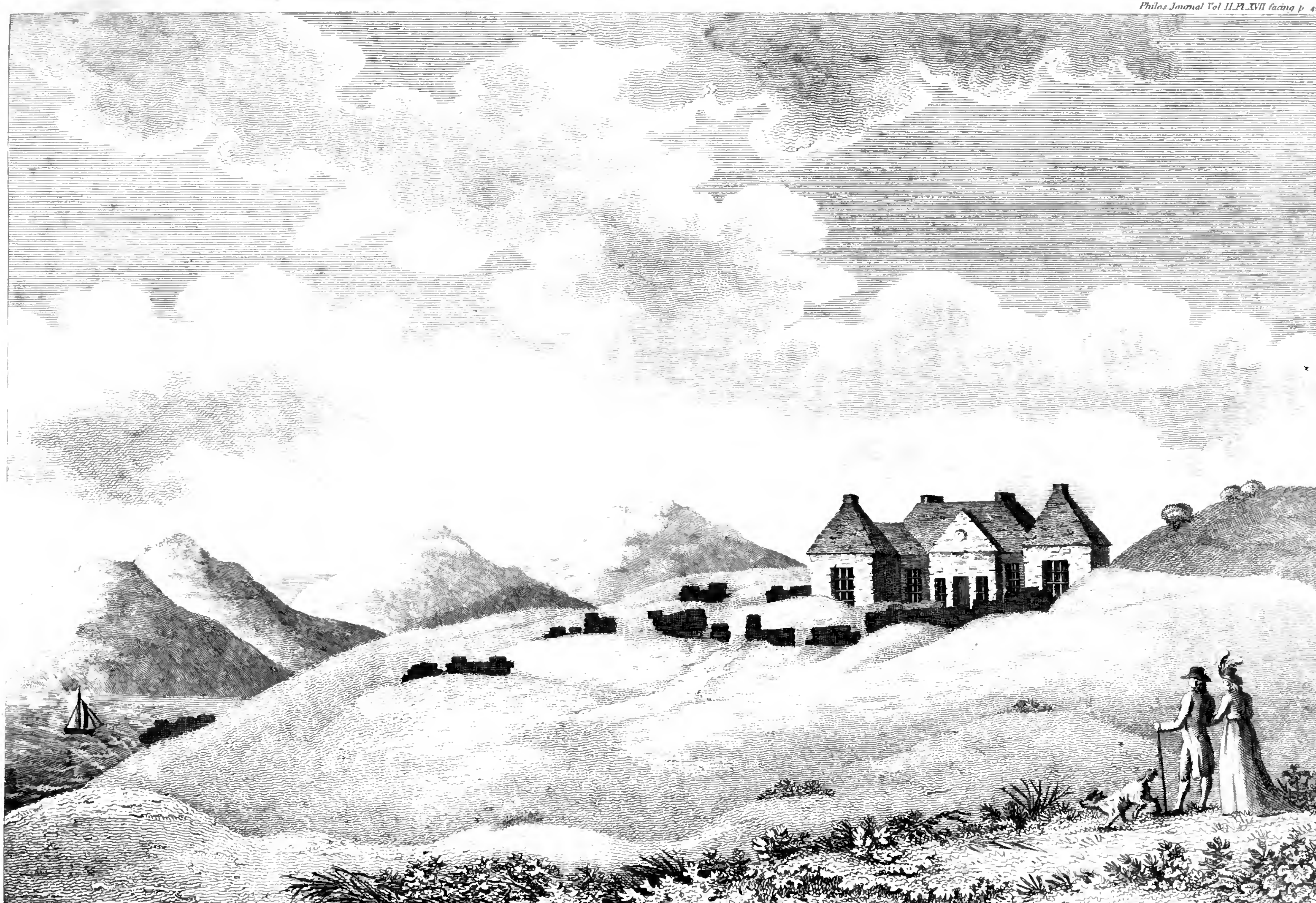
have already appeared in the Annales de Chimie and Journal de Phyfique; but this collection contains, as Lametherie informs us, fome new articles and annotations of the author. The utility of fuch collections is obvious, even if their fole object were to form into one body the fcattered productions of a man of genius. The works of many eminent writers of Memoirs have loft half their utility, for want of this friendly office to fcience and to pofthumous fame.

An Englifh Tranflation of the late Dr. Stewart's "*Propofitiones Geometricæ More Veterum demonftratæ*" is announced for publication by Mr. Leybourn.

It confifts of a feries of geometrical theorems, moftly new, inveftigated firft by analyfis, and afterwards fynthetically demonftrated by an inverfion of the fame analyfis. The utility of fuch a work is evident; and the celebrity of the original, which is now fcarce, will render the prefent tranflation more valuable.

From a paffage in the *Eloge de Leibnitz*, in the volume of the French Academy for 1716, and in the firft of the two volumes of *Eloges par Fontenelle*, I conclude that the George Dalgaru, mentioned in page 345, is fo named by an error of the prefs, which but too frequently occurs in copying proper names from obfcure manufcript. His name appears to have been *Dalgarme*. I fhall give the paffage in Englifh, on account of its curiofity and value. (See page 493 of the laft-mentioned work.)

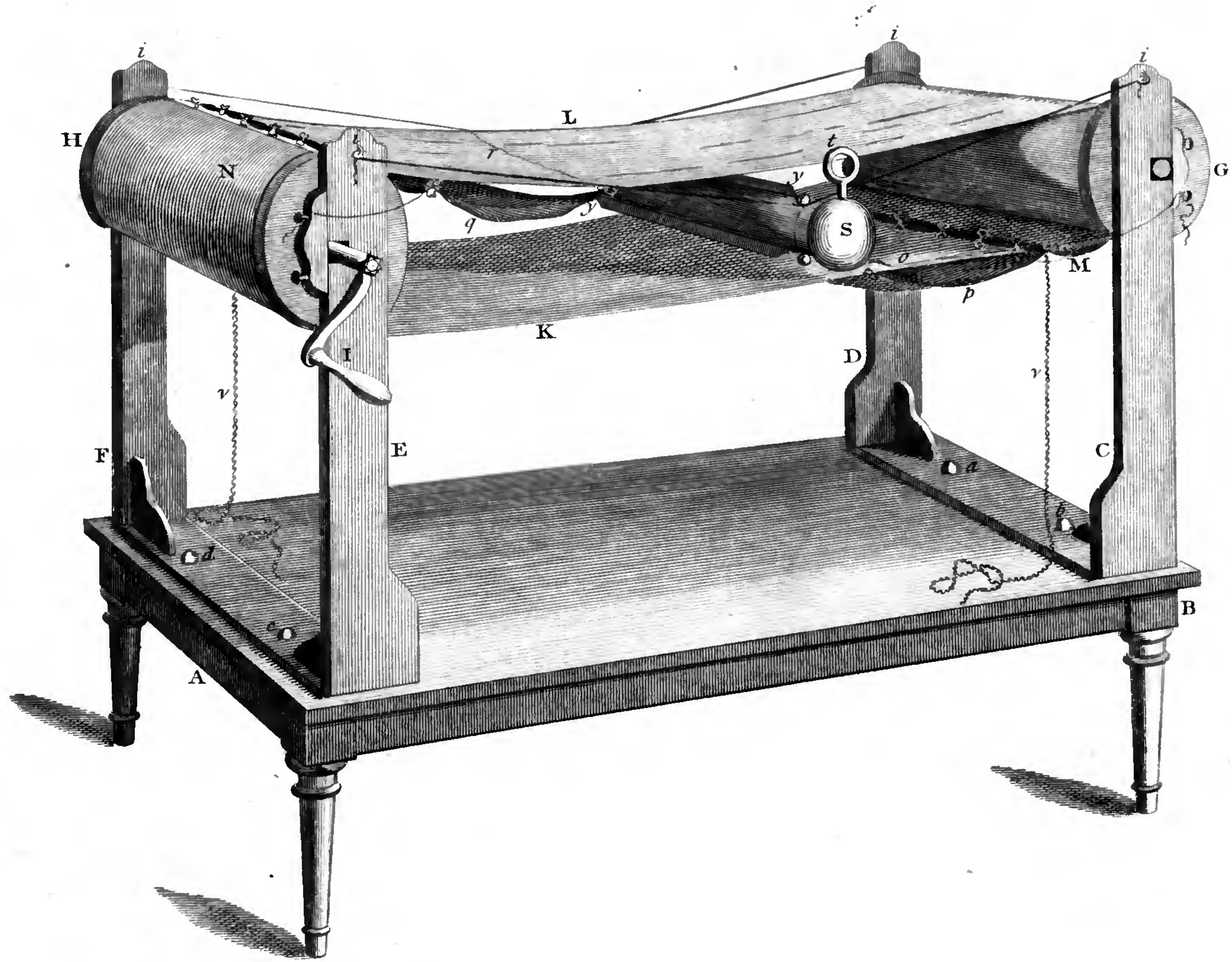
"It might now feem as if we had exhaufted the fubject of the labours of Leibnitz. But "this is not the cafe; not becaufe we have paffed over in filence a very great number of "individual facts, fufficient to have eftablifhed the fame of any other man, but becaufe "what remains to be narrated is of a very different kind from what we have already given. "It is the project of a Philofophical and Univerfal Language, which he had conceived. "*Wilkins* Bifhop of Chefter, and *Dalgarme*, had laboured at this enterprife: but when "Leibnitz was in England, he obferved to *Boyle* and *Oldenbourg*, that he did not think thofe "great men had followed the true method. They might caufe nations whofe languages "were different, to communicate together with eafe; but they had not feized the true "*real characters*, which were the moft delicate inftrument the human underftanding could "avail itfelf of, and were calculated extremely to facilitate the proceffes of reafon, the "powers of memory, and the invention of things. Thefe characters were to refemble as "much as poffible the characters of Algebra, which, in fact, are very fimple and very ex- "preffive, which never prefent any redundancy or equivocal expreffion, and of which all "the varieties are demonftrative. He has fomewhere fpoken of an *alphabet of human thoughts*, "which he meditated. According to every appearance, this alphabet bore relation to his "univerfal language. After the difcovery of this, it would have been alfo neceffary to have "difcovered the art of perfuading the different nations to ufe it; and this would not have "been the fmalleft difficulty of the tafk." With regard to this laft art, it appears to me to be very fimple, and capable of being pointed out in a few words. On this fubject the reader may confult the remarks at pages 189 and 191 of our prefent volume.



Barlow sculp

Mansion of Rosapenna in Ireland, destroyed by the change of Climate during the present Century.

Powerful Electrical Machine of Walckiers & Rouland.



A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JANUARY 1799.

ARTICLE I.

Description of a new Instrument for drawing equidistant and other parallel Lines with great accuracy and expedition; intended principally for the use of Engravers. With Specimens of its Performance. By W. N.

SOME months ago I was informed by the Engraver who executes the plates for this Journal, that an ingenious artist had constructed a machine for ruling the shades and grounds of copper-plate engravings, which he not only used with great success in his own business, but had sold to others under an engagement of secrecy. How far this information might be strictly accurate, was of less consequence at the time this conversation took place, than the consideration that it would be easy to make such a tool, and render its advantages more general by publication. I then promised to make one; and having now finished it, I take the earliest opportunity of communicating it to the world. From the preceding historical statement, the reader will see that I have no claim to the original thought of substituting mechanical operation instead of hand work in this department of engraving; and those who have seen the screw gear of Ramsden's great dividing engine will also perceive, that I have done little more than distribute the parts of this tool in what appeared to me to be the most simple and convenient manner.

Fig. 1. Plate XX. represents the instrument of the size of that which I have made. The outline section, fig. 2, represents the same viewed in a direction parallel to the edge of the moveable ruler. The letters denote the same things in both. A A A A is a frame fixed to the drawing-board. It resembles that of a sliding rule, and serves to guide a sliding piece, which, lying in fig. 1, immediately under the screw, could not with convenience be denoted by any letter. B C is a screw of exactly forty threads in the inch, but might conveniently be made of a coarser thread. G H and E D are two cocks, the former of which, G H, is fixed to the frame A, and bears a clip or pair of nuts, which open and shut with a joint like a pair of compasses, and either embrace the screw by a re-

gularly tapped part when ſhut, or leave it at liberty when open. Fig. 3. ſhews the face of this cock with the clip, of which P is the joint, Q Q tapped part, R a pin to inſure their coming up fairly, and S a claw, which, when drawn upwards, ſerves to cloſe them; or, if thrown back, leaves them at liberty to open by the action of a ſmall ſpiral ſpring, let into oppoſite holes near the joint. E D, fig. 1, repreſents the other cock, which is fixed to the ſliding piece. It carries the ſteel ruler E F, which, though ſufficiently ſtrong, is thin enough to adapt itſelf to ſlight variations of thickneſs of the plate beneath it. In my inſtrument I have made it adjuſtable to much greater variations of thickneſs, by means of an horizontal axis; but as this contrivance adds to the expence, and diminiſhes the ſimplicity of the inſtrument, I would rather recommend that great variations ſhould be allowed for by putting paper or thin ſlips of metal underneath the plate A A, as may be required. The end C of the ſcrew is turned down, and fixed in the cock E D by means of a nut and waſher; or in any other of the methods which are familiar to inſtrument-makers. The upper part of the cock E D is filed round, and cut into teeth, of which fifty would complete the whole circle. The centre of this external circular part correſponds with the axis of the ſcrew (ſee fig. 4.). L, M repreſent two ſhort cylindrical pieces which are hollow, and apply to each other ſo as to form a kind of box. Within, and fixed to the part M, which is fixed to the ſcrew itſelf, there is a ratchet wheel divided into fifty teeth; and within the part L, which is merely ſupported by the cylindrical part or ſtem of the ſcrew, there is a ratchet, which holds when the part L is moved by its handle N from right to left, but eſcapes when that handle is moved in the contrary direction. O is a lever, or arm, likewiſe ſupported by the ſtem of the ſcrew, and occupying the remaining ſpace between the handle N and the cock D. At the outer extremity of this lever there is a ſmall ſteel blade, which, by means of a back ſpring exactly reſembling that of a pocket knife, may be made to form a continuation of the lever itſelf, or, by being placed at right angles to the lever, may be made to reſt in any of the diviſions between the teeth of the circumference of the cock; and conſequently will, by that means, confine the lever to the poſition in which it is placed. The handle N cannot paſs the lever O, becauſe this laſt is too thick, and there is a ſtud or pin T, fig. 4, upon the face of the cock D, which prevents the handle from being moved beyond a certain determinate ſtation to the left hand. And, laſtly, the ruler I K, which is repreſented as broken off at K, but (like E F) may be of any required length, ſerves, by means of a thumb ſcrew at I, and another at the oppoſite end, to ſecure the copper-plate againſt the drawing-board in the uſual manner.

After this deſcription the uſe of the apparatus may be eaſily underſtood. By drawing back the claw S, fig. 3. the ſcrew is ſet at liberty, and the ruler E F may be brought to any required diſtance from I K by hand. The plate may then be duly placed and ſecured to the board, and the clip drawn gently together by the claw S. In this ſituation, ſuppoſe the lever O to be placed at a conſiderable diſtance from the handle N, that handle may be moved to the right, during which the click will gather upon the ratchet wheel; and then being returned to the left, it will carry the ſcrew round. The gentle preſſure, exerted by means of the claw, will tend to cloſe the clip upon the ſcrew, as ſoon as it comes into a fair poſition by its rotation; at which inſtant the claw will ſuddenly fall into its place, and the machine is ready for work, excepting that the adjuſtment for the fineneſs or coarſeneſs of the

the ſtroke muſt firſt be made. This is done by the lever O. If the ſteel blade be dropped into the firſt notch beginning on the left hand, the handle N will be confined; if at the ſecond notch, the handle, upon being moved backwards and forwards between the pin T and the lever O, will move the ſcrew through one tooth, or one-fiftieth of a turn, each time, and conſequently will carry the ruler F through one 2000th part of an inch. If the blade of the lever O be placed in any other of the notches (which are numbered by filing the corners of every fifth notch), the quantity paſſed over, at each return of the handle, will be greater or leſs according to the number. As there are but twenty-ſix notches, the greateſt ſingle ſhift of this inſtrument will be one-eightieth part of an inch; but as the ſhift is ſo readily made, it is eaſy, even with this fine ſcrew, to reach greater intervals, by moving the handle once, twice, or even three times, between ſtroke and ſtroke. Thus for one-fiftieth of an inch, or $\frac{40}{2000}$ths, the number of intervals cannot be paſſed over at one ſtroke; but, if the blade be ſet at the twentieth notch, the ruler will be ſhifted exactly that quantity by two movements of the handle. It is needleſs to multiply directions for this part of the operation; and the more ſo, as the artiſt is much more likely to determine the fineneſs of his ſhade by previous trials, which will render him acquainted with the inſtrument, than by any numerical computation.

An inſtrument like the preſent may alſo be uſefully employed in ſubdividing lines into ſmall parts. If curved lines be required to be drawn at equal diſtances from each other, the ſimpleſt method appears to be that of varying the figure of a temporary edge of horn or metal, which may be fixed to E F: and if lines be required converging to a point, the ruler E F may be made to occupy the radius of a circle of any magnitude whatever, by giving an adjuſtable angular figure to the ſlider, and cauſing it to move againſt two points, upon principles deduced from the thirty-firſt propoſition of Euclid's third book.

As this inſtrument was made at intervals of leiſure, without any account being kept of time or materials, I cannot diſtinctly ſtate the coſt: but I eſtimate that the whole being made of braſs and ſteel, and well finiſhed, with a ſmall drawing-board and the wooden holding piece I K, might be afforded by a ſhopkeeper in the mathematical-inſtrument buſineſs for ten guineas. If the frame and ſlider were of box-wood, which I have no doubt would anſwer very well, and the ornamental covering L M of the ratchet-wheel were omitted, I ſuppoſe the inſtrument could be afforded for conſiderably leſs than half that ſum.

The engraver has uſed this apparatus in the plates of the preſent number.

II.

Memoir on the Climate of Ireland. By the Rev. WILLIAM HAMILTON, *of Favet, in the County of Donegal; late Fellow of Trinity College, Dublin; M. R. I. A. Correſponding Member of the Royal Society of Edinburgh, &c.*

[Concluded from Page 386, Vol. II.]

Of the Effects of theſe Weſtern Tempeſts on the General Temperature of the Climate.

IN a paper, honoured with a place in the ſecond volume of the Tranſactions of the Academy, I recorded the reſult of ſome experiments made in the year 1788, for the purpoſe of aſcertaining the temperature of the earth in our latitudes.

 From

From these experiments it appears, that the medium temperature of the kingdom of Ireland, from its southern to its northern extremity, nearly coincides with the standard assigned to corresponding degrees of latitude in Mr. Kirwan's ingenious tables *.

It is further discoverable, that the general warmth of our island is in no respect inferior to that of other countries in its parallel: from whence we are naturally led to conclude, that the annual quantity of heat received here is not less in our day than it was in former ages.

If these things are so, it may be denied that there is any substantial foundation for the frequent complaint of change in our seasons. The ocean may be agitated by storms; sands be drifted along our coasts, and trees blasted in mountainous and exposed situations; but why should the farmer lament the diminished ardour and shortened period of his summers, as if winter possessed more than its natural portion of the revolving year? Whence should arise the gardener's complaints, that his peaches, however sheltered from storms, rarely now arrive at maturity; and that his May-duke cherries, even in the most favoured aspects, no longer merit that distinguished title?

May I be permitted to assume, as true, the fact which it has hitherto been the purpose of this paper to demonstrate, and in support of which the trees, the sands, and the tides of our island seem to bear constant and unequivocal testimony; namely that, of late years, the western winds have blown with increased violence? From that single principle an answer may be deduced to all these interesting questions.

It is a matter well known, and easily to be explained, that the surface of the ocean varies less from the mean annual temperature of its latitude, than land in the same parallel †: that is, the surface of the sea is colder in summer, and warmer in winter, than the surface of the ground in that latitude.

If then the prevalent winds of any country blow over an ocean situated in its parallel, that country will relatively be denominated temperate; it will be free from all extremes; the heats of summer and the colds of winter will be checked by sea breezes of a contrary property; and the land, influenced by the neighbouring element, must, more or less, partake in its equability of temperature.

Such is the case in almost all the islands of the world: and such from immemorial ages has been the peculiar character of Ireland. Hither come the western winds, modified by the temperature of the broad Atlantic ocean, which they traverse in their career: hither fly the clouds, teeming with moisture collected in a course of three thousand miles along its surface; and here uniformity of temperature, and redundant humidity, have always been marked as the distinguished character of our soil.

Since then the trees, the sands, and the tides demonstrate that these winds have, of late years, blown with unusual violence; since they bear testimony that a large quantity of air thus directed, thus tempered, and surcharged, has passed over our lands; it plainly follows that the climate must have felt the change; that it must have experienced colder summers and milder winters than heretofore, approaching towards that equability of heat and redundance of moisture which the farmer and the gardener at present so heavily lament.

But it is not from increase of quantity alone that these winds have produced their effects.

* See Kirwan's Estimate of Temperatures. † Ibid.

They

They have altered the temperature of the ocean itſelf; and thus have, as it were, multiplied their changeful influence on the land; acting there at once with new properties, as well as with increaſed quantity.

The ſurface of water in a ſtate of tranquillity admits of greater variations of temperature than in a ſtate of agitation. It may become much hotter in ſummer and colder in winter when calm than when diſturbed; for the particles at the ſurface, when heated or cooled, do not immediately give place to others nearer the bottom; the proceſs of commixture, in a tranquil ſtate, is gradual, and the tranſmiſſion of change ſomewhat reſembles the ſlow and retarded progreſs that takes place on the land. Agitation always counteracts this gradual proceſs; a rapid commixture of the particles produces a quick aſſimilation of temperature throughout the whole maſs, and, thus taking away all partial exceſs, reduces the whole toward a medium ſtate of uniformity.

Thus, ſummer tempeſts always tend to diminiſh the ſuperficial heat of an ocean over which they ruſh; while winter blaſts, agitating the waters at conſiderable depths, reſiſt the natural cold of the ſeaſon by a ſupply of relatively warm particles, which ariſe from the bottom toward the ſurface.

That the Atlantic ocean, the vaſt and potent arbiter of our ſeaſons, has ſuffered unuſual agitation of late years, ſeems evident from the natural phenomena recorded in the beginning of this Memoir. The trees and ſands of our iſland bear teſtimony to the ſtorms which ſweep along its ſurface; and the tides come to us as frequent and unerring meſſengers of the tumultuous and agitated ſtate of its billows attendant on their furious career.

From this perturbed abyſs of waters has ariſen an unceaſing influence, equally potent to check the ardour of the ſummer ſolar beams, or to relax the ſhackles of a northern winter. By this prevailing influence, ſummer has been rendered impotent to raiſe and ripen many of our crops; and the farmer, taught by neceſſity, learns now to hand them over ready grown, and prepared for the maturity of ſummer under the mild temperature of an Atlantic winter. Hence too the gardener has, of late years, been compelled to call in the aid of artificial heat to forward the peach, the nectarine, the grape, and every other ſpecies of delicate fruit to perfection.

Hence premiums now vainly attempt the growth of hemp, which formerly peaſants, in the moſt northern counties of Ireland, cultivated without reward around their cabins for domeſtic uſe. And hence the ancient apiaries of our iſland, once ſo celebrated, and guarded with ſuch ſpecial and minute attention by the Brehon laws *, are now extinct; and honey, from being a common article of popular conſumption, has become a rare luxury, or an expenſive medicine.

Winter has likewiſe felt the general influence of this Atlantic temperature; our graſſes ſcarcely droop beneath the froſts; wheat and oats vegetate in the open fields during the very ſolſtice itſelf; myrtles and laurels, in ſheltered ſituations, brave the ſeverity of winter; the Foyle, and other large rivers of the northern province, frequently ſubject to the icy chains of former ages, now run in uninterrupted freedom †.

* See Collectanea de Reb. Hib. Tranſl. Brehon Laws.

† Fifty years have elapſed ſince the river Foyle has been effectually frozen over at Derry. It is alſo obſerved that the river Thames, in Britain, is leſs frequently frozen of late years than formerly. See Archæologia Britannica, vol. iii. p. 55.

Facts

Facts such as these are to be considered as the basis of general opinion concerning the alteration of our climate; while old age, connecting these appearances with the fragility of declining life, and a decayed constitution, has become querulous in proportion to its feelings, and, judging of external phenomena by the exaggerated test of its own acute sensations, emphatically pronounces that the seasons are now less favourable than formerly.

On the Consequences of this Equability of Temperature to Animal and Vegetable Life.

FROM the transient perusal of this Memoir, a rapid and impatient mind would probably draw numberless false conclusions. The facts here recorded seem to wear a gloomy aspect; to mark a gradual deterioration of our seasons; to indicate a climate, harsh, ungenial, and of consequence sterile in its nature; clouded, humid, tempestuous, cheerless, and unfriendly to animal and vegetable life.

All these conclusions contradict experience, and may be overturned by a calm consideration of the phenomena themselves.

Experience teaches us that dry seasons and easterly gales are, in our island, invariable sources of feeble vegetation and numerous diseases*; and the history of the world informs us that winds, whether hot or cold, are in their nature deleterious to animal and vegetable life, in proportion as they become deprived of humidity. The warm air of the African desert breathes desolation over the parched land of Egypt and Syria, before it is yet felt as a tempest †. Even our own westerly winds, the Atlantic messengers of health and fertility to our island, after traversing the cold and dry tract of the vast Siberian continent, bear nothing but sterility, and almost perpetual winter, to the unhappy climate of Kamschatka ‡.

Heat or cold in extremes, dry air in rapid motion, and moist air in a stagnant state, seem to be the principal external sources of human diseases; and climates are generally found favourable to health and longevity in proportion as they are exempt from these natural causes of disorder and decay.

It ought therefore to be inferred, *a priori*, that Ireland, celebrated for the singular equability of its temperature, and the ceaseless motion of an atmosphere always influenced by the moisture of the Atlantic, should be likewise characteristically free from natural disease; and experience proves that this conclusion is true.

The exhausting agues of North America, or the fens of England §; the fatal fluxes which prevail in the low countries of the continent of Europe; the dreadful bilious distempers of both the Indies; the pestilence which desolates the African and Asiatic cli-

* Of this the influenza, attendant on the easterly winds of spring in the present year (1795), affords a strong instance.

† See Volney's account of Egypt and Syria.

‡ The latitude of Kamschatka corresponds with that of Ireland; the westerly winds are prevalent in each; yet the former experiences a rigorous winter of nine months, and the latter rarely of as many days. See Cook's Voyage in 1779, vol. iii. ch. 6.

§ The ague is so rare in several parts of Ireland, that many persons are totally unacquainted with it. In the northern province the author has never met with the disease, and its existence there is generally denied.

mates,

mates, are all either entirely unknown, or but feebly felt in Ireland. There is here no characteriſtic diſeaſe to mark the natural ſource of unhealthfulneſs. There are few diſorders which cannot be directly traced up to ſome artificial cauſe; to ſome intemperance, to ſome neglect, to ſome exceſs, either of luxury or penury, in the ſuffering ſubject. Manufactures in their kind unwholeſome, induſtry exerted beyond its proper limits, irregularity of food or fermented liquors, illicit amours, colds, the conſequence of folly or inattention, exceſs or deficiency of proper exerciſe, anxiety and fretfulneſs of mind, together with the copious liſt of caſualties, afford to the phyſician of this country the cauſes and explanation of almoſt all its diſorders.

From its peculiar ſalubrity, the natives of this iſland are celebrated through Europe for juſt ſymmetry of proportion and an athletic frame; becauſe, from earlieſt infancy to manhood, a check is rarely given to the progreſſive increaſe of animal ſtrength, or the natural and appropriate forms of an undiſeaſed body. From the ſame hygeian ſource flow thoſe ardent paſſions, thoſe exuberant ſtreams of animal ſpirits, which render our natives always cheerful, oftentimes turbulent and boiſterous, the uſual conſequences of uninterrupted health and a vigorous conſtitution. Hence wild adventure, perſonal courage, impetuoſity of purſuit, inattention to conſequences, and improvidence of diſpoſition, become the characteriſtic features of minds ſeldom under the influence of that anxiety, which, flying from the preſent moment and its enjoyments, watches for futurity, and pants after remote felicity.

The general temperature of our climate, in the vicinity of the capital, is ſomewhat lower than the 50th degree of Fahrenheit's thermometer*; and a mean of the hotteſt or coldeſt months of our year rarely varies more than ten degrees from this ſtandard heat †. Winter therefore with us is uſually accompanied with a temperature of forty degrees, ſpring and autumn of fifty degrees, the ſummer of ſixty degrees of the thermometer: and the general heat of any ſingle month of theſe ſeveral ſeaſons ſeldom varies much from the correſponding temperature of that particular ſeaſon to which it belongs.

Of theſe limits the loweſt is not ſufficiently cold to check the growth of any of the natural herbage of our iſland, nor the higheſt powerful enough to parch the ſurface of a moiſt ſoil, or to ſcorch its luxuriant graſſes. Hence it comes to paſs that our fields maintain a perpetual verdure, unimpaired by either ſolſtice. Hence too the farmer is enabled to lay his lands under graſs almoſt at any ſeaſon, even at the commencement of winter ‡: and hence the grazier never loſes the benefit of his rich paſtures at any period of the year, unleſs during the tranſient paſſage of a temporary drift of ſnow; ſo that horſes, cattle, and ſheep, arrive here, with little care, at a degree of perfection unattainable in other countries without infinite expence and trouble.

For the growth of flax, the ſtaple commodity of the kingdom; of potatoes, the general food of its inhabitants; and of barley, from whence the ardent ſpirits and other fermented liquors of the country are derived; the climate ſeems to poſſeſs a peculiar felicity of tem-

* See Hamilton's paper on the temperature of Ireland, in the ſecond volume of theſe Tranſactions.

† See State of the Weather in Dublin, from June 1791 to June 1793, by Mr. Kirwan, Vol. V. of theſe Tranſactions.

‡ See Young's Tour in Ireland, Vol. I. p. 130.

perature,

perature, and moift in all its feafons; and for their neceffary degree of ripenefs the heat of fummer is amply fufficient.

It is for the more delicate fpecies of garden-fruits, for ill conducted crops of oats in bleak and mountainous fituations, and for the complete and perfect maturity of wheat, that uneafy apprehenfions can ever be entertained by the gardener or the farmer.

The former are in themfelves of little importance; and, where fuch luxuries are required, glafs coverings and artificial heat afford an ample fupply, and remove all pretence for difcontent. Human art and induftry, attentive to circumftances, and accommodated to times, has ftolen from the mild temperatures of our winters as much warmth as ferves to compenfate for the deficiency of fummer heats in forwarding and perfecting the crops of wheat; and a little will teach the farmer in our mountainous provinces to ufe the fame artifice with refpect to his oats, whenever neceffity fhall demand his increafed attention.

To fum up matters then with truth and brevity—A denfity of population, furpaffing that of the vaunted millions of undepopulated France *; a copious export-trade in provifions of various kinds, unequalled by any kingdom whofe inhabitants are proportionably numerous †; and a ftaple manufacture, unrivalled in general ufe, in certainty of produce, and intrinfic value ‡; are circumftances which have not fallen to the lot of other nations, and bring with them clear and irrefragable evidence to demonftrate a falubrious country, a genial climate, and a fertile foil in Ireland.

* The inhabitants of Ireland amount to more than four millions. (See Mr. Bufhe's Memoir, Irifh Tranf, Vol. III.)

Hence the denfity of its population is at the rate of 182 perfons to a fquare geographical mile, fuppofing the ifland to contain in round numbers 22,000 fuperficial miles.

Mr. Zimmerman, in his Political Survey of Europe, erroneoufly fuppofing the inhabitants of this country lefs than two millions and a half, deduces a denfity of population only to 117 perfons on each mile. The following table marks the number of individuals on a geographical fquare mile in the northern countries of Europe, as taken from Zimmerman's tables (Ireland being corrected); from whence it will appear that this kingdom rates extremely in the denfity of its population.

	Perfons.
Denmark - - -	12
Sweden - - -	14
Ruffia - - -	20
Scotland - - -	51
Poland - - -	53
Pruffia - - -	67
England - - -	129
Germany - - -	135
France - - -	152
Ireland - - -	182
Holland - - -	236

† Two hundred and twenty thoufand barrels of beef and pork, independent of the various matters connected therewith, and of every other fpecies of provifion, were exported in the year ending 25th March 1793.

‡ After a progreffive increafe of ninety years the exports of linen-cloth alone, in the year 1792, amounted to forty-three million yards, from one million only at the commencement of the prefent century.

CONCLUSION.

IN this Memoir I have endeavoured to prove, from natural and almost incontrovertible registers of the phenomena of later years, that the winds, and particularly the western storms, have swept with increased violence over Ireland.

From this fact I have deduced a necessary change in its climate; a more general equability of temperature through the year; summers less warm, and winters more mild and open: and lastly, I have endeavoured to support this conclusion by general observation, and the enumeration of particular instances where the defect of summer heat, and winter ice, seem to be the most strongly marked.

One question still remains curious in its principle, and interesting in its solution—Why have these western winds blown with unusual and increased violence?

The limits of a Memoir such as this do not permit me to enlarge on this subject, and even appear to include a reproof for the length of the present interruption. I shall therefore trespass no longer on the moments of the Academy than to suggest a few queries, which may stand over for future consideration; leaving it to time, and the ingenuity or better directed observations of others, to verify, to disprove, or to condemn, the hints which may be contained in them.

1st. Have not our winds become more violent, and the temperature of our seasons more equable, since the forests of Ireland were cleared, and the country cultivated? And have not these winds, and that equability of temperature, been nearly proportioned to these as to their causes?

2d. Have not similar changes occurred under analogous circumstances in North America; even in Canada, that country of extremes in heat and cold? And did not the island of Bermudas, though situated so much to the southward of us, become barren of fruit in consequence of the destruction of its timber-trees?

3d. Has it not appeared from observations on the ascent of balloons, and the motion of clouds, that the lower mass of air often pursues a different course from the upper stratum? May not then the limits of our stormy currents of air be often confined within a few hundred yards of the surface of the earth? And if so, is it not possible, and even probable, that the frequent interruption of forests and groves, and hedge-row trees, might have formerly very much retarded, and finally checked, the progress of a tempest?

4th. Have not all the countries of Europe, Asia, and America, within the parallel of Ireland, been very much denuded of their forests within the present century? And has not the increased velocity of the westerly winds been proportioned to this destruction of the forests and trees, as to their natural causes?

5th. Is it not probable, since the prevalent winds of our parallel have a westerly tendency, that circumstances which have removed impediments to their career round the entire globe would give increased velocity to their course?

6th. Should not Ireland, launched as it is into the Atlantic beyond the other coasts of Europe, and denuded as it has been of forests and hedge-row trees, be most sensibly affected by increasing tempests from the west, and the first to experience their influence on its climate?

Whether these queries have any foundation in nature, or are merely to be esteemed the result of an unrestrained imagination; whether so diminutive an animal as man, so temporary

porary in duration, so impotent in strength, acting through the lengthened period and persevering efforts of a large portion of his species, can reasonably be deemed equal to the involuntary production of such vast effects; to a change even of the elements and climates of the earth, may admit of doubt, of opposition, and even of denial. For these reasons I have taken the liberty simply to propose them as matters of enquiry; and only beg the indulgence of the Academy for troubling them with matters that may not be attended with sufficient evidence to bring conviction, or even to escape censure.

POSTSCRIPT.

October 20, 1795.

THE present year (1795) has been comparatively cold in its commencement, and warm in its summer: it has been calm, dry, and in many respects a contrast to the long series of preceding years, whose effects are registered in this Memoir.

Yet the extremes of its temperature have not been equal to those of former years, as far as can be inferred from the recorded effects of heat and cold on natural objects. The large rivers of the northern province have not been bound firmly in the icy chains of winter, nor have the grapes ripened in favourable aspects in any part of the kingdom during summer, as formerly occurred in seasons whose temperature was denominated extreme.

III.

Experiments and Observations on Electricity—Excitation—the two States—Points of Difference between the Action of weak and strong Electricities compared together. (*W. N.*)

THE excitation of electrics by friction still remains a mystery. We possess nothing in the least resembling a theory of the changes of electrical capacity which the rubber and cylinder undergo during their change of relative position in contact. In the year 1789 I communicated several facts to the Royal Society* concerning the mutual action of the silk flap and the electrical cylinder upon each other, and shewed that something like compensation takes place while they remain together. The transition of the surface of the cylinder from the compensated to the uncompensated state is strikingly seen when a hole is cut in the silk, and the cylinder turned under circumstances of considerable excitation. A cascade of fire issues from that edge of the hole which is nearest the cushion; but instead of being dissipated into the air, it bends down again, and unites with the cylinder at the opposite edge of the hole, whence it proceeds as usual to the receiving part of the prime conductor.

The phenomena of the two electricities called plus and minus are singularly distinct in almost every experiment which can be made with the exhibition of electric light. Paper is a good substance for observing the visible passage of electricity. If a strong plus electric stream be let fall on the flat side of an uninsulated sheet of paper, it forms a beautiful star about four inches in diameter, consisting of very distinct radii not ramified. The minus electricity, in circumstances perfectly similar, throws many pointed brushes to the paper,

* Philosophical Transactions, No. lxxix.

but

but forms no ftar upon it. In this experiment I ufed a machine with a cylinder of feven inches diameter.

Hence it feems to follow, that a hollow ball of paper, or a glafs globe covered with paper, might form an amufing part of the electrical apparatus for experiments in the dark.

The laws of action with regard to weak electricities and thofe of confiderable intenfity appear to differ in various particulars, which require further inveftigation. When a number of jars are charged in fucceffion, that is to fay, by infulating the whole feries, and caufing the outer coating of the firft to communicate with the inner of the fecond, and the outer coating of the fecond with the inner of the third, and fo forth, it is well known that the whole may be charged by communicating electricity to the infide of the firft, provided the laft have a communication with the earth; but with this circumftance, that the charge will reftore itfelf by explofion when the quantity of electricity is much lefs than the firft jar would fingly have received and retained. From this refult it has with fome probability been concluded, that glafs refifts the communication of electric energy, and that the jars are fucceffively lefs and lefs charged. Whether this laft conclufion be true, has not I believe been afcertained by experiment.

Under all the uncertainties concerning the place occupied by the electric charge of coated glafs, though it may feem unfair to make any inference refpecting glafs which is uncoated, yet, upon the whole, there appears to be a probability that the interpofition of naked glafs may impede the action of electrified bodies. This queftion more immediately points at the tube in which the gold-leaf electrometer of Bennet is inclofed. To determine whether the tube of the electrometer does affect the electric ftate of the included leaf, either by compenfation or otherwife, I took a piece of window-glafs eighteen inches long, two inches wide, and one-twentieth of an inch thick, which I cleaned very well, and then paffed it feveral times through the hot air over the flame of a candle. In this ftate one end of the glafs was laid gently upon the electrified plate of Bennet's electrometer, and then fuddenly raifed by a turn of the wrift. It was fcarcely poffible to difcern that the leaves were at all affected; but when the electrometer was in the plus ftate a very flight collapfion was produced by raifing the glafs, and the contrary effect was produced when the electrometer was negative. Some days afterwards the experiment was repeated, after the gold-leaf had been changed for other pieces, which were very pointed and delicate in their movements. The refult was, that the glafs was always fhewn by the electrometer to be in a weak pofitive ftate; and, when the electricity of the electrometer was made plus, the collapfion was equal to the divergence when it was minus.

In making thefe experiments I had previoufly fuppofed that the influence of the metallic ftate of the electrometer would produce fomewhat of the nature of a charge upon the glafs; and confequently that the intenfity of the leaves would have been diminifhed during the exiftence of that charge; and alfo, that in fuch a cafe the action of the metal through the glafs would be fubject to the fame diminution as in the feries of jars. But as the glafs did not appear to act in this manner, it feems proper to conclude that clean glafs does not affect the electric ftate of bodies by its vicinity, and that the divergence of the balls or the gold-leaf in the electrometers of Cavallo and Bennet is not diminifhed by the tube which furrounds them.

From a variety of experiments it was clearly aſcertained that the metallic coatings, though by their vicinity they may diminiſh the intenſity of the electric ſtate in the leaves, do nevertheleſs increaſe the angle of divergence by their attraction.

When the gold-leaf electrometer is made with a very ſmall tube, its ſenſibility is ſomewhat increaſed by the nearneſs of the coatings; but the chance of rendering it unſerviceable from caſual friction, which excites the glaſs, and cauſes the gold leaf to ſtick to it, together with the leſs perfect view of the divergence through a tube of ſmall curvature, afford reaſons why a diameter of leſs than an inch ſhould be rejected. Other reaſons of convenience indicate that the diameter of the glaſs ſhould not much exceed this quantity.

I was once induced to think that the conſiderable magnitude of the cap of Bennet's electrometer might render it leſs capable of being acted upon by ſmall quantities of electricity. Experiment did not however give much countenance to this ſuppoſition. By trials with heads of different ſize, the ſmalleſt were found to be rather more ſenſible to extremely minute electricities, and leſs ſo to ſuch as were greater. The influence of very weak electricity may produce the oppoſite ſtate in the whole of a ſmall head, but only in part of a larger; the remaining part of this laſt aſſuming the oppoſite ſtate, and robbing the leaves of part of their intenſity. But in higher electricities the whole of the large head may be urged to give electricity to the leaves, in a quantity which the ſmaller head could not give without acquiring a higher degree of intenſity, and conſequently more ſtrongly reſiſting the deſired proceſs. It appears therefore that the maximum of effect with a given electricity, acting without communication, will not be obtained but by an head of a definite figure and magnitude.

From ſome experiments of Hoadley and Wilſon with a number of prime conductors ſeparately inſulated, it was eſtabliſhed that an electrified body brought near one end of ſuch a ſet of conductors, forming by contact a right line, will produce the contrary ſtate in the nearer conductors, and the ſame ſtate in thoſe which are more remote. Or, upon the hypotheſis of a ſingle fluid, the electricity of the body preſented will repel that of the compound conductor; ſo that by ſeparating them before the influence is removed, and afterwards examining them, the reſpective ſtates of the ſeveral portions of the whole line may be aſcertained.

It was a concluſion obviouſly enough deducible from this experiment, that if two electrometers of Bennet were connected by a metallic bar, there ought to be a difference when an electrified body is preſented at either extremity, or at the middle of the bar. I made this experiment with a braſs bar eighteen inches long. Both electrometers were affected at the ſame inſtant. Their divergence was equal, and the effect was the ſame, whether the glaſs or ſealing-wax was preſented at either end or at the middle of the bar. This reſult ſeems to indicate a difference in the mode of action between the very weak electricity I uſed and the much ſtronger of Hoadley and Wilſon.

The action of pointed bodies and of flame likewiſe exhibits a remarkable difference between the ſtrong and the weak electricities. The prime conductor of an electrical machine is very ſuddenly deprived of the greateſt part of its electricity by the operation of a metallic point; but does not ſeem to be much affected by the vicinity of burning candles.

candles. On the contrary, the electrometer of Bennet will fcarcely communicate the leaft portion of its electricity to a point not abfolutely in contact with its cap; but is readily deprived of its electric ftate by a candle.

Coulomb, by his method of torfion, has determined that the action of weak electricities diminifhes as the fquare of the diftance. I do not know that any experiments have been made to determine the ratio of the repulfion or attraction of bodies by electricity, with regard to the diftance, when the intenfities are confiderable.

To examine this object I caufed a brafs conductor to be made, four inches in diameter, with a fpherical part at each end, of five inches diameter; and, on the whole, twenty inches long. It was fixed with its axis in the vertical pofition. In the upper fpherical part there was a ftage fupporting a pulley, the axis of which ran upon two pair of very delicate friction wheels. The lower fpherical part was perforated in the axis, to receive the ftem of a very light fphere of paper gilt, and of the diameter of five inches. Care was taken that this ftem, which was nearly the whole length of the conductor from the friction wheels to the lower orifice, fhould not touch that orifice; and for greater fecurity there was a fmall frame carrying three friction rollers, againft one of which it was fure to bear in cafe of accidental irregularity. The upper part of the ftem was faftened to a fine filken thread which paffed over the pulley, and was tied to a counterpoife adapted by its figure to receive fmall weights, in order that the equilibrium or preponderance might be adjufted at pleafure. On the axis of the pulley was an index and hand, which moved upon a face without rouching, beneath a convex glafs, and fhewed the defcent of the gilded ball in inches and parts. I expected to have obtained a confiderable fcale on my ftem of twelve inches. But in the trials I made when it was finifhed, in which the afcending power of the ball could be varied at pleafure by addition to the counterpoife, I found either that it did not move at all, or that, if it did, it ran with confiderable velocity through its whole length. This unexpected event, together with fome other imperfections in the inftrument itfelf, which required amendment and alteration, prevented me from purfuing and diverfifying a courfe of experiment that promifed fo little. The facts appear however to fhew, that in great intenfities and fhort diftances, the diminution of effect, if it follow the law of the fquares of the diftances, is, like the attraction of the earth upon projectiles, too fmall to be perceived. But from the indications afforded by Henley's quadrant electrometer, and the floating electrometer of Nollet, there appear to be reafons why the purfuit fhould not be thought unworthy of further experiment.

IV.

Analyfis of the Red Lead of Siberia; with Experiments on the New Metal it contains. By Citizen VAUQUELIN, *Infpector of Mines, and Confervator of Chemical Products at the Mineralogical School.*

(Concluded from page 393, Vol. II.)

Combinations of the Acid of Red Lead with the Alkalis.

THE acid of red lead forms, in its combination with the alkalis, falts which are foluble, cryftallifable, and coloured. The fimpleft procefs for making thefe falts confifts in in boiling upon one part of red lead reduced into fine powder, two parts of carbonate of alkali with forty parts of water. By this means a double affinity is made to operate, by virtue of which carbonate of lead is formed, and falls to the bottom, while a combination of the acid of red lead with the alkali made uſe of remains ſuſpended in the water, by virtue of its ſolubility.

The proportion of alkaline carbonate proper to be uſed for the decompoſition of the native red lead muſt vary according to the proportions which ſubſiſt between the baſes, the acids, and the water of cryſtallization. The proportions ſtated in the laſt paragraph are ſuited to the carbonate of pot-aſh; but the proper quantities in the other caſes are eaſily aſcertained, by uſing a ſmall quantity of the carbonate at firſt, and afterwards gradually increaſing it, till the greateſt part of the red lead is decompoſed. On the whole, it is better to uſe a ſmaller than too large a portion of the carbonate, in order that its baſe may be more completely ſaturated with the acid of the red lead.

There is no reaſon to fear a ſimilar inconvenience with the carbonate of ammoniac. It is proper, on the contrary, to add more than is ſufficient to ſaturate the acid of the red lead; becauſe a portion of this ſalt will always be driven off during the ebullition which is indiſpenſably neceſſary to produce the intended decompoſition. And even in caſe there ſhould remain an exceſs after the complete decompoſition of the red lead, there would be no danger, becauſe this exceſs of alkali is driven off by the evaporation of the ſalt newly formed, and there is always a certainty of obtaining the combination perfectly pure.

The colour of the combinations of the acid of red lead with the alkalis is an orange yellow *. Their ſolutions afford cryſtals nearly of the ſame tinge, the differences being only ſlight variations of ſhade. Theſe ſalts are decompoſed by barytes, lime and ſtrontian. The mineral acids likewiſe decompoſe them by an inverſe operation.

Theſe ſalts afford oxygen gas by the action of heat, and their reſidue has the appearance of a green maſs. The ammoniacal compound muſt however be excepted, as its baſe is partly decompoſed by the oxygen of the acid, and leaves in the retort a pure green oxide, the undecompoſed portion of ammoniac being alſo driven off by the heat.

Theſe ſalts decompoſe by double affinity the calcareous, barytic, magneſian, aluminous ſalts, &c. Moſt of the metallic ſalts are likewiſe decompoſed by theſe ſubſtances, and afford new combinations ſcarcely or not at all ſoluble in water; and for the moſt part red, yellow, orange, or lemon colour.

I have not hitherto been able to determine the figure of theſe ſalts with accuracy, nor their degree of ſolubility in water, for the reaſons I have already explained; neither have I made all the combinations of this acid with the metallic oxides, nor determined its mode of action on the metals. Such experiments would no doubt have afforded intereſting phenomena for the completion of the hiſtory of this metallic acid.

But I indulge the hope that theſe experiments are only retarded, and that ſome fortunate opportunity will hereafter enable us to perform them: and, at all events, the experiments already made will be ſufficient to aſcertain the preſence of the acid of red lead wherever it may be met with, and diſtinguiſh it from every other natural ſubſtance.

* That of the ammoniacal ſalt, which has the form of plated cryſtals, is yellow, and exhibits the metallic brilliancy of gold. V.

SEC-

SECTION VII.

Reduction of the Acid of Red Lead to the Metallic State.

THOUGH the properties of the acid of red lead, defcribed in the foregoing paragraphs, are in ftrictnefs fufficient to convince thofe who are in the habit of treating metallic fubftances, that this fubftance belongs to the fame clafs, it is neverthelefs defirable, in order, as it were, to add the ftamp of evidence to the proofs of demonftration, that this acid fhould be reduced to the metallic ftate.

To obtain this object I took 72 parts of the acid in queftion, extracted from the red lead by the muriatic acid, as defcribed in our fourth fection, which I introduced into a crucible of charcoal placed in another of hard porcelain likewife filled with charcoal powder.

This apparatus, placed in a forge furnace, was expofed for an hour to the action of a very ftrong fire urged by the blaft of a ftrong pair of bellows with three pipes.

When the crucible was cooled and broken, I found, to my great fatisfaction, in the fmall veffel of charcoal, a metallic mafs of a white grey colour, fhining, very brittle, and upon the furface of which were many feathered cryftals of the fame colour perfectly metallic. This metallic mafs weighed forty-three parts.

It appears, from the refult of this operation, that the oxygen does not adhere with very great force to this metallic bafe, and that the acid contains about 0,40 of its weight of this acidifying principle.

SECTION VIII.

Properties of the Metal of Red Lead.

THIS metal, as I have already remarked, is of a white colour inclining to grey: it is very brittle, and cryftallifes in an elevated temperature into a kind of feathered filaments, which rife above the metallic mafs.

The metallic button being broken, prefented internally compact points forming a clofe grain, and in other parts needles croffing each other in all directions, leaving void fpaces between them, which prevented me from determining its fpecific gravity.

A fragment of this metal expofed to the heat of the blow-pipe acquired a tarnifh on its furface, and by continuing the operation it was covered with a light green cruft; but it afforded no fign of fufion.

When heated by the fame apparatus with borax it did not melt, but was in a fmall degree diminifhed, and communicated an emerald-green colour to the falt.

When reduced to a fine powder, and treated with the concentrated boiling nitric acid, it was oxided with much difficulty, and communicated to the acid a green tinge flightly inclining to blue. The nitric acid attacks this metal with fo much difficulty, that it was not till after treating it repeatedly with confiderable quantities of that folvent that I could fucceed in diffolving fix grains.

The different folutions being added together were evaporated to drynefs. Towards the end of the operation the refidue affumed the form and the ductility of a vegetable extract of a red brown colour. Cauftic potafh poured on this refidue diffolved a great part, and affumed a lemon-yellow colour; but part remained of a very beautiful green, which was not acted upon by that re-agent: it was a portion of the metal which had not received

6 from

ſrom the nitric acid the whole quantity of oxygen neceſſary for its acidification. It was ſtill in the ſtate of oxide.

I treated it again with the concentrated nitric acid; and by a ſeries of operations of this nature I at length ſucceeded in completely acidifying it.

The combination of this artificial acid with potaſh exhibited abſolutely the ſame appearances with the different re-agents as the ſalt which is formed with the natural acid. See Section II.

SECTION IX.

Denomination of the Metal contained in the native Red Lead.

WHEN an unknown ſubſtance is to be examined, the only method to aſcertain whether it has been before deſcribed, is to examine its properties, and compare them with thoſe of other bodies; an operation which ſuppoſes a knowledge of all that has been before deſcribed in natural hiſtory. And when, after an accurate compariſon of the properties of the body under examination with thoſe of other bodies, it is found that none of theſe laſt exhibit the whole of thoſe properties, a fair concluſion may be formed that the body is unknown, and conſequently that it is new.

After this point is determined, it becomes neceſſary, in order to make it known to others, that its diſtinctive characters ſhould be clearly aſcertained, and a name given to it, for the purpoſe of brief deſignation, and of inſcription in the catalogue of human knowledge.

This name may be derived from various ſources;—the place whence it was obtained, the author who diſcovered it, or the ſpecific properties it poſſeſſes, &c.: but it is eaſy to ſee that the two firſt ſources are vicious. In fact, the name of the place ſeems to announce that the ſubſtance is to be found excluſively there; and it is very far from being known, at the early period of diſcovery, whether it may not be found elſewhere. The name of the author teaches nothing, except that ſuch a man was the diſcoverer; a circumſtance of little intereſt to ſcience. But the name of a ſubſtance deduced from its leading properties is truly uſeful, becauſe it brings to mind, and in ſome meaſure places the object before the eyes by a faithful ſketch of its attributes.

From theſe conſiderations I have thought fit to adopt the name *chrome*, which was propoſed to me by Cit. Haüy, to deſignate the new metal found in the native red lead. In truth, this name does not perfectly agree with the complete metal, becauſe it has no very diſtinct colour; and becauſe, even if it had one, this would not be a ſufficient reaſon, ſince every metal has a more or leſs peculiar colour.

But it agrees wonderfully well with its combinations with oxygen, which afford a green oxide, or a red acid, according to the proportions of that principle, and becauſe each of its primary combinations communicates its colour to all the ſecondary combinations into which it enters; properties which belong to it almoſt excluſively.

This name appears to me to be ſo much the better founded, as this ſubſtance has not yet been diſcovered except in the ſtate of acid or oxide, and becauſe it may perhaps never be found in the native metallic ſtate.

Neverthelеſs, as I am not diſpoſed to conſider the adoption of one name rather than another as of any importance, provided the name do expreſs ſome remarkable or diſtinctive property of that which it denotes, I ſhall with pleaſure, if a better name be found, conſent to ſubſtitute it inſtead of *chrome*.

SEC-

SECTION X.

Uses of Chrome, of the Oxide of Chrome, and of the Chromic Acid.

THE brittleness of chrome, its resistance to the action of fire, and the small masses in which it has hitherto been found, afford little expectation that this metal will ever be of great use in the arts. But this assertion may be too strong; for a new substance, of which the properties at first appear to be of small interest to society, is sometimes found, in the subsequent processes of investigation, to be capable of very important applications in the arts and sciences.

The acid and the oxide of this metal are certainly not subject to the same observation. The former, by the beautiful emerald-green which it communicates even to enamels, without being subject to alteration in the purity of its shade, will afford to painters and enamellers an additional object to enrich their productions, and add to the perfection of their art: the second, by the beautiful cinnabar red which it assumes and keeps in its combination with mercury; the orange-red colour it affords with lead; the carmelite red it communicates to silver, may become highly valuable for paintings in oil and water-colours.

Chemistry will be greatly benefited by this excellent re-agent to detect the smallest quantities of mercury, silver, and lead, dissolved in acids by the different colours it produces when its action is assisted by an alkali. And, in the same manner as this acid indicates the presence of the metals here spoken of, those metals in their turn may serve to discover the chromic acid, if it be previously put into the necessary condition to produce the before-mentioned effects.

If the chromic acid should hereafter be abundantly found in any other combination besides that of lead, it might, after extraction by carbonate of potash, be used for the artificial composition of red lead, and furnish, in abundance, this valuable orange-red colour for painters' use, which is sold very dear in Siberia, and applied to this purpose with great success. For this object the native red lead, which is disseminated in small crystals, or plates, in the fissures of quartzose or gritstone gangues, in which it is commonly found, might be pulverized, then boiled with a solution of carbonate of potash, and mixed with nitrate of lead; the acid of which will saturate the potash, and afford its metal to the new acid, which will thus produce a colour no less beautiful than the natural, and perfectly clear of the matrix.

There is reason to presume that chrome, either in the state of oxide or of acid, will be found singly or engaged in some other combinations. For the analysis of the emerald of Peru has already shewn me that its colouring part is afforded by the oxide of this metal; a circumstance which gives the most agreeable proof of the goodness and fixity of this colour, as it is known that the emerald can sustain the most violent degree of heat without losing its colour.

I have likewise found that the yellowish green tufted crystals, which often accompany the red lead ore of Siberia, are formed of chrome and lead, both united, in the state of oxide. Green crystals possessing the same form, the same dimensions, and the same situations on the matrix as those of red lead, but which are a combination of the oxides of chrome and of lead, are likewise found in certain specimens of the native red lead. It is probable that these combinations originally existed in the state of chromate of lead, and that in process of time a portion of oxygen having been carried off by unknown caufes, converted them to the ftate of oxide, and changed the red colour to green.

From paragraph III. it follows that the Siberian red lead contains

	By analyfis. Experiment V.	By fynthefis. Experiment VI.
Oxide of lead - -	63,96	65,12
Chromic acid - -	36,40	34,88

V.

Experiments to determine the Denfity of the Earth. By HENRY CAVENDISH, *Efq. F. R. S. and A. S.**

MANY years ago the Rev. John Michell, F. R. S. contrived a method of determining the denfity of the earth, by rendering fenfible the attraction of fmall quantities of matter; but, as he was engaged in other purfuits, he did not complete the apparatus till a fhort time before his death, and did not live to make any experiments with it. After his death the apparatus came to the Rev. Francis John Hyde Wollafton, Jackfonian profeffor at Cambridge, who, not having conveniences for making experiments with it in the manner he could wifh, prefented it to Mr. Cavendifh.

The apparatus is very fimple: it confifts of a wooden arm fix feet long, made fo as to unite great ftrength with little weight. This arm is fufpended in an horizontal pofition by a flender wire forty inches long, and to each extremity is hung a leaden ball about two inches in diameter; and the whole is inclofed in a narrow wooden cafe, to defend it from the wind.

As no more force is required to make this arm turn round on its centre than is neceffary to twift the fufpending wire, it is plain that, if the wire is fufficiently flender, the moft minute force, fuch as the attraction of a leaden weight a few inches in diameter, will be fufficient to draw the arm fenfibly afide. The weights which Mr. Michell intended to ufe were eight inches diameter. One of thefe was to be placed on one fide of the cafe, oppofite to one of the balls, and as near it as could conveniently be done, and the other on the other fide, oppofite to the other ball, fo that the attraction of both thefe weights would confpire in drawing the arm afide; and when its pofition, as affected by thefe weights, was afcertained, the weights were to be removed to the other fide of the cafe, fo as to draw the arm the contrary way, and the pofition of the arm was to be again determined; and confequently half the difference of thefe pofitions would fhew how much the arm was drawn afide by the attraction of the weights.

In order to determine from hence the denfity of the earth, it is neceffary to afcertain what force is required to draw the arm afide through a given fpace. This Mr. Michell intended to do by putting the arm in motion, and obferving the time of its vibrations, from which it may be eafily computed†.

* Abridgment of his paper in the Philofophical Tranfactions, 1798, p. 469.

† Mr. Coulomb has in a variety of cafes ufed a contrivance of this kind for trying fmall attractions; but Mr. Michell informed Mr. C. of his intention of making this experiment, and of the method he intended to ufe, before the publication of any of Mr. Coulomb's experiments.

6 Mr.

Mr. Michell had prepared two wooden ſtands, on which the leaden weights were to be ſupported and puſhed forwards till they came almoſt in contact with the caſe; but he ſeems to have intended to move them by hand.

As the force with which the balls are attracted by theſe weights is exceſſively minute, not more than one fifty-millionth part of their weight, it is plain that a very minute diſturbing force will be ſufficient to deſtroy the ſucceſs of the experiment: and from the following experiments it will appear, that the diſturbing force moſt difficult to guard againſt is that ariſing from the variations of heat and cold; for, if one ſide of the caſe is warmer than the other, the air in contact with it will be rarefied, and in conſequence will aſcend, while that on the other ſide will deſcend, and produce a current which will draw the arm ſenſibly aſide *.

As Mr. Cavendiſh was convinced of the neceſſity of guarding againſt this ſource of error, he reſolved to place the apparatus in a room which ſhould remain conſtantly ſhut, and to obſerve the motion of the arm from without by means of a teleſcope, and to ſuſpend the leaden weights in ſuch a manner that he could move them without entering into the room. This difference in the manner of obſerving rendered it neceſſary to make ſome alteration in Mr. Michell's apparatus: and as there were ſome parts of it which Mr. Cavendiſh thought not ſo convenient as could be wiſhed, he choſe to make the greateſt part of it afreſh.

Plate XIX. is a longitudinal vertical ſection through the inſtrument, and the building in which it is placed. A B C D D C B A E F F E is the caſe, *x* and *x* are two balls which are ſuſpended by the wires *h x* from the arm *g h m h*, which is itſelf ſuſpended by the ſlender wire *g l*. This arm conſiſts of a ſlender deal rod *h m h*, ſtrengthened by a ſilver wire *h g h*; by which means it is made ſtrong enough to ſupport the balls, though very light †.

The caſe is ſupported and ſet horizontal by four ſcrews, reſting on poſts fixed firmly into the ground: two of them are repreſented in the figure by S and S; the two others are not repreſented, to avoid confuſion. G G and G G are the end walls of the building. W and W are the leaden weights which are ſuſpended by the copper rods R*r* P*r*R and the wooden bar *r r* from the centre pin P*p*. This pin paſſes through a hole in the beam H H, perpendicularly over the centre of the inſtrument, and turns round in it, being prevented from falling by the plate *p*. M M is a pulley faſtened to this pin, and M*m* a cord wound round the pulley and paſſing through the end wall, by which the obſerver may turn it round, and thereby move the weights from one ſituation to the other. When the weights are in the poſition repreſented in the engraving, both conſpire, by their action on the ſmall weights *x x*, to draw the arm in the ſame direction; but when, by means of the pulley

* Mr. Caſſini, in obſerving the variation-compaſs placed by him in the Obſervatory (which was conſtructed ſo as to make very minute changes of poſition viſible, and in which the needle was ſuſpended by a ſilk thread), found that ſtanding near the box, in order to obſerve, drew the needle ſenſibly aſide; which I have no doubt was cauſed by this current of air. It muſt be obſerved that his compaſs-box was of metal, which tranſmits heat faſter than wood, and alſo was many inches deep; both which cauſes ſerved to increaſe the current of air. To diminiſh the effect of this current, it is by all means adviſable to make the box in which the needle plays not much deeper than is neceſſary to prevent the needle from ſtriking againſt the top and bottom. C.

† Mr. Michell's rod was entirely of wood, and was much ſtronger and ſtiffer than this, though not much heavier; but as it had warped when it came to Mr. Cavendiſh, he choſe to make another, and preferred this form, partly as being eaſier to conſtruct and meeting with leſs reſiſtance from the air, and partly becauſe, from its being of a leſs complicated form, he could more eaſily compute how much it was attracted by the weights.

 MM,

MM, the observer causes them to move through a considerable portion of a semicircle, they will arrive at a second position, in which their joint action will move the arm in the contrary direction. These weights are prevented from striking the instrument by pieces of wood fastened to the wall of the building.

The situation of the arm is determined by slips of ivory placed within the case near each extremity of the arm. These slips bear a set of divisions of the inch into twenty parts, to which a small slip at each end of the arm applies, without contact, a vernier that affords a subdivision into hundredth parts. Estimation by the eye looking through the short telescopes T and T, gives the position of the arm to still greater nicety. The lamps L and L throw light on the divisions by convex glasses, and no other light is admitted into the room.

FK is a wooden rod, which, by means of an endless screw, turns round the support to which the wire *g l* is fastened, and thereby enables the observer to turn round the wire till the arm settles in the middle of the case without danger of touching either side. The wire *g l* is fastened to its support at top, and to the centre of the arm at bottom, by brass clips, in which it is pinched by screws. The different parts are drawn nearly in the proper proportion to each other.

Suppose the arm to be at rest and its position to be observed, let the weights be then moved; the arm will not only be drawn aside thereby, but it will be made to vibrate, and its vibrations will continue a great while; so that, in order to determine how much the arm is drawn aside, it is necessary to observe the extreme points of the vibrations, and from thence to determine the point which it would rest at if its motion were destroyed, or the point of rest. To do this, Mr. Cavendish observes three successive extreme points of vibration, and takes the mean between the first and third of these points as the extreme point of vibration in one direction, and then assumes the mean between this and the second extreme as the point of rest; for, as the vibrations are continually diminishing, it is evident that the mean between two extreme points will not give the true point of rest.

It may, he adds, be thought more exact to observe many extreme points of vibration, so as to find the point of rest by different sets of three extremes, and to take the mean result; but it must be observed, that, notwithstanding the pains taken to prevent any disturbing force, the arm will seldom remain perfectly at rest for an hour together; for which reason it is best to determine the point of rest from observations made as soon after the motion of the weights as possible.

The next thing to be determined is the time of vibration, which he finds in this manner: He observes the two extreme points of a vibration, and also the times at which the arm arrives at two given divisions between these extremes, taking care, as well as he can guess, that these divisions shall be on different sides of the middle point, and not very far from it. He then completes the middle point of the vibration, and by proportion finds the time at which the arm comes to this middle point. He then, after a number of vibrations, repeats this operation, and divides the interval of time between the coming of the arm to these two middle points by the number of vibrations, which gives the time of one vibration. The propriety of this method is more fully explained in the Memoir itself.

In Mr. Cavendish's first experiments the wire by which the arm was suspended was $39\frac{1}{4}$ inches long. It was of copper silvered, one foot of which weighed $2\frac{4}{10}$ths grains; and its stiffness was such as to make the arm perform one vibration in about 15 minutes. It was immediately

immediately found that it was not ſtiff enough, as the attraction of the weights drew the balls ſo much aſide as to make them touch the ſides of the caſe. It was thought proper, however, to make ſome experiments before a ſtiffer wire was ſubſtituted in its place.

In this trial the rods by which the leaden weights were ſuſpended were of iron, the probability of magnetiſm in which ſeemed to be of no conſequence, as the arm contained nothing magnetical. Some trials however, for greater ſecurity, were made with the rods alone. The effect was ſuch as might juſtify a very ſlight ſuſpicion of magnetiſm; for which reaſon the rods were changed for others of copper. When the weights were hung on, the attraction on the ball was very ſenſible, as it carried the latter through fifteen diviſions, or $1\frac{1}{2}$ inch; and the regularity of the reſults was ſuch, that the extremes did not differ from each other more than one-tenth part. It was found that the effect of the attraction ſeemed to increaſe for half an hour or an hour after the motion of the weights; an effect which might be ſuppoſed to ariſe from a want of elaſticity, either in the ſuſpended wire or ſomething it was faſtened to, which might make it yield more to a given preſſure, after a long continuance of that preſſure, than it did at firſt. This ſuſpicion was put to the trial by keeping the wire for two or three hours in a ſtate of torſion amounting to fifteen diviſions, and then returning it by means of the handle K to its original ſituation. It was not found by two repetitions of the experiment that the wire had acquired any ſet or permanent twiſt. The arm was then ſuſpended by a ſtiffer wire, with which the apparatus, inſtead of performing its vibration in about fifteen minutes, employed only ſeven. The motion of the arm was in this caſe not quite ſix diviſions between the two oppoſite poſitions of the weights, or the deviation from the natural ſtation of the arm was half that quantity.

A variation of the power of the weights upon the balls, in ſome reſpect ſimilar to that obſerved with the ſmall wire, was alſo found to take place in the preſent experiments. It happened that the caſe in which the arm was encloſed was placed nearly parallel to the magnetic eaſt and weſt; and therefore, if there was any thing magnetic in the balls and weights, the balls would acquire polarity from the earth—and the weights alſo, after remaining for ſome time in either of the near poſitions, would acquire a polarity of the ſame kind, and attract the balls more ſtrongly on that account; and on the contrary, when removed to the oppoſite near poſition, if the magnetiſm continued permanent in the weights for ſufficient time, the magnetic power would operate in the way of repulſion. This was in fact the kind of effect pointed out by the obſervations. The accurate habits which ſo eminently diſtinguiſh all the experimental proceſſes of Mr. Cavendiſh did not however permit him to reſt contented with the induction to which theſe facts ſeemed to point. He altered his apparatus ſo as to produce at pleaſure a rotation in the weights upon their vertical axes, without opening the room or diſturbing their actual ſituation. By this contrivance he had it in his power to diſpoſe the imaginary magnetic poles in ſuch poſitions as were beſt calculated to detect their operation. The arm was not affected; and conſequently it follows that the irregularity in queſtion did not ariſe from magnetiſm. This concluſion was ſtill more firmly eſtabliſhed by ſubſtituting two actual magnetic bars in the place of the weights; for it was not obſerved that any alteration was produced by reverſing the poſition of their poles.

The next circumſtance that ſuggeſted itſelf was, that the effect might be owing to a difference of temperature between the weights and the caſe; for it is evident, that if the weights

weights were much warmer than the caſe, they would warm that ſide which was next to them, and produce an aſcending current of air, which would be accompanied by a deſcending current towards the oppoſite ſide, and two horizontal currents near the top and bottom; the latter of which would impel the ball towards the weight. Though it ſeemed ſcarcely probable that the weights ſhould happen to be warmer than the caſe, and that in a ſufficient degree to afford any perceptible conſequence, our author nevertheleſs determined to ſubmit the matter to examination. He therefore placed two lamps under the weights while in the mid-way poſition, and placed a thermometer with its ball cloſe to the outſide of the caſe, near that part which one of the weights would approach when brought into the poſition to exert its attraction upon the ball. The door of the room was then ſhut, and ſome time afterwards the weights were moved to this laſt-mentioned poſition. At firſt the arm was drawn aſide only in its uſual manner; but in half an hour the effect was ſo much increaſed that the arm was drawn fourteen diviſions aſide inſtead of about three, as it would otherwiſe have been; and the thermometer was raiſed near $1\frac{1}{2}°$, namely from 61° to $62\frac{1}{2}°$. On opening the door, the weights were found to be no more heated than juſt to prevent their feeling cool to the hand.

From the great effect of a difference of temperature it became deſirable to aſcertain the temperature of the weight itſelf, and of the air near the caſe, in the ſubſequent experiments. A ſmall thermometer was therefore inſerted in one of the balls, and another placed cloſe to the caſe. A number of experiments were then made, in which the differences of temperature between the weights and the external air were obſerved; the weights being in ſome inſtances heated by a lamp, and in others cooled by means of ice. It was found that a higher temperature in the weight occaſioned a greater deviation or apparent attraction of the ball; and, on the contrary, that a lower temperature cauſed the ball to be driven by a current in the oppoſite direction.

After this minute and ſcientific inveſtigation of the cauſes which are capable of producing error in the reſults, Mr. Cavendiſh proceeded to make his fundamental experiments. Theſe are tabulated at length in the memoir itſelf, and followed by a detail of the method of computing the denſity of the earth. The firſt thing to be done is to determine from the time of a vibration, what force is required to draw the arm aſide; and the next, to find the proportion which the attraction of the weight upon the ball bears to that of the earth upon the ſame ball, ſuppoſing the ball to be placed in the middle of the caſe: and from theſe reſults (ſince the diameter and ſpecific gravity of the weights and balls compared with water, and the diameter of the earth, are alſo known; together with the facts that the quantities of matter are as the attractive forces at like diſtances, and vary in the inverſe duplicate ratio of the diſtance) the mean denſity of the earth itſelf may alſo be found. Several corrections requiſite to be made in this denſity are then ſtated; namely, on account of the reſiſtance of the arm to motion, the attraction of the weights on the arm, and on the remote ball; the attraction of the copper rods, and of the caſe itſelf; and for the alteration of the attraction of the weights upon the balls according to the poſition of the arm; which laſt is of more ſignification than all the reſt. For all which I muſt, for the ſake of brevity, refer to the Tranſactions.

The

The following TABLE contains the Refults of the Experiments.

Experiment	Mot. weight.	Mot. arm.	Do. corr.	Time vibr.	Do. corr.	Denfity.
1.	m to +	14,32	13,42	′ ″	—	5,5
	+ to m	14,1	13,17	14,55	—	5,61
2.	m to +	15,87	14,69	—	—	4,88
	+ to m	15,45	14,14	14,42	—	5,07
3.	+ to m	15,22	13,56	14,39	—	5,26
	m to +	14,5	13,28	14,54	—	5,55
4.	m to +	3,1	2,95	—	6,54	5,36
	+ to —	6,18	—	7,1	—	5,29
	— to +	5,92	—	7,3	—	5,58
5.	+ to —	5,9	—	7,5	—	5,65
	— to +	5,98	—	7,5	—	5,57
6.	m to —	3,03	2,9			5,53
	— to +	5,9	5,71			5,62
7.	m to —	3,15	3,03	7,4 by mean.	6,57	5,29
	— to +	6,1	5,9			5,44
8.	m to —	3,13	3,00			5,34
	— to +	5,72	5,54			5,79
9.	+ to —	6,32	—	6,58	—	5,1
10.	+ to —	6,15	—	6,59	—	5,27
11.	+ to —	6,07	—	7,1	—	5,39
12.	— to +	6,09	—	7,3	—	5,42
13.	— to +	6,12	—	7,6	—	5,47
	+ to —	5,97	—	7,7	—	5,63
14.	— to +	6,27	—	7,6	—	5,34
	+ to —	6,13	—	7,6	—	5,46
15.	— to +	6,34	—	7,7	—	5,3
16.	— to +	6,1	—	7,16	—	5,75
17.	— to +	5,78	—	7,2	—	5,68
	+ to —	5,64	—	7,3	—	5,85

In the fecond column the letter m denotes the middle pofition of the weights when each is equidiftant from the balls, + denotes one of the near pofitions, and — the oppofite near pofition.

I copy

I copy Mr. Cavendiſh's concluding remarks in his own words:

" From this table it appears, that though the experiments agree pretty well together, yet the difference between them both in the quantity of motion of the arm and in the time of vibration is greater than can proceed merely from the error of obſervation. As to the difference in the motion of the arm, it may very well be accounted for from the current of air produced by the difference of temperature; but whether this can account for the difference in the time of vibration is doubtful. If the current of air was regular, and of the ſame ſwiftneſs in all parts of the vibration of the ball, I think it could not; but as there will moſt likely be much irregularity in the current, it may very likely be ſufficient to account for the difference.

" By a mean of the experiments made with the wire firſt uſed, the denſity of the earth comes out 5,48 times greater than that of water; and by a mean of thoſe made with the latter wire it comes out the ſame; and the extreme difference of the reſults of the twenty-three obſervations made with this wire is only ,75; ſo that the extreme reſults do not differ from the mean by more than ,38, or $\frac{1}{14}$th of the whole, and therefore the denſity ſhould ſeem to be determined hereby to great exactneſs. It indeed may be objected, that as the reſult appears to be influenced by the current of air or ſome other cauſe, the laws of which we are not well acquainted with, this cauſe may perhaps act always or commonly in the ſame direction, and thereby make a conſiderable error in the reſult. But yet, as the experiments were tried in various weathers and with conſiderable variety in the difference of temperature of the weights and air, and with the arm reſting at different diſtances from the ſides of the caſe, it ſeems very unlikely that this cauſe ſhould act ſo uniformly in the ſame way as to make the error of the mean reſult nearly equal to the difference between this and the extreme; and therefore it ſeems very unlikely that the denſity of the earth ſhould differ from 5,48 by ſo much as $\frac{1}{14}$th of the whole.

" Another objection perhaps may be made to theſe experiments, namely, that it is uncertain whether in theſe ſmall diſtances the force of gravity follows exactly the ſame law as in greater diſtances. There is no reaſon however to think that any irregularity of this kind takes place until the bodies come within the action of what is called the attraction of coheſion, and which ſeems to extend only to very minute diſtances. With a view to ſee whether the reſult could be affected by this attraction, I made the 9th, 10th, 11th, and 15th experiments, in which the balls were made to reſt as cloſe to the ſides of the caſe as they could; but there is no difference to be depended on between the reſults under that circumſtance and when the balls are placed in any other part of the caſe.

" According to the experiments made by Dr. Maſkelyne on the attraction of the hill Schehallien, the denſity of the earth is $4\frac{1}{2}$ times that of water; which differs rather more from the preceding determination than I ſhould have expected. But I forbear entering into any conſideration of which determination is moſt to be depended on, till I have examined more carefully how much the preceding determination is affected by irregularities whoſe quantity I cannot meaſure."

VI. *An*

VI.

An Inquiry concerning the Chemical Properties that have been attributed to Light. By BENJAMIN, *Count of Rumford, F. R. S. M. R. I. A.*

(Concluded from page 405.)

HAVING been fo fuccefsful in my attempts to reduce the oxide of gold, by means of charcoal, *in the moift way*, I loft no time in making fimilar experiments with the oxide of filver.

Experiment No. 11. A folution of fine filver, in ftrong nitrous acid, was evaporated by drynefs, and the refiduum re-diffolved in diftilled water.

A portion of this folution (which was perfectly colourlefs), diluted with twice as much diftilled water, was poured into a phial containing a number of fmall pieces of charcoal; and the phial, being well clofed with a new cork ftopple, was expofed to the action of the fun's rays.

In lefs than an hour fmall fpecks of revived filver began to make their appearance on the furface of the charcoal; and, at the end of two hours, thefe fpecks became very numerous, and had increafed fo much in fize, that they were diftinctly vifible to the naked eye at the diftance of more than three feet. They were very white, and poffeffed the metallic fplendour of filver in fo high a degree, that when enlightened by the fun's beams their luftre was nearly equal to that of very fmall diamonds.

The phial, which was in the form of a pear, and about $1\frac{1}{2}$ inch in diameter at its bulb, was very thin, and made of very fine colourlefs glafs; the aqueous folution was alfo perfectly tranfparent and colourlefs; and, when the contents of the phial were illuminated by the direct rays of a bright fun, the contraft of the white colour of thefe little metallic fpangles with the black charcoal to which they were fixed, and their extreme brilliancy, afforded a very beautiful and interefting fight.

As the air had been previoufly expelled from the charcoal by boiling it in diftilled water, it was fpecifically heavier than the aqueous folution of the metallic oxide, and confequently remained at the bottom of the bottle.

Experiment No. 12. A phial as nearly as poffible like that ufed in the laft experiment, and containing the fame quantity of diluted aqueous folution of nitrate of filver, and alfo of charcoal, was inclofed in a cylindrical tin box, and expofed one hour to the heat of boiling water in an apparatus ufed for boiling potatoes in fteam for the table.

The refult of this experiment was uncommonly ftriking: the furface of the charcoal was covered with a moft beautiful metallic vegetation; fmall filaments of revived filver, refembling fine flatted filver wire, pufhing out from its furface in all directions!

Some of thefe metallic filaments were above one-tenth of an inch in length. On agitating the contents of the phial, they were eafily detached from the furface of the charcoal, to which they feemed to adhere but very flightly.

Thefe experiments were repeated feveral times, and always with precifely the fame refults.

When the oxide of gold was reduced in this way, the revived metal appeared under the form of fmall fcales, adhering firmly to the furface of the charcoal. May not the difference of of the forms under which gold and ſilver are revived from their oxides, in this proceſs, be owing to the difference of the ſpecific gravities of thoſe metals ?

The following experiments, which were firſt ſuggeſted by an accident, were made with a view to inveſtigate ſtill farther the cauſes of thoſe effects which have been attributed to the ſuppoſed chemical properties of light.

Having accidentally put away two ſmall phials, each containing a quantity of aqueous ſolution of the oxide of gold and ſulphuric ether, in each of which the ether had extracted the gold completely from the ſolution, as was evident by the yellow colour of the ſolution having been transferred to the ether, and the ſolution being left colourleſs ; in one of the phials, which happened to ſtand in a window in which there was occaſionally a ſtrong light (though the direct rays of the ſun never fell on it), I found, in about three weeks, that the oxide was almoſt entirely reduced ; the revived gold appearing in all its metallic ſplendour in the form of a thin pellicle, ſwimming on the ſurface of the aqueous liquor in the phial, and the colour of the ether which repoſed on it having become quite faint; while no viſible change had been produced in the contents of the other phial, which had ſtood in a dark corner of the room.

As theſe appearances induced me to ſuſpect, or rather ſtrengthened the ſuſpicions I had before conceived, that the ſeparation of gold from ether, under its metallic form, when a ſolution of its oxide is mixed with that fluid, is always effected by a reduction of the oxide by means of light, I made the following experiment, with a view to the farther inveſtigation of that matter.

Experiment No. 13. Into a ſmall pear-like phial of very fine tranſparent glaſs I put equal quantities of an aqueous ſolution of the muriatic oxide of gold and ſulphuric ether; and the phial, which was about half filled, being cloſed with a good cork well ſecured in its place, was expoſed to the action of the direct rays of a bright ſun.

A pellicle of revived gold, in all its metallic ſplendour, began almoſt immediately to be formed on the ſurface of the aqueous liquid, and ſoon covered it entirely ; and at the end of two hours the whole of the oxide was completely reduced, as was evident from the appearance of the ether, which became *perfectly colourleſs*.

On ſhaking the phial, the metallic pellicle, which covered the ſurface of the aqueous liquid, was broken into ſmall pieces, which had exactly the appearance of leaf-gold, poſſeſſing the true colour and all the metallic brilliancy of that metal.

On ſuffering the phial to ſtand quiet, the aqueous liquor and the ether ſeparated, and moſt of the broken pieces of the thin ſheet of gold deſcended to the bottom of the phial : the remainder of them floated on the ſurface of the aqueous liquid ; and the ether, as well as the aqueous liquid, appeared to be perfectly tranſparent and *colourleſs.*

By the length of time which was required for the ether and the aqueous liquid to ſeparate, I thought I could perceive that the ether had loſt ſomething of its fluidity ; but as this was an event I expected, it is the more likely, on that account, that I was deceived, when I imagined I ſaw proofs of its having taken place.

On removing the cork, after the contents of the bottle had been ſuffered to cool, there was no appearance of any conſiderable quantity of air, or other permanently elaſtic fluid, having been either generated or abſorbed during the experiment.

Finding that the oxide of gold might be ſo completely and ſo expeditiouſly reduced by means

means of ether, I conceived it might be poffible to perform that chemical procefs, *in the moift way*, by means of effential oils; and this conjecture proved to be well founded.

Experiment No. 14. Upon a quantity of a diluted aqueous folution of nitro-muriate of gold, in a fmall pear-like phial, about 1½ inch in diameter at its bulb, was poured a fmall quantity of etherial oil of turpentine, juft as much as was fufficient to cover the aqueous folution to the height of $\frac{2}{10}$ of an inch; and the phial, being well clofed with a good cork, well fecured, was expofed one hour to the heat of boiling water in a fteam-veffel.

The gold was revived, appearing in the form of a fplendid pellicle, of a bright gold colour, which floated on the furface of the aqueous liquid. The oil of turpentine, which, at the beginning of the experiment, was as pale and colourlefs as pure water, had taken a bright yellow hue; and the aqueous fluid, on which it repofed, had entirely loft its yellow colour.

On fhaking the phial, its contents were intimately mixed; but, on fuffering it to ftand quiet, the oil of turpentine foon feparated from the aqueous liquid, retaining its bright yellow hue, and leaving the aqueous liquid colourlefs.

On fhaking the phial, *before it had been expofed to the heat*, and mixing its contents, and then fuffering it to ftand quiet, the oil of turpentine, on taking its place at the top of the aqueous folution, was not found to have acquired any colour; nor was the bright gold colour of the folution found to be at all impaired. When fulphuric ether was ufed inftead of the oil of turpentine, the effect was in this refpect very different.

To find out whether the oil of turpentine ufed in this experiment, and which had acquired a deep yellow colour, had loft that property by which it effected the reduction of the metallic oxide, I now poured an additional quantity of the aqueous folution of the oxide into the phial, and, fhaking the phial, expofed it with its contents to the heat of boiling water.

After it had been expofed to this heat about two hours I examined it, and found, that though a confiderable quantity of gold had been revived, yet the aqueous liquid ftill retained a faint yellow colour.

The oil of turpentine had acquired a deeper and richer gold colour, approaching to orange.

To the contents of the phial I now added about half as much diftilled water, and, mixing the whole by fhaking, I expofed the phial again, during two hours, to the heat of boiling water; when the remainder of the oxide was reduced, and the aqueous liquid left perfectly *colourlefs*.

On repeating this experiment with oil of turpentine, and varying it, by ufing a folution of the oxide of *filver* (an aqueous folution of nitrate of filver,) inftead of that of *gold*, the refult was nearly the fame: the metal was revived, and the oil of turpentine acquired a faint greenifh-yellow colour.

I alfo revived the oxides of gold and of filver with *oil of olives*, by a fimilar procefs, with the heat of boiling water. The oil of olives ufed in thefe experiments loft its tranfparency, and became deeply coloured: that ufed in the reduction of the oxide of filver taking a very deep dirty brown colour, approaching to black; and that employed in reducing the oxide of gold being changed to a yellowifh-brown, with a purple hue.

 In

In the experiment with the oxide of filver, the infide of the phial, in the region where the oil repofed on the aqueous folution, was beautifully filvered, the revived metal forming a narrow metallic ring, extending quite round the phial; and, in both experiments, fmall detached pellicles of revived metal were vifible in the oil, and adhered in feveral places to the infide of the phial, forming bright fpots, in which the colour of the metal and its peculiar fplendour were perfectly confpicuous.

Experiment No. 15. As *carbon* is one of the conftituent principles of fpirit of wine, as well as of effential oils and fulphuric ether, I thought it poffible that I might fucceed in the reduction of the oxide of gold, by mixing alcohol with an aqueous folution of nitro-muriate of gold, and expofing the mixture in a phial well clofed to the heat of boiling water; but the experiment did not fucceed.

By pouring upon this mixture a fmall quantity of oil of olives, and expofing it again to the heat of boiling water, the gold was revived.

Is it not probable that the reafon why the oxide was not reduced by alcohol, is the mobility of thofe elements, which ought to act on each other, in order that the effect in queftion may be produced? I have no doubt but the oxide would be reduced, could the alcohol be made to reft on the furface of the aqueous folution without mixing with it.

I wifhed to have been able to have collected and examined the elaftic fluids, which probably were formed in moft of the preceding experiments; but my time was fo much taken up with other matters that I had not leifure to purfue thefe inveftigations farther.

In order to fee what effects would be produced by the heat generated at the furface of an opaque body, of a nature different from thofe hitherto ufed in the reduction of the metallic oxides, and one that is little difpofed to form a chemical union with oxygen, (*magnefia alba*) when, being immerfed in an aqueous folution of the oxide of gold, the rays of the fun were made to impinge on it, I contrived the following experiment.

Experiment No. 16. I took four fmall thin phials, A, B, C, and D, of very fine glafs, and putting into each of them about five grains of dry *magnefia alba*, I filled the phial A, nearly full, with a faturated aqueous folution of the oxide of gold.

I filled the phial B, in like manner, with fome of the fame folution, diluted with an equal quantity of diftilled water; and the phials C and D were filled with the folution ftill farther diluted.

Thefe phials, open or without ftoppers, were expofed one whole day to the action of the direct rays of a bright fun, their contents being often well mixed together during that time by fhaking.

The contents of all thefe phials changed colour more or lefs, but they acquired very different hues. The contents of the phial A became of a very deep rich *gold colour*, approaching to *orange*, the earthy fediment being throughout of the fame tint.

The contents of the phial B, which were at firft of a light ftraw colour, firft changed to a light green, and then to a greenifh blue. The phial having been fuffered to ftand quiet feveral days, in an uninhabited room in a retired part of the houfe, the folution became nearly colourlefs, and the fediment was found to be of a dirty olive colour.

The

The colour of the contents of the phials C and D was changed nearly in the ſame manner; and having been ſuffered to ſtand quiet two or three days to ſettle, the ſolution was found to be quite colourleſs, and the ſediment to be deeply coloured. There was, however, a very remarkable difference in the hues of the two phials; that of the phial C being of a light greeniſh-blue; while that in the phial D was indigo, and of ſo deep a tint that it might eaſily have been taken for black.

Theſe appearances were certainly very ſtriking, and well calculated to excite my curioſity; but I am ſo much engaged in public buſineſs that it is not at preſent in my power to purſue theſe inquiries farther. I wiſh that what I have done may induce others, who have more time to ſpare, to devote ſome portion of their leiſure to theſe intereſting inveſtigations.

VII.

Some Account of the Perſian Cotton-Tree. By MATTHEW GUTHRIE, *M.D. F.R.S. &c. &c.**

COTTON is a plant of both the old and the new world; at leaſt it is found wild in both: but I have my doubts whether it was a native of America before the Europeans carried it over, and ſhall aſſign reaſons for my incredulity when I come to treat of the *Perſian cotton*, which is the very ſpecies that is ſaid to be American.

Five ſpecies of the cotton-tree are enumerated by Linnæus; and there is reaſon to ſuſpect the exiſtence of a ſixth, if what we are told of the extreme fineneſs and ſilky nature of a particular kind reared in ſome of the Antilles be literally true. This curious variety is called Siam cotton, becauſe the reed was originally obtained from Siam.

The firſt ſpecies of cotton is the *goſſypium arboreum*, or *Indian cotton-tree*, which has been cultivated and manufactured in the Eaſt Indies from the remoteſt period of the authentic hiſtory of that country, or between three and four thouſand years. It delights in a ſandy ſoil.

The ſecond ſpecies is the *goſſypium religioſum*, which is likewiſe a native of India, and a tree, or at leaſt a high ſhrub; but why Linnæus dignified it with ſo ſingular a ſpecific name I ſhall leave the learned Aſiatic Society in Bengal to determine, as they muſt know if it be uſed for any religious purpoſes by the Bramins. This ſpecies of cotton is ſaid to be that which is cultivated by the French in Martinico.

The third is the *G. barbadenſe*, a ſpecies of biennial cotton ſhrub cultivated in our Britiſh iſland of Barbadoes, from which it obtains its ſpecific name. I believe it is likewiſe the ſame ſpecies which is cultivated in Jamaica.

The fourth is the *G. hirſutum*, an American *perennial* cotton ſhrub in the warmer provinces, but *annual* in the colder, as is ſometimes the caſe with plants in climates where their roots loſe their vegetating power by winter froſt.

The fifth and laſt ſpecies is the *G. herbaceum*, or *G. annuum*, an annual cotton plant which riſes to the height of three or four feet, and is ſown and reaped, like corn, twice a

* Mancheſter Memoirs, Vol. V. Part I.

 year

year in hot countries, and once a year in colder climates. It bears a large yellow flower with a purple centre, and fruit about the ſize of a walnut containing the cotton.

This is the famous *Perſian cotton* properly the ſubject of the paper, although a ſlight mention of other ſpecies was neceſſary to give a more complete view of the ſubject. Linnæus calls it a native of America; and there is no doubt but that it is become ſo, although there is much more reaſon to ſuppoſe America naturalized a Perſian plant than that Perſia got it from the new world; eſpecially if we are to credit a paper lately preſented by a Britiſh merchant to the Economical Society at Peterſburgh, in which it is poſitively aſſerted that ſeveral of the European nations furniſhed their American colonies with *Perſian cotton* ſeed procured at Smyrna. Now this fact (if ſufficiently authentic, which I do not doubt from my knowledge of the veracity of the author) will eaſily account for the *G. herbaceum* being found wild in America; when we recollect the wonderful proviſion of nature for the wide diſperſion of ſeeds, and Linnæus's aſſertion that the *Erigeron canadenſe* was diſperſed from the botanic garden of Paris by the winds over a great part of Europe, and ſeveral other plants* from the botanic garden of Upſal over a whole province.

My reaſons for ſuggeſting theſe doubts relative to the native country of this ſpecies of cotton are, that all vegetables of this genus are ſuppoſed to have been indigenous in Perſia excluſively, and that even the Eaſt Indies derived the cotton plants from thence; a conjecture which ſeems to have acquired ſome degree of credit from the late diſcovery of Sir William Jones, viz. that the Hindoos, or inhabitants of India, were originally a colony of the ancient Iran or Perſia, which ſeems to have been the cradle of the human ſpecies, ſince its ancient language appears to have been the mother of all thoſe now exiſting (with the exception of the Arabic and Tartarian), of which neverthelefs it contained many words.

Now it is very poſſible that the firſt colony carried the cotton plant with them to India, and that it was afterwards diſperſed from Hindoſtan to the adjacent countries and iſlands. The cotton plant is widely diſperſed likewiſe throughout Europe and ſome parts of Africa, particularly the annual or herbaceous ſpecies (the very plant treated of here) reared in the north of Perſia, and which is alſo cultivated in Malta †, Sicily, Chio, Lemnos, and other iſlands of the Archipelago, although poſſibly the cotton of theſe iſlands may be varieties of the ſpecies from difference of ſoil, climate, &c.

The beſt of the European cotton is brought from Cyprus; but Smyrna, Aleppo, Damaſcus, Jeruſalem, &c. furniſh likewiſe a quantity of cotton at leaſt equal to the European.

Cultivation of Cotton in Perſia.

THE annual cotton, or this laſt ſpecies, of which we have treated more amply, is much cultivated in the northern or colder provinces of Perſia bordering on the Caſpian Sea (as the perennial is in the ſouthern); and it is from thence that the ſeeds now ſent to Portugal have been obtained through the Bucharian Tartar merchants, and are the production of the *Goſſypium herbaceum* of Linnæus, the *Goſſypium annuum* of Pallas. It is ſown in Perſia from the end of March to the end of April, and reaped in September. This ſpecies requires a rich ſoil mixed with ſand; and therefore where the land is not rich enough they

* The Antirrhinum minus, the Datura ſtrantonium, the Gnaphalium americanum, &c.

† There is a kind of cotton cultivated in Malta, of a nankeen colour, which exceeds in fineneſs all other cotton, and is much ſuperior even to that from the Antilles.

manure

manure it with cow or sheep dung; although we are told that when the plants are once raised above the ground any species of soil will answer. The ground is worked in the spring, and the seeds are planted at the distance of eight or ten inches from one another, whilst care is taken to weed it, to give air to the young plants. Dry summers give the best crop, as rain is more particularly hurtful when it falls in great quantities during the flowering and ripening of the cotton. It is gathered, as said above, in September, care being always taken to collect a sufficient quantity of seed for the next year. Lastly, watering the young plants with a mixture of wood-ashes and water in certain situations is sometimes necessary to guard them from destructive worms.

The Russians have cultivated the same species of Persian cotton in the government of Caucasus, and rear enough of it to serve their own national manufactures, which are not as yet either numerous or considerable; but on the Terck, at the foot of the Caucasus, where it is reared, they do not sow till the middle of May, lest a late spring frost, which is sometimes felt in those parts, should destroy the hopes of the planter. With that one exception, the Russians strictly observe the Persian mode of culrivation.

There is a species of silky cotton much cultivated at present in Germany, which possibly may merit the attention of Portugal for their plantations in America. It is the *Asclepias syriaca* of Linnæus, and affords so fine a species of cotton (if I may so name it) that fabrics have been erected in Saxony, where stuffs are made of it which rival in lustre, &c. the true animal silk. But this new vegetable silk has circumstances attending it that seem to recommend its cultivation in some of the American colonies and islands: First, because it is originally the native of a hot climate, as Linnæus's specific name indicates; and of course it is likely to be in its greatest beauty and excellence in climates which approach nearest to that of its native country. Secondly, because its stalks afford a coarse sort of cloth well calculated to clothe negroes, whilst from the pith of them paper is made.

VIII.

Facts and Observations concerning the Measure and Expence of first Movers, namely, Wind, Water, Steam, and Animal Strength, and on other Objects of general Utility. (*W. N.*)

THE consideration of the value and importance of natural first movers is of consequence not only to practical engineers, but to every individual in cultivated society. There are numberless situations, even in the spirited manufacturing kingdom we inhabit, where large revenues are expended to perform mechanical and hydraulic operations by the force of human labour, or by horses, which might be for the most part saved by substituting a steam-engine or windmill, or making use of a stream of water now running to waste. It is well known, that, since the extension of the cotton and other works, estates of small rent in the neighbourhood of Manchester and elsewhere have been, and continue to be, let at more than twenty times their original rent, merely from the fortunate circumstance of their possessing a small stream of water falling with a sufficient declivity to give motion to a mill. If the proprietors of lands and manufacturers in general were better acquainted with the simple methods of estimating the forces of those currents of water which run neglected through

through their grounds and premifes, and which an intelligent obferver need not walk many miles in any country to point out, their property and revenue might immediately receive a confiderable acceffion; and the community would be ftill more effectually benefited. The inceffant demands for the employment of fuch forces in grinding corn, colours, drugs, tobacco; in cutting bark and other tanners' and dyers' materials; in fawing wood; in laminating, drawing, or fafhioning metallic bodies; in fpinning, weaving, fulling, &c. the products of the organized kingdoms by arts already practifed, exclufive of the many improvements which may be expected in their application, are too numerous to afford the leaft reafon for any proprietor to fear a want of employment, or to confider the erection of a mill in a proper fituation as a fpeculation of the leaft danger or probable difadvantage. Similar obfervations are to a certain extent applicable in favour of the ufe of horfes inftead of men, and fteam inftead of horfes, in every cafe where the power is required to be great or long continued, and the fkill either little, or capable of being fupplied by machinery.

I am tempted to digrefs for a moment from my fubject by the natural recurrence of a political reflection, fo obvious that it fcarcely ever fails to be made when the extenfion of machinery and the application of inanimate powers are confidered. It is ftated by certain humane but miftaken objectors, that the fcheme of mechanical and chemical improvement is pointed againft the human fpecies, and tends to drive them out of the fyftem of beneficial employment. Two creatures offer themfelves to me for employment and fupport—a man and a horfe. I muft invariably prefer the latter, and leave the former to ftarve. Two other beings—a horfe and a fteam-engine, are candidates for my favour. My preference to the latter tends to exterminate the fpecies of the former. In both cafes it is ftated, that the number of intelligent creatures capable of the enjoyment of happinefs muft be diminifhed for want of fupport; and that, on the whole, the fum of the propofed improvements is not only a lefs proportion of good to fociety, but a pofitive acceffion of much mifery to the unemployed poor.

On this wide and extended argument, which can in fact be maintained againft improvements in no other way than by infifting that the favage ftate, with all its wants, its ignorance, its ferocity, and its privations, is preferable to the focial intercourfe of effort and divifion of labour we are habituated to prefer, it may be fufficient to obferve, that the topic includes matter not only for reafoning and induction, but alfo for experiment. By reference to the matter of fact, though it muft be allowed that new improvements, which change the habits of the poor, muft at firft expofe them to temporary inconvenience and diftrefs, againft which, in fairnefs, it is the duty of fociety to defend them; yet the invariable refult of fuch improvements is to better the condition of mankind. The nations which have fhewn the moft ingenuity and induftry in this way are not only the richeft, the moft populous, the moft intelligent, and the beft defended; but the provinces of thofe nations are feen to flourifh in proportion to their refpective degrees of activity in this refpect. And from thefe exertions it is, as Smith* emphatically remarks, that "the accommodation of an European prince does not always fo much exceed that of an induftrious and frugal peafant, as the accommodation of the latter exceeds that of many an African king, the abfolute mafter of the lives and liberties of ten thoufand naked favages."

* Wealth of Nations, i. ch. i.

But

But to return to our ſubject. In this practical and popular communication I ſhall avoid entering into any diſcuſſion of the theory of windmills, which has employed the attention of ſo many eminent men for a century paſt. The advantages of theſe engines for ſuch work as will admit of being performed and ſuſpended by intervals are ſufficiently known, but perhaps not ſufficiently attended to. There can be no doubt but that the Dutch, who uſe windmills for ſawing, pumping, and various other works, as well as grinding, muſt have found them very profitable, ſince their country is overſpread with them. It may be proper however to take notice, that many writers have copied one from another the determination of Maclaurin, that the beſt angle for windmill-ſails to make with the line of direction of the wind is $54° 44'$, which is only true, as that excellent mathematician obſerves, at the very commencement of the motion, and requires to be enlarged as the velocity of the ſails increaſes: for the law of which, and other eſſential objects, his account of Sir Iſaac Newton's Philoſophical Diſcoveries, and his Treatiſe on Fluxions, may be conſulted. Smeaton, who had much experience in the buſineſs of a civil engineer, and whoſe data may always be depended on, though his theories are not conſtantly accurate, made a ſet of experiments on the conſtruction and effects of windmill-ſails, which are deſcribed in the Philoſophical Tranſactions for the year 1759*. This engineer ſtates, that the mean rate of work for mills with Dutch ſails is when they make about thirteen turns in a minute, which is when the velocity of the wind is $8\frac{2}{3}$ miles in an hour, or $12\frac{2}{3}$ feet in a ſecond: and this wind in common phraſe would be called a freſh gale. Taking the maximum of Deſaguliers, hereafter to be mentioned, as his ſtandard for computation, he deduces the ſize of a windmill-ſail of the figure juſt mentioned, and alſo according to a figure conſtructed from his own experiments, which ſhall be equal in mean power to one man; and thence he arrives at the inference, that one of his own ſails, thirty feet in length, will, when working at a mean rate, be equal to the power of 18,3 men. He had an opportunity of verifying this in the large way in a mill uſed for cruſhing rape-feed. The mean power of a windmill is therefore very conſiderable; but what may be the annual or average quantity of work ſuch an apparatus is capable of performing under all the viciſſitudes of the wind, I poſſeſs no means of aſcertaining.

In the ſame treatiſe Smeaton makes ſeveral very juſt remarks on thoſe windmills which are acted upon by the direct impulſe of the wind againſt ſails fixed to a vertical ſhaft. His objections have, I believe, in every inſtance been juſtified by the inferior efficacy of theſe mills when compared with the charges of erecting them. He alſo maintains that water-mills with oblique ſails, upon the principle of the common windmill, cannot prove beneficial to the undertaker. It is indeed probable that moſt of the circumſtances of running-water are likely to render the common over and underſhot-wheels cheaper and more effectual, and that the oblique float-board will in no inſtance come near the effect of a cloſe overſhot wheel. But it is alſo certain that ſuch wheels are uſed in China, in the ſouth of France, and elſewhere, with much more effect than Smeaton appears diſpoſed to think them capable of: and at all events, the ſubject of theſe wheels deſerves to be conſidered.

* This account has ſince been republiſhed, together with other valuable papers of the ſame author, under the title of An Experimental Inquiry concerning the Natural Powers of Wind and Water to turn Mills, &c. By John Smeaton, F. R. S. 8vo, printed for Taylor, London, 1794.

The enquiry which moft immediately interefts land-holders, and others who have the advantage of a current of water, is to afcertain whether it will afford fufficient power to juftify the erection of a mill, and what that power may be. If the ftream be ample without much fall, it muft neceffarily be applied to move an underfhot wheel by its impulfe, and the power will be determinable from the velocity of the water, and the quantity which paffes through the fection of its bed. One of the eafieft methods of afcertaining thefe data is that given by Defaguliers *, as follows. Obferve a place where the banks of the river are fteep and parallel, fo as to make a kind of trough for the water to run through, and by taking the depth acrofs make a true fection of the river. Stretch a ftring at right angles over it, and at a fmall diftance another parallel to the firft. Then take an apple, an orange, or other fmall ball, juft fo much lighter than water as to fwim in it, and throw it into the water above the ftrings. Obferve when it comes under the firft ftring by means of an half-fecond pendulum, a ftop-watch, or any other proper inftrument, and likewife when it arrives at the fecond ftring. By this means the velocity of the upper furface, which in practice may generally be taken for that of the whole, will be obtained. The fection of the river at the fecond ftring muft be afcertained by taking the depth as before. If this furface or fection be the fame as the former, it may be taken for the mean fection; if not, add both together, and take half the fum for the mean fection. The area of the mean fection in fquare feet being then multiplied by the diftance between the ftrings in feet, will give the contents of the water in folid feet, which paffed from one ftring to the other during the time of obfervation. And this, by the rule of three, may be adapted to any other portion of time. Suppofe, for example, the time had been 12″, and the hourly expenditure of water were required, the proportion would be: As 12″ are to 3600″, fo is the number of cubic feet obferved to the hourly expenditure in cubic feet. If the mere velocity be required in proportion to any fixed interval of time, the fame proportion will give it, provided, inftead of the folid contents in the third term, there be taken the diftance between ftring and ftring.

The intelligent obferver may in general abridge this operation, by taking notice of the arrival of the floating body oppofite two ftations on the fhore, efpecially when it is not convenient to ftretch a ftring acrofs. The arch of a bridge is a good ftation for an experiment of this kind, becaufe it affords a very regular fection and two fixed points of obfervation: and in fome inftances the fea practice of heaving the log may have its advantages. Where a time-piece is not at hand it may be equally convenient, provided two obfervers attend, to note the time with a half or quarter-f conds pendulum. The half-feconds pendulum is made by fufpending a fmall round button, or other fpherical weight, by a thread looped over a pin of fuch a length that the diftance from the bend of the loop to the centre of the weight fhall be 9,8 inches. The quarter-feconds pendulum is one fourth of this length. If, by obfervations at feveral ftations above and below any particular point of the river, the velocity is not found to vary, the fection of the river in all that fpace may be concluded to be uniform; and it will not be neceffary to determine more than one fection by actual meafurement.

In the cafe of an overflowing pond, or fmall ftream, which will admit of a dam acrofs it,

* Courfe of Experimental Philofophy, ii. 419.

the

the quantity of water afforded may be aſcertained by ſuffering it to run through a notch in a board, or a vertical hole of an inch ſquare, according to the following table, which Deſaguliers ſays he calculated from repeated experiments*.

A TABLE of the Expence of Water through an Inch-ſquare Hole, and through a Cut an Inch wide and of different Depths.

Inches below the ſurface.	Expence of water through a hole of a ſquare inch at different depths, according to the number of inches in the firſt column.	Expence of water through a notch of different depths, according to the number of inches in the firſt column
	Tuns in an Hour.	
1	1,04	1,04
2	1,46	2,50
3	1,79	4,29
4	2,08	6,37
5	2,31	8,68
6	2,53	11,21
7	2,74	13,95
8	2,92	16,87
9	3,12	19,19
10	3,28	22,47
11	3,44	25,81
12	3,58	29,39
13	3,74	33,13
14	3,88	36,91
15	3,95	40,86
16	4,16	45,02
17	4,28	49,30
18	4,40	53,70
19	4,52	58,22
20	4,62	62,84
21	4,76	67,60
22	4,87	72,47
23	4,94	77,41
24	5,06	82,47
25	5,2, or 5 tuns and 50,4 gallons.	87,67

A cubic foot of water weighs very nearly 62½ pounds averdupois, and an hogſhead of water weighs about 550 pounds.

* Courſe of Experimental Philoſophy, ii. 129.

In the consideration of power or force to be derived from water in motion, the water may be taken as a determinate mass falling through a given height in a given time. In order that this descending weight may cause another weight to ascend, or may overcome some resistance in the way of work with that degree of speed which shall be the most profitable, it is necessary that the resistance or work to be done should be neither too great nor too little. If it be too great, the slowness of operation will diminish the quantity of work; and if it be too small, the speed will not sufficiently compensate for this smallness. When the power is therefore known, it remains to deduce what may be the effect. But in the first place, as the height from which the water flowing in a river may have descended, in order to acquire its velocity, is, from a variety of circumstances, difficult to be ascertained, and also very different from that height which would immediately and without impediment produce the same velocity, it becomes necessary to compute this last height, which hydrostatical writers usually call the height of the virtual head. These writers teach, that the velocity of a fluid spouting through an orifice in a thin plate is the same as would be acquired by a body falling in clear space from the height of the surface of the fluid above the orifice. Hence, from the common doctrine of falling bodies, if the uniform velocity of a stream be expressed in feet per second, the virtual height of the fall will be found by multiplying the given velocity into itself, and dividing the product by 64,2882; the quotient will be the required height expressed in feet *.

The effect of undershot and overshot-wheels has been treated by various authors, who have given results extremely different from each other. Smeaton, in the Treatise often quoted in the course of this communication, observes, that Belidor in his Architecture Hydraulique, i. 286, endeavours to demonstrate that water applied undershot will do six times more execution than the same applied overshot; while Desaguliers, whom he (Smeaton) misquotes by overlooking the difference of fall, is said to have given the advantage as ten to one in favour of the overshot †. The particular experiments of Smeaton himself, as well as his experience, point out the following results.

The effect in undershot-mills in the large way is at best one third of the power; that is to say, the wheel, being driven with two-fifths of the velocity of the stream, will raise a quantity of water equal to one-third of the column, which strikes the float-boards, to an height equal to that of the virtual head or fall: and the effect of an overshot-wheel will be, at a medium, twice that of the undershot. Mills having a breast-wheel, or other kind of wheel on which the water acts, partly by its weight and partly its impulse, will produce more or less effect, accordingly as the circumstances approach more nearly to those of the over or undershot-wheels.

For the advantage of such as are least conversant in subjects of this nature, for whom chiefly the present memoir is intended, I shall illustrate the subject by an example.

Suppose a stream to pass through an estate without any evident fall, with a velocity of nine feet per second, and affording sufficient room to place an undershot-wheel with a pro-

* The proof of this is simple, but may also be seen in Desaguliers, ii. 520.—Those who use logarithms may with less trouble *subtract the constant log.* 1.8081312 *from twice the log. of the velocity, and the remainder will be the log. of the virtual height.*

† The proportion of work of the two actual mills, compared by Desaguliers, is as 3,25 to 1 in favour of the overshot.

per

per number of float-boards, each six feet long and two deep: It is required to determine, 1st, What quantity of water might be raised by that power, during the day of ten hours, to the height of thirty feet? or, 2dly, What number of bushels of wheat, or malt, might be ground in the same time? or, 3dly, What number of men or horses might this wheel supply the place of, in performing any other kind of work or manufacture?

In the first place, to find the virtual fall, multiply 9 feet by itself, which gives 81, and divide this by 64,2882: the quotient will be 1,26 feet.—Again, the surface of the float being 6 feet by 2 feet, is 12 square feet; which, multiplied by the velocity 9, gives 108 = the number of solid feet of water which has in effect fallen from the virtual head in a second. But this power will partly be consumed in producing eddies and lateral motions, and partly remain uncommunicated in the tail water as it flows off. For which reasons, as has already been remarked, the effect will only be one-third of the power. That is to say, one-third part of 108, or 36 cubical feet of water will be raised in a second to the height of 1,26 feet. The question however is directed to the height of thirty feet, and the quantities of water, or other weights, raised by equal powers, are inversely as their heights: that is, 30 feet : 1,26 feet :: 36 cubical feet : 1,512 cubical feet raised 30 feet in a second. But the hour consists of 3600 seconds, and the day of ten hours, = 36000, which multiplied by 1,512 produces 54432 cubic feet, or 6437 hogsheads, (reckoning the gallon at 231 solid inches) which are raised 30 feet in the day *.

With regard to the operation of grinding, it is allowed that about 3420 cubic feet of water with a fall of 10 feet will, by an overshot wheel, grind one bushel of wheat into flour, and the same force will cut five quarters of malt. But our undershot wheel will only do half the work. From the foregoing calculations our stream is equivalent to 13,6 solid feet per second with a fall of ten feet, or 816,5 solid feet per minute. Therefore as 3420 cubic feet : one bushel of wheat :: 816,5 cubic feet : 0,239 of a bushel which would be ground per minute by an overshot wheel; or 0,1195 by our undershot wheel. This last number multiplied by 60 produces 71,7 bushels per hour, or nearly 72 bushels, or 9 quarters of wheat per day of ten hours. And by the proportion of malt to wheat just mentioned, it will follow that 360 quarters of the former grain might be cut in the same time.

Animal strength is of so fluctuating a nature, that it is difficult to subject it to any estimate. Physical causes must affect both the quantity and duration of the efforts

* This is an outside estimate, and gives no allowance for frictions and errors in the construction of the hydraulic work. The reaction in Smeaton's Experiments was afforded merely by a weight and pulley, which is greatly in favour of the mill in deducing the effect. I find in the "Reports of the late Mr. John Smeaton, F. R. S." printed in quarto, London 1797, the power of the water called "Hubbert's mill stream" is deduced from the quantity and fall, (p. 245.) by a rule which he does not mention, but is evidently this, for overshot or close breast wheels: *Multiply the number of cubic feet of water discharged per minute by the fall in feet: this number may be called the power. Divide the power by two, and the quotient may be called the effect. Assume any height at pleasure in feet: divide the effect by the height, and the quotient will be the cubic feet of water, which a good hydraulic apparatus will raise to that height per minute.*

The undershot wheel will raise half that quantity: or in general less than one-sixth of the power.

I have retained the proportion of the Treatise on mills in the text, because the ratio of the effect to the power can be easily altered at pleasure in computing.

possible

possible to be made either by man or beast, and the former is more particularly influenced by his moral habits. From these last it is that the influence of reward, the expectation of favour or patronage, and various other similar motives, have operated in the temporary exhibition of hydraulic machines, to produce results contrary to every sound deduction from permanent work, and most perniciously delusive to the parties concerned in supporting or encouraging such engines. Desaguliers*, who has taken much pains to ascertain the maximum of power in this respect, has determined that a man can raise of water, or any other weight, about 550 lbs. or one hogshead ten feet high in a minute; and he states that a horse will raise five times that quantity; or, which is the same thing, that quantity through five times the height. His deduction respecting the man, though he says it will hold good for six hours, appears from his own facts to be too high, and certainly such as could not be maintained one day after another. Smeaton † considers this work as the effort of haste or distress. He reports ‡ that six good English labourers will be required to raise 21141 solid feet of sea water to the height of four feet in four hours. This quantity is of the same weight as 21669 cubic feet of fresh water, with which, and the above rate, it will be found by an easy calculation that the men will raise a very little more than six cubic feet each to the height of ten feet in a minute. But the hogshead containing $8\frac{1}{2}$ cubic feet, Smeaton's allowance of work proves less than that of Desaguliers in the proportion of 6 to $8\frac{1}{2}$. And as his good English labourers, who can work at this rate, are by him estimated to be equal to a double set of common men picked up at random; it seems very proper to state that, with the probabilities of voluntary interruption, and other incidents, a man's work for many days together ought not to be estimated at more than half a hogshead raised ten feet high in a minute. In the same report last quoted, Smeaton states, that two ordinary horses will do the work in three hours and twenty minutes, which amounts to a little more than two hogsheads and a half § raised ten feet high in a minute. One horse will therefore do the work of five men.

To apply these deductions to our example, it must be recollected that the quantity raised in ten hours to the height of thirty feet was inferred to be 6437 hogsheads, which are equivalent to 32 hogsheads raised ten feet high per minute. Consequently at the rate of one man for each half hogshead, the stream would perform the work of 64 men, or nearly thirteen horses.

With regard to streams which fall by a considerable declivity, the water may be conveyed by the well known means of a dam or trough to the buckets of an overshot wheel, placed in that part of the stream which is found the most convenient in point of expence and local situation. Suppose, for example, the fall amounted upon the whole to fifteen feet, upon a length of 200 yards, it might be more convenient to lead the whole stream in a wooden trough supported upon posts, with a slight declivity, to the wheel near the lower end of the current; or in other circumstances, according to the face of the land, the work might prove cheaper or more durable if the wheel were placed near the upper end of the stream, and the channel sunk so as to convey away the tail water with no more fall than should be necessary for that purpose. In this case it is obvious that the fall must be ascer-

* Course of Lectures, ii. 498, 505, 536. † Reports, i. 216. ‡ Ibid. i. 323.

§ He elsewhere (ibid. p. 229.) rates an horse at 250 hogsheads, ten feet high, in an hour; but I prefer the deduction in the text.

tained by the operation of levelling, and not by computation as in our former supposition. This operation is easy, and may be performed upon such short distances with sufficient accuracy, with a carpenter's square and a staff.

For this purpose it will be necessary to drive a pin into the handle, or thickest part of the square, near the corner, upon the flat side. A line must be drawn from the pin, parallel to the edge of this handle, and a looped thread supporting a small weight must be hung upon the pin. In this situation the instrument is ready for taking levels. For if it be held in such a position that the plumb line may cover the line drawn from the pin, the blade will lie horizontal, and by looking along its upper edge the eye will be directed to some object on the same level with itself. Let the observer provide himself with a staff five or six feet long; which, if divided into feet and inches, will be the more useful. This is to be pitched at or near the lower end of the stream, and against it the observer is to press the handle of his square, which may be conveniently done by grasping both it and the staff together in the same hand. Then, holding the apparatus so that the plumb line may lie fair, he is to direct the blade of the square to some stone or other remarkable object higher up the stream; for which purpose it will be necessary to slide the handle of the square up or down the staff till the positions are accurately obtained; that is to say, till the blade points fairly to the object at the same time that the plumb line denotes that the blade is level. When this is done, a memorandum must be taken of the feet and inches from the bottom of the staff to the upper edge of the blade, and the observer must proceed to the stone or object to which his sight was directed, and, planting his staff there, repeat the same operation with regard to some other object still higher up the stream, and again note down the feet and inches observed upon the staff. This process continued will, by a few reiterations, bring him to the upper extreme of the water which we suppose to be within his grounds or power. The sum of all the measures taken upon the staff is the difference of level or whole fall of the water. The quantity of water in cubic feet may be ascertained by either of the means before mentioned; or, if the stream be very small, it may actually be dammed, and measured off with two tubs or measures, of as large a size as can conveniently be managed.

I should be inclined to apologize to such of my readers as are intimately acquainted with these subjects, for the minuteness of detail into which I have entered, if I were not assured that they will be the first to perceive, that many individuals who possess beneficial streams are totally uninformed of their value, or of the means of determining the same.

(To be continued.)

IX.

Enquiries concerning the Invention and Practice of the Art of Hat making.

To Mr. NICHOLSON.

SIR, Newcastle, 15th Dec. 1798.

YOUR Publication being open to enquiries relative to the advancement of manufactures, as well as the sciences, will you favour me with a place for the following queries, on on a bufinefs but little known? And probably, amongft your fubfcribers, they may be anfwered through the fame medium.—How, and at what time, was hat-making invented, and by whom?—Were there ever any engines made ufe of in that bufinefs?—Are there any at prefent? And if fo, in what part of the country are they made ufe of? And how far do they go in the procefs?—Are there any machines for the cutting of hare, rabbit, or beaver fkins? And if any, where are they worked?—Your inferting the above in your next number will oblige, Sir, yours,

N. L.

There is no mention made of the above, either in the Tranfactions of the Royal Society of London, or in Beckman's Hiftory of Inventions.

THE above did not come to hand early enough for me to make any enquiries refpecting the invention, hiftory, and practice of the art of hat-making. Refearches of the kind pointed out by this correfpondent are peculiarly adapted to the views and intention of a Journal of the Arts; and I hope he will not be difappointed in his expectations from others of my readers, whofe purfuits may have enabled them to throw light upon the fubject. At all events, I fhall certainly have fome information to communicate in the next number, which, for want of time, I cannot at prefent arrange and digeft.

W. N.

X.

New Obfervations on the Method of producing very loud Fulminations with various Bodies, by Means of Phofphorus. By Cit.* BRUGNATELLI.

I WAS aware that the oxygenated muriate of potafh produces effects fuperior to nitre, when mixed with charcoal and fulphur and converted into gunpowder; that it detonates by percuffion or trituration with a great number of combuftible fubftances; but I did not expect to produce effects much more confiderable by ufing the fimple nitrates, and even the metallic oxides mixed with phofphorus and ftruck with a hammer.

Experiment 1. I took a gros of the cryftallized nitrate of filver, which I placed on an heavy anvil, and laid a very thin flice of phofphorus in the midft of the cryftals. The temperature of the atmofphere during thefe experiments was not higher than 6° above the zero of Reaumur's thermometer. The materials being thus difpofed, I ftruck the mixture rather fmartly with a hammer. The confequence was one of the moft terrible detonations I ever witneffed, which fhook the anvil and its wooden fupport. Streaks of filver were obferved upon the anvil five or fix lines in length. One of the edges of the hammer was bended and turned upwards. I was perfectly ftunned, and my clothes were torn in various places.

* Tranflated by Van Mons from the Italian manufcript of the author into French, and inferted in the Annales de Chimie, xxvii. 72.; from which work the prefent tranflation is made.

I have

I have frequently repeated the fame experiment, even in my lectures; and though I operated with no more than a few grains of the falt, the noife of the explofion was never lefs than that of a mufquet.

The lapis infernalis has nearly the fame effect as the nitrate of filver.

Experiment 2. I placed on an anvil a pinch of the oxygenated muriate of potafh with a fmall portion of phofphorus, and ftruck the mixture with a hammer. The detonation was exceffively ftrong.

Experiment 3. The dry nitrate of bifmuth detonated very ftrongly. The fame effect was produced with all the metallic nitrates which were to be found in the laboratory, and particularly with the nitrate of tin.

Experiment 4. I repeated the fame experiment with the fufed nitrate of mercury, of which I put fix grains with a fmall quantity of phofphorus on an anvil, and ftruck them with a hammer. The phofphorus fimply took fire, without producing any noife: but when the hammer was flightly heated, the fame mixture detonated with a fhocking noife. After the fulmination the mercury was found reduced, having, as it were, filvered the anvil in very brilliant radiations.

Experiment 5. I afterwards tried the alkaline nitrates, particularly that of potafh. When a fmall flice of phofphorus was laid on a pinch of nitre and ftruck with a cold hammer, no fulmination was produced even by repeated blows; but having flightly heated the hammer, in order that the affinities might act more decifively, the very firft blow produced a very loud fulmination, incomparably ftronger than that of gunpowder.

Experiment 6. I obtained no fulmination or detonation with the fulphates of alumine, of copper, or of iron ftruck with phofphorus in the before-mentioned manner, though I heated the hammer more than ufual.

Experiment 7. Neither did I obtain any fulmination from the fimple muriate of filver, commonly called luna cornea, by the fame treatment with phofphorus.

Experiment 8. I likewife tried the oxygenated muriate of filver, obtained by decompofing the nitrate of filver by the oxygenated muriatic acid. A pinch of this falt (with phofphorus), ftruck with a warmed hammer, produced a much weaker fulmination than was obtained with the cryftallized or fufed nitrate of filver, or the other falts before mentioned. The oxygenated muriate of mercury, treated in the fame manner, afforded a very weak detonation.

Experiments 9 *and* 10. I endeavoured to produce fulminations with the metallic oxydes treated with phofphorus. Thofe of manganefe, of zinc, of copper, of iron, of antimony, and of lead, produced no effect, even when ftruck with the hammer confiderably heated; but I obtained fulminations with the yellow oxyde of mercury (turbith mineral) and the grey oxyde of the fame metal.

Turbith mineral does not fulminate in contact with phofphorus when it is pulverifed. It muft be in a lump to produce this effect. The fame thing happens with regard to the grey oxyde of mercury by the nitric acid, except that the fulmination is ftronger.

Experiments 11 *and* 12. I was curious to afcertain whether I fhould obtain fimilar fulminations with the falts before mentioned, by fubftituting another acidifiable combuftible fubftance inftead of phofphorus; as, for example, fulphur and charcoal.

I accordingly took nine grains of lapis infernalis and three grains of pulverifed fulphur, which I ſtruck with the cold hammer. The ſulphur took fire without affording any noiſe; but, when the hammer was uſed hot, a detonation was heard, and rays of ſilver appeared on the anvil.

Having repeated the ſame experiment with lapis infernalis and charcoal, I could produce no more than a very dull detonation, though the hammer was well heated.

Experiment 13. I took ſeveral of the ſalts which had fulminated by the ſtroke, and threw them, together with the phoſphorus, into the liquid oxygenated muriatic acid; but no detonation was produced. In an atmoſphere of the gas of the ſame acid, the fulminating mixtures burned with a ſlight crepitation, ſimilar to that which is excited by phoſphorus alone.

Experiment 14. I wrapped moſt of theſe detonated mixtures in ſmall pieces of paper, and threw them one after the other into a red-hot crucible. They burned with a very lively flame, but did not fulminate nor detonate.

Conclusion.

1ſt. THE nitrate of ſilver, whether cryſtallized or fuſed, fulminates when ſtruck with a hammer, together with phoſphorus, even at a low temperature. *Exp.* 1.—All the ſalts of ſilver do not fulminate equally, nor in the ſame manner. *Exp.* 7.

2d. Moſt of the metallic nitrates fulminate with phoſphorus. *Exp.* 3 and 4.

3d. Common nitre, of which the fulminating property was before known, in its mixture with various combuſtibles, elevated to a certain temperature, or put into contact with an inflamed body, as in gunpowder or fulminating powder, is now found to detonate with phoſphorus alone. *Exp.* 5.

4th. Thoſe ſalts into which the nitric acid has not entered as a component part do not fulminate.

5th. The oxygenated muriates of potaſh, of ſilver, and of mercury, fulminate with phoſphorus, but the latter with much leſs effect than many other ſalts. *Exp.* 8 and 2.

6th. The ſalts are not the only bodies which fulminate with phoſphorus: ſeveral metallic oxydes have the ſame property. *Exp.* 9 and 10.

7th. Phoſphorus likewiſe is not the only acidifiable and ſolid combuſtible matter capable of producing fulminations. Charcoal produces the ſame effect at a more elevated temperature. *Exp.* 11 and 12.

8th. Thoſe ſubſtances which fulminate with phoſphorus produce no effect when thrown into the liquid oxygenated muriatic acid. *Exp.* 13.—Neither do they fulminate when expoſed together with phoſphorus to an elevated temperature. *Exp.* 14.—The blow of the hammer is neceſſary to throw the component parts of theſe bodies into a ſtate of oſcillation, in order to determine the affinities with effect.

ADDITION BY VAN MONS.

THE facts obſerved by the learned editor of the Italian Annals of Chemiſtry appeared too curious and important for me to loſe any time in repeating his experiments. My ſucceſs was as follows:

The

The cryſtalliſed nitrate of ſilver detonated very ſtrongly, emitting only a weak flame. The ſilver was reduced to the ſtate of a blackiſh oxyde.

Lapis infernalis alſo detonated, but much more weakly. The metal was completely reduced.

The nitrate of tin fulminated with conſiderable ſtrength.

The fuſed nitrate of mercury produced a ſtrong effect.

The different-oxydes tried by Brugnatelli, that of manganeſe excepted, produced no noiſe by friction; and the detonations under the ſtroke were at leaſt doubtful. The oxydes were all in part reduced, and the mixtures burned in a lively manner, throwing out ignited ſparks to a diſtance; but the phoſphorus alone, after three or four ſtrokes with the hammer, exhibited the ſame appearance.

The oxygenated muriate of mercury afforded perceptible marks of detonation.

I produced theſe ſeveral fulminations without heating the hammer for any one of them. Friction, or a few ſlight previous blows, heat the materials ſufficiently to cauſe the effect to take place with the laſt ſmart blow.

I tried a great number of times to make the experiment with ſulphur and lapis infernalis. In ſome inſtances I rubbed the two ſubſtances together with all my force in iron or marble mortars, and in others I ſtruck them with a heated hammer; but I did not obtain the moſt feeble detonation. The matter itſelf did not even take fire, excepting when I ſtruck it ſmartly in an iron mortar ſtrongly heated with a peſtle equally hot. I was not more fortunate with charcoal. Similar experiments, which I have ſince tried with various other ſubſtances, afforded me the following reſults:

The nitrate of gold by evaporation produced a louder detonation than is afforded by the oxygenated muriates of alkali. The marble mortar in which I made the experiment was covered with ſmall particles of exceſſively thin plates of reduced gold. The metal ſeemed to have undergone a very liquid fuſion.

The muriate of mercury detonated with a force at leaſt equal to that of the nitrate of gold. The metal was almoſt entirely diſſipated.

The nitrate of lead produced a weaker detonation than the foregoing. The metal was found reduced to the ſtate of black oxyde.

The oxygenated muriates of antimony, zinc, and tin, produced a much weaker effect. I ſucceeded, however, once in producing a very ſtrong and inſtantaneous detonation with the firſt of theſe ſalts.

The oxydes of gold, ſilver, and mercury, by fire, were found to occupy the firſt rank among the fulminating ſubſtances. The oxygenated muriate of potaſh does not afford effects equally conſtant with thoſe of the oxydes.

The oxyde of lead, in its different degrees of oxydation, afforded in no inſtance any appearance but that of inflammation.

I formerly exhibited in my public lectures the detonation which may be produced by percuſſion in a mixture of the nitrate of potaſh and phoſphorus. I had even produced exploſions ſufficiently violent to induce the audience to ſuppoſe that I operated with the oxygenated muriate. I ſubmitted the nitrate of ammoniac to the ſame treatment, and produced a detonation ſo terrible that it ſhook the doors of my laboratory.

 The

The nitrates of barytes, of ftrontian, and of magnefia, by deficcation, detonated with nearly the fame force as the nitrate of potafh.

I had ftill remaining five grains of the oxygenated muriate of ammoniac, of which I took the half, with about four grains of phofphorus. I flightly crufhed thefe two fubftances, with the intention of mixing them together, when a detonation fo terrible was heard that the whole houfe was alarmed. The phofphorus was for the moft part thrown upon my hat, which it burned for a long time before I perceived it. The violence of the blow caufed the hammer to fly out of my hands. I very diftinctly felt it raifed by the expanfion of the gafes. The other half of the oxygenated muriate of ammoniac was placed alone upon the bottom of an inverted iron mortar, and ftruck rather brifkly with the cold hammer. The firft blow caufed it to detonate, but no light was difengaged. I regretted that no more of this falt remained to make the experiment with fulphur and charcoal.

I alfo ftruck the nitrate of ammoniac alone. The fourth blow caufed it to fulminate. I fay it fulminated, becaufe the detonation was accompanied with a difengagement of light.

I obtained an effect equally confiderable from the white nitrate of mercury and ammoniac. I think that all the nitro-ammoniacal triple falts will detonate more or lefs ftrongly with phofphorus.

I was defirous of trying the folid oxygenated muriatic acid; for which purpofe I caufed this acid to cryftallize by artificial cold. A few cryftals of this acid were put together, with a fmall piece of phofphorus, upon an iron mortar placed in a cooling mixture. When thefe were ftruck with a hammer, a dull detonation was heard at the fixth or feventh blow. The fufed acid was projected to a diftance.

I repeated a great number of thefe experiments with pyrophori and the phofphori called artificial carefully prepared; and in almoft every cafe I obtained a detonation. I fhall defcribe my experiments on thefe fingular inflammable preparations in a feparate article.

Whenever I ftruck the before-mentioned mixtures with a heated hammer, I always obtained a weaker detonation, at the fame time that the inflammation was ftronger; and when the hammer was too hot, no detonation or noife was produced. This obfervation I had already made with the oxygenated muriate of potafh; and it appears to me to explain the phenomena of detonation. The blow with the cold apparatus, by ftrongly compreffing the matter, and perhaps exciting fome heat, produces a partial combuftion of the phofphorus, and confequently engages only part of the oxygen of which the other portion affumes the elaftic ftate, and produces the noife of the detonation. At a very elevated temperature the effects are not altogether the fame. All the oxygen is at once employed to burn the combuftible, whence the inflammation is the ftrongeft, and no detonation takes place. In fact it is obfervable, that the more fudden and fonorous the noife, the lefs is the developement of fire; and the contrary. To this principle alfo is referable the obfervation made by Brugnatelli, which I have found to be true, that the oxygenating body muft in fome inftances poffefs the form of a lump to obtain the effect; becaufe it applies too extenfively to the combuftible matter when powdered.

Thefe experiments fucceed much better by friction than by a blow, and the effects take place more readily upon a rough than a fmooth body. I ufually place the two fubftances on the bottom of an inverted marble mortar, which is left rough from the faw.

Upon

Upon these I press the face of an iron hammer, holding the handle with the left hand, and suddenly slide the hammer forward so as to produce a kind of shock. In this manner the detonation or noise is likewise more distinct, and its force more easily determined. Simple compression produces the detonation in many cases.

I think it proper to warn those who are not familiar with experiments of this kind, that when the operation is performed with more than a grain and a half, or at most two grains of phosphorus, there will be danger of burning their hands or clothes by the excess of this combustible, which flies off in a burning state.

I cannot finish this article without restoring to its true proprietor the portion of honour which is due to the discoverer of the detonating property of the oxygenated muriate by percussion or strong friction, which Citizen Fourcroy has attributed to me in the Memoir * he has communicated to the National Institute, no doubt in consequence of my having mentioned this phenomenon in my edition of the Philosophy of Chemistry, under the article of *Oxygenated Muriates*. Professor Wurzer, of Bonn, was the first who observed this fact, by triturating with some force, in a mortar, a mixture, scarcely weighing a grain and a half, of three parts of the oxygenated muriate of soda and one part of sulphur. He obtained a detonation which rendered him deaf for several days. See Crell's Chemical Annals for the year 1792, vol. ii. page 402.

TUNNEL BENEATH THE THAMES.

Reports, with Plans, Sections, &c. of the proposed Dry Tunnel or Passage from Gravesend in Kent to Tilbury in Essex, demonstrating its Practicability, and great Importance to the two Counties and to the Nation at large; also, on a Canal from near Gravesend to Stroud, with some Miscellaneous and Practical Observations. By R. Dodd, Engineer. Quarto. 28 pages, with 3 plates: viz. 1. Plan and Sections of the proposed Tunnel. 2. A View of Gravesend and Tilbury, with the Section of the River, shewing the Strata and Depth of Water: and, 3. A Map of the Country within twenty Miles of Gravesend. London, printed for J. Taylor, 1798.

SOME account of this undertaking was given in this Journal a few months ago. (II. 239.) The present Reports will afford satisfactory information of its detail and progress, and the public will hear with pleasure that it is likely to be carried into effect.

In the month of May 1798, Mr. Dodd circulated an Introductory Report or Address to the Nobility, Gentry, &c. of Essex and Kent, in which he states that the extended scale of commerce on the river Thames forbids the construction of a bridge at Gravesend; but that the practice of making passages, tunnels, or drifts under rivers has been adopted to a great extent in various parts of the kingdom; namely, at the coal works under the rivers Tyne and Wear, and at Whitehaven under the very ocean; that a tunnel is actually intended to be made under the mouth of the river Tyne, to answer the purposes of a bridge for carriages and passengers;—that the measure now proposed will save a circuit of near

* Philosophical Journal, I. 168.

fifty miles to carriages passing from one county to the other; and that the situation at or near Gravesend is peculiarly eligible from its natural facilities, as the greatest part of the ground to be passed through is chalk. These observations are followed by the proposal and estimate which have already been given at the page of our Journal last quoted. Mr. Dodd observes that 12 l. per running yard must be considered as a handsome allowance, since it is a fact, that some of the most considerable tunnels in this kingdom on a scale of nineteen feet by seventeen, which is larger than the tunnel at present recommended, have been executed at that price.

On the 18th of July 1798, a meeting was held in the Town Hall of Gravesend, the Earl of Darnley in the chair; when it was resolved, That there being no reason to doubt the practicability of so desirable a measure, the co-operation of government, the assent of the neighbouring land owners, or the willingness of the public to subscribe to its accomplishment, the opinion of the meeting was, that transferable shares of 100 l. each would afford the best means of effecting it; and that a subscription should be opened at the office of Messrs. Evans and Son, at Gravesend, for its support and encouragement.

In the report and estimate which the engineer presented to this meeting, he states, that from the actual survey he finds no reason to add to the former estimate, but rather to abate somewhat more than 3000 l. on account of the excellent chalk which he finds will be afforded at the most eligible place for the excavation. The deviation from an horizontal line of the tunnel, which passes at about thirty feet below the bed of the river, will be four inches in the yard. Several military and commercial advantages of the undertaking are likewise clearly stated, and an estimate given of the cost of a twelve feet tunnel, admitting carriages to pass only one way at a time, in case the sixteen feet tunnel should be thought too expensive. In the subsequent observations the author enlarges still more upon the same interesting topics, and makes a comparison between the communication by tunnels and the great modern improvement of iron bridges, which last erection he shews to be much less durable, and more liable to speedy destruction by an enemy, than the tunnel. He remarks, that the destructive elastic fluids, which are known to be extricated in mines, cannot affect the present work when once finished, because of the clear communication with the external air at both ends; and he gives some practical remarks on the discovery of inflammable air or fire-damp, which, on account of the interesting nature of the subject, I shall here transcribe.

" On entering any excavation under ground, where this fire-damp is suspected to lie, the candle ought to be held in the left hand (the smaller the candle the more to be depended upon), and the flame thereof to be shaded by the right-hand fingers being placed horizontal, and on a level with the top of the flame of the candle. If the air be good, and perfectly free from any inflammable air or hydrogen, a small brown top or pinnacle is seen on the top of the flame, the same as is seen in the chamber of a house; but on approaching the real fire-damp another top or spire is seen of a blue colour, above the brown just described as arising therefrom; and on advancing a step or two forward the damp is commonly more dense, and consequently the appearances over the candle assume a more serious aspect by the brown spire or small top beginning to disappear; another step perhaps reduces it altogether, on which the danger commences; and the spire or top sits down altogether blue on the flame. At this last stage it is not safe to proceed further without

great

great circumſpection joined with ſteadineſs: a little further the blue top is ſeen to be more perfect and denſe, which if general through the place, all lights ſhould be extinguiſhed, and the workmen be made to quit the place. Perhaps on advancing a very little further a light blue bead appears to circumſcribe the other dark blue, as if approaching to flame, and having a more than ordinary pointed ſpire terminating like a white thread; and the danger becomes very great if to remain any time in this poſition. The next ſtage ſhews the ſprents to have taken place, which ſprents are ſimilar to thoſe produced by ſqueezing the oil from the rind of a lemon into the flame of a candle: at this period ſometimes has been obſerved a ſmall buſhy dark-coloured cloud hanging over the top of the flame, ſtill more and more attracting the fire; when in an inſtant perhaps after this laſt obſervation the whole appearance expands into flames, and one general conflagration takes place. The miſchief which enſues is in proportion to the accumulated quantity of this pernicious vapour; which if great flies to the neareſt aperture leading to the atmoſphere, being about eleven times lighter than common air, with a dreadful exploſion and extraordinary eruption. But in caſe the ſaid fire-damp or inflammable air be mixed with ſtyth, or black damp, theſe appearances are ſomewhat varied; and by being mixed with a very dark brown is in general more ſafe.

"Black damp or ſtyth" (doubtleſs carbonic acid, or hydro-carbonate) "which ariſes in mines is directly the reverſe of inflammable air or fire-damp; the former extinguiſhes the flame of a candle as quickly as if put into water or any other fluid; the other cauſes the candle to burn too faſt. This ſtyth or black damp prevails moſt where there is little or no inflammable air, and when the due circulation of atmoſpheric air is neglected."

It was of eſſential conſequence, by way of ſhewing the practicability of the preſent undertaking, that Mr. Dodd ſhould impreſs the minds of his employers with the fact that ſuch tunnels had been made with ſucceſs on a much larger ſcale. For this purpoſe he relates various hiſtorical matters, beſides thoſe already mentioned, from which we may notice, that the earlieſt tunnel for inland navigation was executed by M. Riquet, to convey the canal of Languedoc through a mountain near Bezieres. The firſt executed in this country was by the celebrated Brindley on the duke of Bridgewater's canal near Mancheſter. The next is the famous tunnel of Hare Caſtle Hill, in Staffordſhire, by the ſame engineer, which is 2880 yards long, and paſſes through a variety of ſtrata, quickſands, &c. more than ſeventy yards below the ſurface of the earth, and ſerves as the receptacle for part of the Grand Trunk Canal. The tunnel of Saperton is two miles and three quarters long, and was carried through two miles of ſolid rock. Many other drifts or tunnels have been made in this kingdom, through rocks and obſtacles of various kinds, in a courſe of time and at expence much leſs than would be apprehended by perſons unacquainted with works of this nature.

Mr. Dodd's report and eſtimate on the projected dry tunnel under the river Tyne contains illuſtrations and argument of nearly the ſame import. His pamphlet concludes with a propoſal for an inland canal from the Thames near Graveſend, to the Medway near Stroud; which, by a ſhort courſe of about ſix miles, would ſave the circuitous and leſs certain paſſage of forty-ſeven miles by the way of the Nore. The peculiar advantages of this cut to the country at large, and to the government eſtabliſhments on theſe rivers, as well as the nature of the ground itſelf, are ſtrongly and perſpicuouſly ſtated.

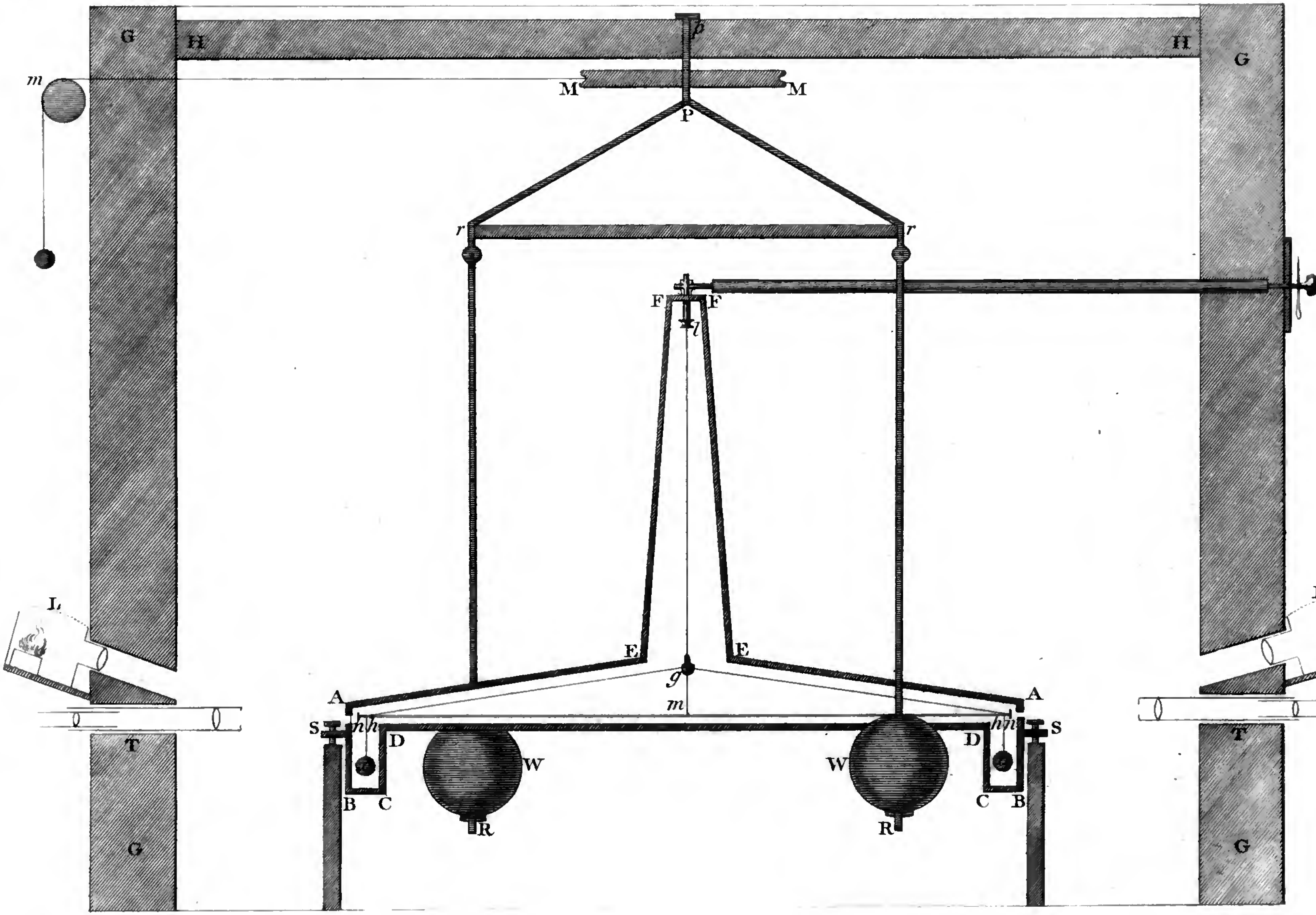
The

The last public meeting on the former business was held at the London Tavern on the 14th of last month (December); when it appeared by the Report that the subscriptions amounted to upwards of ten thousand pounds. The managing committee of subscribers consists of the Earl of Darnley, Lord Petre, the Hon. J. T. Townshend, M. P. the Hon. Robert Petre, Sir William Geary, Bart. M. P., Mr. Ald. Lushington, M. P., John J. Angerstein, and Claude Scott, Esqrs. Lieut. Col. Twiss, Royal Engineers, Capt. Schank, R. N. T. Woodruffe Smith, Esq, John Mavor, Esq, Geo. Hawks, Esq, and Benjamin Harrison, Esq.—Claude Scott, Esq. is Treasurer.

The undertaking will be begun and carried on by those who shall first subscribe to the proposed shares, which are three hundred at a hundred pounds each; but the actual work will not be commenced till the whole sum of thirty thousand pounds shall have been subscribed.

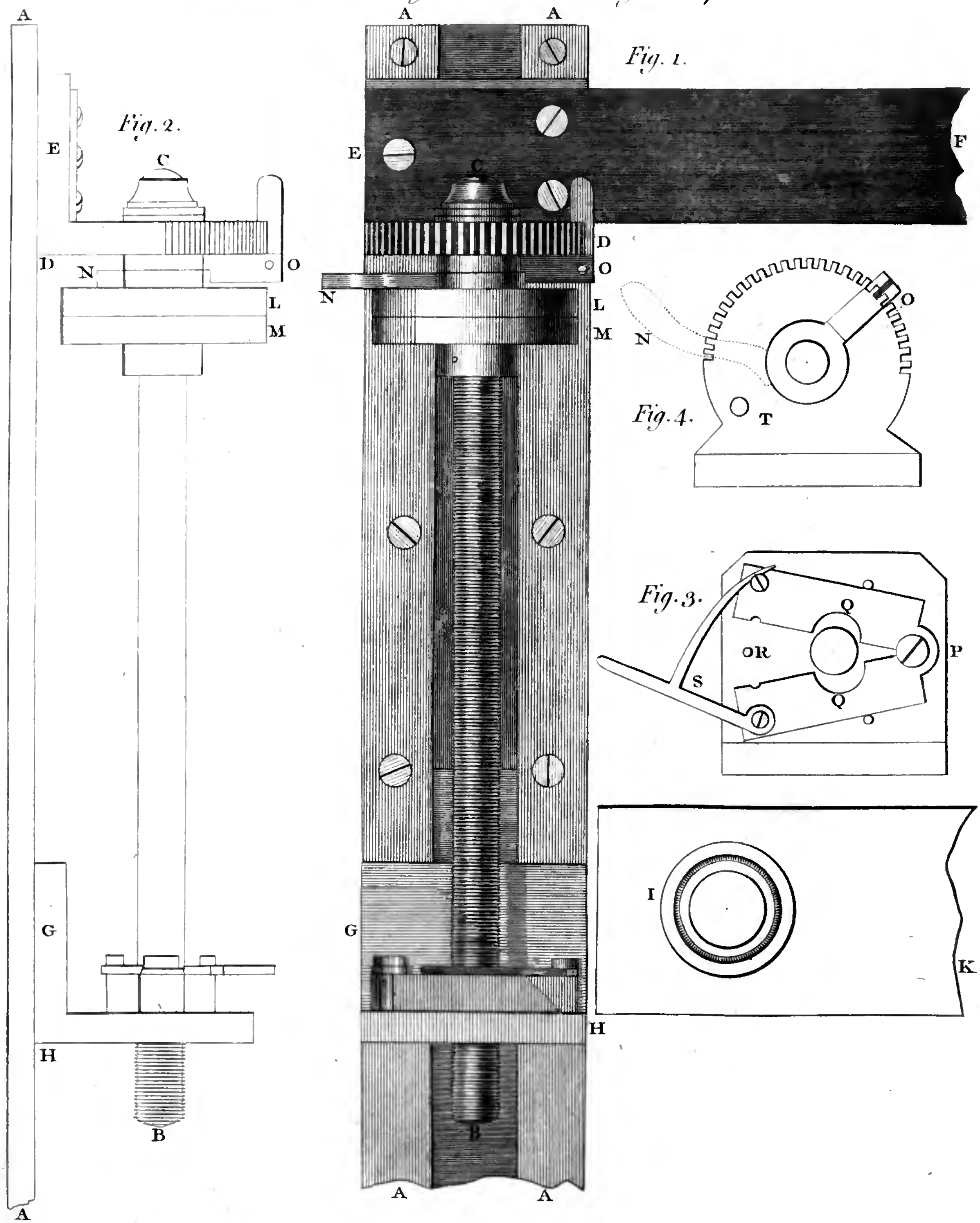
A fellow-labourer in the cause of science, Mr. A. Tilloch, objects totally to the account I gave of Mr. Cartwright's apparatus for rendering the pistons of steam-engines tight by metallic fittings, at page 365 of the present volume. As the whole of his remarks appear to me to be obviously erroneous, and the matter is before the public, I must be excused from entering into controversy. He will himself perhaps, on second thoughts, recollect that a circle, even though in diameter equal to that of the earth, will not be converted into a triangle by cutting it in three pieces; and that it is absurd to suppose the third law of nature, that *action and re-action are equal and contrary*, can either be dispensed with or explained away. He will then probably look (not to the vacuum, but) to the lower plate of Mr. Cartwright's piston for the re-action; which, by means of the piston rod and the work required to be done, is made to act beneath the moveable pieces of the apparatus, while the steam presses their upper surface with no inconsiderable force; admitting its elasticity to be equivalent to a single atmosphere only: a force sufficient, in my apprehension, to prevent ground surfaces from sliding freely, if at all, upon each other. But, as we are all liable to mistakes in new practical matters, I could have wished that Mr. Tilloch had reasoned less diffusely, and referred at once to the facts. If it be true that there was a steam-engine at work six months ago, when Mr. T. invited the public to inspect it, at Mr. Rowley's, in Cleveland-street, Mary-lebone*; or if he can bring evidence that such an engine has been at work for any considerable part of the time since he published his description, and that the facility of operation, the power, and the durability of the apparatus are such as he conceives them to be;—I cannot but think that he has, unfortunately, overlooked his best argument. To this argument, if offered, I must grant my assent; and I beg leave to assure him that I shall most readily attend, inspect, and report concerning this engine, if he or the inventor will give me the opportunity, without in the least regarding whether this part of my duty to the public shall confirm or overthrow those opinions which the present state of the facts has compelled me to give.

* I went there at the time, and saw the parts of a small model or engine, which was not at work; and, upon late enquiry, I understand there is none there at present. I could gain no information whether there was any engine of this construction at work, or in progress, at any other place.



Barlow sculp.

Instrument for ruling the Shades of engraved plates.



Barlow sculp.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

FEBRUARY 1799.

ARTICLE I.

On the Corundum Stone from Asia. By the Right Hon. CHARLES GREVILLE, *F. R. S* *.

Analyfi cryftallorum, tam ejufdem quam diverfæ figuræ, multum lucis fcientia expectat.
BERGMAN, Opufc. de Terra Gemmarum.

HAVING contributed to bring into notice the mineral fubftance from the Eaft-Indies, which is generally called adamantine fpar, I beg leave to lay before the Royal Society the following account of its hiftory and introduction.

About the year 1767, or 1768, Mr. William Berry, a very refpectable man, and an eminent engraver of ftone, at Edinburgh, received from Dr. Anderfon, of Madras, a box of cryftals, with information of their being the material ufed by the natives of India to polifh cryftal, and all gems but diamonds. Mr. Berry found that they cut agate, cornelian, &c. but in his minute engraving of figures upon feals, &c. the fuperior hardnefs of the diamond appeared preferable; and its difpatch compenfated for the price: the cryftals were therefore laid afide as curiofities. Dr. Black afcertained their being different from other ftones obferved in Europe; and their hardnefs attached to them the name of adamantine fpar. My friend, Colonel Cathcart, fent me its native name, *Corundum*, from India, with fome fpecimens given to him by Dr. Anderfon in 1784, which I diftributed for analyfis.

When the native name was obtained, it appeared from Dr. Woodward's catalogue of foreign foffils, publifhed about the year 1719, that the fame fubftance had been fent to him by his correfpondent Mr. Bulkley.

In his firft catalogue of foreign foffils, p. 6. ζ. 17. " *Nella corivindum* is found in fields where the rice grows: it is commonly thrown up by field rats, and ufed, as we do emery, to polifh iron."

* Philofophical Tranfactions, 1798, p. 403.

Page 11. λ. 13. "*Tella convindum*, Fort St. George, Mr. Bulkley. 'Tis a talky ſpar, grey, with a caſt of green: it is uſed to poliſh rubies and diamonds."

In Dr. Woodward's Additional Catalogue of Foreign Foſſils, publiſhed in 1725, p. 6. ζ. 10. "*Nella corivendum* is found by digging at the foot, or bottom, of hills, about five hundred miles to the ſouthward of this place. They uſe it as emery, to clean arms, &c. it ſerves alſo to grind rubies, by making it like hard cement, by the help of ſtick lac mixt with it. Eaſt-India. Mr. Bulkley."—Theſe, with a few others in Woodward's Catalogues, are the only inſtances by which any author, prior to 1768, appears to have noticed this ſubſtance.

This information being unſatisfactory, and every appearance of the ſtone indicating it to be part of a ſtratum, I wrote repeatedly to friends in India, to aſcertain, if poſſible, the ſituation of the rock, and if near the ſea, to ſend a conſiderable quantity as ballaſt, with a view of applying it to cut and poliſh granites, porphyry, and other ſtones, which the high price of cutting and poliſhing excluded from uſeful or ornamental work. But my inquiries at Madras were fruitleſs: by ſome I was aſſured it came from Guzarat. From Bombay I obtained no ſatisfactory information. At laſt, in the year 1793, I obtained a ſatisfactory account. Sir Charles Oakley was diſpoſed to oblige me: he was then Governor of Madras; and his ſucceſs is due to the activity and judgment of Mr. Garrow.

Mr. Garrow knew how difficult it was to avoid the cauſes of my failure, from every Hindoo being occupied by the duties of his caſt; ſcarcely thinking on any thing elſe, and wherever his intereſt is concerned, being ſuſpicious and reſerved. Mr. Garrow, in the firſt place, aſcertained the caſt connected with corundum, to be the venders of glaſs *bangles*; that they uſed it in their buſineſs, and ſold it to all other caſts. This caſt of natives at all times had free acceſs to every part of Tippoo's country; nor until the diſtricts about Permetty were ceded to the Engliſh, could it be procured in any other way. Mr. Garrow depended on his perſonal inſpection; the particulars are contained in the following letter communicated to me by Sir Charles Oakley.

SIR CHARLES OAKLEY, BART.

"Sir, Tritchinopoly, 10th Nov. 1792.

"I derived ſo little ſatisfaction from the various accounts given me of the corundum, from the indifference of the natives to every ſubject in which they are not immediately intereſted, that I reſolved to aſcertain the particulars I wiſhed to know, on the ſpot where the ſtone is found. The glaſs-men agreed in one material circumſtance, that the place was not far from Permetty: in other particulars they diſagreed, apparently with intention to miſlead.

"It is near a fortnight ſince I diſpatched a ſervant I could depend on to Permetty, with one of theſe people, who on his arrival there, probably through fear of his caſt, ſaid he knew no farther. My ſervant perſevered, and informed me he had found the place I wiſhed to ſee.

"I arrived at Permetty, by the route of Namcul, the 6th; and learning that the diſtance to the ſpot was about 3½ hours, or 14 miles, I left Permetty in time to arrive there about ſunriſe the next morning. At this time no perſon but my ſervant was preſent, and from a continued excavation at different depths, from 6 to 16 feet, in appearance like a water-courſe,

running

running in length about a mile and a half east and west over the brow of a very rising ground, I saw at once the place from which the stone was procured. The prodigious extent that at different times appears to have been dug up, with the few people employed, shews that it has been a business of ages.

"The ground on which the vein of excavation runs, and of course the mineral, commands one of the finest and most extensive prospects it is possible to conceive. The surface of the ground is covered with innumerable fine alabaster stones, and a variety of small shrubs, but not a tree sufficient to shelter my palanquin.

"There is not the appearance of an habitation within three quarters of a mile. The nearest village is called Condrastra Pollam. In this village are about 30 small thatched houses: among these are 5 families, who, in descent by prescriptive right, are the miners, and dig in the pits. The nearest place of any consequence in Rennel's Map, is Caranel, on the south side the Cavery. The distance of the pits from the river is above four miles; but the ground between prevents its being seen in a direct line. A fine view of the river is seen near Erode; which fort, as well as Sankerdroog, are plainly visible with the naked eye, as is also the Coimbitoor country, south and west of the river, to an immense extent.

"I procured at Permetty a cadjan, from the Bramin manager to the head man of the Pollam; which on my arrival at the pits I sent to him; and soon after three of the miners came from the Pollam, with their implements, and families following with provisions. As they came up they inquired of my servant how they were to address me, having never seen an European before.

"I followed them into a pit in the line of the excavation, above 14 feet from the ground level. The instrument they used is a very heavy iron crow, ending in a broad point, with a straight wooden handle clampt with iron. The soil they cut through is of different colours, but composed chiefly of a gritty granite; and at the depth of seven feet are layers of a substance not unlike dried pitch, which crumbles into small flakes when taken out. With considerable labour the miners, with the points of their crows, cut out several pieces of the strata, of some pounds weight each; and when a considerable quantity was broken off, it was carried up, and crushed to pieces with great force by the iron crow. Among these broken lumps the corundum stone is found; but in many of the pieces there was none. The mode of getting it, made it difficult to get any with the stratum adhering to it; this, however, after several trials, I obtained very perfect, and shall forward to Madras, with specimens of the strata at different depths. The stone is beyond all comparison heavier than the substance which encrusts it.

"It appears extraordinary how this stone, so concealed, should, under such difficulties, have been sought for, and applied to any purpose; and that the knowledge of the few people who dig for it, and who do so from father to son, is confined entirely to the finding the stone. For they told me they knew none of its uses, and that the labour was so hard, and their gain so small, that they would, through choice, rather work in the fields; that the sale of it, from the spot, is confined solely to the glass-sellers, who vend it over the whole country, and who had, while I was there, above forty Parriar horses, bullocks, &c. ready in the

 Pollam

Pollam to carry it to Tinnevelly, and the fouthern countries; through which track, if the ftone is known in Europe, I apprehend it has found its way by means of the Dutch.

"The people on the fpot declare it is to be got in no other fituation, or place, whatever; and the ftone-cutters tell me, they can do nothing without it. It pays no duty either where dug up or retailed.

"The colour of the ftone is either very light brown, or purplifh, in the proportion of twenty to one of the latter; but in ufe no preference is given, and they are ufed equally. To an indifferent perfon the moft ftriking circumftance is its great weight.

"As the fpot I have been fpeaking of now compofes a part of the Company's territories, the moft minute information on the fubject may be acquired.

"I felt particular fatisfaction at having been the firft European who was ever at the place; and I fhall be much gratified, if the account given meets with your approbation.

"I fhall difpatch a load of the ftone in a day or two, which I got at the Pollam, with the charge of it. The diftance from this place, by Namcul, is 84 miles.

"*The charges of 50lb. weight of Corundum.*

"Nine Tritchinopoly meafures of the corundum ftone weigh 50lb.

"Average and Coft at the Pits *.	*P.*	*F.*	*C.*
"1½ Madras farams per meafure	0	13	40
Cooley, from thence to Tritchinopoly	0	28	40
Ditto from Tritchinopoly	1	13	40
Pagodas	2	10	40

"The ftone is delivered by meafures, and paid for at the Pollam in the gold fanam.

"I am, &c.

"Nov. 15, 1792. "EDWARD GARROW."

This letter contains very interefting topographical obfervations on the mine. The fpecimens fent were of one fort of a greyifh colour, with a fhade of green. The entire cryftals which I felected among the broken ones were, of courfe, few in proportion; but with the addition of fome diftinct cryftals, which Col. Cathcart, and Capt. Colin Macauley had fent me, have been fufficient to afcertain the ftructure and form of the cryftals, of which an analytical defcription will clofe this paper. I fhall, therefore, now fay nothing concerning their form, but proceed to give an account of the varieties of corundum ftone, which I have obtained from India and China.

In the year 1786, Col. Cathcart fent me a fmall fragment of a ftratified mafs from Bengal, with this label: "Corundum, much inferior in price to that of the coaft." It is of a purplifh hue; its fracture like compact fand-ftones; and a confufed cryftallization appears in all

* The above is the prime coft. I have been informed by correfpondents who purchafed fome in retail, that it was fold for about fix fhillings a pound at Madras.

parts

parts of the ſtone, by fibres of a white colour, from which the light is reflected as in feld-ſpar, &c.

I have ſince obtained a larger lump of the ſtone of the ſame texture, but rather paler in its purpliſh hue. Sir John Macgregor Murray informed me, that it is called by the natives of Bengal, *corone*, and uſed for poliſhing ſtones, and for all the purpoſes of emery.

Its ſpecific gravity is 3,876.

Capt. Colin Macauley procured a lump of corundum from a *ſikuldar* (a poliſher, this term is moſt appropriate to poliſhers of ſteel), in whoſe family it had been above twenty years, employed for grinding and poliſhing ſtones or gems. The uſe to which it had been ſo long devoted, had occaſioned grooves in its ſurfaces which facilitated greatly the examination of its ſtructure. It is about $5\frac{1}{2}$ inches long, $3\frac{1}{2}$ inches broad, and above two inches thick. On one of its broad ſurfaces are two oval grooves; one of them is four inches long, one broad, and $\frac{3}{4}$ of an inch deep. On the oppoſite ſide is a ſhorter oval groove, above $2\frac{1}{2}$ inches long, $1\frac{1}{2}$ inch broad, and one inch deep. In theſe grooves, the ends of the laminæ of the claſs reflect the light like the cryſtals. It ſerves as a ſpecimen of the ſimple apparatus of an Indian lapidary. Stones poliſhed in theſe grooves, would be of the common India poliſh, and form *en cabochon*, which is often called tallow-drop from the French lapidaries' term *goutte de ſuif*, convex, oval, or circular. A very ſmall quantity of the corundum powder would be required, as the action of the powdered corundum and gems on the lump of corundum would, as appears from the depth of the grooves, wear away from it a ſupply of powder for the operation of poliſhing. It appears to be part of a larger maſs, is of a purpliſh colour, and of the ſame laminated texture as the cryſtals of corundum; it has this peculiarity, there appear cracks branching irregularly acroſs the laminæ of the lump, which are filled with homogeneous matter, diſtinguiſhed, however, by the ſuperior purity, which might be expected to ariſe from the degree of filtration required for its depoſition in the fiſſures. Some of theſe cracks, which terminate on the ſurface, appear to have the ſame cryſtallized arrangement which characterizes the laminæ of corundum. The cracks not being, in any degree, influenced in their direction by the laminæ of the cryſtallized maſs, it is probable they had not been conſolidated when they cracked; and from this ſpecimen, we may expect to find corundum cementing maſſes of ſtone by the ſame proceſs of ſtalactitical cementation, by which quartz and calcedony connect great nodules and maſſes of ſiliceous ſtones.

In this ſpecimen, I conſider the veins as pure corundum, that is, having the ſame ſpecific gravity, hardneſs, and texture, as corundum cryſtals; and I found the whole lump poſſeſſed all the qualities of corundum, except its ſpecific gravity, which amounted only to 2,785; and, in this property, it correſponded nearly with the matrix of the corundum cryſtals, or the vein in which corundum is before ſtated to be found; the ſpecific gravity of which is 2,768. The texture of the matrix appears ſometimes like adularia, and confuſedly cryſtallized; often compact like cipoline or primitive marble; ſometimes ſparry, ſometimes granulated; and on the outſide of the vein, and near fiſſures, decompoſed and becoming opaque. In all its ſtates it ſcratches glaſs, but not rock-cryſtal, poſſibly from want of adherence of its particles:

particles: and in this, it differs from the fubftance of the above lump, which cuts glafs and rock-cryftal with great facility.

This lump, and the matrix of corundum, appeared to poffefs the fame properties as corundum, when examined by the blow-pipe with the different fluxes.

The matrix of corundum having fometimes an appearance like adularia and feldfpar, I afcertained, by Mr. Hatchett's fcales, the fpecific gravity of adularia to be 2,558, and of feldfpar 2,555. The corundum, and the lighter corundum of the lump, cut adularia and feldfpar; the latter effervefced, and combined with foda, which the former did not.

It is, therefore, evident that the matrix of corundum, or fubftance of the vein, is a diftinct fubftance from adularia and feldfpar, and nearly connected with corundum.

The matrix, or vein, contains alfo a black fubftance like thorl, which, on clofer examination, appears to be *hornblende*. This fubftance, Mr. Garrow had remarked to have the appearance of charcoal; and, on that account, he had attributed the formation of thefe ftrata to the agency of fire. Other gentlemen, from the appearance of the matrix of corundum, have ftated it to be a calcareous vein.

Mr. Garrow obferved that there ran through the ftrata, in which the corundum was found, veins of a fubftance like dried pitch, apparently on their edge, which feparated like a pack of cards. It is a brown micaceous fubftance, which, in drying, foliates, and fhews a certain degree of regular arrangement of the component parts; in this cafe, the fragments of the folia fubdivide with fome degree of regularity into rhombs, whofe angles are 60° and 120°: it is more fmooth and lefs flexible than pure mica.

Thefe are all the forts of corundum which I procured from India.

I now proceed to the refult of my inquiries in China.

I requefted Capt. Cumming, in 1786, at that time commanding the company's fhip, Britannia, to take a fpecimen of corundum to China, to afcertain its nature, and to obtain fpecimens, if poffible, adhering to their matrix, and regularly cryftallized. On his arrival at Canton, he collected the information I wifhed, with the good fenfe and zealous defire which he always exerts for his friends. He afcertained that the ftone I inquired for was in common ufe with the ftone-cutters; and he brought me the ftone in its rude and in its pounded ftate, taking care to felect the moft regularly cryftallized pieces, and others adhering to the rock. A ftone-cutter was fawing rock-cryftal with a hand-faw, which he alfo brought to me; it is a piece of bamboo flit, about three feet long, and $1\frac{3}{4}$ inch broad, thickened at the handle by a piece of wood, rivetted with two iron pins; having a lump of lead tied with a thong of fplit rattan, fteadying an iron pin, on which the end of a twifted iron-wire is faftened, which being ftretched to the handle, is paffed through a hole in the bamboo with the fuperabundant wire; a wooden peg being preffed into the hole, keeps the bow bent, and the wire ftretched, and ferves to coil the fuperfluous wire, till, by fawing the cryftal, the ftretched wire is worn, and requires to be renewed from the coil. The twifted wire anfwers the purpofe of a faw, and retains the powder of corundum and water, which are ufed in this operation. Dr. Lind had before brought fpecimens fimilar to the above from China.

From Sir Jofeph Banks, I obtained Dr. Lind's fpecimens, and fome in powder, which Mr. Duncan, fupercargo, in China, had fent him with the Chinefe name, pou-fa. The matrix

trix being mixed with a red and white ſparry ſubſtance and mica, is generally called red granite; but it appears to me of the ſame nature as the matrix of corundum from India. The white is more fibrous, and like cyanite; the red part of it is compact and opaque; other parts appear to foliate, and pure mica is in conſiderable patches, and generally adheres to the cryſtals. This corundum is of a darker brown, and more irregular on the ſurface, than the corundum of the coaſt, and often mixed with black iron ore* attractable by the magnet.

It is deſcribed as the third modification of the corundum cryſtal in the analytical deſcription which follows. The *châtoyant*, or play of light, on theſe dark cryſtals, is very remarkable: ſome are of a bright copper colour; others exhibit the accident of reflection of light, which, in a poliſhed ſtate, gives varieties to the cat's-eye, ſtar-ſtone, ſun-ſtone, &c; which as yet are claſſed from ſuch accident without ſtrict attention to their nature, which is various, and, in general, has not been aſcertained.

Theſe are the circumſtances connected with the ſtrata, worth mentioning. The examination of corundum, on which our preſent knowledge reſts, is nearly that which an India mineralogiſt might derive of the hiſtory of feldſpar from a lump of Aberdeen granite out of one or two different quarries. He might aſcertain a few modifications of the cryſtal of feldſpar, its fracture, and matrix; but he would have no knowledge of the pureſt or more beautiful ſorts, which other quarries produce in Scotland, at Baverno, at St. Gothard, and Auvergne. I therefore think it eſſential to mention that corundum, under circumſtances favourable to its cryſtallization, becomes glaſſy in its fracture, and of various colours. I have not only obſerved in cryſtals of corundum ſpecks of a fine ruby colour, but I have fragments of cryſtals in texture and every reſpect like the colourleſs corundum, of a fine red colour. It is certain that we obtain from India, corundum which may paſs for rubies. I have ſent to India ſome of the corundum with ſmall ruby ſpecks, which were not ſufficiently diſtinct or large, either for meaſurement or analyſis, in hopes of being enabled to aſcertain correctly the form of *Salam* rubies found in corundum; in the mean time, I have the corundum of a fine red colour. Looking over ſome poliſhed rubies from India, I ſelected one which appeared laminated like corundum, and had alſo the *chatoyant*, or play of light, on its laminæ, which formed an angle in the ſtone. The lapidary called it an oriental ruby. I altered the form of the cutting ſo fortunately that the reflected rays formed a perfect ſtar; a phænomenon I had obſerved in the ſapphire, and expected in corundum, but not in the octoedral ruby. The ſpecific gravity of this ſtone being 4,166, confirmed my opinion that it is one of the *Salam* rubies, ſo much eſteemed by the natives on the coaſt or peninſula of India, which are found in the corundum vein. The ſpecific gravity of a colourleſs ſapphire very little leſs opaque than corundum, forming alſo a perfect ſtar, was 4,000; that of a deep blue ſapphire, and of a ſtar-ſtone, 4,035; all which I connect with the corundum; the ſpecific gravity of a

* A ſmall group, conſiſting of three or four octoedral cryſtals, preſents the leaſt common variety of this kind of iron ore; the edges of the octoedra being replaced by planes, which almoſt cover the triangular planes. *Romé de l'Iſle.* Criſtallog. vol. IV. plate 4, fig. 69.

diſtinct

distinct crystal of which was 3,950; of a fragment of ruby-coloured corundum, 3,959; and of a fragment of corundum with vitreous lustre, 3,954.

It may be objected to me, that Bergman has stated the variety of specific gravity in gems to be so great, as to leave no certain rule of judging thereby of the species. He observed, that the topaz generally prevails in weight, being from 3,460 to 4,560; the ruby from 3,180 to 4,240; then the sapphire from 3,650 to 3,940 *. But, in the preceding page, he had said, "Analysi crystallorum tam ejusdem quam diversæ figuræ multum lucis scientia expectat. Illæ quarum antea compositionem explorare licuit naturali forma per artem privatæ erant." It is not, therefore, an hypothesis unworthy of examination which I advance, that gems, derived from the rectangled octoedra, whose specific gravity is above 3,300 to 3,800, will be found to be diamonds, or octoedral rubies; and these will be easily distinguished from each other by their lustre and hardness. Diamonds, whether red, yellow, blue, or white, being hardest, though their specific gravity will be less; viz. from 3,356 to 3,471, as I found among different diamonds in my collection: whereas the octoedral ruby was from 3,571 to 3,625, and inferior in hardness not only to the diamond, but to the corundum; the specific gravity of which, in its different appearance of form and colour, I found to vary from 3,876 to 4,166; and I suppose it to be subject to a variation from 3,300 to 4,300: after which the jargon will come with a specific gravity of 4,600; easily distinguished also by its crystallization from the above-mentioned gems. The above specific gravities Mr. Hatchett very obligingly assisted me in taking with his accurate scales, in the temperature of 60°. It will not be understood that I depend entirely on the specific gravity; on the contrary, I connect this quality with crystallization: hardness is the next criterion; and analysis must separate the component parts, and demonstrate the analogy, or identity, of substances, or of compounds. The improvements of Mr. Klaproth's process are evident by the comparison of his first analysis, and in his last analysis of corundum.

In the first it consisted of

Corundum earth - - - -	68 0
Siliceous earth - - - - -	31 50
Iron and nickel - - - - -	0 50
	100

By the last analysis of Mr. Klaproth, the corundum of the peninsula of India consisted of

Argillaceous earth - - - -	89 50
Siliceous earth - - - - -	5 50
Oxide of iron - - - - -	1 25
Loss - - - - - -	3 75
	100

* De Terrâ Gemmarum. Berg. Opusc. vol. II. p. 104.

The

The corundum of China,

Argillaceous earth	- - - -	84 0
Siliceous earth	- - - -	6 50
Oxide of iron	- - - - -	7 50
Lofs	- - - - - -	2 0
		100

That the analyfis of fapphire of Mr. Klaproth may be compared, it is here added.

Argillaceous earth	- - - -	98 50
Calx of iron	- - - - -	1 0
Calcareous earth	- - - -	0 50
		100

Iron-ore cryftallized is often mixed with the Chinefe corundum, as I have before ftated, and may be confidered as accidentally interpofed, not combined. In the corundum of the coaft, the greenifh colour may indicate the combination of iron, as the blue colour does in the fapphire; and the proportion of iron in both is nearly alike.

There, then, is the $\frac{5\ 50}{100}$ and $\frac{6\ 50}{100}$ of filex in corundum, evidently an integral part of the coarfe corundum cryftal, and not of the fapphire; but it will require an analyfis of the vitreous or pellucid corundum to decide that filex is a conftituent part of corundum: there will then remain to account for the calcareous earth; and, having eftablifhed its being a conftituent part of the fapphire, the fmall proportion of $\frac{0\ 5}{100}$ cannot be expected to produce a very notable difference.

It is not neceffary to do more than thus to hint at what further analyfis and examination of former experiments are required, to afcertain the analogy or identity of the fapphire and oriental ruby with corundum.

I have before ftated, that I have corundum (which has the fame texture and fracture as the common colourlefs corundum) of a ruby red, and alfo of fapphire blue, and of fapphire blue and white colours.

I have fapphires yellow and blue, white and blue, brown and greenifh, and of a purplifh hue; thefe I fhould confider as corundum, with fracture of vitreous luftre.

(To be continued.)

II.

An Account of a fingular Halo of the Moon. In a Letter from WILLIAM HALL, *Efq. of Whitehall, F.R.S. Edin. to Sir* JAMES HALL, *Bart. F.R.S. Edin.**

DEAR SIR JAMES, Whitehall, near Berwick, April 2, 1796.

I SEND, under cover, the reprefentation of a very fingular halo of the moon (pl. XXII. fig. 25.) feen here on the night of the 18th of February laft, about ten o'clock; and this I have

* Edinburgh Tranfactions, vol. IV. page 173.

have hitherto delayed, in order, if poſſible, to gain farther information in the neighbourhood concerning it.

During the ſhort continuance of the ſmall halo, which did not exceed ten minutes after I got notice of it, I could not lay my hands on any other inſtrument, to take the angles, but a Siſſon's theodolite, which unlucklily having been conſtructed ſo as not to admit of a vertical angle ſo great as the moon's altitude then was, I laid it aſide, not recollecting that it might have meaſured ſeveral of the ſmaller angles. But I obſerved ſundry marks, from which I took the angles as exactly as I could next day.

The moon was about ſouth-weſt, and her altitude nearly 54°, which, of conſequence, was alſo the higheſt altitude of the limb of the greater halo where it was higheſt, and where it paſſed through the moon; the altitude of its oppoſite limb was 14°; ſo that its diameter ſubtended an angle of no leſs than a hundred and twelve degrees.

The diameter of the ſmall halo, which appeared to be a perfect circle, with the moon in its centre, I found, after repeated trials, was under 12°, and more than 8°; but as the different diameters of the large halo were not meaſured, it cannot poſitively be affirmed, that it was an exact circle; on the contrary, its limb did not ſeem to interſect the ſmall circle quite ſo much at right angles as the circular arch delineated in the plan. It may, therefore, have been ſomewhat elliptical.

The ſmall circle was remarkably bright, particularly at Weſt Reſton, about five miles to the northward, the only other place where the halo was obſerved, and where it was thought to ſend forth flame. The ſmall halo alſo continued there much longer than here, where ſome thin fleecy clouds ſoon put an end to it; but the large halo continued with us near an hour.

The weather about this time was, for the ſeaſon, remarkably mild, particularly on the day of the halo. The ſky was pretty clear all that day, and alſo in the evening; but, at the time of the halo, there was a ſmall degree of hazineſs, particularly towards the north, which did not, however, prevent the moon from ſhining with brightneſs; and the ſtars were even viſible within the circle of the ſmall halo: there was little or no wind.

The circles, or belts, of both halos are repreſented in the plan nearly of their apparent breadth, or, perhaps, a little broader: the light of both was whitiſh, and conſiderably bright, without colour; that of the large circle was the paler of the two, particularly where it paſſed through the ſmall circle: to the northward it was ſomewhat obſcure.

By means of the angles taken as above, after having aſcertained, on a vertical circle of the heavens, the ſituations of the moon, of the ſmall halo, and of the north-eaſtern limb of the large halo, whoſe ſouth-weſtern limb paſſed through the moon, the whole was projected on the horizontal plane, as in the figure already referred to. The moon, a little more than half, is placed in the centre of the ſmaller halo; and both halos are repreſented in their true ſituations, relatively to the horizon, and in the circular ſhape which they appeared to have; though they ought, perhaps, to have been ſomewhat foreſhortened, and thrown into an elliptic form.

This halo, as you will ſee by the above deſcription, appears to be of the kind called by the learned a *corona;* and as it ſomewhat reſembles the famous one of the ſun obſerved at Rome

Rome in the year 1629, and defcribed by Scheiner*, it deferves the more attention, efpecially as the great halo on the prefent occafion, having its fouth-weftern limb elevated to the height of 540°, and its north-eaftern depreffed to within 14° of the horizon, was in an obfique pofition, not eafily reconciled with the theory of Huygens, which feems to require that fuch circles fhould be equally elevated above the horizon all round. It alfo fhews that Scheiner's original plan of the halo at Rome, which reprefented it as oblique, may have been right, and that Huygens' correction, which makes it parallel to the horizon, was probably an erroneous conjecture.

I am,

dear Sir James,

your humble fervant,

WILLIAM HALL.

III.

Experimental Refearches concerning the Principle of the lateral Communication of Motion in Fluids, applied to the Explanation of various Hydraulic Phenomena. By Citizen J. B. VENTURI, Profeffor of Experimental Philofophy at Modena, Member of the Italian Society, of the Inftitute of Bologna, the Agrarian Society of Turin, &c.

(Continued from page 426, vol. II.)

EXPERIMENT XXIII. The two tubes A B C, D E F, fig. 14. Plate XXII. are 15 inches long; their diameter is 14,5 lines. The conical portions A, D, have the form of the contraction of the vein of fluid, and are applied to the orifice P, fig. 1. Plate VIII. of the prefent volume, which is 18 lines in diameter, with 32,5 inches depth, or charge of fuperincumbent fluid. The elbows, or flexures, B C, E F, are made in the plane of the horizon. Thefe two pipes are made of copper foldered with filver, and the workmanfhip carefully executed. The curvature B C was drawn out, or bended, into the form of a quarter of a circle, by filling the tube with melted lead, in order that it might preferve its diameter during the act of bending. The elbow D E F is conftructed in a right angle. The expenditure through thefe two tubes was compared with that afforded through a right-lined cylindrical tube of fimilar dimenfions, and in like circumftances. The four cubical feet of water flowed out of the cylindrical tube in 45″; out of the curved tube A B C in 50″; and out of the angular tube D E F in 70″.

4. It is of importance that the tube B C, fig. 13. Plate VIII. fhould be of an equal diameter throughout. It is not enough that care be taken that there fhall be no contraction, it is alfo neceffary that it fhould not be enlarged at any part. For fuch enlargements have nearly the fame bad effect in the expenditure as contractions. The pipe A O, fig. 12. affords a much lefs quantity of fluid with the dilatations D E, H I, than if it were of a diameter equal to that at B throughout its whole length. The following experiment agrees with the theory.

* Smith's Optics, vol. I. § 534.

Experiment XXIV. The circular orifice A, fig. 12. has the form of the contraction of the vein, and the remaining part of the tube is interrupted by various enlargements of its diameter. This tube is applied to the aperture P, fig. 1. The dimenfions of its parts meafured in lines are as follows. Diameter at A = 11,2. Diameter at B, C, F, G, &c. = 9. Length of B C = F G, &c. = 20. Length of C D = E F = G H, &c. = 13. Diameter of the enlarged parts = 24. The length of each of the enlarged parts was variable. The firft time of trial it was 38 lines, the fecond 76, and the refult of the experiment was the fame in both cafes.

Number of enlarged parts.	Time during which four cubical feet iffued out.
0	109″
1	147″
3	192″
5	240″

I afterwards applied to the fame orifice a tube, having the fame form, and the fame diameter, as A B C, but cylindrical throughout, without any enlargements, and its length was 36 inches, the fame as that of the tube with five enlarged parts; in this cafe the expenditure of four cubical feet was made in 148″.

When the fluid paffes from C to the middle of the enlarged part D E, part of the motion is diverted from the direction C F towards the lateral parts of the enlargement. This part of the motion is confumed in eddies, or againft the fides. Confequently there remains fo much the lefs motion in the following branch F G. This is alfo the caufe which deftroys, or weakens, the pulfe in the arteries beyond an aneurifm.

From this confideration we are juftified in concluding, that if the internal roughnefs of a pipe diminifhes the expenditure, the friction of the water againft thefe afperities does not form any confiderable part of the caufe. A right-lined tube may have its internal furface highly polifhed. Throughout its whole length, it may every where poffefs a diameter greater than the orifice to which it is applied; but, neverthelefs, the expenditure will be greatly retarded if the pipe fhould have enlarged parts, or fwellings. This is a very interefting circumftance, to which, perhaps, fufficient attention has not been paid in the conftruction of hydraulic machines. It is not enough that elbows and contractions are avoided; for it may happen, by an intermediate enlargement, that the whole advantage may be loft, which may have been procured by the ingenious difpofitions of the other parts of the machine.

PROPOSITION VIII.

In the machine for blowing by means of a fall of water, the air is afforded to the furnace by the accelerating force of gravity and the lateral communication of motion, combined together.

The Academy of Touloufe, in the year 1791, invited philofophers to determine the caufe and the nature of the ftream of air which is produced by the fall of water in certain forges. I propofe, in this place, to develope the complete action of this kind of blowing apparatus, and to afcertain the beft form of conftruction. Kircher is the firft I know of who has explained

plained the production of wind by a fall of water *. Barthes, the father, has given a theory which appears to me to be defective in many respects †. Dietrich was of opinion, that this wind is produced by the decomposition of water ‡. Fabri had a similar notion in the last century §. Most philosophers are well acquainted with this kind of engine ‖.

I shall begin with an idea, the foundation of which did not escape the penetration of Leonardo Da Vinci. Suppose a number of equal balls to move in contact with each other along the horizontal line A B, fig. 16. Plate XXII. Imagine them to pass with an uniform motion, at the rate of four balls in a second. Let us take B F, equal to 16 feet English. During each second four balls will fall from B to F, and their respective distances in falling will be nearly B C = 1, C D = 3, D E = 5, E F = 7. We have here a very evident representation of the separation, and successive elongation, which the accelerating force of gravity produces between bodies which fall after each other.

The rain water flows out of gutters by a continued current; but during its fall it separates into portions in the vertical direction, and strikes the pavement with distinct blows. The water likewise divides, and is scattered in the horizontal direction. The stream which issues out of the gutter may be one inch in diameter, and strike the pavement over the space of one foot. The air which exists between the vertical and horizontal separations of the water which falls, is impelled and carried downwards. Other air succeeds laterally; and in this manner a current of air, or wind, is produced round the place struck by the water. I went to the foot of the cascades which fall from the Glacière of La Roche Mêlon, on the naked rock at La Novalêse, towards Mount Cenis, and found the force of the wind to be such as could scarcely be withstood. If the cascade falls into a bason the air is carried to the bottom, whence it rises with violence, and disperses the water all round in the form of a mist.

The water which is precipitated in the hollow internal parts of mountains carries the air with it, which afterwards issuing forth from apertures at the foot of the mountain, produces those natural blasts, those *ventaroli* ¶, which are most frequently observed in the volcanic mountains, because these mountains are most commonly hollow within.

Let B C D E, fig. 16. represent a pipe, through which the water of a canal A B falls into the lower receiver, M N. The sides of the tube have openings all round, through which the air freely enters to supply what the water carries down in its fall. This mixture of water and air proceeds to strike a mass of stone Q; whence rebounding through the whole width of the receiver M N, the water separates from the air, and falls to the bottom at X Z, whence it is discharged into the lower channel or drain, by one or more openings, T, V. The

* Mundus Subterr. lib. XIV. cap. 5, edit. 1662.

† Mémoires des Savans étrangers, vol. III. p. 378.

‡ Gites de Minerai des Pyrénées, p. 48, 49.

§ Physic. tract I. lib. II. prop. 243.

‖ Art des Forges, part II. Mariotte des Eaux, part I. disc. III. Transact. No. 473, &c.

¶ These *ventaroli* are sometimes produced by the difference of temperature between the air of the cavern and the external air. V. From the effects they seem to be oftener produced by this last cause, than by a fall of water. On this subject in general, namely, the cold winds which issue out of the earth, see Philos. Journal, I. 229.—N.

air

air being lefs heavy than the water, occupies the upper part of the receiver, whence being urged through the upper pipe O, it is conveyed to the forge.

Experiment 25. I formed one of thefe artificial blowing engines of a fmall fize. The pipe B D was two inches in diameter, and four feet in height. When the water accurately filled the fection B C, and all the lateral openings of the pipe B D E C were clofed, the pipe O no longer offered any wind.

It is, therefore, evident that in the open pipes the whole of the wind comes from the atmofphere, and no portion is afforded by the decompofition of water. Water cannot be decompofed, and transformed into gas, by the fimple agitation and mechanical percuffion of its parts. The opinion of Fabri and Dietrich have no foundation in nature, and are contrary to experiment.

It remains, therefore, to determine the circumftances proper to drive into the receiver, M N, the greateft quantity of air, and to meafure that quantity. The circumftances which favour the moft abundant production of wind, are the following.

It is known that in the parabola, if $d\,x$ be affumed as conftant, $d\,y$ will decreafe in the ratio of $\frac{1}{\sqrt{x}}$. The feparation of the balls in fig. 15. is more rapid in the upper fpaces of the fall than in the lower. In order, therefore, to obtain the greateft effect from the acceleration of gravity, it is neceffary that the water fhould begin to fall at B C, fig. 16. with the leaft poffible velocity; and that the height of the water F B fhould be no more than is neceffary to fill the fection B C. I fuppofe the vertical velocity of this fection to be produced by an height or head equal to B C.

2. We do not yet know, by direct experiment, the diftance to which the lateral communication of motion between water and air can extend itfelf; but we may admit with confidence, that it can take place in a fection double that of the original fection, with which the water enters the pipe. Let us fuppofe the fection of the pipe B D E C to be double the fection of the water at B C; and in order that the ftream of fluid may extend and divide itfelf through the whole double fection of the pipe, fome bars, or a grate, are placed in B C, to diftribute and fcatter the water through the whole internal part of the pipe.

3. Since the air is required to move in the pipe O with a certain velocity, it muft be compreffed in the receiver. This compreffion will be proportioned to the fum of the accelerations, which fhall have been deftroyed in the inferior part K D of the pipe. Taking $K\,D = 1{,}5$ feet, we fhall have a preffure fufficient to give the requifite velocity in the pipe O. The fides of the portion K D, as well as thofe of the receiver M N, muft be exactly clofed in every part.

4. The lateral openings in the remaining part of the pipe B K, may be fo difpofed and multiplied, particularly at the upper part, that the air may have free accefs within the tube. I will fuppofe them to be fuch that 0,1 foot height of water might be fufficient to give the neceffary velocity to the air at its introduction through the apertures.

All thefe conditions being attended to, and fuppofing the pipe B D to be cylindrical, it is required to determine the quantity of air which paffes in a given time through the circular fection K L. Let us take in feet $K\,B = 1{,}5$; $B\,C = B\,F = a$; $B\,D = b$. By the common theory of falling bodies, the velocity in K L will be $7{,}76\sqrt{(a+b-1{,}4)}$; the circular fection $K\,L = 0{,}785a^2$. Admitting that the air in K L to have acquired the fame velocity as the

the water, the quantity of the mixture of the water and air which paſſes in a ſecond through K L is $=6,1\ a^2 \surd\ (a+b-1,4)$. We muſt deduct from the quantity $(a+b-1,4)$ that height which anſwers to the velocity the water muſt loſe by that portion of velocity which it communicates to the air laterally introduced; but this quantity is ſo ſmall that it may be neglected in the calculation. The water which paſſes in the ſame time of one ſecond through B C is $=0,4\ a^2 \surd\ (a+0,1)$. Conſequently, the quantity of air which paſſes in one ſecond through K L, will be $=6,1\ a^2 \surd\ (a+b-1,4)-0,4\ a^2 \surd\ (a+0,1)$, taking the air itſelf, even in its ordinary ſtate of compreſſion, under the weight of the atmoſphere. It will be proper, in practical applications, to deduct one-fourth from this quantity; 1, on account of the ſhocks which the ſcattered water ſuſtains againſt the inferior part of the tube, which deprive it of part of its motion; and, 2, becauſe it muſt happen that the air in L K will not, in all its parts, have acquired the ſame velocity as the water.

If the pipe O do not diſcharge the whole quantity of air afforded by the fall, the water will deſcend at X Z; the point K will riſe in the pipe, the afflux of air will diminiſh, and part of the wind will iſſue out of the lower lateral apertures of the pipe B K.

I ſhall not here examine the greater or leſs degree of perfection of the different forms of water-blowing machines, which are uſed at various iron forges, ſuch as thoſe of the Catalans, and elſewhere. Theſe points may be eaſily determined from the principles here laid down.

PROPOSITION IX.

It is poſſible, by means of a fall of water, to drain a piece of ground, without the help of machines; even though the ground ſhould lie on a lower level, than the eſtabliſhed current, below the fall.

The means of doing this is pointed out in the firſt experiment of this treatiſe. We have ſeen that the water contained in the veſſel D E F B, fig. 3. Plate VIII. iſſues through the channel M B V, which is higher than the ſurface of the water itſelf, becauſe the fluid which paſſes through A C carries with it the water contained in the veſſel.

In the artificial fall, which is procured in channels to give motion to mills, when the water ruſhes down by a rectangular trunk of wood, D B C F, fig. 17. placed nearly horizontal in the middle of the lower channel, the ſurface of the water at K is one or two feet beneath the inferior current (or back-water) F L*. The water at F tends to return and deſcend along F K; but the current, by its lateral action, conſtantly carries it away, and does not permit it to ſlide down to K. If an opening G be made in the lateral ſides of the trunk, the waters from lands lower than the current of the inferior ſtream F L may be drained off. In a commiſſion with ſeveral of my colleagues, I once propoſed, that this principle ſhould be applied to a caſe of practice. The project was adopted, and the drainage ſucceeded very well.

The rectangular conduit D D F C muſt be prolonged to a certain extent along the lower channel, otherwiſe the water might flow back from F to K, and oppoſe the drainage through G. The mill-wrights are aware of the utility of this prolongation. Experiment has taught

* This depreſſion of the level has already been noticed in K. Guilielmi, della natura dè fiumi, cap. 7, fig. 46. Boſſut, art. 721. The wheel alluded to in the text muſt, I preſume, be of that kind which we call a breaſt-wheel.—N.

them

them, that it prevents the water from returning back fo readily in the time of floods, which might check the motion of the water-wheel. For this purpofe, they make the upper part D F at the height of the waters, which the mill-ftream can refift or fupport. The town of Final, in the territory of Modena, having charged me with the direction of changing the courfe of part of the waters of the Panaro, which the circumftances of the town required to be done; I availed myfelf of this prolongation of the tail-pipe D F, combined with other artifices, to maintain the action of mills in the new channel; and I fucceeded not only beyond the expectation of the inhabitants, but even beyond my own hopes.

PROPOSITION X.

The eddies of the water in rivers are produced by motion, communicated from the more rapid parts of the ftream to the lateral parts, which are more at reft.

Few authors have examined the caufe and the effects of the eddies of water in rivers; and thofe who have undertaken this inveftigation, do not appear to have been very happy in their refearches.

The water which moves in the channel M N H, fig. 19. meets the obftacle B A, which impedes its courfe, and caufes it to rife and difcharge itfelf in the direction A C with an increafed velocity. Suppofe the water in B D C A to be dormant, the current A C communicates its motion to the lateral particles E (Prop. I.), and conveys them forward; the furface of the dormant water becomes depreffed at E, and the moft remote particles towards D are urged according to the laws of the equilibrium of fluids to fill the depreffion. The current A C continues to carry them off, and the fpace B D C A continues to be exhaufted. The water of the current A C, by virtue of the fame laws, is acted upon by a conftant force which urges it towards the cavity E, while its natural courfe or projection carries it towards A C. Under the agency of thefe two forces, the water A C acquires a curve-lined motion in C D, and defcends as it were through an inclined plane, becoming retrograde in D E, whence it would proceed to ftrike the obftacle B A, and the current A C, after which, it would undergo feveral ofcillations previous to acquiring a ftate of equilibrium and repofe. But the current A C continues its lateral action: a fecond time it draws away the water through C D into E, and forces it to renew its motion through the curve C D E; in which manner the eddy continues without ceafing.

If the river fhould pafs through a contraction of its bed at N, it will produce eddies on both fides, at P and at Q, fimilar to thofe we have contemplated at D C.

Suppofe the ftream of water, after having ftruck the bank G H, to be reflected into a new direction H S, the lateral communication of motion will excite eddies in the angle of reflection R.

When two currents of unequal velocity meet obliquely in the middle of the river, the moft rapid current will produce eddies in that which is the leaft rapid.

Suppofe a ftream of water to flow over a bed of unequal depth. If the longitudinal fection of the inequalities of the bottom exhibit a gentle flope, as at A B C, fig. 20. the fuperior water will imprefs its motion by lateral communication upon the inferior water, which is near the bottom, beneath the line A C, and a current will take place through the whole depth of the

the fection M B. The current which is formed near the bottom at B, is turned out of its courfe by the flope B C, and proceeds to rife above the furface at Q; fometimes in the form of a curling wave or vertical whirlpool. If the extremities of the hollow place form an abrupt angle, as D E, F G, eddies will be produced even at the bottom, in the vertical direction at D, and fometimes alfo at G. Thefe phenomena may be obferved in an artificial channel with glafs fides.

Every eddy deftroys a part of the moving force of the current of the river. For the water which defcends by a retrograde motion, in the inclined plane C D E, fig. 19. cannot be reftored in the direction of the current of the river but by a new impulfe. It is as it were a ball, which is forced to rife on an inclined plane, whence it continually falls back again to receive new impulfions. It is the labour of Sifyphus.

Hence I deduce, as a primary confequence, *that in a river, of which the courfe is permanent, and the fections of its bed unequal, the water continues more elevated than it would have done, if the whole river had been equally contracted to the dimenfions of its fmalleft fection.* The caufe of this phenomenon is the fame as that which retards the expenditure through the tube with enlarged parts. (Prop. VII. No. 4.) The water which defcends from the elevation above the contracted part N, into the bafon P Q, fig. 19. lofes nearly the whole of the velocity it acquired by defcending from it; becaufe the narrow part has a curved flope towards the lower part of the river, which directs the velocity of the ftream in an horizontal direction. Guilielmini has well remarked, that a fall does not influence the velocity of the lower ftream, becaufe the eddies of the water in the bafon P Q deftroy the velocity produced by the fall. This velocity increafes the depth, and enlarges the width of the channel at P Q. Eddies are formed on each fide, at the bottom, and at the furface; both in the horizontal and vertical directions. It would be to no purpofe to atttempt to prevent this hollowing out and enlargement of the channel by fuch a fall, by adopting the means of clofe walls; for the bafon would then obtain its enlargement, where thefe conftructions might end.

If the channel have a number of fucceffive contractions and dilations, M N, without cafcade or dam, there will ftill be formed, at each dilatation, eddies which will diminifh the velocity more than if the channel had an uniform fection equal to that in M or N. It will, therefore, follow, that the furface of the water, after each dilatation, muft rife, in order to recover the velocity it loft by the eddies. If we call the height to which the water muft rife, above the elevation neceffary to have overcome the retardations of a bed of uniform fection, $= a$, and that the number of equal and fucceffive alternate dilatations and contractions be $= m$, the height of the rife in the ftream thus alternately dilated beyond that of the fame river uniformly contracted will be $= am$. I here fuppofe the bottom of the river to be uniform. If this bottom be of fuch a nature to be attacked by the current, the contracted parts will be hollowed out, and the matter will be depofited in the enlarged parts.

The fecond confequence which I draw from the principle here eftablifhed, refpecting the lofs of force, caufed by the eddies, is of confiderable importance in the theory of rivers, and appears to have been neglected by thofe who have treated on this fubject. The friction of the water along the wet banks, and over the bottom of rivers, is very far from being the only caufe of the retardation of their courfe, which, confequently, requires a continued defcent to

 maintain

maintain its velocity. One of the principal, and moſt frequent, cauſes of retardation in a river, is alſo produced by the eddies, which are inceſſantly formed in the dilatations of the bed, the cavities of the bottom, the inequalities of the banks, the flexures or windings of its courſe, the currents which croſs each other, and the ſtreams which ſtrike each other with different velocities. A conſiderable part of the force of the current is thus employed to reſtore an equilibrium of motion, which that current itſelf does continually derange.

(To be continued.)

No. IV.

Concerning a new Variety of argillaceous Iron-ore. By SAMUEL L. MITCHILL, *M.D. of New York.*

SIR,

THE annexed deſcription of a mineral ſubſtance, and the remarks upon it, are taken, by permiſſion, from Dr. Mitchill's manuſcripts. As the ſubject appears to me worthy to be made known to the mineralogiſts of Europe, I have forwarded you a copy for publication, in your uſeful and inſtructive Journal; I have the ſatisfaction to inform you at the ſame time, that Mr. Bruce, a young gentleman who goes paſſenger to London in the ſame ſhip which conveys this note, is in poſſeſſion of a ſpecimen of this remarkable foſſil.

I am, Sir,

Your very obedient ſervant,

New-York, November 17, 1798. ADOLPH. C. LENT, M.D.

To Mr. W. Nicholſon.

A new Variety of Iron-ore of the argillaceous Kind, and figured ſomewhat like Baſaltes.

The ſpecimen of columnar iron-ore in my collection is from Germany, and correſponds very well with Mr. Kirwan's ſecond variety of his firſt family of argillaceous iron-ores. (2. Elements of Mineralogy, Dublin, 1796.) I knew of no other iron-ore of that character, until a few months ago. As I was walking on my farm upon Long-iſland, ſurveying the various mineral productions that lay thick under my feet, I obſerved a ſmall ſpot, that appeared to be paved, with ſtones of a regular figure; on taking up ſome of them and examining them, they were evidently of the ſame conſtitution with the common argillaceous iron-ſtone, which lay ſcattered about in rude lumps. They were, however, of a ſingular ſhape. They were about four inches long; and ſtood erect, ſide by ſide of each other, in a ſtiff loam. Each of them had five ſides and five angles; and though not exact pentagons, were readily diſtinguiſhable in moſt of the ſpecimens from one end to the other. This ſhape was moſt perfect at the two ends, which were conſiderably larger than the middle part, and about one inch and a half acroſs. So that when a ſpecimen of this ore was graſped in the hand, it had ſome reſemblance of a double-headed peſtle. In ſome, however, four ſides only were to be traced.

The

The number of these figured bodies was about thirty-six, though not all of them equally perfect. And they evidently had been formed from a solid circular mass of argillaceous iron-ore, of about two feet and five inches in breadth, and four inches in thickness, at the central parts, and becoming thinner towards the edges. In the natural fracture of the stone, the fragments took upon themselves these regular forms, very much resembling basaltes; and, like basaltes, though *figured* they evidently are not *crystallized*, for they are as perfectly terrigenous and opaque as any argillaceous ore of iron whatsoever.

I brought away some of the pentagonal pieces, and could easily get more of them, for in taking out the few that I brought away, I disturbed the rest as little as possible.

Do not these specimens go very far towards deciding the dispute, if any doubts still remain, about the *igneous* or *aqueous* origin of basaltes? they support, by the most powerful evidence, the *Neptunian* origin of that kind of figured bodies, and are directly opposed to the *Plutonic* system. The specimens before me prove, that argillaceous iron-ore, which nobody has supposed to be a volcanic production, can take on a basaltic figure. With Bergman, Weideman, and Kirwan, I am inclined to believe the weight of testimony is opposed to the formation of basaltic columns by fusion, and that the true manner, in which many, if not all, of them have been produced, is in the moist way. This new proof, added to Mr. Kirwan's very able paper on *traps* and *basaltes*, I think settles the controversy in his favour.

V.

Concerning the Invention of the Electrical Doubler. By Mr. John Read.

To Mr. NICHOLSON.

SIR,

IN your journal, Nov. 1798, page 368, you charge me with want of candour, in my description of what I call the spectacle doubler of electricity. Whether this charge be just, or not, must be left to the decision of a judicious public: who, it is hoped, will have candour enough to compare your account of the doubler with mine. I was of opinion, that after giving you the priority of invention, which I have expressly done in page 29 of my work, entitled Summary View of the spontaneous Electricity of the Earth and Atmosphere, no further acknowledgement could be required. The passage is, " and to give the plates a considerably more extended insulation than that made by Mr. Nicholson, without augmenting the size of the instrument, &c."

That your charge is frivolous, will more fully appear from the following circumstance, namely, that Dr. Priestley did actually deliver my original manuscript into your hands, with full power to correct, erase, or add whatever you chose, and after you had retained it more than three months in your possession, I received the manuscript from you, and found that you had made no alteration at all in it: of course it went to the press in the dress it is now in.

It is not my intention, at present, to criticise on all you have said in the passage alluded to. As to the adopting some of your own words into my description, of nearly the same instrument, could not well be avoided; nor can it be thought an illiberal proceeding. But if you disliked it—

it—if you thought it improper, you ought to have mentioned it at a proper time, viz. when you returned the manuſcript, or before it was ſent to the preſs; and your requeſt would have been literally complied with. With regard to the inſtrument itſelf, it is at preſent uſeleſs; and it is allowed by all, that it has totally failed for want of a perfect inſulation. If this defect ſhould at ſome future time be overcome, the doubler of electricity, will *then* be the moſt *uſeful* and the moſt *noble* inſtrument in the whole group of electric apparatus.

I remain, ſir,

Your very humble ſervant,

JOHN READ.

Quadrant, in Knightſbridge,
January 17th, 1799.

Though the above letter relates to perſonal incidents, which may not, perhaps, be conſidered with any great degree of intereſt, yet I conceive my readers will admit the propriety of publiſhing it, in order that Mr. Read, whoſe candour, as an author, has been called in queſtion, may juſtify himſelf to that public, which he has eſſentially ſerved during a long life, employed as an inſtrument-maker and operative philoſopher. I am well content, that the deciſion reſpecting the doubler, ſhould be made by the tribunal to which he refers, and to which it of right belongs. If a careful review of what I have written, together with his preſent communication, had led me to alter my ſentiments, it would be my duty to ſay ſo in this place. But I think I have faithfully ſtated the facts in the paſſage he alludes to, and have only to remark, that the indirect mention of my name, in page 29 of his work, was little, if at all, calculated to deſtroy the concluſion which his readers would obviouſly be induced to make, from the unacknowledged copy in the former part of the chapter; in proof of which, the inference of the foreign philoſophers, who were miſled in his favour as the inventor, is nearly deciſive. His narrative reſpecting my having poſſeſſed his manuſcript, previous to its publication, is not quite correct in the manner, becauſe it does not communicate the whole of what happened. Either Dr. Prieſtley, or elſe Mr. Read with the Doctor's recommendation, did deliver to me certain manuſcripts about ſix or ſeven years ago, which I believe to have been part, or the whole, of what was ſince publiſhed, under the title of *A Summary View, &c.* but they were not put into my hands with the notion that any relation might ſubſiſt between me and their contents, which could require the full powers Mr. Read mentions;—but ſimply to correct and prepare them for the preſs, on condition of being paid for my labour. As I have always declined this kind of employ, excepting when motives of perſonal acquaintance or friendſhip have led me to it, I returned this copy untouched, and unperuſed, to the author, and ſuppoſed of courſe, that he had employed ſome other perſon to reviſe it. I hope and believe, therefore, that notwithſtanding this incident, upon which Mr. Read ſeems to place ſo much reliance, it cannot be thought to afford any ground for an inſinuation, that I have been induced to ſpeak as I have done, of his ſhare in the invention of the doubler, from any motives, but ſuch as ought to guide an independent narrator of philoſophical facts.

With

With regard to the inftrument itfelf, which is fo truly honourable to the fagacity of Lichtenberg, Klincock, Volta, and Bennett, but in which my fhare is certainly very trifling, I do not know that its imperfections are greater than was long fince afcertained by the Englifh electricians; of which an account is given in a paper, at page 394 of the firft volume of this Journal. If Mr. Read is in poffeffion of any new facts refpecting its operation, the public will, no doubt, receive them with as much attention and refpect, as they have paid to the refults of his former purfuits.

VI.

Inquiries refpecting the Conftruction of a Water-wheel, and the Manufacture of Bricks. By a Correfpondent.

Mr. Nicholfon will oblige a friend to his ufeful Journal, by giving, therein, information on the under points.

December, 1798.

On a ftream where the fall is $6\frac{1}{2}$ feet, what diameter ought the water-wheel to be?

Whether a bucket-wheel, or float-board-wheel?

If the former, ought the water to be brought in at the height of the fall; or a portion given for head? and what portion?

Is there any practical direction in print, to direct the procefs of brick-making?

If there is not, Mr. N. would do a material fervice to numbers, who are in fome parts of the country, remote from the workmen who are fkilled in this art, by collecting and publifhing fuch practical directions of the nature of the materials and the procefs, as will enable the uninformed to fupply themfelves with this ufeful article.

The data, refpecting the above-mentioned ftream, are not fufficiently precife to determine whether a bucket-wheel, or float-board-wheel, would be preferable: the quantity of water afforded per minute ought to have been mentioned. The bucket-wheel appears, upon the whole, to be beft adapted to fmall ftreams of water with a confiderable fall; but, in contrary circumftances, the clofe breaft-wheel appears to be preferable, that is to fay, a wheel with float-boards, moving in a channel fo well fitted, as to permit the leaft poffible quantity of water to efcape, without acting upon the wheel.

The diameter of the wheel may, in theory, admit of confiderable variation. Admitting it to be a breaft-wheel, its radius muft be fomewhat more than the height of the fall. The velocity of the furface of fuch wheels, as are driven by the gravitating power of water, lies between two feet and fix feet per fecond. Three feet may be confidered as a good practical velocity, as determined by experience. It is of no advantage, but, on the contrary, a lofs, to confume any part of the head, in throwing the water againft the floats with a confiderable impulfe; it is only neceffary that the ftream fhould be delivered upon the wheel with fomewhat

what more velocity than that of the wheel itſelf. If the ſtream can be brought on with the velocity of three feet at the top of the fall, it will, undoubtedly, be beſt; but if the water ſhould flow from a pond, having no perceptible current, the portion or ſpace required to produce this velocity will be about one foot and a half.

Theſe remarks are equally applicable to bucket or overſhot wheels.

The art of brick-making is conciſely deſcribed in the French Encyclopedie. Bergmann has attended to this ſubject in a direct chemical way. A tranſlation of his treatiſe into Engliſh, may be ſeen in the third volume of his Phyſical and Chemical Eſſays; or an abridgement of the ſame in my Chemical Dictionary, article Brick. As the art itſelf is of extenſive utility, and of conſiderable ſimplicity in the practice, I ſhould, in this place, have deſcribed the proceſs, with remarks, if I were not deſirous of again inſpecting the methods which are practiſed in the neighbourhood of this metropolis. When I ſhall have done this, the account he deſires will appear.

VII.

On the Combuſtion of Phoſphorous.

To Mr. NICHOLSON.

SIR,

UPON putting a ſmall piece of phoſphorus in a ſhovel over the fire, in ſome experiments I lately made for the entertainment of a company of friends, that ſubſtance ſmoked, began to melt, and took fire as uſual. When the combuſtion had ceaſed, and we were looking at the ruddy coloured reſidue, or acid, one of the company wetted the reſidue by ſpitting upon it; at which inſtant, the combuſtion was renewed, with a crackling noiſe, at every part where the moiſture had reached. I ſuppoſe the reſidue to have been oxygenated phoſphorus, or phoſphoric acid; and that the addition conſiſted chiefly of hydrogen and oxygen. What then was the new combination, which was attended with the farther extrication of heat? Will you have the goodneſs to explain this fact, or ſubmit it to your correſpondents.

I am, Sir,

Your obliged reader,

January 4, 1799. R. S.

I have complied with the requeſt of this correſpondent, by publiſhing his letter; upon which my other friends will make their remarks, if the ſubject ſhould appear to require it. If phoſphoric acid, at an elevated temperature, in contact with iron, can be made to emit heat and light by the addition of water, as R. S. apprehends, the true explanation of the play of affinity muſt be ſought from varied experiments, in which all the products, as well gaſeous as fixed, ſhould undergo examination. But in the inſtance before us, I am much inclined to think the renewed combuſtion is produced by a portion of phoſphorus, which eſcaped the atmoſpheric action in the firſt burning. That is to ſay, when a piece of phoſphorus is burned,

burned, in the circumstances of the present experiment, the upper or greatest part of the mass becomes acidified, and covers a portion beneath, so as to prevent that lower part from combining with the atmospheric oxygen; in consequence of which, the combustion ceases before the whole is burned. But when this dense acid becomes diluted with water, it boils, and most probably rises, at least so far as to leave the surface of the phosphorus below uncovered, and to permit the combustion to be renewed. Instances of volatility being given to various substances by water, are sufficiently numerous in chemistry; of which the acid of borax is one of the most striking. To what degree, the same effect may take place in the phosphoric, I am not aware. Scheele (on Air and Fire, §. 73), indeed, affirms, that water gives it fixity; but he says this in so loose a way, that I am not disposed to rely on the assertion, as universally true, in contradiction to some other facts, which shew that the diluted acid does partly rise by heat.

VIII.

Pyrometrical Essays to determine the Point to which Charcoal is a Non-conductor of Heat. By Citizen GUYTON *.

SINCE the experiments and researches of philosophers have been directed to the matter of heat, the state in which it is found, and the various manners in which it affects bodies, it has been well ascertained that charcoal is one of the worst conductors of heat. From this observation it has been proposed to form a double wall to furnaces, and to fill the intermediate space with charcoal; and, very recently, an happy application of this property has been made in the construction of vessels designed for the preserving the temperature of warm infusions.

I am not, however, acquainted with any researches which may have been made to fix, even comparatively, the limits of this kind of insulation. The effect of the non-conducting power of charcoal has been so slightly considered, even by the most accomplished chemists, that they have not thought it necessary to take any account of this effect in the result of their operations. Hence it is, that the celebrated Klaproth, in a series of experiments upon the alteration which stones undergo when exposed to extreme heat, seems to think that the heat might have been nearly equal in such as were contained in crucibles of porcelain, and those which were entirely surrounded with charcoal. The reduction of the oxides of tungsten, titanium, and uranium, has also been attempted in crucibles of charcoal, though it is well known that the highest degree of heat is here required for the fusion, and that, in other respects, every endeavour must be made to expose them to the most intense ignition.

These reflections have led me to conclude, that it might be of some importance to obtain a more accurate measure of this insulating effect of charcoal. The following are the experiments which I have made upon this subject.

Out of the same parcel I took two pyrometic pieces perfectly alike, and placed one, which I shall call A, in a crucible filled with pure siliceous sand dried over the fire. The crucible

* Read to the French National Institute, 6th Germinal, in the 6th year of the Republic (March 26, 1798), and inserted in the Annales de Chimie, XXVI. 225.

was

was 8 decemeters high, 6 in diameter at the mouth: the cylinder of clay was placed in the middle, and the crucible had its cover luted on.

The other piece, B, was placed in a ſimilar crucible, with this difference, that the crucible was filled with powder of charcoal, which had been previouſly ignited in a cloſe crucible.

The two crucibles were then placed beſide each other on the grate of a large melting furnace, in which the fire was kept up for about three quarters of an hour.

When the crucibles were cooled, the cylinder A was taken out of the ſand, and preſented to the pyrometic ſcale of Wedgewood; it had undergone a contraction of 89 degrees. The cylinder B was then taken out of the charcoal, and ſtopped in the gage at 60,25 degrees. It had acquired a grey tinge, but without any appearance of glazing.

Hence it follows, that the tranſmiſſion of heat through the ſand, is to the tranſmiſſion through the charcoal nearly in the proportion of 3 to 2. In proportion as this difference is in itſelf ſtriking, the more it becomes neceſſary to attend to ſuch precautions as are required to prevent deception ariſing from foreign circumſtances. The crucibles had moſt aſſuredly undergone the ſame degree of fire. The ſtate in which they were found exhibited the traces of its action: the conditions were therefore as equal as poſſible; but it might, poſſibly, be ſuſpected that ſome defect, or want of uniformity in the pyrometic piece, or ſome imperfection in its compoſition, or fabrication, might have altered its diſpoſition to contract equally, and proportionally to the heat it might undergo. There was a very ſimple method of removing theſe doubts; namely, to aſcertain whether the ſame piece put into the ſand, and expoſed to a much ſtronger heat, would reſume the common courſe of contraction, and agree with the former. This, in fact, was performed; the ſame two pieces, A and B, were incloſed in one and the ſame crucible filled with ſand, ſo that they were not more diſtant from each other than about 7 or 8 millimetres, and the crucible was expoſed for half an hour to the moſt violent heat of a forge, urged by three twyers.

The crucible, when cold, was found to have loſt ſome of its thickneſs by vitrification, ſo that there was a crack in one of its ſides. The ſand, however, was not deranged within.

The piece A marked 163,5 upon the pyrometic ſcale, it weighed no more than 1,491 grammes; its ſpecific gravity was 2,232.

The piece B exhibited 160 degrees on the pyrometer, it weighed 1,53 grammes, and its ſpecific gravity was 2,346. It had almoſt loſt the grey tinge which it had acquired in the charcoal, and was no longer diſtinguiſhable from the other but by a black vitreous point, produced by the acceſſion of ſome foreign matter.

I confeſs I did not expect to be ſo completely ſucceſsful in this verification: the ſmall difference of $3\frac{1}{2}$ degrees is nothing, when we conſider that the piece firſt incloſed in the charcoal, and which had ſtopped at 60, was ſtill capable of contracting through an additional 100 degrees. It is, beſides, known to be phyſically impoſſible, that two bodies placed in the ſame crucible, and in contact with the ſame ſubſtance, ſhould be ſtrictly in the ſame ſituation as to the reception of heat, particularly when the blaſt is directed from three different nozzles, which are neceſſarily unequal. The advanced ſtate of one of the ſides of the crucible, with reſpect to the fuſion, is a proof that this was in fact the caſe.

We may therefore conclude from theſe eſſays, that the body included in the charcoal in the

the fame fire, does not receive more than about two-thirds of the heat of a body furrounded with quartzofe fand; that the reduction of metals which do not flow at a lower degree of heat than 130 degrees, cannot be effected in charcoal; that the pyrometric pieces do not afford an accurate judgment of the heat communicated, unlefs they be in contact with fubftances of the fame kind as thofe which furround the body, on which the chemical procefs is performed; and laftly, that with thefe attentions we may make an advantageous ufe of this inftrument, of which the applications will not be fufficiently known, until we fhall be able to procure, at pleafure, cylinders fabricated after the manner of Wedgewood, of a clay fufceptible of lofing 0,18 of its volume by contraction; or, which is the fame thing (fuppofing no elaftic matter to fly off), of paffing from the fpecific gravity of 2,05 to 2,45, between the degree of ignition required for baking the bifcuit of earthen ware, and the higheft heat of furnaces, without undergoing a commencement of fufion.

I have made a great number of experiments, with the intention of difcovering a clay which might naturally exhibit thefe properties, or which might acquire them by eafy and cheap preparation. I hope I fhall be fhortly able to communicate fatisfactory refults to the clafs of the inftitute.

IX.

Defcription of an Air-pump, of a new Conftruction. By the Rev. JAMES LITTLE, *of Lacken, in the County of Mayo, in Ireland*.*

THE pump here defcribed is a portable one, contrived fo as to be confined in a very fmall fpace; but the author obferves, that it may be made of a different form, and with two barrels, though not conveniently.

Fig. 1. plate XXI. is a perfpective view of the whole machine, as it lies before the operator. Fig. 2. is a back view of the fame.

The barrel A A (fig. 1.) is almoft fifteen inches long, and internally in diameter juft two inches. The pifton (fig. 3.) is folid, without any perforation; it confifts of circular plates of leather, preffed together between the round plate *a* (in the focket of which, *b*, the pifton rod was inferted, and faftened by a crofs-fcrew, before the leathers were put on), and the two upper plates *c* and *d*; of which the outer one, *d*, being fcrewed upon a fcrew formed on the outfide of the focket, preffes down the under plate, and this the leathers, which latter are not reprefented in the figure. The upper plate, when fcrewed on, lies even with the upper part of the focket, that when the pifton is raifed to the top, it may touch the plate covering the upper part of the barrel, leaving no air above the pifton. Thefe two plates of the pifton, efpecially the lower one, are made fo large as juft to move in the barrel without touching it, and care was taken that when the pifton is put down, it fhould be every where in contact

* Irifh Tranfactions. VI. 319. The paper from which the above is abftracted, could not be given on account of its extent. It occupies 77 pages. Some of the early air-pumps were made with an horizontal barrel. There is a plate of one in Senguerdii Philofophia Naturalis, the fecond edition of which, in 4to, was publifhed at Leyden, 1685.

 with

with the plate at the bottom of the barrel, this plate being turned in the lathe upon the piston rod, which fitted its socket exactly, so that not the least space might be left for lodgment of air under the piston. The leathers are of the best buck-skin, dressed in the usual manner, firm in its texture, but not harsh; and, being well dried, were soaked in a mixture of three parts suet, melted with one part oil, before they were put together: they were then, when cold, turned in a lathe on the piston with a very sharp tool *.

The cylindrical iron rod of the piston is well finished, and moves through a collar of leathers. As it is altogether necessary that this rod should move most exactly in the middle, or axis, of the barrel, care was taken that the position of the box, and of the socket annexed to it, should be secured, by having a prominent part of the plate of the box of leathers inserted into the pump barrel, and a like projecting part of the lid of the box inserted into the box itself; also that the piston rod should most exactly fill the socket in the lid. Over the collar of leathers, within the box, lies a tinned brass plate, which is pressed down upon them by the ends of three screws 3 3, screwed through the lid of the box.

The piston is moved up and down by the toothed flat bar, or rack, F, whose end is furnished with an arm G (to be taken off occasionally), which arm is secured against a shoulder formed on the end of the piston rod by a nut H screwed on the rod; the rack is moved by a small steel wheel L, of twelve teeth, turned by the handle X on its axis L. This wheel is sustained by the cheeks K K, fig. 1. and 2. furnished with projecting sockets, through which its axis passes, and these cheeks are screwed to the iron bar M, which is a part of the frame supporting the whole machine; by this bar it is fastened by clamps to whatever table it is placed on. From the middle of the bar, and at right angles with it, extends horizontally an arm N (rivetted, and brazed upon the bar), the under surface of the arm being on a level with the upper one of the bar; this arm supports the gage-glass Y, and terminates in a cross-piece (making together the form of the letter T), to which piece and to the arm the receiver-plate is screwed by screws underneath, inserted into its thick margin.

The bar M supports the upright pieces, or pillars, *o*, *o*, terminating in the semicircular arms *p*, *p*, on which the barrel is fastened by four screws at *p*, *p*, screwed a little way into its projecting rings. These pillars are made of iron, and very strong, being half an inch in thickness; they are also most firmly fixed, and should be brazed in the bar which supports them; this being necessary, as all the force of the handle of the wheel, by which the rack is worked, bears against these pillars; and if they were to be shaken, the cement, by which the receiver-pipe is connected with the pump, would be broken, which cannot otherwise happen. These pillars support also the iron case, or sheath, P P, extended underneath the pump-

* If tanned leather were used for the piston, &c. it might be soaked in oil alone; but, besides that, it is apt to grow too hard by compression, and also to corrode the brass of the barrel more, from the acid imparted by the bark, with which it is tanned; it also emits a greater quantity of elastic fluid in vacuo than leather which is not tanned. On these accounts, I have used buck-skin leather for the piston; but the texture of this being very loose, oil only would not fill its pores, so as to make the piston air-tight, even when the leathers were closely pressed; and it was necessary to fill its grain with the above stiffer composition. Yet this leather will also generate air, and is harder to be made air fast; it also moves so stiffly in the barrel, that, perhaps, good shoe leather is preferable to it.—L.

barrel,

barrel, within which case slides the toothed bar, or rack, F, moved by the wheel, and this rack (with its case) is fixed exactly parallel to the axis of the cylinder, that it may draw out the piston rod precisely in that direction in which itself moves: the bar is $\frac{3}{10}$ of an inch in thickness, but is an inch in breadth from the bottom of its teeth to the back of it. It is made thus strong, that the end of it may not be strained upward from the proper direction, when it is urged forward by the wheel, and yet checked by the piston rod, after the piston is raised to the top of the barrel. To confine the rack in the true line of its motion, it is made perfectly straight, and of the same dimensions in its whole length, and its case the same, so as to fit each other most exactly, that it may be kept in its due position, when the greatest part of it is drawn out of the case; for which purpose also it is made (as likewise the case) so much longer at either end than the part necessary to be toothed, as to permit a great part of it to remain in the case, when the piston rod is drawn out to its utmost extent: accordingly in this pump it acts, in this respect, as well as could be wished*. A notch is cut out of the case at I, to allow the teeth of the wheel to take into those of the rack; and to keep the case firmly in its place, little notches are cut in the upper edge of it, into which the contiguous parts of the pillars are let, and it is secured so by wedges, 2 2, underneath. It will be known that the case of the rack has its due position, when the arm G being taken off, both the rack and the piston rod pulled out to their limit, are found to be parallel. The pillars are made as short as possible; to favour which the frame M has a furrow cut in the middle of it, under the wheel I, to let the teeth of the wheel descend almost to the table on which the machine rests; and the pump barrel is placed as near as may be to the rack.

On the upper part of the box, containing the collar of leathers, is a projecting part of the metal, in the shape of a cube, forming a little pedestal Q: on this is placed the valve *f*; from beneath which descends a straight duct into the barrel, without penetrating the box: the form of the parts of this valve (all made of brass, and of the size of the originals belonging to the pump) is represented in fig. 4. in a vertical section. A A is a square plate (to be taken off if it should require to be ground and polished anew in contact with the valve), which is fastened on the pedestal with cement; it has a hole in the middle, being the opening of the duct, in which is inserted the little elevated pipe *c*, to be occasionally taken out; whose use is to prevent the oil applied to the valve from being blown down into the duct by the air rushing into the barrel: the elevated part of the plate *a*, *a*, is circular, and has its upper surface made plane and polished, on which rests the valve D; which is so far hollowed within, that only its lower edge, being about $\frac{1}{30}$th of an inch thick (which is also well polished), may be in contact with the plate under it, and also that its cavity may rise above the little pipe: the valve

* By this contrivance of fixing the barrel of the pump horizontal, and its rack underneath the barrel, it is made so portable, that I have packed it (the gage-glass and receiver being taken off) in a box two feet long, eighteen inches wide, and seven in depth; and it should be remembered that the most operose parts of it here described, are the frame and machinery necessary to render a pump, with *so long a cylinder*, portable; a great part of which machinery, if it were not portable, would not be spared, but merely exchanged for the huge frame of those so constructed.—L.

 is

is almoſt cylindrical, tapering but a little from the baſe upward, and being laid on the plate, with a ſmall drop of oil interpoſed, the hollow cylindrical cap E, very little wider internally than the valve, is put over it, being faſtened down on the projecting circular part of the plate *a a*. To let the air paſs through this cap, three little holes, *e*, *e*, are made in its ſides, the centres of which are juſt as high as the lower edge of the valve D, when it is raiſed to touch the top of the cap; the holes being higher than the plate under the valve, that the oil may not run out by them. When this valve is found to admit air into the barrel, it is occaſioned either by the oil's being blown away, or ſome particle of duſt, &c. getting between it and the plate (which would produce the ſame effect in any valve), and is remedied by taking it off, wiping it clean, and applying another drop of oil: the greater the quantity of air is, which paſſes by the valve, the more is the oil diſſipated; and, conſequently, this chiefly happens in the beginning of the exhauſtion, when much air is drawn from the receiver, and thrown out of the barrel at each ſtroke; ſo that whenever great rarefaction is requiſite, I often found it convenient, towards the end of the operation, to apply a freſh drop of oil to the valve, keeping the piſton in the mean time elevated to the top of the barrel, whilſt the valve is taken off, until it be replaced; which is done in an inſtant: thus it will perform perfectly well if quite clean and free from duſt; but as the ſmalleſt particle of dirt, or mucus (which is apt to be produced from the oil's corroding the braſs, and becoming clammy), will render the valve not air-tight, I am, therefore, perſuaded, that no valve will ſo perfectly or certainly exclude air as a ſtop-cock; and this I take to be the chief reaſon why Mr. Smeaton's pump, as improved by Meſſrs Haas's and Hurter's contrivance, for raiſing the valve at the bottom of the barrel, does not, in practice, anſwer as well as any other: for if ſome air did not return into the receiver by that valve in pumps on his conſtruction, it would be hard to tell why it ſhould not perform as well as a pump of any other conſtruction.

To the bottom of the barrel is faſtened by four ſcrews, paſſed through its prominent rim, the circular plate R, forming in part the ſhell of the cock S, and which has the high ridge T on the back of it; all theſe being only one piece of metal, which is repreſented ſeparately in fig. 5. and was caſt in that form: the round plate is about $\frac{1}{4}$th of an inch thick; but the cavity of the cock intrudes ſo far on this, that when the key of the cock is put in, the thickneſs of the plate in the part directly over the key, where the ducts open into the barrel, is not more than $\frac{1}{8}$th of an inch: the length of the ſhell of the cock is three inches; that of the key $\frac{1}{8}$th of an inch leſs: the diameter of the key, at the thicker extremity is 1 $\frac{4}{10}$ths of an inch, and at the ſmaller end 1 $\frac{1}{4}$th of an inch. It is turned by the handle *u*, faſtened on its axis as near to the end of the ſhell as may be; viz. at the diſtance of $\frac{1}{4}$th of an inch. Care was taken that the metal of the cock ſhould be free from pores, by which, if air were admitted in the working, it would be very hard to diſcover it, as this might take place only in *certain poſitions* of the key; and the ointment might ſometimes prevent and ſometimes ſuffer it, ſo that it might elude trials; the cock was alſo ſo truly and ſmoothly ground, as to produce an intimate contact of the key and ſhell throughout (for air will penetrate where liquids would not); it was alſo made of a true conical ſhape, its ſides being perfectly ſtraight from end to end. The cock

muſt

must be lubricated, and made air-tight by ointment *; and as all oil or greasy ointment has an acid in it (of which, perhaps, it can never be divested without destroying its lubricity) which corrodes the brass, and stiffens the oil or ointment itself, by impregnating it with green rust or verdegris, I found it necessary, after the cock was finished, to have the key and the inside of the shell coated with tin.

The key of the cock terminates in a little knob *x*, fig. 2. against which the end of the thin spring lever *y* presses to keep the key constantly firm in its shell: this spring is made to urge onward the key, with any degree of force requisite, by the screw at *y*, screwed through the outer part of the little arch, or frame, which frame is itself fastened by screws on the prominent ring of the barrel, as appears at *z*, fig. 2. The lever has a little ridge, or elevated part, which rests in a notch in the frame; by which the lever is fixed in its proper place, the notch being the fulcrum: by withdrawing the screw the ridge may be raised out of the notch, and the lever pulled back from the knob, to let the key be taken out when it requires more ointment; which will be known when it has worked into the shell nearly to its limit; and this should be carefully learned, by marking how far it can be inserted into the shell without any ointment interposed. The form of the key, and its lever, is exhibited in fig. 6. in an horizontal section through the middle of them.

Through the shell of this cock are two round perforations for two ducts, through the cock, each one inch distant from the other, and the same from the end of the cock on either side: they are in a plane, or section, passing through the axis of the key, and also through the axis of the barrel; and both pass through the ridge of metal T at the back of the shell. The duct next the smaller end of the key (being a part of the duct communicating with the receiver) is in diameter $\frac{1}{4}$th, and the other duct is only $\frac{1}{6}$th of an inch: through the key, also, are ducts answering to the perforations in the shell; but the ducts in the key are not both in the same section of it, but one of them is at right angles to the other; so that when one of the ducts through the cock is open, the other will be always shut, and *vice versâ*.

There is a continuation of each of those ducts formed in the piece of metal V, which is joined (by cement interposed) to the ridge of the shell of the cock by the four screws *g*, *g*, fig. 1. by two openings (into the two ducts in the piece of metal) opposite to those in the ridge; and these ducts in the piece V have two other orifices at *a*, and *c*, where the two pipes *a b*, fig 1. and *c d*, fig. 2. communicate with the two canals in the piece of metal, being connected with them only by the help of cement, that the barrel with its cock may be occasionally disjoined from them: thus a separate communication is formed between each of the pipes, and the barrel.

The pipe *a b* is only $\frac{1}{12}$th of an inch in diameter, except at its orifices, which are widened;

* This I have made of one part of common rosin, one part oil, and one part and a half fresh suet: the oil and rosin is first melted, and when a little cooled the suet is added, that the heat requisite to melt the rosin may not burn the fibrous part of the suet, and destroy its firmness: more oil is to be added in cold, than in warm weather. The quality of the ointment for the cock is of the utmost consequence; for if ever the parts of the metal of the cock came into immediate contact, it will not be air-fast; oil will not render it so (nor consequently would a valve); but the ointment must be stiff and adhesive; yet not too stiff, as then it would be hard to turn the key, and it could not be distinguished whether its stiffness was owing to the ointment, or to the friction of the parts of the metal getting into immediate contact, which would be pernicious.—L.

once

one of them is connected at *b*, with a duct formed in the piece of metal *k*, attached to the box of leathers C at the top of the barrel, which duct turning in an angle, without penetrating the box, opens into the barrel: this pipe and duct are joined by a simple application of their orifices, which are made in the contiguous sides of both, and the joint made air-tight, by cement melted around with a blow-pipe; and the same might be effected by the pressure of a screw closing the surfaces with oiled leather interposed. By this pipe when the valve is shut, and the key of the cock so placed that the communication of this pipe with the bottom of the barrel is open, if the piston be depressed from the top, all the air which is *under* it will be forced to pass by this pipe into the barrel *above* the piston; and *vice versâ* if the piston be elevated from the bottom; so that according to the motion of the piston, the air under, or over it, within the barrel, is made to change its place, and to circulate from the space under or above the piston to the contrary; from which effect of this pipe, I call it the *circulating pipe*.

The other pipe, or duct, is represented at *c d*, fig. 2. which exhibits a back view of the pump: its extremity *c* enters the block of metal V to communicate with the duct therein, and so with that through the cock into the barrel: its end *c* has a wing soldered to it, by which it is firmly screwed to the piece V; this being necessary lest the cement by which alone the joint there is made air-fast, should be cracked by any shock moving the barrel; or it might, perhaps, be secured by the screw with only leather interposed: this duct is a very wide one, that the air may the sooner pass through it; it has a turn at right angles at *d*, and another at *e*, from whence it extends under the pillar B, and pump-plate C D, through the middle of which it rises at A; it has a branch turning at right angles from A under the pump-plate, the end of which terminates in a round orifice with an inside screw, that by means of a pipe furnished with a stop-cock; or otherwise another pump-plate and receiver, or several of them, may be connected with it, being laid on the same table which supports the pump: when such are not used, the orifice is closed with a stop-screw as at D. From this pipe rises a branch E, inserted into the top of the brass cap F of the glass vessel Y, and opening into the vessel by a duct made in the thick plate of the cap. G is a reduced barometer tube, to be filled in the most perfect manner with mercury, and immersed into a pool of mercury in the bottom of the glass vessel. The interval between the tube and the neck of the vessel is closed by cement, which, as the neck must be a good deal wider than the tube, is effected by a deep ring (I) fitted to the tube, having a round plate at the bottom of it; which being let down on the tube after it has been immersed in the cistern, is joined with cement to the top of the cap. The tube was easily immerged in the mercury within the vessel, by covering its open end, and sustaining the mercury in it by a little plate, or scale of iron (P), fig. 7. having two threads tied to it, through two holes made near its edges; which threads were grasped together with the top of the tube between the fingers, and the tube was inverted, and let down into the mercury in the vessel: the little plate was then pulled up by one of the threads, it being made so small as to pass through the neck of the glass while the tube was within it, and remained immersed in the mercury*: the

* I thought this the best way of fixing the reduced barometer, for if there were a separate vessel as a cistern for the mercury, and a receiver placed over it and the tube, the eye when on a level with the surface of the stagnant mercury, could not see the altitude of that in the tube with precision through two glasses.—L.

vessel

veſſel was then cloſed at the top with melted cement; and as it communicates by the pipe E with the conduit-pipe, it is exhauſted with the receiver, and the mercury in the tube will ſink as the rarefaction advances: if a perfect vacuum could be made in the receiver, and gage-veſſel, it would ſink more or leſs *beneath* the level of the ſurrounding mercury, as the tube is narrower or wider, even ſo as to diſappear if the tube were very narrow; ſo that it muſt not be leſs than $\frac{3}{10}$ths of an inch in diameter; but it would be better if it were $\frac{4}{10}$ths. If it were obſerved, how much the mercury in a tube open at both ends would ſink beneath the level of that in a ciſtern in the open air, it might be pretty nearly eſtimated how great is the rarefaction in this gage-veſſel, and conſequently in the receiver*.

In order that each of the ducts here deſcribed ſhould alternately communicate to the barrel, it is neceſſary, that the cock ſhould be moveable through a quarter of a turn, and confined to that motion. This is effected, by a pin, *n*, fixed perpendicularly in the back of the handle, fig. 1. which, when the handle is fixed in its place, deſcribes a quadrantal arc, under the ſhell of the cock, where there is juſt room to allow ſuch a motion.

The action of this pump may be readily apprehended from the conſtruction of its parts. When the piſton is at the bottom of the barrel, and the key of the cock turned to its limit which opens the communication, between the barrel and the receiver and at the ſame time ſhuts that with the circulating pipe; the piſton being then drawn to the top of the barrel diſcharges the air through the valve, while other air from the receiver ruſhes into the barrel, and follows the piſton. As ſoon as the piſton has arrived at the upper, or more remote end of the barrel, the key of the cock is again turned, and ſhuts off the connection with the receiver, and opens that with the circulating pipe. The piſton being then depreſſed, drives the air through the pipe, into the upper, or remote end of the barrel. When the piſton has arrived at its limit, the ſtroke is ended, the key of the cock is to be again turned to open the receiver, and a ſecond ſtroke may be made, with the ſame conſequences as the firſt; and in this manner the proceſs may be continued at pleaſure. The limit of exhauſtion will be when the air in the receiver ſhall have become as rare as the air in the barrel would be when the piſton is up; ſuppoſing no communication to have taken place between it and the receiver.

When the pump is required to condenſe, the air is made to move in a reverſe direction. The cock of the gage-veſſel is ſtopped, that it may not be burſt; the valve is taken off; the communication of the bottom of the barrel, to the circulating pipe, opened by the key of the cock, and the piſton raiſed to the top. The external air paſſes into the lower part of the barrel; and by turning the cock, ſo as to open the receiver, and ſhut the circulating pipe, and then depreſſing the piſton, the air is forced into the receiver, which ought of courſe to be of ſufficient ſtrength, and properly ſecured. By ſhutting the receiver, raiſing the piſton, opening the receiver again, and returning the piſton to its former ſituation, another ſtroke is made, and theſe alternations may be carried on to any deſired or practicable extent.

* The author prefers the ſhort barometer-gage to the ſyphon-gage, becauſe in this laſt he thinks the motion is impeded by the tube, and the mercury becomes foul on that ſide which communicates with the receiver.—N.

The

The remainder of Mr. Little's paper, consists of numerous observations and remarks, with some experiments; a considerable number of the former of which are familiar to such as have attended to this branch of natural philosophy: but the whole, though evidently short of what the author must have originally intended, will be read with interest and satisfaction, by those who may consult the memoir itself.

X.

Observations on Chemistry and Natural History. By PROFESSOR VANDELLI*.

1. *Fossil Prussian Blue from Minas Geraes in the Brazils.*

IN digging a mine at S. Ioao del Rey, to the depth of more than forty palms (about 27 feet English), large brittle bones, and some teeth of a cetaceous animal were found in the clay, which occupied a space of more than fifty palms square (about 35 feet English). These bones, and the clay with which they were intermixed, were white when first extracted, but soon became blue when exposed to the air †.

These bones and clay, in the blue state, being exposed to the action of a moderate fire, became green, emitted a weak flame, and afterwards lost their colour. The reddish earth which remained was, for the most part, attracted by the magnet.

The Prussian blue was soluble in the marine acid, and in this state lost its colour. By addition of a fixed alkali, the precipitate was afforded of a green colour, which afterwards became blue, of more intensity then at first.

The mixture of bones and clay, being fused with a proper flux for iron, afforded three parts of their weight of iron, which was intirely attracted by the magnet.

Respecting the fossil Prussian blue, as the reader may consult the works of Wallerius, Bergman and Kirwan, I shall not enter into any further detail.

2. *Native Copper of Brazil.* This mass of copper was found in a valley two leagues from Cachoeira, and fourteen from Bahia. It weighs nearly 2616 arratels (or nearly pounds English), and its figure is rhomboidal, the upper surface being irregular, on account of some cavities and protuberances. Its greatest height is three feet two inches, its width, at the base, two feet and a half, and its greatest thickness ten inches, Paris measure ‡.

The external colour of the mass is deep reddish, with spots and particles of a greenish blue, produced by the decomposition of the copper. On the lower surface appear some yellow spots of ochre of iron.

At various parts of the surface, particularly the lower surface, several pieces, large and small, are observed, which, at first, seem to be *ferrum micaceum*; but when examined by fire they prove to be indurated oxide of copper. For an ounce of this substance afforded thirteen parts out of sixteen pure copper.

* From the Transactions of the Royal Academy of Sciences at Lisbon, I. 259.

† For an explanation of this phenomenon, see Proust in this Journal, I. 455.

‡ The foot-royal of Paris is to that of London as 1,0000 to 0,9383.—N.

This

This copper being assayed, afforded no indications of gold or silver.

Another piece of virgin copper was found at the same place, but much smaller than the foregoing. A mass of copper, of the magnitude here described, has very rarely been met with, as Mr. Monnet remarks*.

XI.

On the Manufacture of Hats, and other Objects. By a Correspondent.

To Mr. NICHOLSON.

SIR, Newcastle, 9th January, 1799.

THE obliging manner in which you have noticed my enquiries, respecting the hatting business, induce me to send you others on another business, equally productive of mischief, to the morals and the health of a class of industrious, but depraved, fellow men—I mean the journeymen flax-dressers; those and the journeymen hat-makers, are almost proverbially vicious, and I confess I look with sanguine expectations of reform, to the period that will exhibit machinery for each.

Is it possible to construct a machine, for the purpose of dressing flax? There are machines, I understand, in Scotland, invented about 40 years ago (vide English Encyclopedia, article Flax Dressing), for the breaking and scutching of that article: might these not be extended to the further process of the heckles? Perhaps the claim is equally upon humanity as ingenuity, for I have been told (and I have, from my own knowledge in that business, scarce any doubt of its truth), that the journeymen are obliged to give it over, about the age of 40, from an approach of *consumption;* and one, whom I am told was lately opened at his death, *had his lungs covered with a thick crust, composed of the dirt received upon them from his business.*

Accept the following information, as the best I can yet afford you, on the subject of my queries in your last (p. 467); they have been obtained since my writing to you on the subject. Hats were invented at Paris, A.D. 1404, by what circumstance I am yet ignorant. First made in London, 1510. The above, though an anonymous communication to me, I believe will not be found far from the truth. There is a house in Derbyshire, name Cooper Bibby and Downal, at Lea Wood, near Cromford, in that county, who, I understand, use machines in the making of hats, but how far in the process I am not yet able to discover: perhaps some of your correspondents may have informed you, in consequence of my queries. The common account amongst the journeymen is, that *the property of wool to felt* was first discovered by a shepherd, who had wool in his shoes; his name was Clement, and they keep the 25th of November, as a day *sacred to dissipation and an old shepherd saint.* Saint Catherine, I understand, is the patroness of the journeymen flax-dressers. It is probable, *they* will celebrate the 25th of November.

* Nouveau Système de Mineralogie, page 314, Mines de Cuivre. The Cabinet of Mines at Freyberg possesses a specimen of this kind, weighing ten pounds, which is the finest and largest specimen of native copper hitherto known.

Theſe ſuggeſtions may, I hope, afford a thread to others, more converſant in the theory of Mahuzzim, or ſaints protectors, than I am, to favour you with information of more conſequence on the ſubjects of the manufactories in queſtion. The editors of the Engliſh Encyclopedia ſay, that, on the ſubject of dreſſing flax, they refer their readers to ſome obſervations in the Gentleman's Magazine, for June, 1787: I have not that book at hand, but I hope your readers may find in it ſome ground-work for future machinery.

I am, ſir,

Yours, &c.

N. L.

In the liquor for boiling of wool hats, after bowing and baſoning, one part of human urine is made uſe of, and two parts clean ſoft water: Could not ſomething be ſubſtituted in place of the urine, which might be *equally uſeful in hardening the hood*, and exclude ſo dirty an ingredient*?

XII.

Extract of a Letter from Citizen Ramond, Aſſociate of the National Inſtitute of France, and Profeſſor of Natural Hiſtory at Tarbes, to Citizen Haüy, Member of the Inſtitute at Paris, reſpecting two excurſions to Mount Perdu, the moſt elevated ſummit of the Pyrenean Mountains.*

Bareges, 5 complem. day, in the year V.

I FLATTER myſelf, Citizen, that you will not hear, without intereſt, ſuch events as have proved moſt remarkable in the reſults of my travels of the preſent year. I haſten to communicate them, with the hope that the portion of your time which I ſhall engage will be well repaid by the geological fact which is the object of this letter.

Mount Perdu is the moſt elevated mountain in the chain of the Pyreneans. In my former travels, I proceeded along the baſes. Reboul, who has ſucceeded in determining its height by obſervations made from various elevated points, had likewiſe approached it in a different direction. It is certain that the whole of the ſurrounding group is calcareous, and the aſpect, which can ſcarcely deceive thoſe who are habituated to behold and contemplate mountains, had determined my belief that the entire pic was of the ſame nature.

Abundance of calcareous matter forms one of the diſtinct characters of the Pyrenean mountains; but to behold this genus in poſſeſſion of the very creſt of the chain, the place which the granite occupies in every other known chain of mountains, was a phenomenon too ſingular not to inſpire me with the ſtrongeſt deſire to aſcertain its exiſtence.

The enterprize was not without its difficulties; and among them, that which was the leaſt

* The preparations for my change of reſidence, as mentioned on the wrapper, have induced me to defer my own obſervations and report, concerning hatting, till next month.—N.

† Read to the French National Inſtitute at the ſitting of the 21ſt Vendemiaire, in the 6th year of the Republic (12th October, 1797), and inſerted in the Journal de Mines, No. 37, of the ſame year.

foreſeen,

forefeen, was the abfolute ignorance in which I found all the country people, with regard to the real pofition of Mount Perdu. It is not vifible, but from the elevated fituations, and difappears as foon as you defcend. It was neceffary, therefore, to feek the road, as chance might direct, through the moft horrid deferts, which are never frequented, either by the hunter, or by fhepherds.

On this occafion, I had the good fortune to enjoy the company of our colleague, La Peyroufe; and I ufed every effort in my power to fecure his company in thofe regions, in which I forefaw fo many interefting obfervations would prefent themfelves. But his ftrength did not permit him to accompany me longer than the firft day, and part of the fecond. I left him at the foot of the firft glaciere, taking with me his fon, and one of his pupils, together with four of my own. I will not fatigue you with the detail of our own fufferings, nor alarm you with an account of our dangers; but fhall fimply obferve, that after having imprinted our fteps, during three hours, in the hardened fnows, which the inclination of their plane rendered very dangerous, we arrived at a fummit oppofite Mount Perdu, which raifed us to its middle part. I never beheld, even in the Alps, any object of greater magnificence: I do not except the approaches to Mount Blanc. The calcareous mountains poffefs a fimplicity of form and majefty which is peculiar to them: now Mount Perdu is calcareous, abfolutely calcareous, and of fecondary compofition. I defcended from my fituation towards a vaft lake, ftill frozen, which repofes at its feet. I croffed this lake; I vifited every fpot where the external fnows, and horrid glacieres, have fuffered the naked rocks to penetrate. Every where I found grit, breccia, and compact calcareous ftone, covered with the prints and remains of marine bodies: marine remains at the fummit of the Pyrenees, and on the pic which predominates over all thefe mountains!—This phenomenon acquires a character ftill more wonderful, when we confider that the entire chain exhibits none of thofe atteftations of the former refidence of the ocean; and that it is in vain we feek them in our fecondary calcareous ftones, our bituminous ftones, or flates, all which are deprived of the prints of fhells and vegetables. I paffed from the flanks of Mount Perdu into the valley of Pinede, where it pours its waters; I came to the port or paffage of Pinede, one of the moft elevated of the Pyrenees. The fame phenomena every where prefented themfelves; every where I faw grit, breccia, and common calcareous ftones, abounding with marine bodies; and by cafting my eyes along the extenfive valley of Pinede, I faw, in all parts, the continuation of the banks in which I had obferved thefe remains.

La Peyroufe had feen the oppofite fide of the mountains which I vifited: he coafted along them to meet me at the port of Pinede. He obferved part of thefe wonders, and partook of my harveft. Stormy clouds having covered the pic, at the time of my arrival at my firft excurfion, it became neceffary to revifit it at a more favourable feafon; I, therefore, refumed the route of Mount Perdu, fifteen days ago, with a couple of friends only, and fome hardy mountaineers. All the glacieres were then uncovered, by the diffolution of the fnow which had laid upon them; the lake was unfrozen, and the dangers of approach were confiderably increafed. It was neceffary to fecure our footfteps in the hardeft ice by heavy ftrokes of the hatchet. We fuffered much in this excurfion, and I could not reach the fummit; but the fky was admirably ferene, and I touched, as it were, thofe rocks which I could not

 afcend.

ascend. At this time, I beheld the whole structure of the mountains, the direction and inclination of the strata, the entire succession of its layers; every fact was manifested to my sight, and I completed my collection of marine bodies, which are contained in these mountains.

To the west, as well as to the east, every part is secondary, and full of shells. I sent one of my pupils towards Vignamale, who brought me a cornu ammonis. It is, as I have ventured to affirm in my printed observations, an enormous secondary mass, superposited on the edifice of the chain, and which covers the southern part to the thickness of ten or twelve kilometres, and is in length, forty.

The most perfect among the remains of marine bodies which I found in these regions is a perfect ammonite, the exact impression of a pectinite, the prints of asterites, many oysters in the solid, caryophillites, and a multitude of madrepores. I shall not enter into more circumstantial details. The geologic and geographic results; the nature and inclination of the different banks; their connection with the rocks which support them; the state of vegetation; the insects observed in these elevated regions;—will form materials for a memoir, which I intend to draw up, when in a less interrupted state, for the purpose of transmitting it to the National Institute. But I am desirous that the principal facts should be communicated to that body, with all the interest of novelty, and I think them sufficiently interesting to request your mediation in this respect.

If you think it proper likewise to transmit them to the Council of Mines, I request that you will permit me to give you the trouble of performing this office.

XIII.

An Abstract of a Memoir upon the Fossil Bones of Animals. By Citizen CUVIER.*

THE intention of the Author, in this memoir, was to collect as much as it was in his power all the fossil bones appertaining to each species of animal, whether of such as he himself had seen, or those of which he merely had a description in authors, to form or recompose the skeletons of these species, and to compare them with those which now exist on the surface of the globe, in order to determine their relation and differences. The following is a series of the species to which his attention has been directed.

1. The animal which afforded the bones and teeth, called the bones and horns of the mammouth, by the Russians, and inhabitants of Siberia. Similar fossile remains are also found in Europe. It is a species of elephant, resembling the elephant of Asia; but from which it differs in the alveolæ of its teeth, and its tusks being longer, the angle of its lower jaw

* Communicated to the Societé d'Histoire Naturelle at Paris. This abridgment is translated from the bulletin of the Societé Philomatique, No. 18, year VI.

being

being more obtuse, and the laminæ of which its grinders are composed being thinner. The true analogous living animal is not known, though it has been hitherto considered as the ordinary elephant.

2. The animal, of which the remains are found on the banks of the Ohio in North America, which the Americans and English have also named mammouth, though it differs much from the former. Remains of this animal are also found in Europe and in Asia. It must have been nearly the height of the elephant, but more bulky; its tusks are smaller; its grinders are armed with large cutting points, of which the section by wear presents double transversal lozenges. There are three molar teeth on each side, one of four, one having six, and one eight, points.

3. The animal of which the teeth tinged by copper afford the turquois stone, and of which there was a mine at Simore, in Languedoc. The remains of this same species is found in the department of Ain, in Peru, and elsewhere. It must have considerably resembled the former, but the points of its molar teeth are round, and when worn, their section presents, first, a circle, then a semi-oval, and afterwards, a figure of a trefoil, which has caused them to be confounded with the teeth of the rhinoceros: some of these teeth have twelve points, others six, others four.

4. The rhinoceros. The feet and fragments of the jaws of this animal are found in France, and elsewhere, in which the author has hitherto observed nothing which differs from the common rhinoceros; but, as he has not yet seen an entire bone, he cannot positively affirm that they are identical.

5. The species of rhinoceros, with an oblong craneum, which is found in Siberia, Germany, and other countries. The author has seen teeth, and parts of the jaw-bones, found in France, which appeared to him likewise to belong to this animal; the principal character of this species consists in the long closure of the nose: the living analogous animal is unknown.

6. A molar tooth with two transversal eminences, which is in the possession of Citizen Gillet; and of which the National Museum possesses a young tooth that resembles neither the teeth nor the germs of any animal yet known, whether living or fossile: the only tooth which this slightly resembles is the last molar tooth of the rhinoceros. This tooth, therefore, indicates the existence of a sixth fossile species, of which the living analogous animal is unknown.

7. The animal, twelve feet in length, and six in height, of which the skeleton was found under ground at Paraguay, and is preserved in the royal cabinet at Spain, at Madrid. The author proves by a detailed comparison of the bones, with those of all the known quadrupeds, that it is a proper and distinct species, more nearly approaching the sloth than any other genus, and that it may be called the giant sloth. Citizen Cuvier, in this place, communicates the interesting discovery he has made, that the sloth *(bradypus tridactybus,* Lin.) has naturally and constantly nine cervical vertebræ. It is the first known exception, established by Citizen Dauberton, that all quadrupeds have neither more nor less than seven cervical vertebræ.

8. The animal, of which the remains are found in the caverns near Gaylenreuth and Muggendorf, in the margraviate of Bayreuth, in Franconia. Various authors have considered

dered it as a white bear; but it differs from this animal, as well as from all the known bears, in the form of its head, which is particularly characterized by the projection of the front, by the absence of the small tooth, which all the known bears have behind each canine tooth, by the osseous channel of the humerus, in which the brachial artery passes; and by several other circumstances in the figure and proportion of the bones. This animal, however, resembles the bear more particularly than any other kind.

9. The carnivorous animal of which the bones are found in the plaister-stone of Montemartre: the form of its jaws, the number of its molar teeth, and the points with which they are armed, indicate that this species is referrible to the genus canis; but it does not completely resemble any species of this genus. The most striking distinctive mark is that the seventh molar tooth is the greatest in the animal of Montemartre, whereas the fifth is the largest in dogs, wolves, foxes, &c.

10. The animal of which the lower jaw was found near Verona, has been considered by Joseph Monti as a portion of the cranium of the sea-cow; a notion which all the geologists have adopted, though it be the contrary to the most simple notions of comparative anatomy. This jaw, according to Cuvier, has belonged to an animal resembling, though specifically different from, the mammouth, the animal of the Ohio, and that of Simore. Its most particular character consists in the curve which forms its symphysis.

11. The animal of the stag kind of which the bones and the antlers are found in Ireland, in England, at Maestricht, &c. It is sufficiently different from all the stags, and even the elk, to which it has been referred, by the enormous magnitude of its antlers, the flattening of their superior part, and the branches which spring from their base. Several figures of these are given in the Philosophical Transactions.

12. The genus of the ox or beeve alone affords several fossil species: the craniums of two were found in Siberia, which have been described by Pallas, who referred one of them to the ordinary buffalo; but he has since attributed them to a peculiar species, natives of Thibet, named arni. Citizen Cuvier proves, by osteologic comparison, that those craniums have not belonged to the buffalo. The other appeared to Pallas to have belonged to the buffalo of the Cape, or the musk ox of Canada. Citizen Cuvier shews that they cannot have belonged to the former, but not being in possession of the cranium of the arni, nor the musk ox, he makes no decision respecting their identity with the fossile craniums.

The author likewise describes two kinds of craniums which have been found in the turf pits of the department of La Somme, which greatly resemble our common ox, and that of L'Aurouchs, but are more than one fourth longer.

From this enquiry, the Citizen Cuvier concludes, 1. That it is not true to affirm that the animals of the south have formerly lived in the north, their species not being perfectly identical. 2. That in every country there have lived animals which do not at present exist, either on the same spot, or elsewhere in any known country. Hence he leaves to geologists, the task of making, in their systems, such changes or additions as they may think best suited to explain the facts which he has thus established.

XIV.

Extract of a Memoir of Prouſt, entitled Enquiries concerning Tin. By CITIZEN DARCET.*

THE author of this memoir obſerves, in the firſt place, that he conſiders his work as a ſupplement to that of Pelletier, on the different degrees of oxygenation which tin can acquire, when diſſolved in acids.

When tin is diſſolved without heat, in very weak nitric acid, it acquires a degree of oxygenation, different from that which it would obtain if diſſolved in a concentrated acid, or if heat had been employed. In the firſt caſe, the nitric acid is not decompoſed, and affords but little oxygen; but this is not the caſe with the water, which in fact, affords the quantity neceſſary for its oxygenation.

This ſolution, which is of a yellow colour, gradually precipitates, more eſpecially if it be heated, and the ſeparation of the tin is made without re-action, for there is no diſengagement of nitrous gas; and if it be ſaturated with cauſtic pot-aſh, there is a diſengagement of ammoniac, as was already obſerved by Bayen and by Pelletier. The oxyde of tin, as well that portion which remains ſuſpended in the fluid, as that which is precipitated, and re-diſſolved in muriatic acid, equally decompoſe the ſuperoxygenated muriate of mercury, and the mercurial oxydes, &c. being very different, in this reſpect, from the oxyde of tin, which is precipitated from the ſolution, in concentrated nitric acid, or the ſolution which has been made with heat. In this, the nitric acid is decompoſed, and the precipitate is inſoluble in the muriatic acid, and no longer acts upon corroſive ſublimate, or the oxydes of mercury. This difference ariſes from the circumſtances, that in the firſt caſe, the oxyde of tin, whether it remain in the liquor, or fall down, is oxyded to the minimum, as Prouſt affirms, and in the ſecond caſe, to the maximum. Hence it is, that in the firſt caſe, there is no diſengagement of elaſtic fluid, whereas in the ſecond, the diſengagement is conſiderable.

When an acid of the ſtrength of 25 or 30 degrees is uſed (ſp. gr. 1,23), the ſolution is ſo violent, that there does not remain an atom of tin in the fluid, but the precipitate is oxyded to 40 parts of oxygen; but when the very weak nitric acid is uſed, or the ſolution is made without heat, the metal is oxyded only to 30.

It is aſcertained by hepatic water, that all the tin is ſeparated from the acid; for this re-agent, will totally precipitate tin, copper, or lead, if the fluid contain any. Prouſt has obſerved, that this re-agent may prove eminently uſeful. For example, ſuppoſe a ſolution to contain lead, copper, zinc and iron, the hepatic liquor will ſeparate them all in ſucceſſion; firſt the copper, then the lead, next the zinc, and laſt of all the iron, with this remarkable event, that the iron which was oxyded to the maximum in the ſolution, is thrown down at the minimum, and is precipitated green, by ammoniac and by lime. This happens, becauſe the hepatic water ſeizes a portion of the oxygen from the oxyde, and becomes decompoſed, leaving the iron at the leſs degree of oxygenation.

It is known, that very fetid hydrogen is extricated, during the ſolution of tin in the muri-

* Annales de Chimie, XXVIII. 213.

atic acid, particularly when the tin contains arſenic, which may be very well aſcertained, by burning the gas under a glaſs veſſel, when the arſenic is depoſited on the ſides. Prouſt alſo obſerves, that this gas is very hurtful to the brightneſs of the purple of powder of caſſius. He therefore very properly adviſes, that the ſolution ſhould be heated before it is uſed: an obſervation of the greateſt importance for porcelain and enamel painters.

If a few drops of the muriate of tin, be added to the muriatic acid of commerce, the yellow colour of the latter inſtantly diſappears. The tin deprives the iron of the exceſs of oxygen, which rendered it red, and ammoniac then precipitates it of a green colour. The ſame thing happens with the ſolutions of the ſulphates, nitrates, and red muriates of iron.

But, if a ſtronger doſe of muriate of tin be poured into the muriatic acid of commerce, a grey powder falls down, which Prouſt has aſcertained to be mercury.

When a ſolution of the muriate of tin is diſtilled, the whole of the liquid comes over, with a portion of the muriate. Towards the end, the matter ſwells up, and afterwards ſettles in the ſtate of calm fuſion, of a green colour; and if the fire be raiſed, puffs of fuming vapour are driven off, but nothing comes over in the fluid form. If the muriate of tin contains a ſmall portion of ſulphuric acid, that acid is decompoſed, and the ſulphur partly unites with the tin, forming a ſulphuret, in which the tin is oxyded to the minimum. This muriate requires a ſtronger heat for its diſtillation, than the ſmoking muriate in the ordinary proceſs.

But that ſubſtance which riſes, and is condenſed in the neck of the retorts, proves, after waſhing, and ſeparating the fuming muriate, to be nothing but tin half oxyded, to which acid only need be added, in order to diſſolve it again. This diſtilled muriate effectually decompoſes the ſublimate of mercury, and all the mercurial preparations, or oxydes; while the fuming muriate, being oxyded to the maximum, decompoſes nothing.

The muriate of tin affords, with alcalis, a precipitate, which cauſtic pot-aſh abundantly diſſolves. This ſolution reſerved, in a cloſe veſſel, at the end of 12 or 15 days, affords a metallic group, in the form of cauliflowers, which conſiſts of tin, nearly pure. In this proceſs, one part of the tin, oxyded to the mininum, robs the other, and becomes ſaturated; and this muriate of tin, ſo ſaturated, is no longer capable of altering corroſive ſublimate.

The ſame thing happens, if a mixture of the carbonate of copper, and the oxyde of tin, ſeparated from the muriate by pot-aſh, be kept under water. The oxyde of tin robs all the copper of its oxygen, and the portion of carbonic acid; whence the copper is found reduced, in cryſtalized plates, among the oxyde of tin.

Laſtly, the muriate of tin diſoxygenates indigo, and changes it to green. This experiment is analogous to other diſoxygenations of the ſame fecula, known to dyers. Prouſt mentions this circumſtance as an object deſerving of great attention, upon which he means to make further reſearches.

The ſulphates, nitrates, muriates, acetates, and carbonates, of copper, and alſo, the red, blue, or green oxydes, mixed with the muriate of tin, are generally converted into a white oxyde, which is collected at the bottom of the veſſel; and if this muriate be paſſed again upon a new oxyde, it becomes itſelf green, and is ſaturated to ſuch a degree as no longer to alter the colour of oxydes. The powder, thus robbed by the muriate of tin, aſſumes different ſhades of colours, violet, blue, black, &c. which are more particularly governed by the

degree

degree of expofure to light. It is fufible by heat, and affumes the appearance of a muriate of filver, in which ftate Prouft propofes to keep it.

The fulphuric acid has no action on this fubftance, but the muriatic acid immediately diffolves it, and it cryftallizes in tetrahedrons. The nitric acid alfo diffolves it, during which it paffes through various fhades of colour; but at laft it retains the appearance of a folution of nitrate of copper, in which the prefence of muriatic acid may be eafily detected. But the difengagement of nitrous gas which takes place during the folution, proves that it is not faturated with oxygen.

Prouft, obferving that this muriate was without colour, and that it was alfo foluble in ammoniac without colouring it, thought at firft that he might conclude, contrary to the principle eftablifhed by Lavoifier, that the copper was abfolutely clear of oxygen; but the analyfis which he made foon undeceived him.

He diffolved 100 grains of this white fufed muriate in very pure nitric acid, which he afterwards precipitated by the nitrate of filver, and obtained 142 grains of muriate of filver, which reprefent $24\frac{3}{4}$ grains of marine acid. On the other hand, 100 grains of very white muriate of filver treated with nitric acid, and carbonate of pot-afh, afforded him 113 grains of carbonate of copper, which, according to experiment, anfwer to $62\frac{7}{9}$, or 63 of copper, fo that from his analyfis he found thefe products:—marine acid $24\frac{3}{4}$; oxyde of tin 1; copper 63; and the $11\frac{1}{3}$ deficiency remain for oxygen.

Whereas he was fatisfied, that in the green muriate, in the fulphate, in the nitrate, and in the acitite of copper, the metal which is at the maximum of oxygenation, contains about 26 of oxygen in the centenary.

Laftly, it is proved that while the copper is oxyded no farther than 17 or 18, its folutions are white, and it cannot then give colour either to its muriate, or to ammoniac.

The following, as Prouft informs us, is another proof of the prefence of oxygen in this white powder. If a fmall quantity be put into water with iron filings, the copper foon feparates, and the iron becomes oxyded to the minimum, fo that when diffolved the alcalies precipitate it of a green colour.

If it be demanded whence the oxygen comes in the white folution of copper in the muriatic acid, Prouft replies, that it is from the water. The water is decompofed, as Berthollet had before fhewn. The oxygen combines, and oxydes the copper. Prouft even relates an experiment in which the hydrogene of the water was rendered perceptible to the fmell, by the affiftance of a fmall quantity of fulphur which was formed, and was diffolved and volatilized with the hydrogene. This experiment confifts in boiling the blue fulphate of copper in muriatic acid. A folution is obtained, which is precipitated by water, during which the hydrogene becomes perceptible to the fmell.

When a green muriate is diftilled, it may eafily be brought to the point of cryftallization; but if the diftillation be carried further, it is decompofed, and lofes the portion of oxygen, which makes the difference between the oxyde containing 25 parts, and that containing only 18; but the diftillation then affords oxygenated muriatic gas. At length this gas ceafes, and there remains at the bottom of the retort a grey, well-fufed mafs, which is the white muriate of copper. This is an eafy method of procuring the white muriate. Prouft adds, that the copper in the green ſand of Peru, and in the native muriate of Chili, are in this ſtate.

It is of importance to remark, that the chemiſt might be induced to believe, that when a metallic ſubſtance is diſoxygenated in any manner whatever, whether by the application of ſtrong heat, or by the affinities of other metals for oxygen; or, in the manner of Prouſt, by hepatic gas, as happens in the diſoxygenation of the tunſtic and molybdic acids; the chemiſt might think, that theſe ſubſtances, at their tranſition to the violet blue, or black colour, were reduced to the metallic ſtate: but the effect is, as he remarks, merely an incomplete diſoxygenation. The metal is merely depreſſed to the minimum, as happens with iron and copper, when by various means they are brought from their entire ſtate of oxygenation, to that in which the colour becomes blue, black, or red, more or leſs deep. There are no metals but mercury, gold, and arſenic, in which a complete diſoxygenation is obſerved.

But to return to tin: Prouſt has found, with Bergman, that tin oxyded to the minimum acquired no more than an addition of 30 parts in the hundred, and even in this ſtate is not exempt from marine acid. If it be ignited in a crucible it loſes weight, and emits the vapours of muriate of tin; and when the tin is oxyded to the maximum it is charged with 40 per cent. But it is eaſy to reduce it 30, in which ſtate it is blueiſh, and inſoluble in acids. The author here terminates his memoir, by remarking, that although Pelletier has not mentioned the white muriate of copper, he is not leſs perſuaded that it was known to that chemiſt; and this more particularly, becauſe he has ſpoken ſlightly of the diſoxydation of that metal by tin, as if he meant to ſpeak more fully on another occaſion. But while he renders this juſtice to Pelletier, he affirms with the open freedom of truth, that though his reſults coincide with thoſe of that chemiſt, they were not undertaken ſubſequent to his experiments.

There are a great number of other facts and obſervations in the Memoir of Prouſt which deſerve to be related; but the abundance and denſity of theſe facts are ſuch, that the attempt to communicate them would convert this abridgement into a memoir no leſs ample than the original.

XV.

*Obſervations on the Differences which exiſt between the Acetous and Acetic Acids. By J. A. Chaptal.**

THE acid of ſour wine preſents to our obſervation two very diſtinct ſtates, which are known by the names of the acetous and the acetic acids.

Chemiſts have hitherto referred this difference to the varying proportion between the oxygen and the radical, and it has generally been believed, that the acetic acid differs from the acetous ſimply in a ſtronger doſe of the acidifying principle†.

Citizen Adet has reſumed this intereſting reſearch, and has preſented to the ſociety a courſe of experiments, from which he concludes:

* Annales de Chimie, XXVIII. 113.

† We may except Citizen Perès, who, in the Journal des Pharmaciens, has announced, that the difference between the acetous and acetic acids conſiſts in the proportion of carbone.—C.

1. That

1. That no fuch thing as the acetous acid exifts.

2. That the acid of vinegar is always at the higheft poffible degree of oxygenation; and, confequently, is always acetic.

3. That the difference between the acid obtained by the diftillation of vinegar, and that afforded by the like procefs from the acetate of copper, depends on the lefs quantity of water contained in the latter.

I muft confefs, that notwithftanding the high degree of confidence I place in the labours of Citizen Adet, it has been impoffible for me to adopt his conclufions in this refpect; and as I was very far from calling his experiments in queftion, I have thought it proper to analyfe, compare, and difcufs, his refults, much lefs with the view of afcertaining their accuracy, than to determine whether they juftify the confequences deduced from them by their author.

I conclude, therefore, immediately with Citizen Adet, that it appears to be demonftrated by his experiments, that the oxygen exifts nearly in equal proportions in the diftilled acetous acid and the acetic acid; that it is equally difficult, and even impoffible, to oxygenate the acetous acid, and to difoyxgenate the acetic; and that moft of the falt feparately formed by thefe two acids are of the fame nature. Thefe confequences, no lefs new than interefting, belong to Citizen Adet, and naturally flow from his experiments.

But to conclude, from the fame facts, that the acetous and the acetic acids are exactly the fame, and differ only in their proportions of water, is more than the experiments will juftify; and while we muft agree with Citizen Adet as to the truth of his firft confequences, it may alfo be fhewn, that there is a very great difference between the acetous diftilled acid, and the acetic. I fhall endeavour to afcertain the caufe of this difference.

The two acids in queftion differ, no doubt, in fmell, in tafte, and folvent power; but as the caufe of thefe differences might be referred to their refpective degrees of concentration, I have thought it proper to bring both to the fame fpecific gravity, by diluting the acetic acid with a fufficient quantity of diftilled water; and it was with thefe two acids, at the fame degree, that I made the following obfervations and experiments:

1. The fmell and tafte fhew the difference between thefe two acids. The effects, in this refpect, are more evident, and the fenfation is much more penetrating on the part of the acetic than the acetous acid.

2. The action of the acetic acid on the lips, and upon the metallic oxydes, is more fpeedy and energetic than that of the acetous acid. Thefe two acids, fet to digeft in equal portions upon the oxyde of copper, precipitated from the fulphate by pot-afh, wafhed, and then diftilled, prefented very different refults. The acetic acid diffolved the oxyde, and formed beautiful cryftals by cooling; but the acetous was fimply coloured of a blueifh green, and let fall nothing but a green faline cruft, on the borders of the fluid.

Neither of thefe acids perceptibly attacked copper. They merely acquired, by long digeftion, a light green fhade of colour.

Eleven parts of the acetic acid required for their faturation 6,98 of pure pot-afh; the fame quantity of acetous acid required only 5,73.

There, confequently, exifts a difference between thefe oxydes. The following experiments will fhew in what this difference confifts, and render us acquainted with its caufe:

 I. If

I. If ſulphuric acid be poured on the concentrated acetic acid, and then diſtilled, the firſt conſequence is the production of conſiderable heat, the colour of the mixture becomes red, and increaſes in intenſity, until at length it becomes black. A great quantity of carbone is precipitated by the continued action of the heat, at the ſame time that much ſulphureous gas is diſengaged.

II. I put ſeparately into two glaſs retorts equal parts of the acetic and acetous acids, at the ſame degree of concentration; I poured upon each one-fourth of its weight of ſulphuric acid. This mixture produced a very ſtrong heat.

The colour of the mixture of ſulphuric acid with the acetic acid at firſt appeared of a pale yellow, while that of the acetous acid did not change colour.

The mixture of acetous acid arrived at the point of ebullition ſooner than the other. It became yellow after the evaporation of one-third of the fluid, and its colour grew deeper and deeper, until it had acquired the tinge of highly-coloured wine.

The mixture of the acetic acid was not coloured in the ſame proportion, and it never acquired a deeper tinge than that of ſtraw-coloured white wine.

The diſtillation being long kept up, and urged by a ſtrong fire, cauſed copious white vapours of ſulphureous acid to paſs over; and towards the end the two reſidues became colourleſs, and contained nothing but the concentrated ſulphuric acid.

The two firſt third parts of the product of the diſtillation had nearly the ſame ſmell and taſte, and the acetous acid appeared to me to have been brought to the ſtate of acetic acid by its decarbonization in the retort.

III. I ſaturated, with pure pot-aſh, 100 pots of each of theſe two acids, and put the ſolution to evaporate, and obtained, from both, white foliated deliqueſcent ſalts. I put equal parts of each of theſe ſalts into two retorts, and expoſed them to an equal heat, which was gradually increaſed till it became violent. There paſſed at firſt into the receiver of the apparatus in which the acetate was placed, two or three drops of offenſive ſmelling acrid water, which indicated no acidity to the taſte. The diſtillation of the acetite produced nothing but certain vapours, which emitted a ſimilar odour.

The ſalt was firſt liquified, and then became black, in both the retorts. In proportion as I increaſed the heat, the retorts and receivers became lined with a white fume, which afterwards diſappeared.

In both retorts were left black reſidues, upon which I poured boiling diſtilled water. I waſhed them ſeveral times, in order to deprive them of all ſoluble matter.

Theſe reſidues being dried, preſented all the characters of carbone. Their weight compared with that in ſalt made uſe of, afforded the following reſults:

The acetate afforded one-ſeventeenth part of its weight in carbone.

The acetite afforded one-thirteenth part.

There is, conſequently, a difference between the acetous and the acetic acids; and this difference ariſes from the greater proportion of carbone in the acetous beyond that in the acetic acid.

It appears to me, that the phenomena preſented by the diſtillation of the acetite of copper, ought to lead us to the ſame conſequence. In fact, the acetite of copper is merely a ſolution of of the oxyde of copper in the acetous acid, and when this falt is diftilled, the acetous acid is decarbonated. Part of the carbone combines with the oxygen of the oxyde of copper, and efcapes in the form of carbonic acid; while the other part remains in its folid form in the retort along with the oxyde itfelf. The acetous acid, thus deprived of a portion of its carbone, paffes into the receiver with characters which no longer appertain to the acetous acid, and the oxyde of copper is nearly reduced to the metallic ftate.

The acetous does not therefore become acetic acid, but by a fubtraction of carbone. It appears that the metallic oxydes alone, and fome of the acids, are capable of effecting this decompofition.

The acid appears, therefore, to exift in the ftate of acetous acid in the falt of copper, improperly called acetite. It does not become acetic acid but by diftillation, becaufe it is by this procefs only that it lofes a portion of its carbone.

Independent of the proofs we have exhibited in fupport of this truth, we may adduce the two following facts:

1. When verdigreafe is manufactured with the diftilled acetous acid of copper, a pure falt is obtained, which affords by diftillation the fame products as the cryftals of Venus, or the acetate of copper. I have mentioned this fact in a memoir concerning the compound proceffes of the fabrication of verdigreafe. 2. Every one knows that vinegar is fo much the ftronger, and more completely approaches in its fmell to the acetic acid, in proportion as it is more completely deprived of the extractive matter with which it was combined. We may, in this cafe, confider the acid of vinegar as originally exifting in a ftate nearly faporaceous, which diminifhes its action, and weakens its properties. It is difengaged from this addition, 1. by fimple repofe, which fuffers part of the extractive matter to precipitate, or fall down; 2. by the fulphuric acid, which decompofes and more completely carbonizes the extractive matter; 3. by the oxyde of copper, which retains a portion of the fame principle with which it appears to form a pyrophoric combination.

I was defirous of afcertaining whether the acetic acid could be brought back to the ftate of acetous acid, by caufing it to refume, in the courfe of diftillation upon carbone, that portion which it had loft: but all the methods I made ufe of to fucceed in this refpect were ineffectual.

From the preceding facts I conclude: 1. That there is a difference between the acetous and the acetic acids. 2. That this difference arifes from a fmaller proportion of carbone in the acetic acids, than in the acetous. 3. That the acidis in the acetous ftate in metallic falts. 4. That it does not pafs to the acetic ftate but by decarbonization. 5. That the difference between this acid and fome others, equally fufceptible of modifications by a change in the preparation of their conftituent parts, is, that in this the oxygen does not appear fufceptible of addition, or fubtraction; but its carbone alone undergoes which variation, and determines all its changes; whereas in the other acids the oxygen is the principle which is more particularly fubject to variation, and occafions the changes obfervable in their properties.

SCIENTIFIC

SCIENTIFIC NEWS, AND ACCOUNT OF BOOKS.

Lantern Pinions of Glaſs for Mill-work.

CITIZEN MOLARD has communicated to the French Inſtitute an acoount of the advantages of making the trundles in lantern pinions of glaſs; to work againſt the hard wooden teeth of wheels in mill-work. The trundles are ſet in the ſame manner as thoſe of iron, only taking the neceſſary care which the nature of the material muſt require. A pinion of this kind, in which the trundles were two inches in diameter, did not undergo the leaſt wear, or alteration of the ſurface of the glaſs, in 18 months' uſe; and the wooden teeth had loſt about one-twelfth part of an inch. Iron trundles wear out the teeth in about four months, and load the work with much more friction. The kinds of glaſs which were tried, were green window-glaſs, white ſheet-glaſs, and the white glaſs for goblets *(gobletrie)*. This laſt was the hardeſt and beſt. The inventor, Cit. Renaut, of the department of La Meurthe, thinks bottle-glaſs might be of good ſervice. He does not blow the trundles, but makes them ſolid. It is probable, that with us the glaſs of wine bottles might be the cheapeſt, and moſt valuable for its hardneſs and tenacity, if it can be eaſily wrought to the figure by the workmen who are employed upon it.—*Decade Philoſ. No.* 5, *Year* VII.

Inſtitute of the Ligurian Republic.

THE lately eſtabliſhed Inſtitute of the Ligurian Republic is compoſed of 72 members, of which 36 are reſident, and 36 aſſociates, inhabitants of the republic, divided into two claſſes, and each of theſe into three ſections.

The firſt is the claſs of mathematical and phyſical ſciences, of which the three ſections are: 1. Agriculture, commerce, and manufactures. 2. Nautical ſcience, mathematics, natural philoſophy, and natural hiſtory. 3. Chemiſtry, botany, anatomy, medicine, and ſurgery.

The ſecond claſs includes (moral) philoſophy, literature, and the fine arts; of which the ſections are: 1. The art of reaſoning, and analyſis of the operations of the underſtanding, grammar, eloquence, and poetry. 2. Politics, hiſtory, and antiquities. 3. Arts of deſign.

On the 14th Brumaire (Nov. 4) the Executive Directory of the Republic repaired to the Hall of Seſſion of the Inſtitute, and the eſtabliſhment was opened by the formality of proclaiming the members, &c. after which the Inſtitute proceeded to chooſe their preſident and ſecretaries, and to organize their future labours.

*Inſtitute of Cairo.**

THE learned men who accompanied Buonaparte in his expedition have formed a national Inſtitute at Cairo, compoſed of four claſſes, each conſiſting of twelve members. They hold their meetings on the firſt and ſixth day of each decade.

The names of the members of each claſs are as under:

Claſs I. Mathematics. Andréoſſy, Buonaparte, Coſtaz, Fourier, Gerard, Lepére, Leroi, Malus, Monge, Nouette, Queſnot, Say.

* From the Magazin Encyclopedique, IV. 552. Nivoſe VII.

Claſs

Clafs II. Natural Philofophy. Beauchamp, Berthollet, Champi, Conté, Delifle, Defcotils, Defgenettes, Dolomieu, Dubois, Geoffroy, Savigny. (There is one place vacant.)

Clafs III. Political Economy. Caffarelli-Dufalga, Gloutier, Pouffielgue, Sulkowfky, Sucy, Tallien. (Six places vacant.)

Clafs IV. Literature and the Arts. Denon, Dutertre, Nery, Parceval, Redouté, Riegel, Venturi, Raphael, a Greek prieft. (Four places vacant.)

On the 6th Fructidor, in the year VI. (Aug. 23, 1798), at feven in the morning, the Inftitute of Egypt held its firft fitting, at which Buonaparte prefided. The officers were elected: Monge, prefident; Buonaparte, vice-prefident; and Fourier, perpetual fecretary; in whofe place, on account of his abfence at Rofetta, Coftaz was provifionally appointed.

In this firft fitting Buonaparte made the following propofitions:

1. *What are the beft methods of improving the conftruction of ovens to bake bread for the army?* The commiffaries named to examine this queftion were, Berthollet, Caffarelli, Say, and Monge.

2. *What product can be fubftituted inftead of hops in making beer?* Commiffaries, Berthollet, Malus, Coftaz, Gloutier, and Defgenettes.

3. *How may the waters of the Nile be clarified, and rendered pure?* Commiffaries, Monge, Berthollet, Coftaz, and Venturi.

4. *Are wind, or water mills, to be preferred? (in the prefent circumftances).* Commiffaries, Caffarelli, Malus, Say, and Coftaz.

5. *What are the refources for procuring gunpowder?* Commiffaries, Andréoffy, Malus, and Venturi.

6. *What is the ftate of legiflation in Egypt, and how can it be meliorated?* Commiffaries, Say, Sulkowfky, Tallien, and Coftaz.

7. *A project of regulation (of the Inftitute?)* Commiffaries, Monge, Caffarelli, Tallien, Geoffroy, Coftaz.

(To be continued).

Cafes of the Diabetes Mellitus, with the Refults of the Trials of certain Acids, and other Subftances, in the Cure of the Lues Venerea. By JOHN ROLLO, Surgeon-General, Royal Artillery. Second Edition, with large Additions.

THE fecond edition of this treatife was announced at p. 427 of the prefent volume, but time did not then permit me to give an account of the additions and facts now firft publifhed.

The additional cafes and communications on this diabetes, appear to eftablifh the author's opinion refpecting the efficacy of diet confifting entirely of animal food, when rigoroufly perfevered in. The diffections likewife prove, that the fweet urine does not proceed from any organic affection of the kidneys, but moft probably from fome vitiated ftate of the ftomach, and organs of digeftion.

The additional cafes are, two from Dr. Gerard of Liverpool; a continuation of Walker's cafe, by Dr. De la Rive; four from Dr. Cleghorn of Glafgow, with a continuation of his former cafes; fome remarks and cafes by Dr. Storer of Nottingham, with a continuation of Dr. Aldrich's cafe; one from Dr. Jamefon, furgeon of the Royal Artillery; one from Mr. Sherriff

Sherriff of Deptford; one from Mr. Houſten of London; three from Dr. Pearſon of St. George's Hoſpital, with remarks; one from Dr. Marſhall, with the diſſection; one from Dr. Willan; and one from Mr. Thomas of Leiceſter-ſquare, with the diſſection.

Dr. Rollo informs us, that, ſince the firſt diſperſion of his notes on Capt. Meredith's caſe, in Jan. 1797, the number of diabetic patients, which he has either ſeen or heard of, amounts in all to 48; a circumſtance which proves, that this complaint has been frequently overlooked, and is by no means ſo rare as has been generally ſuppoſed. At the end of the firſt part are ſome experiments and obſervations on urine and ſugar, by Mr. Cruickſhank.

In the ſecond part, we find a continuation of the trials of the nitrous acid, and other oxygenating remedies, in the lues venerea; which appear to eſtabliſh their efficacy in this diſeaſe, both in its primary, and ſecondary ſtages. The number of caſes cured in this way, ſince March 1797, amount to 155; of theſe, 59 were cured by the nitrous acid; 59 by the oxygenated muriate of pot-aſh; 7 by the oxygenated muriatic acid, and muriate of manganeſe; 3 by lemon juice; 11 by the nitrous and other acids, combined with the oxygenated muriate of pot-aſh; and 16 by a combination of mercury, with the new remedies.

In a few inſtances, where theſe medicines had not been continued for a ſufficient length of time, relapſes occurred, and the ſecondary ſymptoms made their appearance; but all theſe were afterwards completely cured, by perſevering in the ſame mode of treatment.

At the concluſion of this part are ſome uſeful and neceſſary obſervations, with regard to the management of theſe remedies, more eſpecially the oxymuriate of pot-aſh: but for particulars, the reader muſt refer to the work itſelf; of the character and importance of which, the public is ſufficiently aware.

Moyens d'apprendre à compter ſurement et avec facilité, ouvrage poſthume de Condorcet; or, Methods of learning Accounts with certainty and eaſe. One volume, 8vo. 132 pages. Sold by Maulardier at Paris, year VII.

The diſtinguiſhing character of theſe elements, ſays the editor, are, that they exhibit the elements of arithmetic and logic at the ſame time. The author uſes the decimal ſyſtem. Mag. Encycl.

Mr. Lowry, engraver, of Titchfield-ſtreet, the artiſt alluded to at page 429 of the preſent volume, has ſhewn me his machine for ruling, which is very different from mine, in all its parts. As he has no actual diviſion in the part which produces the ſhift, he can regulate his diſtances to incommenſurate, as well as commenſurate, meaſures. The parallax of the ruling point, againſt which I had made no proviſion, is, by a very ſimple and happy contrivance, taken away in common ruling, or rendered variable at pleaſure, for the purpoſe of thickening the ſtroke in ſhading. This, together with his great profeſſional ſkill and ſcience, has given Mr. Lowry's engravings the degree of preciſion and effect for which they are ſo juſtly admired.

In July 1786, I communicated to the celebrated Mr. Troughton, of Fleet-ſtreet, a ſmall apparatus, for ſecuring the point from lateral deviation, in dividing mathematical inſtruments, which I did not think neceſſary to add to the inſtrument in plate 20.—The execution of that plate, in which the point was left to the uſual management of the artiſt, ſhews, however, that ſuch a contrivance is neceſſary, and I ſhall take an opportunity of deſcribing it hereafter.

Air Pump by the Rev. James Little.

Fig. 5.

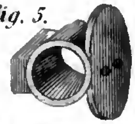


Fig. 3.

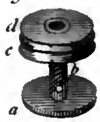


Fig. 4.

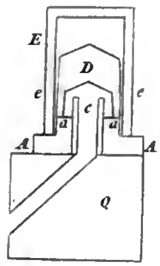


Fig. 1.

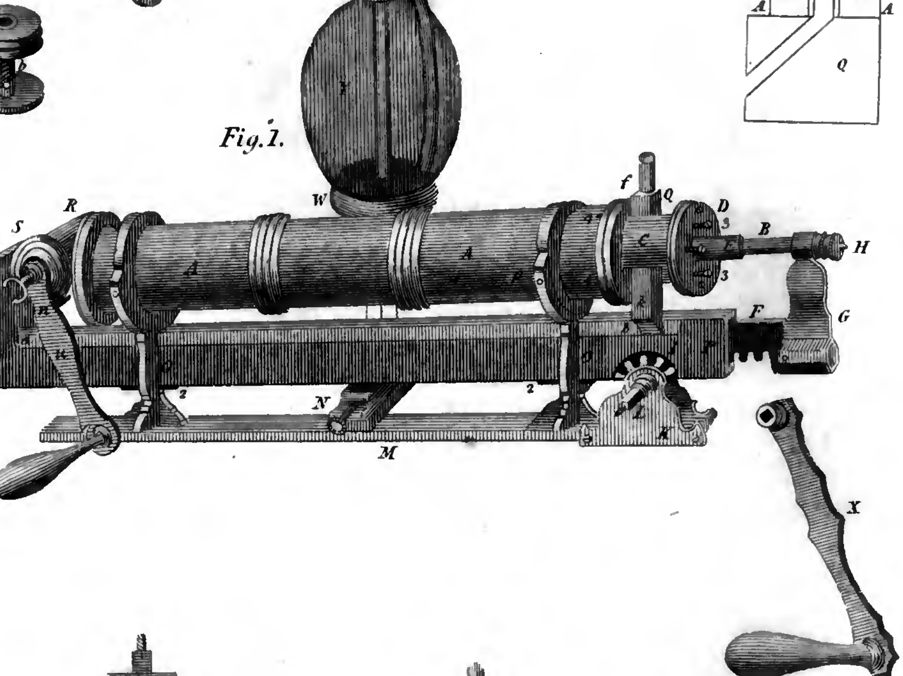


Fig. 6.

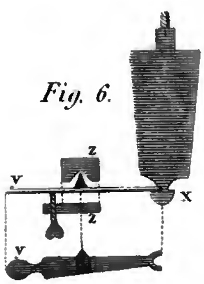


Fig. 7.

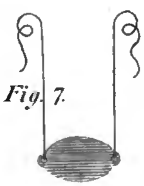
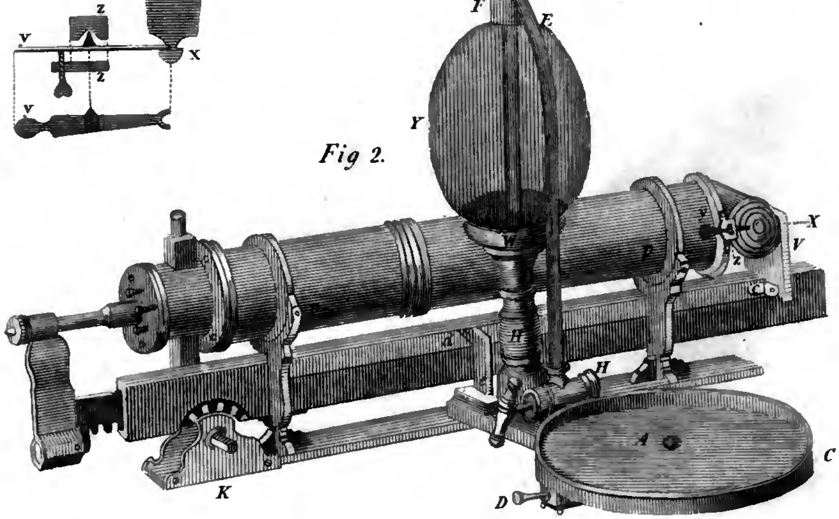


Fig 2.



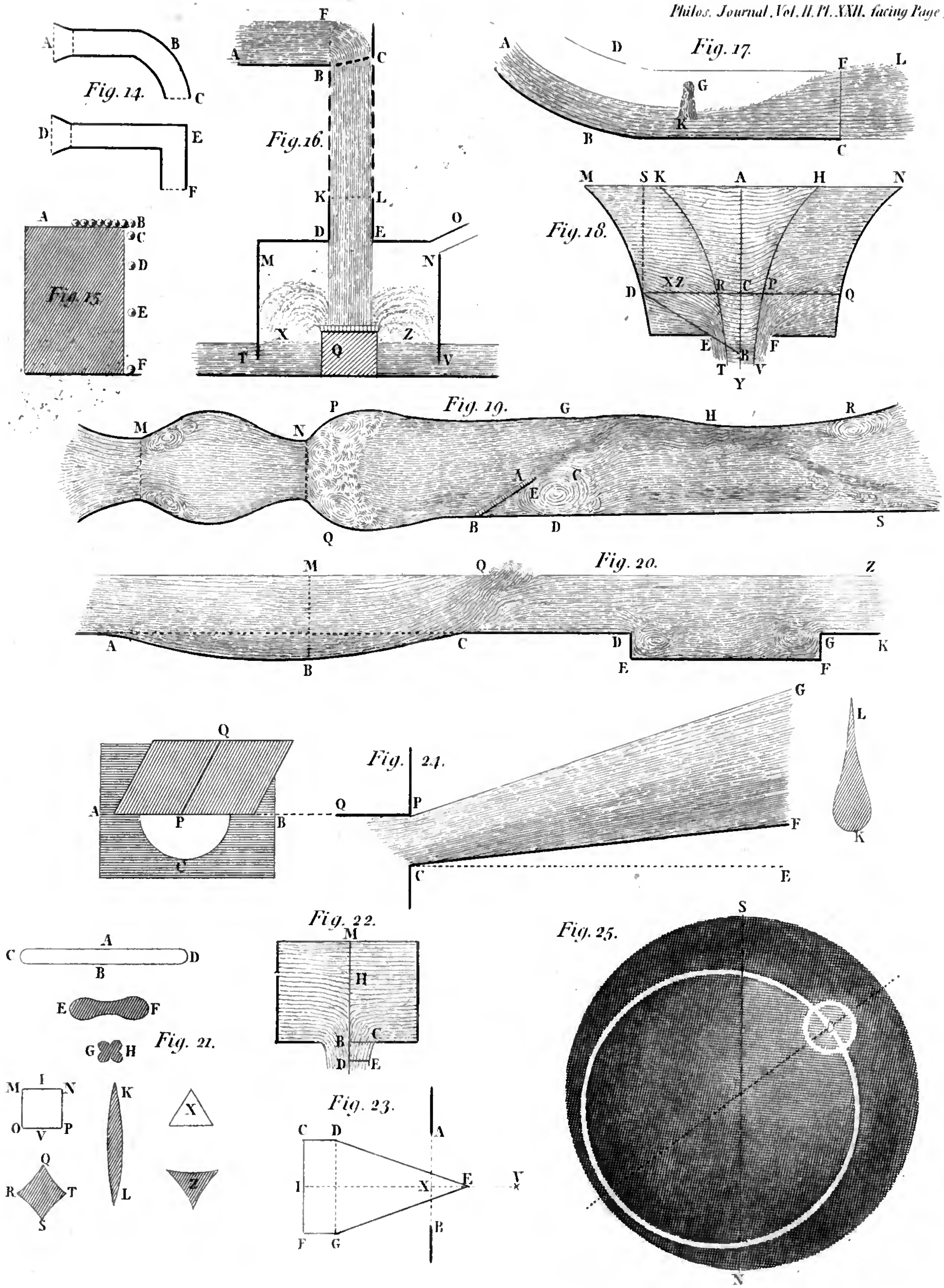


Fig. 14.
Fig. 15.
Fig. 16.
Fig. 17.
Fig. 18.
Fig. 19.
Fig. 20.
Fig. 21.
Fig. 22.
Fig. 23.
Fig. 24.
Fig. 25.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MARCH 1799.

ARTICLE I.

An Account of Improvements in Electrical Batteries; a Method of augmenting their Power, with Experiments, shewing the proportional Lengths of Wire fused by different Quantities of Electricity, and a Description of a new universal Electrometer. By MR. JOHN CUTHBERTSON *, No. 53, *Poland-street, London,* 1799.

IN the year 1774, Mr. E. Nairne made an Electrical Machine, far superior in acting power to any that had been made before†, and a Battery more judiciously constructed and larger than any former one, with which he made a number of interesting experiments. One in particular, affords an accurate measure of the power of his battery, compared with such batteries as have been made since that time. I mean the experiment of melting or dispersing a metallic wire. His battery contained 50 square feet of coated surface; and he found that it was capable of receiving a charge so high, that the discharge melted 45 inches of iron wire of $\frac{1}{150}$ part of an inch in diameter, which answers to about $\frac{7}{8}$ of an inch in length for each square foot, which was the greatest length of wire ever melted. We have no account of this experiment having been afterwards repeated on a scale of any considerable magnitude till the year 1785; when I constructed a battery for the Teylerian society, at Haerlem, containing 135 square feet of coated surface. With this battery 180 inches of the same sort of wire was melted, which seemed to be much more than in proportion to the size of the battery, as this was about 1,3 inches for each square foot. This battery was afterwards increased to 225 square feet of coating, and with this 300 inches of the same sort of wire melted, which was also at the rate of 1,3 inches for each square foot. Some time after this, I made another battery for the same society, containing 550 square feet of coating, composed of 100 jars of $5\frac{1}{2}$ square feet each.

* Communicated by the author. † Philos. Trans.

each. The ſame ſort of wire was not tried with this; but it could be calculated from other ſorts of wire which it melted, that it was capable of melting 65,5 inches, being alſo at the rate of 1,3 inch for each ſquare foot. This increaſe of power, which is almoſt double that of Mr. Nairne, might be attributed to the acting power of the machine; for though Mr. Nairne's machine poſſeſſed the ſtrongeſt acting power of any machine made at that time, yet it could not be ſuppoſed to poſſeſs that high charging property of the Haerlem machine.

Since my return to London, I have made ſeveral batteries, commonly compoſed of 15 jars, each containing 168 ſquare inches of coated ſurface, conſequently the whole battery contains 17 ſquare feet of coating. This battery, according to the proportion of that made by Mr. Nairne, ſhould fuſe 6,3 inches, and in proportion to the Haerlem batteries, it ought to fuſe 22 inches; but inſtead of following that proportion, it is found to fuſe 60 inches, which is an aſtoniſhing increaſe of force. For the battery is only about one-third part of that of Mr. Nairne, and fuſes a much greater length of wire; and though it is only $\frac{1}{32}$ part of that at Haerlem, yet it fuſes $\frac{1}{10}$ of the length of wire. It ſeems difficult at firſt ſight to account for this advantage. I have before remarked, that the proportional difference between the charge of the battery at Haerlem and Mr. Nairne's, might be accounted for, from the high charging power of the great machine; but the reſult of the laſt-mentioned experiments overturns that notion; as it can by no means be ſuppoſed, that a ſingle 2-feet plate machine, which I have uſed to charge the battery of 17 ſquare feet ſo high as to fuſe 60 inches, can have a higher charging power than that at Haerlem; ſo that it muſt proceed from ſome other cauſe. It might be queſtioned whether all the batteries were alike judiciouſly conſtructed. As to Mr. Nairne's, it had certainly faults, both with reſpect to the coating and the mounting of the jars; but the batteries at Haerlem were as judiciouſly conſtructed as my preſent one, which I am ſpeaking of, and which exceeds them in ſuch an aſtoniſhing degree in its proportional force. The only difference between my preſent batteries and theſe at Haerlem is in the glaſs. They were compoſed of glaſs, blown in Bohemia, and thoſe which I make here are of white flint-glaſs. I mention this fact, but I am not inclined to think that the cauſe of the difference depends on the glaſs, becauſe I remember to have melted the ſame quantity of wire with one jar of that kind of glaſs when in Amſterdam, as I do at preſent with white flint-glaſs; ſo that it only remains now to be ſought for in the manner of uſing or charging each battery, and here we ſhall probably find a means of ſolving this paradox.

With regard to the batteries at Haerlem, they were never attempted to be charged but in dry weather, being ſuch as was then commonly called favourable for electrical experiments. There was no convenience in the room where the machine and batteries were uſed, for making a fire, which was therefore ill calculated for electrical experiments: the batteries previous to charging, were made as clean and dry as poſſible; and if they received a charge ſo high as to cauſe a ſpontaneous exploſion, they were then looked upon to be in their moſt favourable ſtate.

It was about this time that we were told by Mr. Brooke, that a coated jar would take a higher charge when dirty, than when clean; but the degree of dirtyneſs was ſo ill defined, that I muſt own I never could dirty a ſingle jar ſo as to anſwer, or to come near what was ſaid of it; and to pretend to bring all the jars in a large battery, containing upwards of two hundred, into that ſtate of dirtyneſs was never attempted; neither does it appear that Mr. Brooke ever thought

thought of dirtying his battery jars, as he only mentions trying two small bottles, whose charging property was very differently increased by his method of dirtying.

Some time afterwards, in the year 1792, I happened casually to discover that a coated jar, when it was a little dampish in the inside above the coating (which is always the case when a jar is fresh coated), would take a higher charge than it would do after it had been coated for some time, and was quite dry in the inside; and also, if the atmosphere was in a moist state, and the jar not dryed in the inside, it would take an equally high charge. From this it appeared evident to me, that if I could, by any means, render the inside of jars damp, it would answer the same purpose. Breathing into a jar was tryed, and the success was such, that it would receive and retain nearly double the quantity of electric fluid it could retain when dry: and in trying to fuse wire with the charge of one jar in a dry state, no more than 5 inches could be fused, though after breathing into it, 12 inches were fused.

This method appeared at first sight to have increased the force to more than double; but notwithstanding so evident and striking an effect, I did not think of trying what would be the result of charging a battery, after the jars had been breathed into; being deterred, as I suppose, from the idea of its being so contradictory to the common method of using batteries, which was never attempted to be done, but when the atmosphere was in a tolerably dry state, and the jars previously cleaned. But in March 1796, being engaged in a course of experiments, when the atmosphere was so very dry, that a spontaneous discharge always took place before I had a sufficient force to answer my purpose, it then occurred to me to try what the effect of breathing into the jars of the battery would be. In this trial, or research, it became necessary first, to ascertain the real charge that the battery was capable of receiving, before a voluntary explosion took place. This battery contained 17 square feet of coated glass, and was composed of 15 jars: it was found in the then state of the atmosphere to be incapable of fusing a greater length of wire than 18 inches. But after breathing into each jar through a glass tube, it took a charge which fused 60 inches, to my very great surprise and satisfaction, as I then thought I had obtained a method of making one battery perform the function of three; because three times the quantity of wire was fused, as appears by comparing this with what had been performed, by increasing the surface of batteries by former electricians. This notion seemed to be justified, by observing in Dr. van Marum's works, that I had enlarged his batteries at three different times; his first contained 135 square feet coating, the second 225 square feet, and the third 550; and the highest charge of the first was just sufficient to fuse 180 inches of iron wire of $\frac{1}{150}$ of an inch diameter, or 6 inches of iron wire of $\frac{1}{40}$ of an inch diameter; the highest charge of the second fused 300 inches of the first-mentioned wire, or 10 inches of the last-mentioned; the highest charge of the third fused 25 inches. We find that these batteries increased in power in the same proportion as the coated surface was increased. I was present when the wire was fused by the two first-mentioned batteries, but at the third not; however, we have no reason to doubt Dr. van Marum's report. These experiments supported me in my first notion, that I had discovered a new method of increasing the force of a battery to three times its usual power; but being unable to account for it to my own satisfaction, I resolved to make a course of experiments, in order to throw some light on the subject.

The chief experiments which have been made on the force of batteries, by Mr. Brooke*, at Norwich, in the year 1786, and by Dr. Van Marum, in 1785 and 1795. The refults were very different. Some experiments which I made in Holland, and afterwards repeated here, did not feem to confirm either of the two. All that had been done either by Dr. van Marum, or myfelf, was done without the help of fuch an electrometer as could indicate the proportional quantities of electric fluid with a fufficient degree of accuracy.

Mr. Brooke was poffeffed of an inftrument of his own invention, with which it was poffible to afcertain the comparative ftrength, if managed with the fame dexterity as Mr. Brooke himfelf poffeffes. But this inftrument came fo high in price, and was fo very difficult in its ufe, that few electricians provided themfelves with it; which, perhaps, is one reafon, why this fubject has fo long remained in obfcurity. I have lately had the good fortune to invent an electrometer which has all the properties that fuch experiments require, and is very fimple and eafy in its ufe; and with this I found myfelf enabled to go through fuch experiments as were neceffary, with greater accuracy than any which had been made before.

The electrometer is reprefented in Pl. xxiii. G H is a long fquare piece of wood, about 18 inches long, and fix inches broad, in which are fixed three glafs fupports, D E F, mounted with brafs balls, *a b c*. Under the brafs ball *a*, is a long brafs hook; the ball *c* is made of two hemifpheres, the under one being fixed to the brafs mounting, and the upper turned with a groove to fhut upon it, fo that it can be taken off at pleafure. The ball *b* has a brafs tube fixed to it, about three inches long, cemented on to the top of F, and the fame ball has a hole at the top, of about one-half inch diameter, correfponding with the infide of the tube. A B is a ftraight brafs wire, with a knife-edged centre in the middle, placed a little below the centre of gravity, and equally balanced with a hollow brafs ball at each end, the centre, or axis, refting upon a proper fhaped piece of brafs fixed in the infide of the ball *c*; that fide of the hemifphere towards *c* is cut open, to permit the end *c* A of the balance to defcend till it touches the ball *a*, and the upper hemifphere C is alfo cut open to permit the end *c* B to afcend; *i* is a weight, weighing a certain number of grains, and made in the form of a pin with a broad head; the ball B has two holes, one at the top, and the other at the bottom; the upper hole is fo wide, as to let the head of the pin pafs through it, but to ftop at the under one, with its fhank hanging freely in *b*; a number of fuch pins are commonly made to each electrometer of different weights; *k* is a common Henley's quadrant electrometer, and when in ufe, it is fcrewed upon the top of *c*.

It is evident from the conftruction, that if the foot ftand horizontal, and the ball B be made to touch *b*, it will remain in that pofition without the help of the weight *i*; and if it

* Though I had read Mr. Brooke's book, as I thought, with a fufficient degree of attention when it was firft publifhed, I did not, till lately, obferve that it contained any experiments relating to this fubject, till I begun to write this paper, and had occafion to look into his book for fome references. I believe thefe experiments had efcaped Dr. van Marum's notice likewife, as I never heard him fpeak of them when he was making others of the fame kind. Though Mr. Brooke's experiments were conducted with much fkill and intelligence, they are fo confufedly arranged, that this had entirely efcaped my notice; and I doubt not but that it had alfo efcaped the notice of feveral other electricians.

fhould

ſhould by any means receive a very low charge of electric fluid, the two balls *b*, B, will repel each other; B will begin to aſcend, and, on account of the centre of gravity being above the centre of motion, the aſcenſion will contiuue till A reſt upon *a*. If the balance be ſet again horizontal, and a pin *i*, of any ſmall weight, be put into its place in B, it will cauſe B to reſt upon *b*, with a preſſure equal to that weight, ſo that more electric fluid muſt be communicated than before, before the balls will ſeparate; and as the weight in B is increaſed or diminiſhed, a greater or leſs quantity of electric fluid will be required to effect a ſeparation.

When this inſtrument is to be applied to a jar, or battery, for which purpoſe it was invented, one end of a wire, L, muſt be inſerted into a hole in *b*, and the other end into a hole of any ball proceeding from the inſide of a battery, as M*: *k* muſt be ſcrewed upon c, with its index towards A; the reaſon of this inſtrument being added, is to ſhew, by the index continuing to riſe, that the charge of the battery is increaſing, becauſe the other part of the inſtrument does not act till the battery has received its required charge.

If this inſtrument be examined with attention, it will be found to conſiſt of three electrometers; and anſwers three different purpoſes, namely, a Henly's electrometer, Lane's diſcharging electrometer, and Brooke's ſteelyard electrometer; the firſt not improved, but the two laſt, which were very defective when firſt invented, I flatter myſelf are here brought to perfection. As the only uſe of Henley's electrometer to this inſtrument is, as I have ſaid before, to ſhew, by its continuing to increaſe in divergency, that the battery continues to receive a ſtill ſtronger charge, it required no improvement; but Lane's electrometer, in its primitive ſtate, could by no means anſwer the required purpoſe for batteries, becauſe the ball intended to diſcharge the battery, was neceſſarily placed ſo near to the ball of the battery, that duſt and fibrous particles were always attracted by and adhered between the two balls, ſo as to retard the charging, and often render a high charge impoſſible: whereas, in this, they are placed at four inches aſunder; and when the deſired height of charge is obtained, and not before, the ball of the electrometer moves of itſelf nearer to the ball which is connected with the outſide of the battery, and cauſes a diſcharge. The defects in Brooke's ſteelyard electrometer were, 1ſt, that it could not cauſe a diſcharge, and 2dly, the difficulty of obſerving the firſt ſeparation of the balls cauſed great error. If it were not placed in an advantageous light (which the nature of the experiments could not always permit), it would not be ſeen, without the attention of an aſſiſtant, which is ſometimes unpleaſant, and cannot always be commanded. But the inſtrument which I have deſcribed, requires no attention or aſſiſtance; for as ſoon as the ſeparation takes place between B and *b*, the ball A deſcends, and diſcharges the battery of itſelf.

By this combination and improvements, we poſſeſs in the preſent inſtrument all that can ever be required of an electrometer; namely, by *k*, we ſee the progreſs of the charge; by the ſeparation of B, *b*, we have the repulſive power in weight; and by the ball A, the diſcharge is cauſed, when the charge has acquired the ſtrength propoſed.

* A chain, or wire, or any body through which the charge is to paſs, muſt be hung to the hook at *m*, and carried from thence to the outſide of the battery, as is repreſented by the line N.

Experiments

Experiments made with a View to determine in what Degree the charging Capacity of coated Jars is increased by breathing into them before the charging.

Experiment 1. Prepare the electrometer in the manner shewn in the plate, with the jar M annexed, which contains about 168 square inches of coating *; put into B the pin, marked 15; take two inches of watch-pendulum wire, fix to each end a pair of spring tongs, as is represented at G *m*, hook one end to *m*, and the other to the wire N, communicating with the outside of the jar; let the uncoated part of the jar be made very clean and dry; and let the prime conductor of an electrical machine, or a wire proceeding from it, touch the wire L; then if the machine be put in motion, the jar and electrometer will charge, as will be seen by the rising of the index of *k*, and when charged high enough, B will be repelled by *b*, and A will descend and discharge the jar through the wire, which was confined in the tongs, and the wire will be fused and run into balls.

Experiment 2. Put into the tongs eight inches of the same sort of wire as before, hang one pair of tongs to the hook *m*, and apply the other to the wire which forms the outside communication: take out the pin in B, and put in its stead one marked 30; all the other part of the apparatus remaining as before, and the uncoated part of the jar being previously cleaned and dried; the machine being then put in motion, the jar and electrometer will charge, as is shewn by the rising of the index as before; but as soon as the jar has received a greater quantity of electric fluid than before, a spontaneous explosion will happen without affecting the balls B *b*, because the discharge will have passed along the uncoated part of the jar from the inside coating to the outside: whence it follows, that while the jar remains in that clean state, it is incapable of receiving a charge high enough to affect the balls, or even a higher charge than it had received in the first experiment. Let the uncoated part of the jar be therefore rendered, in a slight degree, damp; which is easily done, by breathing into the inside, through a glass tube; put the machine in motion, and no spontaneous explosion will happen, but the balls B *b* will repel, as in the first experiment, and the discharge will happen from A to *a*, and pass through the wire placed in the circuit; and though it was eight inches, it will be fused in the same degree as two inches in the last experiment, namely, the wire seen red hot the whole length, and then fall into balls.

Very different degrees of fusion are caused by electric discharges, which may cause great mistakes, if not well attended to. It is proper to adhere to the degree above-mentioned, and particular care ought to be taken to lay the wire, intended for fusion, straight, without any bendings or angles in it. The wire used in the two last experiments, was that which is commonly called watch-pendulum wire, which is flatted; and as it approaches very near to such a sharp edge as might be supposed to affect the experiment, by permitting a dissipation of the electric fluid in its passage, round wires were tried, and the result was the same.

* Take out the pin in B, and observe whether the ball B will remain at rest upon *b*; if not, turn the adjusting screw at C, till it just remains upon A.

By

By the laſt experiment it appears, that breathing into the jar had increaſed its charging capacity nearly in the ſame proportion as it had done the batteries: after breathing, it received a charge ſufficient to fuſe four times the length of wire it did when clean; but by the weight in the electrometer, and alſo by the greater number of revolutions given before the diſcharge happened, it might be ſuppoſed that the jar had received only a double charge.

The following experiments are intended to ſhew the lengths of wire, which are juſt fuſed by various quantities of electric fluid at the ſame intenſity.

Experiment 3. For this purpoſe, a ſecond jar was placed at the wire L, the pin marked 30 was taken out, and 15 put in its place, two inches of the ſame ſort of wire as uſed in the laſt experiment was placed in the circuit, every other part of the apparatus remaining unaltered; the machine was then put in motion, till B begun to aſcend, when it was ſtopped, and before A could reach *a*, one of the jars was puſhed from the wire L (to do which, there is always ſufficient time while the electrometer is in motion), the diſcharge was effected, and the two inches of wire was juſt fuſed.

Experiment 4. The jar which was puſhed away in the laſt experiment was diſcharged, and placed at the wire L, as before, and eight inches of the ſame ſort of wire placed in the circuit; the outſide coating of the jars either touched each other, or had a metallic communication. All the other part of the apparatus remained as before, and the machine was put in motion, till B begun to aſcend; the jar was not removed, as in the laſt experiment, but ſuffered to diſcharge with the other, and the eight inches of wire was fuſed in the ſame degree as the two inches in the laſt experiments.

It is evident from the poſition of the apparatus, that the quantity of electric fluid diſcharged in the laſt experiment muſt be double that of the former; yet, in repeating the experiment, I had different reſults, which made me again ſuſpect the edges of the wire, I therefore reſolved to take round wire, and of as large a diameter as could be conveniently fuſed.

Experiment 5, *with three jars.* Iron wire of $\frac{1}{150}$ part of an inch in diameter, and ſix inches in length, was placed in the circuit; three jars were placed ſo that the balls proceeding from their inſides touched the wire L, and their outſide coatings touched each other. The machine was turned till B begun to aſcend, the diſcharge was cauſed, and the whole length of the wire was juſt run into balls.

Experiment 6, *with three jars, one removed.* Two inches of the ſame ſort of wire was placed in the circuit in the ſame manner as the laſt, and the three jars remained; the machine was turned till B begun to aſcend, then one of the jars was drawn away, conſequently only two diſcharged, and the wire juſt run into balls as the laſt.

Experiment 7, *with four jars.* Wire of $\frac{1}{150}$ part of an inch was taken, and four jars placed in contact with the wire L, with their outſide coatings in contact with each other, and eight inches of wire was placed in the circuit; the weight in the electrometer remained as before; the machine was then put in motion till B begun to aſcend, the diſcharge was effected, and the wire was fuſed, and run into balls. The experiment was repeated with the ſame ſort of wire $8\frac{1}{2}$ inches long; the diſcharge was juſt ſufficient to run it into balls.

Repeated with nine inches of the ſame ſort of wire, and the diſcharge cauſed it to be red hot the whole length.

Experiment.

Experiment 8, *with four jars, two removed.* Two inches of the ſame ſort of wire was placed in the circuit, all the jars remaining as in the laſt experiment, the machine put in motion, till B begun to aſcend, then two of the jars were drawn away; the diſcharge was cauſed, and the wire was fuſed, and run into balls.

Repeated with the ſame ſort of wire $2\frac{1}{2}$ inches long, the diſcharge cauſed it to be red hot the whole length.

Experiment 9, *with fourteen jars.* Wire of $\frac{1}{100}$ part of an inch diameter was taken, eight inches long, and proceeded according to Experiment 7; it was fuſed and run into balls.

Experiment 10, *with fourteen jars, ſeven removed.* Two inches of the ſame ſort of wire was taken, and proceeded with, according to Experiment 8; it was fuſed and run into balls.

The reſult of the foregoing experiments proves ſufficiently, that double quantities of electric fluid, in the form of a diſcharge, will melt four times the length of wire of a certain diameter; and Experiments 5 and 6 prove that when one-third part is added to two, three times the length of wire was fuſed.

Theſe experiments give reaſon to apprehend ſome error in Dr. van Marum's experiments, becauſe he found his batteries to increaſe in power only in the ſame proportion as the coated ſurface was increaſed, viz. that double ſurface of coated glaſs only could fuſe double lengths of wire of the ſame diameter.

The doctor might, perhaps, have been led into a miſtake in the following manner: firſt, he may not have charged the batteries to an equal height, as he did not, at that time, poſſeſs an electrometer of ſufficient accuracy for that purpoſe; and, ſecondly, he may not have been aware of the different degrees of fuſion cauſed by electric diſcharges, but only judged of the force by the wires being converted into balls; by which great miſtakes may happen. For if a wire be taken 18 inches long, and of ſuch a diameter, that when a jar or battery is charged to ſuch a height as juſt to cauſe it to run into balls, much ſhorter lengths of that ſame ſort of wire may be ſubjected to the ſame force, and ſtill be only converted into balls by it; even if only ſeven inches were taken, nothing but balls will appear; the only difference will be, that the balls will be ſmaller, and diſperſed to a greater diſtance, which might be eaſily overlooked. If ſix inches of the ſame ſort of wire be taken, it will be converted into balls and flocculi, or brown oxyde of iron; ſo that to be accurate in this point, the loweſt degree of fuſion muſt be had, which is known when the charge has paſſed, by the wire being ſeen red-hot the whole length, and afterwards run into balls.

Having now ſufficiently proved by experiment, in what proportion different quantities of electric fluid act upon different lengths of wire, which was required to be known, in order to explain in what proportion the charging capacity of a jar or battery is increaſed by breathing into it, before the charging begins, I ſhall proceed in the next place to explain this point.

The opinion that I had at firſt entertained (though ſupported by Dr. van Marum's experiments), that I had found out a method of increaſing the charging capacity of batteries to three times their uſual force, was not ſupported by the facts that the uſual power of a clean and dry battery, containing 17 ſquare feet coated ſurface, namely, that of fuſing from 18 to 22 inches of iron wire of $\frac{1}{150}$ part of an inch in diameter, will be increaſed by breathing into the jar, ſo as to become capable of fuſing 60 inches. If the firſt-mentioned effect be taken at a mean a mean, it will be 20, then the increafed effect, gained by breathing, will be juft $\frac{2}{7}$, as determined by the wire; and experiments 5 and 6 prove, that in order to produce fuch an increafed effect, an addition of $\frac{1}{7}$ part of the coated furface muft be added to the battery, which is about 816 fquare inches. This would amount to an addition of 54 fquare inches to each jar; or, in other words, if that quantity of coating could be added to each jar, the fame effect would be produced as when breathed into. But this would require the coating to be within an inch of the top, which would render the battery unchargeable, at leaft, to that degree. A battery of 15 jars conftructed in the ufual manner, will, therefore, by this treatment, become equivalant in power to 21 jars of the fame kind, if clean and dry.

To explain the effect of breathing into the jars, appears to be a matter of fome difficulty. This experiment has been fhewn to feveral electricians, and different opinions have been advanced, moft of which feem to imply, that breathing acts as a coating to the uncoated part, which will appear in the fequel to be abfurd. Mr. Nicholfon's opinion (fee Philof. Journal, II. 219) comes much nearer to the truth, though it does not appear to me to be fufficient to account for the effect produced. I admit, with him, that a fpontaneous explofion over the uncoated part is moft commonly caufed by undulation; but that this undulation is caufed by the difcharging of different charged zones, will be difficult to prove, becaufe fuch zones cannot exift upon clean and dry glafs.

When the uncoated part of a Leyden jar is made perfectly clean and dry, and the jar fet to the conductor of a machine in action, it will begin to charge, and, while charging, the coated part of the jar, and the wire which is connected with it, become equally charged, and each endeavours to throw off that furplus of electric fluid which is forcing into them; the coating from its edges upwards, and that part of the wire which is above the coating and within the jar, will endeavour to throw it in all directions, which will caufe it to be furrounded by an electric atmofphere, increafing in denfity as the charge increafes. This atmofphere, together with that given out by the coating, fills the whole jar. Part of the electric fluid forced into the coating enters the furface of the glafs, but the uncoated part, being clean and dry, both withinfide and without, the infide refifts the fluids entering its furface, which is kept fufpended at a diftance, becaufe the natural electric fluid contained on the outfide, finds no means of efcape. But the action of the machine ftill continuing, preffes it ftill clofer to the furface, and at laft overcomes that refifting force, and fome of the particles on the outfide give way, which caufes an undulation in the infide, and the electric fluid clofes inftantly in upon its infide furface, and forces a greater quantity from the outfide. Flafhes, or corufcations, are thus caufed, which are always feen when a jar is charging in the abovementioned circumftances: the charge ftill continuing to be made, forces another quantity from another part of the outfide of the jar, and caufes a fecond corufcation and undulation, which may be fo ftrong as to caufe a fpontaneous difcharge; or two or three more corufcations and undulations may happen, before the difcharge, according to the fteadinefs or unfteadinefs of the action of the machine, the quantity of electric fluid thrown off from the outfide at each undulation, and alfo the degree of drynefs and cleannefs of the uncoated part of the jar. A difcharge fometimes happens without having previoufly occafioned any perceptible corufcation. This is the cafe when the firft undulation has been fo ftrong, as to caufe the whole

 difcharge

difcharge with the firft corufcation, the one being fo quickly followed by the other that it is imperceptible.

A jar will fometimes, while it is charging, give a great many fmall corufcations, quickly fucceeding each other, which afterwards ceafe without having caufed a fpontaneous explofion, though the action of the machine be continued. This happens when the uncoated part is nearly clean and dry, but not perfectly fo; its furface ftill containing fome conducting particles, but not fo connected, that the electric fluid can pafs from one to the other without leaps, or fmall corrufcations on the outfide, which permit the electric fluid to fpread gradually over its infide furface, and prevent the undulations from being fo ftrong as to caufe a difcharge.

After this explanation of the caufe of the flafhes, or corufcations, which are feen upon the uncoated part of a jar while charging, and alfo that fuch corufcations produce undulations, which terminate in a fpontaneous explofion; it remains now to explain how a jar is charged when the corufcations are prevented by breathing upon the uncoated part.

When a coated jar is breathed into, and then fubjected to the procefs of charging, the electric fluid is forced into it, along the wire in the infide to the coating, where it inftantly and equally fpreads itfelf over the whole coated part, and at the fame time, though with difficulty, and confequently gradually, it fpreads itfelf over the uncoated part, taking the condenfed film of humidity for its conductor, as it proceeds from the edges of the coating upwards towards the mouth of the jar, according to the arrangement of the particles of moifture, and rifes higher or lower, depending entirely on their arrangement, and the force with which it is repelled from the machine. If the conducting particles be almoft uniformly diffufed over the uncoated part, the whole jar, in the infide, will become charged, though the uncoated part will be charged in a much lefs degree than the coated, on account of the imperfection of the conducting particles which has adhered to its furface; no corufcations will be perceived, on account of the gradual and equal diffufion of the electric fluid over its infide furface: and though the charging be continued, yet, if the exhaled conducting particles be favourably diffufed, no fpontaneous explofion will happen from one coating to the other, along the uncoated furface, but the jar will either be perforated, or, if it be of fufficient ftrength to refift that effect, the electric fluid will be feen to run in a ftream over the mouth of the jar, as quickly as the machine fupplies it. Whenever a fpontaneous electric explofion happens, it muft be from a body of fufficient bulk and conducting property to contain that quantity of electric fluid at that point from which it explodes, otherwife no explofion ever happens. But the humid conducting particles are juft fufficient merely to admit the electric fluid, by the action of the machine, to be fpread over the furface of the glafs, but in no part of fufficient denfity, either to receive, or contain an explofion. If, therefore, a fpontaneous explofion do happen, it muft either proceed from the infide coating, or the wire which is connected with it to the outfide; and, if we examine the ftate of the coating, we fhall underftand, that the edge of the coating (from which part only it is ever poffible to explode), and alfo above it, to a fhort diftance upwards, is as ftrongly charged as the coated part; and by the action of the machine it is fo ftrongly loaded with electric fluid, that it is repulfive in all directions, which keeps back, or entirely ftops, a fpontaneous explofion from the

the edge of the coating. With regard to the wire, the only place from which it explodes fpontaneoufly, is that part which is nearly of an equal height with the edge of the mouth of the jar. The fluid is nearly as much condenfed on this part as on the other, fo that an explofion from the wire is hindered by the fame caufe as from the coating. A jar under fuch circumftances cannot, therefore, explode fpontaneoufly; but the fluid will run over the edge of the jar as quickly as the machine furnifhes it, when its charging capacity is full.

I have ftated, at page 530, that a jar of the dimenfions there given, being clean and dry, can only contain a charge fufficient to fufe 2 inches of a certain wire, and when breathed into, its charging capacity will be fo much encreafed, that it will contain a charge fufficient to fufe 8 inches of the fame fort of wire; and a battery of 15 jars, in the firft-mentioned ftate, can only fufe 20 inches, and in the laft-mentioned, 60 inches. This increafed charging capacity, proceeds, no doubt, from the particles of moifture, though not from their acting as a coating, as has been fuppofed, but by their being brought into a ftate or capacity of refifting a fpontaneous explofion, fo that a ftronger charge is forced in upon the coated part. Some of the electric fluid which was forced upon the uncoated part to a certain height (perhaps ½ an inch, more or lefs, according to the degree of dampnefs, and the fituation of the particles) may, indeed, be difcharged along with that from the coated part; but this is of little importance, and by no means capable of producing that increafed effect, which, as I have fhewn by experiment, would require an addition of feven jars to a battery of fifteen.

II.

Difcovery of Sulphate of Strontian, near Sodbury, in Gloucefterfhire. By G. S. GIBBES, B. M. F. R. S.

To Mr. NICHOLSON.

SIR,

IF the following imperfect analyfis fhould meet your approbation, I fhall feel myfelf flattered by feeing it noticed in your excellent monthly publication.

I have the honour to be,

Sir,

Your humble fervant,

GEORGE SMITH GIBBES.

Bath,
No. 28, Gay-ftreet.

A friend of mine, the Rev. Mr. Richardfon, fome little time fince, fhewed me a fpecimen of a fubftance which, he faid, was found in great abundance in the neighbourhood of Sodbury, in Gloucefterfhire, where it was ufed for the purpofe of making gravel walks. The ftone was compofed of a vaft number of fmall cryftals, which cohered together with but little force. The cryftals were eafily reduced to powder, and were not affected by any acid. I expofed equal parts of thefe cryftals in the ftate of a fine powder and charcoal to the action

action of heat, and I found, on pouring the muriatic acid on the ſubſtance, when cool, that there was a great diſengagement of ſulphurated hydrogenous gas.

I filtered this ſolution in muriatic acid, and I found the cryſtals exactly ſimilar to thoſe which I had often formed, by adding the muriatic acid to the ſtrontian earth, found in Scotland. This muriate poſſeſſed the property of changing the flame of the candle to a moſt beautiful red colour. A copious precipitation was formed, when the ſulphuric acid was added to a ſolution of this muriate. From theſe experiments, and from the external characters of the ſtone, I have no heſitation in believing, that it is compoſed of the ſulphuric acid and ſtrontian earth.

P. S. Should you wiſh to poſſeſs a ſpecimen of this ſubſtance, on receiving a line from you, I will take the firſt opportunity of ſending one to you.

III.

On the Corundum Stone, from Aſia. By the Right Honourable CHARLES GREVILLE, *F. R. S.*

(Continued from p. 485, Vol. II.)

MR. Tranckell, who reſides in Ceylon, and from whoſe communications I derived lately much information, had, about five years ago, a ſapphire, the greater part blue, and the remainder of a ruby colour. I ſaw, in Romé de L'Iſle's collection, at Paris, a ſmall gem, which was yellow, blue, and red, in diſtinct ſpots, and he called it oriental ruby. M. de la Metherie, to avoid the confuſion of the denomination oriental ruby, with octoedral ruby, calls it a ſapphire: with more correctneſs, I think, the above-mentionened gems ſhould be claſſed as argillaceous, under the denomination of corundum. I am not uninformed, that corundum is ſaid to be found in France. The Count de Bournon is convinced, that the ſpecimens mentioned in Crell's Journal, as having been found by him in a granite in the Forez, were corundum. M. Morveau alſo ſays, he found it in Bretagne; but the Abbé Hauy, in No. 28, of the Journal des Mines, aſſerts, that the corundum, found in France, is titanite; he does not ſay whether this obſervation extends both to the corundum of Bretagne and that of the Forez. In the ſame manner I had obſerved in the ſpecimens, which Mr. Raſpe called jade, or a new ſubſtance from Tiree, on the weſt coaſt of Scotland, a great reſemblance to corundum; but, having then only had a curſory view of the ſubſtance, I am indebted to Mr. Hatchett for the examination of a ſpecimen of it, which he had from Mr. Raſpe's collection. The Tiree ſtone reſembles cryſtallized corundum of the coaſt, in texture and colour; it is alſo as refractory, when examined by the blow-pipe, with different fluxes. Its ſpecific gravity is 3,049; conſequently nearer the ſpecific gravity of pure corundum than the above-mentioned lump, 2,785, and the matrix of corundum, 2,768. The Tiree ſtone will ſcratch glaſs readily, but not rock cryſtal; its hardneſs, therefore, correſponds with that of the matrix of corundum. The ſubſtance of the lump, deſcribed in page 481, cuts glaſs and rock cryſtal, and the Tiree ſtone, readily.

It will, therefore, be ſufficient for me to ſay, that there is great probability corundum may be be found in Great Britain, and on the Continent of Europe, as well as in Afia; and the above flight effays may fhow, that obfervations on corundum, in its different ftates of purity, may lead to accurate diftinction between fubftances hitherto imperfectly known, and will lead to a revifion of the filiceous genus, whereby the argillaceous gems may obtain its due pre-eminence in mineralogy. When gems, by art, or by rolling in the beds of rivers, have been deprived of the angles of their cryftals, they are unavoidably fubjected to uncertain external characters, which even great practice cannot render certain; and hence the unwillingnefs of European Jewellers to deal in coloured gems. I have fome fpecimens of a fapphire-blue ftone, India cut, very fmall and pellucid; they were purchafed in India as fapphires, and were fuppofed to be fluor by a lapidary in London, but are cyanite. The above could fcarcely have happened, if the ftones had been of fufficient fize and value to require much examination, the weight and degree of hardnefs being exceedingly deficient. The colour, therefore, will not be a fafe guide. The diamond, whether white, blue, red, yellow, or green, can be diftinguifhed by its cryftal, or by its fpecific gravity and hardnefs, or, when it is polifhed, by its luftre. Other ftones, which compofe the order of gems, might equally depend on their cryftallization, fpecific gravity, polifh, and hardnefs, for a diftinct arrangement. The near relation of argil, which Bergman gave to this order, is daily confirmed; and it will be, perhaps, to Mr. Klaproth, more than to any other exifting chemift, that we fhall owe our correct information on the fubject of other gems, as we do on the fubject of corundum.

Many of the varieties of corundum, particularly the coloured and tranfparent forts, with their regular cryftallizations, are yet *defiderata.* Many cryftallized ftones, from defect of colour, luftre, &c. are of little value in the market, fuch as jargon, chryfolite, tourmaline; and an infinity of unnamed ftones of Ceylon, Pegu, Siam, &c. would be valuable to the mineralogift, if obtained adhering to their ftrata, and in cryftals, when external form is not obliterated. I have no doubt, when it is known how much fuch information will tend to illuftrate the hiftory of the earth, and particularly that of gems: the fpirit of enquiry, fo laudably afloat in Britifh India, will be directed to attain it. I have not heard of any metallic veins being found in corundum, unlefs a ftone, which Alonfo Barba, *lib.* 1, *c.* 13, defcribes, fhould give an inftance. "The chumpi, fo called from its grey colour, is a ftone of the nature of emery, and contains iron; it is of a dull luftre, difficult to work, becaufe it refifts fire long. It is found at Potofi, at Chocaya, and other places, with the minerals, negrillos, and roficleres."

Having mentioned the varieties of cryftallized and amorphous corundum, and the mifcellaneous facts relative to my collection of that fubftance, from India and China, it might be fufficient to give an *icon* of the cryftal, and clofe a paper already prolix; but having with fatisfaction obferved, within the laft years, the fcience of mineralogy gaining ground in Great Britain, from the knowledge acquired by feveral gentlemen, who have examined the mines, and formed perfonal acquaintances with the moft learned and experienced men on the continent, and alfo from ingenious foreigners, who have communicated their obfervations on Englifh foffils, and connected them with the moft approved fyftems, it may, perhaps, be accepted as a fufficient apology for what follows; that I confider it as a defideratum to Englifh mineralogifts, to be invited to a preference of permanent characters, which the ftudy of cryftallization has collected, and which promifes

promifes to be a certain method of afcertaining the laws, by which elective attraction arranges and combines molecules of matter. It is true, the progrefs of cryftallography has been extremely flow, and different nations have contributed to its prefent improvement. It is rather remarkable, that the earlieft treatife on metallurgy, of authority, was publifhed in Italy, by Vanoccius Biringuccius, juft before Agricola publifhed his Treatife, in 1546, in Germany; and the firft treatife, on the Structure of Cryftals, I know, is alfo from Italy, by Nicolas Steno; *Prodromus Differtationis de Solido intra Solidum naturaliter contento. Florientiæ, 1669, in 4to.* A work of great merit. Louis Bourguet of Neufchatel, in his *Lettres fur la Formation des Sels et des Cryftaux. Amft.* 1729, 12*mo.* connected, by obfervation and meafure, triangular and rhomboidal, and cubic, and pyramidal tetraedal molecules, for all different fubftances. His contemporary, Maurice Antoine Capeller*, attempted to deduce a fyftem from geometrical principles; and in this ftate did Linnæus find the fubject, when he attempted to reduce the fcience of minerals to external characters, and cryftallized bodies to falts.

None of the obfervations of Linnæus will prove ufelefs to fcience; but his fyftem alarmed the chemifts and mineralogifts, who rejected every other criterion, than internal character from analyfis, and the fyftem of Cronftedt was preferred by general affent. By this means, a fpirit of controverfy deprived the chemift and lythologift of mutual affiftance; and the general opinion was correct, on the fuppofition, that a mixed fyftem of chemical and external characters would be irreconcileable; but it has been admitted, even by thofe who moft decidedly oppofed Linnæus's fyftem, that the beft fyftem of mineralogy fhould be founded on external and internal characters combined†. Among the few, who ventured to exprefs their obligations, at the fame time, to Linnæus, and to Cronftedt, was Baron Born, whofe abilities and character, in addition to his diftinction, as one of the counfellors of mines of his Imperial Majefty, obtained his inrolment among the fellows of the Royal Society. He connected the intrinfic and extrinfic characters of minerals, in the Index Foffilium, which he publifhed in 1772. In Sweden, Bergman's Treatife on the Forms of Cryftals, publifhed in the Upfal Tranfactions, in 1773, was a more authoritative recommendation to the inveftigation of the principles of cryftallization; and it can be of little importance for me to add, that fince I have poffeffed the collection of Baron Born, in 1773, I have had every confirmation of the fame opinion. The progrefs of chemiftry, and of cryftallography, applied to mineralogy, has rendered the examination of ftrata, and of mines, a fource of amufement as well as inftruction; and the arrangement of interefting facts, in the chemiftry and mechanifm of nature, fuits my occafional refearches in geology, which, from variety of avocations and circumftances, have been very much interrupted. My acknowledgment of obligation to the learned, who have made this progrefs in fcience, is the beft recommendation I can give to others to examine their works. Thofe whofe talents and time are devoted to the inveftigation of every mineral fubftance can have no refpite to their labour; minerals, in every ftate of their formation,

* Prodomus Cryftallographiæ, &c. and Litteræ ad Scheuzerum, de Cryftallorum Generatione. Act. Nat. Cur. vol. 14. Append. p. 9.

† Nullum itaque eft dubium, quin hujufmodi methodus mixta, quæ notis characterifticis tam extrinfecis quam intrinfecis fimul combinatis, eft fuperftructa, proxime ad naturalem accedens, maximam indicans fymmetriam, reliques fit præferenda methodis. J. G. Wallerius, de Syftemate Mineralogico rite condendo. §. 102.

formation, perfection, and decomposition, as they occur in mines, must have their qualities immediately ascertained, and be reserved for profit, or thrown away on the heap. The practical miner could not, without external characters, make any progress. The valuable minerals are soon pointed out by assay, and their appearance numbered. The accuracy of selection depended, in all periods, much on the experience of the miners. It remained for Mr. Werner to give the utmost degree of accuracy which irregular external characters can acquire, by fixing appropriate terms to all the characters which occur, and which the senses can discriminate. In 1774, he opened his system of external characters of minerals; and the perfection he has since given to it, has rendered it very general. The Leskean collection, arranged after Mr. Werner's method, has procured in Mr. Kirwan a powerful support to the introduction of that system in this country; and we have already some other valuable publications, to recommend and introduce other favourite systems of the continent. It is, therefore, at this time, the English mineralogist should be invited to examine, if not to prefer, permanent characters, so far as the progress of crystallography has collected them, or at least to give them a distinguished rank among external characters of bodies.

If prejudice too long has retarded the union of intrinsic and extrinsic characters, it has also occasioned a schism among the advocates of crystallography.

Romé de L'Isle, in the year 1772, published the first edition of his Essay on Crystallography, which he states to be a supplement to Linnæus; and, by the assistance of a very few friends, he was enabled to increase the number of crystals in a degree to assume the appearance of a system. He told me that the accuracy of his measurement of angles of minute crystals was the acquirement of great practice; but that the Count De Bournon, after a short practice, attained equal correctness, and afforded him assistance, which he acknowledges, in his second edition, to have received, particularly by the discovery of crystals in Dauphiné, Auvergne, Franche-Comté, &c.

The Abbé Hauy, an accurate and patient observer, and a good mathematician, considered crystallography as founded on certain laws, reducible to demonstration by calculation. In the beginning, the differences of Bourguet and Capeller were not more pointed than those of Romé de L'Isle, and the Abbé Hauy; but the progress of observation and calculation having demonstrated their mutual utility, the observer and measurer of crystals will now rest satisfied only when calculation confirms actual measurement. To the Abbé Hauy is also due a late scheme to simplify calculation, by expressing, according to algebraical formulæ, the different laws which determine the modification of crystals. So far as they are the result of calculation and measurement, we may admit the laws of calculation; for whenever the superposition, or subtraction, of simple or compound molecules, on a nucleus, shall, by calculation, give a series of planes and angles, which corresponds exactly to the angles and planes measured on natural crystals, it will amount to no more nor less than a demonstration of the rule or arrangement of elective attraction by figures.

These laws may be reduced to simple practice; for instance, the Abbé Hauy, by measuring the rhombic plane of corundum, found its two diagonals to be as two to three: which gives

to

to its acute angle 81° 47′ 10″, and to its obtuse angle 98° 12′ 50″; the same as martial vitriol*. The forms of fragments in corundum are all acute rhomboids. The cosine of the little angle in corundum is one-seventh of the radius; but in calcareous spar, the cosine is one-fifth of the radius; in schorl two-fifths of the radius; in the garnet, one-third; and in rock-crystals, one-seventeenth. Thus the application of general laws, to ascertain constant character, after they shall have been fully verified, may be very simple and general. It will not require perfect crystals; for when crystals separate into laminæ, which subdivide into fragments, and shew the form or arrangement of their molecules, it is easy, from such fragments, to connect them with their primitive crystal, and, consequently, with their class. It will be a great step, to obtain one regular and permanent external character.—Attention to other characters will be necessary, to ascertain the nature of the substance; and other external characters, such as irregular fracture, colour, &c. must be resorted to, when no permanent characters exist; but from their nature they are fallible, and, in fact, are seldom conclusive.

The progress of crystallography appearing, to me, of consequence to the progress of mineralogy, induced me to desire the Count de Bournon, above mentioned, one of the honourable victims to his allegiance to his king, to describe such crystals, in my collection, as shewed the different known modifications of corundum; which will develope the theory of crystallization, so far as is consistent with the avowed object of this paper. The subject, I believe, has not hitherto been submitted to the consideration of this society. The translation of the Count de Bournon's description has been carefully made, to preserve its clearness; and, I hope, it will be favourably received by the Society, and make some amends for my tedious introduction. After it, I have added a table, connecting in one view the specific gravities of corundum, &c. herein mentioned, with those given by other authors.

An Analytical Description of the Crystalline Forms of Corundum, from the East-Indies, and from China. By the Count DE BOURNON.

THE most usual form of corundum is a regular hixædral prism (Plate XXIV. Fig. 1.); in general, the surface of the crystal is rough, with little lustre, owing to unfavourable circumstances, under which it is crystallized.

The crystals of corundum, hitherto found, were not formed in cavaties, where each crystal being insulated, its surface could preserve that smoothness, and natural brilliancy, which are common to all substances that freely assume a crystalline form. Like the crystals of feldspar which we meet with in the porphyroid granites, the corundum crystals have been enveloped, at the time of their crystallization, by the substance of the rock, which was forming at the same time with themselves, in an imperfect and confused crystalline mass; and the corundum crystal, before it had acquired its perfect solidity, necessarily received on its surface the im-

* This result is extracted from the Journal de Physique; but it appears, from the Journal des Mines, No. 28, that the Abbé Hauy has since rectified this measure, and given 86° 26′ for the acute angle, and 93° 34′ for the obtuse angle.—G.

pression

preſſion of the different particles of the rock which enveloped them: this naturally renders the ſurface rough and dull. Cryſtals of feld-ſpar, formed in the granitic porphyroid rocks, exhibit the ſame kind of appearance, from the ſame cauſe.

The corundum cryſtals are, in general, opaque, or at leaſt they have only an imperfect tranſparency at the edges: when broken into thin fragments, the pieces are ſemi-tranſparent; when held between the eye and the light, and examined with a powerful lens, it will be perceived that their interior texture is rendered dull, by an infinite number of ſmall flaws, croſſing each other, much reſembling the medullary part of wood, when it is viewed in the ſame manner. The degree of tranſparency of the ſmall interſtices, which are between theſe flaws, is further evidence that this texture of ſmall flaws occaſions opacity, which augments in proportion to the thickneſs of the fragments.

This kind of internal ſtructure has alſo a very ſtrong analogy with that of feld-ſpar in granite and porphyry. The endeavour to ſplit theſe cryſtals in a direction, either perpendicular, or parallel to their axes, meets with a very conſiderable reſiſtance: they may, indeed, be broken in theſe directions; but the rugged and irregular ſurface of the broken parts, clearly proves that the direction in which the cryſtalline laminæ have been depoſited one upon another has not been followed.

The regular hexaedral priſm of theſe cryſtals cannot therefore be conſidered as the form of the nucleus of the cryſtal; and, conſequently, is not the primitive form of the cryſtals of this ſubſtance.

If, in order to diſcover the direction of the cryſtalline laminæ, a variety of cryſtals be examined, ſome will hardly fail to be met with, which, on their ſolid angles, formed by the junction of the ſides of the priſm, with the planes of the extremities, preſent ſmall iſoſceles triangles. Theſe are ſometimes greater, and ſometimes ſmaller, and form ſolid angles, of 122° 34′, with the extreme planes of the cryſtal. They are, in ſome inſtances, real faces of the cryſtal, but moſt frequently they evidently are the effect of ſome violence on that part. The ſmoothneſs and brilliancy of theſe ſmall faces, in the latter caſe, ſhew that a piece has been detached in the natural direction of cryſtalline laminæ. It is, indeed, much leſs difficult to ſeparate a portion of the cryſtal at thoſe angles, than at any other part; and in following the natural direction of the faces, with a little patience and dexterity, all the cryſtalline laminæ may be detached, and progreſſively increaſe the ſize of the triangular face.

This operation, however, cannot be done indiſcriminately on all the ſolid angles of the cryſtals, but only on the alternate ones at the ſame extremity, and in a contrary direction to each other. As to the other angles, they may be broken, but it is impoſſible to detach them. When, inſtead of the ſolid angles of an hexaedral priſm, ſmall triangular planes are met with (which frequently happens, whether cauſed by violence or otherwiſe), they are always placed in the direction above mentioned. If by following this indication of nature, we continue to detach the cryſtalline laminæ, we ſhall at laſt cauſe the form of the hexaedral priſm to diſappear totally, and in place of it, a rhomboidal parallelopiped will be obtained (fig. 2.), of which the plane angles at the rhombs will be 86° and 94°; the ſolid

 angles

angles at the ſummit * will meaſure 84° 31′; and that taken at the reunion of the baſis will be 95° 29′.

We can ſplit this parallelopiped only in a direction parallel to its faces; it will ſtill conſequently preſerve the ſame form, which is that of the nucleus of this ſubſtance, and its primitive form. It is, therefore, by a modification of the rhomboidal parallelopiped (fig. 2.) that nature has formed the regular hexaedral priſm (fig. 1.) which this ſubſtance preſents.

For if we conceive that in any period whatever of the increaſe of the rhomboidal parallelopiped, a ſeries of laminæ, or cryſtalline plates, has been depoſited on all the ſides of the parallelopiped; and that theſe laminæ have all undergone a progreſſive decreaſe of one row of cryſtalline molecules, at the acute angle which tends to form the ſummit; and alſo along the ſides of the oppoſite acute angle (fig. 3. and 4.); there will neceſſarily reſult from the continuation of this ſuperpoſition, to a certain period, an hexaedral priſm, terminated by two triedral pyramids, placed in a contrary direction; and their planes, or faces, which form a ſolid angle, of 147° 26′, with the ſides of the priſm, will be either pentagonal (fig. 3.) or triangular (fig. 4.). They will alſo have, in place of a ſummit, an equilateral triangular plane, ſometimes greater and ſometimes ſmaller.

If the ſuperpoſition continues, the equilateral triangular plane, on the ſummit, will become nonagonal, and there will remain no other traces of the primitive planes of the rhomboidal parallelopiped than ſmall iſoſceles triangular planes (fig. 5.): if the ſuperpoſition ſtill continues, until the laſt cryſtalline lamina is reduced to a ſingle molecule, or point, no appearance of the rhomboidal parallelopiped will then remain; and the cryſtal reſulting from this operation of nature will be a regular hexaedral priſm (fig. 1.).

In the ſame manner, viz. by a decreaſe on the lower edges of the laminæ, the primitive rhomboidal parallelopiped of calcareous ſpar paſſes to a regular hexaedral priſm of that ſubſtance, though more frequently it does ſo by a decreaſe on the lower angles of the laminæ. When the laminæ of the corundum cryſtal have, during their ſuperpoſition on the planes of the primitive rhomboidal parallelopiped, experienced a progreſſive decreaſe at one of their acute angles, and along the ſides of the other, at the ſame time, and in the ſame proportion, it is eaſy to conceive that the height of the hexaedral priſm muſt be the ſame as that of the rhomboidal parallelopiped, upon which it has been formed. The height B C (fig. 1.) muſt therefore bear the ſame proportion to the line A B, drawn through the middle of the two oppoſite ſides of the planes, on the extremities, as the whole height E F of the rhomboidal parallelopiped (fig. 2.) bears to the ſmall diagonal G H, from one of the rhombs, that is, nearly as 6, 45 : 5.

But although this exact proportion appears in a very great number of corundum cryſtals, yet we meet with ſome, whoſe lengths are more or leſs conſiderable; and this is owing

* For greater clearneſs, this rhomboidal parallelopiped may be conſidered as being formed by the junction of two triedral pyramids, baſe to baſe; and the two ſolid angles (each of which is formed by the reunion of three of the acute angles on the planes of the rhomb) will then be conſidered as the ſummits of theſe pyramids.

to

to different circumſtances, which have exiſted at the time of their cryſtallization. We may conceive, for inſtance, that if before the progreſſive decreaſe of the cryſtalline laminæ, in the manner above mentioned, the increaſe of the rhomboidal parallelopiped had taken place, by a ſuperpoſition of laminæ, in which the rows of cryſtalline molecules experienced a progreſſive decreaſe along the edges of the acute angle of the baſe only (fig. 6.), and that (the ſides of the priſm having already acquired a certain length) the ſucceeding cryſtalline laminæ had experienced a decreaſe at the acute angle of the ſummit, the ſame regular hexaedral priſm would have reſulted from this proceſs; but the proportion between the height, and the line drawn from two of the oppoſite ſides of the planes, on the extremities, would have been much greater than that of 6,45 : 5, and conſequently this priſm would have been longer than that of the rhomboidal parallelopiped, which ſerved as its nucleus. On the other hand, if the increaſe of the rhomboidal parallelopiped had taken place, by a ſuperpoſition of cryſtalline laminæ, decreaſing at the acute angle of the ſummit, and ſome time after decreaſing alſo along the ſides of the acute angle of the baſe (fig. 7.), the regular hexaedral priſm reſulting from this proceſs, would have been ſhorter, in proportion to the duration of the mode of decreaſe in the cryſtalline laminæ, which were firſt depoſited. There are ſome of the hexaedral priſms, in corundum cryſtals, which are ſo ſhort, that they appear no more than ſegments. Calcareous ſpar offers the ſame phenomenon; as do likewiſe all the ſubſtances in which the hexædral priſm has any analogy of formation, with that which we have here deſcribed.

It happens frequently, when the ſuperpoſition of the cryſtalline laminæ does not go on equally on all the faces of the rhomboidal parallelopiped, that one or two only of the ſolid angles of the hexædral priſm, taken alternately, ſtill ſhew, by ſmall iſoſceles triangular planes, ſome remains of the faces of the parallelopiped, while the others do not ſhew any at all.

Mr. Greville, in his collection of this ſubſtance, has a cryſtal of corundum, upon one ſide of which only two of the planes of the rhomb have experienced an equal and perfect ſuperpoſition, while there has been but a very ſmall number of cryſtalline laminæ depoſited on the third plane. Conſequently this cryſtal preſents a regular hexaedral priſm, one of whoſe ſolid angles is ſo much truncated, that the half of the plane of the end of the hexaedral priſms diſappears (fig. 8.); and this cut, or ſection, forms an angle of 122° 34′ with the plane on the extremity.

It is unneceſſary to obſerve, that the regularity of the hexaedral priſm depends on that of the rhomboidal parallelopiped, on which it is formed.

When, by detaching the laminæ from the alternate ſolid angles of the regular hexaedral priſm, the planes reſulting from this operation begin to run into one another; and the cryſtal begins to aſſume the form of the rhomboidal parallelopiped, to which it owes its origin; we frequently ſee the ſurface of theſe new planes divided into an immenſe number of ſmall rhombs, formed thereon by the interſection of lines, that are parallel to the ſides, which belong to the rhomboidal form of the new faces (fig. 9.).

Theſe lines are owing to the extremities of the laminæ, which have been depoſited on the inferior faces, correſponding with thoſe on which we obſerve them; and they ſerve to corroborate

roborate ſtill further, the demonſtration we have given of the formation of the regular hexaedral priſm in this ſubſtance.

(To be concluded in our next.)

IV.

On Water-wheels.

To Mr. NICHOLSON.

SIR,

YOUR attention to the inquiries ſent laſt month, induces me to tranſmit the annexed paper for your conſideration.

Your opinion that a float-board-wheel is preferable to a bucket, agrees with that of an experienced mill-wright.

I hope, through the medium of your valuable Journal, the ſubject will receive further diſcuſſion; as the ground on which the preference is given is not obvious.

It will, no doubt, be agreeable to you to learn, that the ſphere of your Journal's uſeful communication is extended to the northern verge of Ireland; and that, amidſt the tumult of political ſtrife, the arts of peace, and the intereſts of ſcience, are not yet forgotten.

I am, your obliged reader,

February 1798. DERRIENSIS.

On Water-wheels.

THE effect of water-wheels, ariſing from the gravitating power of the water; to retain the water, ſeems the point neceſſary to be attended to, in the conſtruction of the wheel.

The form of the bucket-wheel ſeems beſt adapted to this end; yet in ſtreams, where the fall is not conſiderable, a preference is given to float-board-wheels.

Why is this preference given to float-board-wheels? Suppoſe a ſtream which has a fall of ſix feet, and a ſupply which renders it equal to the power of ſix horſes:

Allow this power to be adequate to raiſe a given weight, one foot high, thirty times in a minute.

It is convenient to have a water-wheel of ten feet diameter. A bucket-wheel is made of a width adequate to the ſupply of water. Will this wheel be leſs competent to the work than a large float-board-wheel? If ſo, to what is the loſs of power in the ſmall, or the gain of power in the large, wheel owing?

WITHOUT entering minutely at preſent into the ſubject here offered for conſideration, which I leave to my other correſpondents, I ſhall only remark, that the preference ſeems to have been given to the cloſe breaſt-wheel, for ſtreams affording conſiderable quantities of water

water with a low fall, for the following reafons: 1. The load upon the bucket-wheel produces more friction, on the extremities of the axis, than is produced in the breast-wheel by the water, during its paffage down the channel. That this reafoning is good, may be fhewn by a very familiar experiment. Let any wheel be made to reft on its pivots, and then turned round; it will gradually lofe its velocity, by the friction, to a certain point, when it will ftop all at once. The fuddennefs of this ftop will indicate the magnitude of the friction. If this be fmall, the velocity will much more gradually decay than in the contrary cafe. On the other hand, to fhew the friction at the furface of a fluid, let a bowl be filled with water, and the fluid be ftirred, fo as to give it an horizontal motion; it will be found, that this large and heavy mafs, bearing on a furface fo very much exceeding that of the pivots of the wheel, will turn round for a confiderable time, and lofe its motion by very imperceptible gradations: whence it will follow, that the friction is much lefs. 2. In the ufual conftruction of overfhot-wheels, part of the fall is loft, in delivering the water, at a certain diftance below the vertex, and part of its action is loft, by its running out of the buckets, before it has arrived at the loweft point of the revolution. 3. It is a general notion, that large wheels go fteadier, from their operating in the manner of a fly, by means of their inertia; and wheels have been actually conftructed of iron, with a view to this effect. The breast-wheel is always larger than the overfhot-wheel, under like circumftances; and it may be conftructed, under certain limitations, of as large a fize as the engineer may think fit. 4. It is true, that a breast-wheel, of any fize, might be converted into a bucket-wheel, and carry down the water in its buckets, inftead of its being fuffered to flide down the ufual channel. But, when we imagine this converfion to take place, we fee, at once, that the action would be lefs fteady, from the fucceffive filling of the buckets, and that the lower portion of the loaded periphery would difcharge fome of its contents much earlier than the point at which the breast-water ftream would have ceafed to act.

The numerical computation of the difference of effect between one wheel and the other, according to the dimenfions given in the prefent communication, may be made from the data at page 466 of our prefent volume. It is there deduced, that one horfe will raife two hogfheads and a half of water, ten feet high, in a minute; and this effect, from the note at page 465, would require the fall of twice that quantity of water, as a power to overcome friction, and produce velocity of working. The ftream mentioned by my correfpondent muft, therefore, afford 415 cubic feet of water per minute, with a fall of 6 feet, if it be equal in power to fix horfes.

If certain dimenfions be affumed for the buckets of the overfhot-wheel, and an ordinary breast-wheel be compared with it, the quantity and fall of the water, ufelefsly expended on the former, will fhew the difference of effect.

W. N.

V.

On the Glaſs Trundles of Citizen RENAUT; *and the Duration of the Teeth of Mill-work. By C. B.*

To Mr. NICHOLSON.

SIR,

I OBSERVE in your Journal for January laſt, an account of Citizen Molard's having ſubſtituted glaſs in the place of iron for the trundles of lantern pinions, which he ſtates to be much preferable to the latter; as he ſays, caſt-iron wears out the wooden teeth they act againſt in about four months; whereas, the glaſs, in the experiment he made, wore the wooden teeth about one-twelfth of an inch only in eighteen months.

The uſe of glaſs, in the trundles of lantern pinions, is liable to many objections; experience, however, may point out methods of obviating many of theſe: but I think it right to mention, that Citizen Molard is extremely wrong, when he ſays, caſt-iron wears out the wooden teeth in four months; if it does ſo in France, it is otherwiſe in this country: I have had a mill of my own at work for theſe four years, in which wooden teeth act againſt iron, and I have never been obliged to get ſo much as a new tooth. Some months ſince I ſaw a ſteam engine, at Meſſ. Fiſh and Yates', in St. John-ſtreet, which turns machinery for his grinding of ſnuff and cutting tobacco. This machine was erected by Mr. Rennie, engineer of this city, in the year 1786, and has been conſtantly at work for about fifteen hours per day ever ſince; yet the wooden teeth, in the firſt motion, which act on caſt-iron, had not worn above one-ſixth of an inch; and the iron teeth did not ſeem to be worn $\frac{1}{50}$ part of an inch: and from every appearance, they will work without repair for ten or twelve years to come; as the workmen told me, that they had not, apparently, worn any thing for theſe ſix years paſt. They appeared to be very well executed; but not better than Mr. Rennie generally does ſuch work.

By publiſhing the above, you may, perhaps, prevent an ill-founded prejudice from being taken againſt the uſe of caſt-iron, in mill-work; which, from experience, I can ſay, is the beſt material I have known to be uſed.

I am, Sir,

Your moſt humble ſervant,

Feb. 4th, 1799. C. B.

I have viſited the manufactory of Meſſ. Fiſh and Yates', and find that the ſtatement given above is perfectly correct. It may be preſumed, that the iron trundles, mentioned by Citizen Renaut, at page 522, were either ill finiſhed, or rough from the forge; and, at all events, it appears, that they ought not to afford ground for any prejudice againſt good work in the uſual materials.

W. N.

nO

VI.

On Dr. Parr's Theory of Light and Heat. By a Correſpondent.

To Mr. NICHOLSON.

SIR,

ACCIDENT prevented my ſeeing, for ſome time, your Journal, publiſhed in December laſt; and I now take the liberty of making a few obſervations on ſome remarks of your correſpondent, reſpecting Dr. Parr's theory of light and heat*. He obſerves, " with reſpect to Dr. Parr's theory, that light and fire repel each other, when diſengaged from matter, it appears to me too fanciful to be ſolid, and too inconſiſtent to be ſupported," &c.—I do not quote the whole paſſage, becauſe I mean not to commence a controverſy upon the ſubject, as from the obſervation juſt tranſcribed, as well as from what follows, it is evident that your correſpondent has ſeen only the quotation in Dr. Pearſon's paper; and as the eſſay was publiſhed before the appearance of your Journal, I requeſt only room for a ſhort analyſis of the theory, that it may not be, as I ſuſpect it has been in the paſſage before me, miſunderſtood. I ſhall take up a little ſpace only in your valuable publication, but you will allow me to add, that M. Gadolin has ſince publiſhed an opinion not very different; and that, when Count Rumford ſpeaks of changes effected, not " by any chemical combination of the matter of light with ſuch bodies (as are expoſed to it), but merely by the heat which is *generated* or *excited* by the *light* which is *abſorbed by them*," he in reality uſes the ſame language.

Dr. Parr does not merely aſſert an opinion, but ſupports his hypotheſis by numerous and well-connected facts. Light he conſiders, with the chemical philoſopher of the preſent day, as a ſubſtance capable of combining with the various bodies it meets, producing by that means many chemical changes. Among theſe, he traces with peculiar care its influence upon vegetation; and from the decompoſition of water, afforded in the leaves of vegetables, ſeemingly by its power, he explains the great variety of gaſes expired by plants in different ſituations.

As light thus ſeparates oxygen, and as the latter probably contains heat in a leſs compounded ſtate than any other ſubſtance, he ſteps over the next difficulty, and ſuppoſes that it ſeparates heat. This is the only ſtep unſupported by immediate facts; but if aſſumed for a moment as an hypotheſis, the concurrence of numerous other facts will raiſe it to a higher rank. By what means light and heat are brought together in union, when they are ingredients in inflammable bodies, he pretends not to explain; but if they are really different elements, as is now generally believed, our not knowing the bond of union, is no proof againſt its exiſtence. In the examination of the various exploſive ſubſtances, Dr. Parr traces in each the ſource of its light and heat, and ſhows by what means the balance ſubſiſting between theſe active ingredients is deſtroyed; ſometimes by the addition of the one, ſometimes by that of the other principle. There are various exploſive ſubſtances which will not ex-

* Eſſay upon Light, in a miſcellaneous volume, publiſhed by a ſociety at Exeter.

plode

plode without light, however intense the heat, and *vice versâ*.—This part of the essay, where he examines the source of light in the composition of the body, and explains the different modes by which the violent separation is effected, as well as the consequences of the explosion, appears highly ingenious.

Such, Sir, is the outline of the paper which I think is misunderstood and misrepresented by your correspondent. The different parts can be only fairly appreciated by an examination of the facts. This is a business to which I cannot now attend, and which is not suited to your Journal. Your giving me a little room for this analysis will oblige, Sir,

Yours,

A CONSTANT READER.

SCIENTIFIC NEWS AND ACCOUNTS OF BOOKS.

Institute of Cairo.

(Continued from page 523, vol. II.)

THE second sitting was held on the 11th Fructidor, at seven in the morning, at which Citizen Andreossy made a report on the fifth question proposed by Buonaparte. He observed, in the first place, that Egypt possesses no sulphur, but formerly imported it from Venice. The charcoal is obtained by burning the lupine in a trench, and afterwards sifting it. The saltpetre is native, and is even said to be found in veins *(par veines)* round Cairo. It is refined in the same manner as (commonly) in Europe. It is a true nitrate of potash, and not of lime, as in France. The boiling is made with the stalks of Turkey-wheat, and it is purified with whites of eggs. The gunpowder is made by hand, and the workmen are naked. Each mortar contains fifteen pounds, and the process lasts seven hours. The pestles weigh from nineteen to twenty-five paros. A small quantity of water is added, and the granulation is performed by a sieve. The reporter affirms that *this powder is very good*; but that to render it still better it is necessary that the proportions of France should be followed. Two thousand cantars (were) formerly made at Cairo, of which much was sent to Leghorn. The beys had very little powder. Murat Bey had no more than 1,500 cantars. It would be easy, says the reporter, to augment this fabrication, and even to export large quantities to Europe.

At the same sitting, the citizen Monge read a memoir on the phenomenon, called *le mirage* by French seamen, and by ours *looming*. This effect of refraction was remarkably seen by the French, in the course of their march through the desert. Villages seen at a distance appeared elevated, and as if built on an island in the middle of a lake. The surface of this apparent water became narrower as they approached, until at length, when they were only at a small distance, it disappeared; but the same illusion presented itself, with regard to the next

next remote village. The author afcribes this phenomenon to a diminution of the denfity of the lower ftratum of the atmofphere, occafioned by evaporation of water from the fands. It is impoffible to deduce the particulars of his theory from the mere report; but it feems to be nearly the fame as that of Mr. Huddart*.

The third fitting was held on the 16th Fructidor. Berthollet read a memoir on the formation of ammoniac. Citizen Sulkowfky read a defcription of the route from Cairo to Salehia. Egypt has hitherto been known only on the banks of the Nile. The road followed by the French army in purfuit of Ibrahim Bey was entirely unknown. On going out of Cairo by the gate of Nafr, you enter the defert, where feveral forfaken houfes were obferved. The village of Elmaria, on this road, is the ancient Heliopolis. In another village, called Elmaria, there were thoufands of palm-trees. On the right of this road, there are nothing but deferts of fand; but, on the left, are many cultivated lands. The places which the army paffed are Lacoubey, Elhanea, Elmenia, Belbeys, Souva, Coraim, Salehia, and many others, which it merely paffed through in hafte.

Citizen Say fpoke of the methods of obtaining better means of grinding, and fhewed that water-mills ought to be conftructed.

C. Balholet (q. Berthollet?) read a memoir, in which he examines the gun-powder of Cairo. It contains only five parts out of thirty-two of falt-petre; the reft is fulphur, charcoal, earthy matter, and muriate of foda: fo that *the only means of rendering it ufeful, is to wafh out the falt, and manufacture it a fecond time.*

Citizen Monge read a memoir on the antient monuments of Cairo. The fafar, in the ftreet which leads from the inftitute to the caftle, prefents a vafe of granite with hieroglyphics within and without. It was a tomb, and refembles that of the mofque of Alexandria. Citizen Monge propofes that it be removed by the Inftitute, in order to convey it to the mufeum of France. Near the caftle, after having paffed the palace of Jofeph, there is the cell of a gate with hieroglyphics well preferved.

The fourth fitting, 21ft Fructidor. Citizen Sulkowfky read a memoir on a buft of Ifis. Say, another on the materials for fuel in Egypt. The commiffion for grinding, announced their intention of erecting a wind-mill. Citizen Geoffroy read a memoir on the oftrich.

The fifth fitting, 26th Fructidor. The commiffion for fuel, reported that the ftems of carthamus, reeds, and ftraw, may be ufed for heating ovens, at lefs charge than in France.—Buonaparte prefented the *Connoiffance des Temps* for the year VII, and invited the Inftitute to compofe an almanack. A commiffion was appointed for this object, confifting of Monge Beauchamp, Nouette, and Raphael. It was at the fame time decreed, that the three following almanacks fhould be united, namely, that of the Copts, the Muffulmen, and the French. Citizen Fourier read a memoir on the folution of algebraic equations. Citizen Parceval, a tranflation of a fragment of Taffo. Citizen Defgenettes read a differtation on the diarrhœa, the dyfentery, and the endemic opthalmia, which are more peculiarly the diforders of Egypt.

* Philofophical Journal, I. 145.——This curious fubject, which does not yet feem to be adequately explained in all its particulars, has employed the meditations of various obfervers; for fome of which, the reader may refer to the authors quoted at page 418 of our prefent volume.—N.

The sixth sitting, on the first complementary day. Citizen Beauchamp presented an annuary, ready for immediate printing, and also several astronomical observations. Citizen Berthollet read a letter from Citizen Laplace, announcing a correction in the metre. He also gave an account of the manufacture of indigo, and pointed out considerable improvements. Citizen Fourier communicated a project of a wind-mill, to water lands.

Sixth sitting, on the 6th Vendemaire, year VII. Citizen Poussielque presented a sketch of a new method of analytically demonstrating geometrical theorems, by Corancez the younger. Citizen Norry read a memoir on Pompey's pillar. Savigny described a new species of nymphæa. A commission was charged with the establishment of a school of design, among whom were Redouté and Norry. Citizen Costaz read a memoir on the colours of the sea, and Citizen Parceval another translation from Tasso.

Eighth sitting, Vendemaire, year VII. Fifty mummies of birds, sent to the Institute, were delivered for examination to a commission, composed of Buonaparte, Geoffroy, Dolomieu, and some others. Porte, a native of France, and inhabitant of Cairo, who is employed on Indigo, presented samples to the Institute. Citizen Larrey read a memoir on the opthalmia. Citizen Beauchamp read another on his voyage to Trebisond. He indicates the longitude of Ispahan, and observes, that the longitude of Trebisond is 37° 18′ 5″ from Paris, and not 43°, as Bonne affirms; which deducts more than eighty leagues from the Black Sea. Citizen Delisle read a memoir on the palm-tree, which bears the fruit called domm. It is the cassiophora of Theophrastus. Citizen Dolomieu read a memoir on the study of ancient and modern geography. He fixes the position of the ancient Alexandria between two hills of calcareous sand-stone, and explained the subsequent changes. He thinks the sea must have risen a foot since the time of the Ptolemies. Citizen Norry made a report concerning the school of design; and Citizen Parceval read a translation from Tasso.

Since the commencement of the VIIth year, a literary journal has appeared, under the title of *Decade Egyptienne, Journal litteraire et d'Economie politique.* The prospectus was signed *Tallien,* and is composed of thirty-eight pages. The Journal is to appear every decade, each number containing two and a half or three sheets, in octavo. The price per number is to be 1 franc French money, or 10 francs per 12 numbers. Subscriptions are taken by Citizen Marc-Aurell, printer to the army, quartier des François, at Cairo. The first number appeared the 10 Vendermaire, year VII.

An Account of the Operations carried on for accomplishing a Trigonometrical Survey of England and Wales, from the Commencement in 1784, to the End of the Year 1796. Begun under the Direction of the Royal Society, and continued by Order of the Honourable Board of Ordnance. First published in, and now received from, the Philosophical Transactions. By Captain William Mudge, F.R.S. and Mr. Isaac Dalby, Vol. I. Illustrated with 22 Copper-plates. London, printed by Bulmer, and Co. for Faden, Charing-Cross, 1799; 437 pages, Price 1l. 8s. boards.

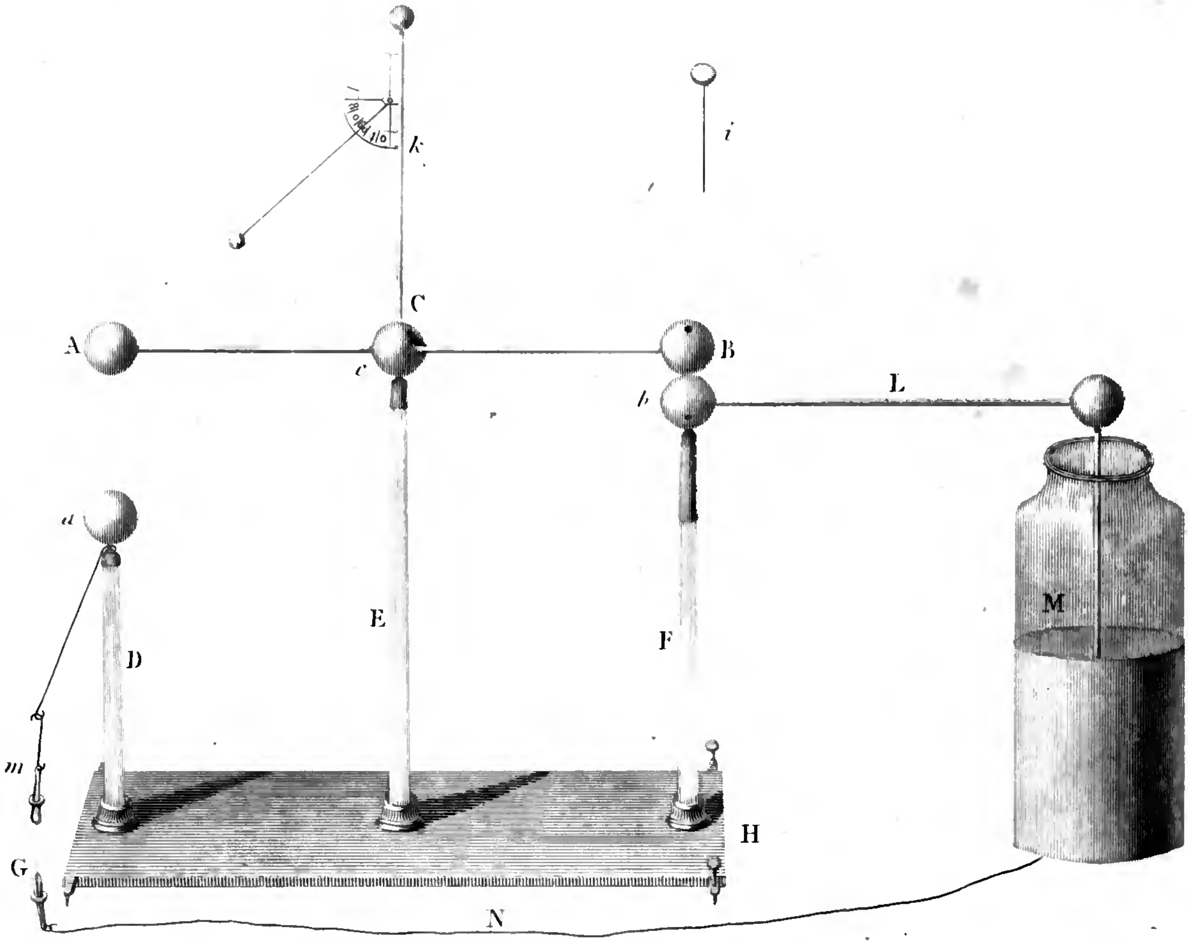
ALL Europe is acquainted with more or less of the particulars of this survey, which is a work of the utmost utility, and an honour to our government, and the parties to whom the execution

execution has been intrufted. The accounts having hitherto been publifhed only in the Philofophical Tranfactions, muft neceffarily have been of confined circulation, while the number and expence of the engravings have prevented any fatisfactory detail from appearing in other publications. The geographer and man of fcience will, confequently, learn with great pleafure, that the account of the operations, as far as they have yet been carried, together with the plates of inftruments, furveys, and other refults, are to be had, in a beautifully printed quarto volume, at a very eafy price.

The original defign had, for its immediate object, the afcertaining of data, by which the difference in longitude, between the obfervations of Greenwich and Paris, might be determined. Soon after the death of General Roy, the general furvey of the kingdom was commenced, of which accounts were publifhed, in the Tranfactions for 1796. And in 1798, Mr. Faden having determined on republifhing all the papers relative to this object, the prefident and council of the Royal Society rendered him the very effential fervice of furnifhing him with the original copper-plates, and the mafter-general of the ordnance granted him permiffion to reprint the accounts of the fubfequent trigonometrical furvey. This is now done under the fuperintendence of the able men whofe names appear in the title page, who have omitted no care to render this collection of equal value with the originals, and in fome few refpects more fo. An account of the changes, which the editors have made in the original papers, is given in the preface; where we alfo obferve, that Mr. Faden, with the permiffion of the Board of Ordnance, intends to publifh a very fuperior map of Kent, in the courfe of the prefent year, from documents fupplied by the labours of the gentlemen employed on the general furvey by government.

A public inftitution for diffufing the knowledge and facilitating the general introduction of ufeful mechanical improvements, and for teaching by courfes of lectures and experiments the application of fcience to the common purpofes of life, has been propofed to be eftablifhed, by fubfcription, in London, by fome gentlemen of the firft refpectability. I have feen a printed paper, containing the outlines of a plan, and a lift of gentlemen, who have fubfcribed fifty guineas each, for this purpofe. As the plan itfelf is ftill under deliberation, with regard to various effential particulars, and the prefent lift of fubfcribers does not contain the whole of the names, on account of the rapidity with which that lift increafes, I fhall enter into no further detail at prefent, than fimply to obferve, that the plan appears to have already met with a degree of fupport, which is no lefs honourable to the public fpirit of the fubfcribers, than diftinctive of its own value and importance.

Mr. Cuthbertson's new universal Electrometer.



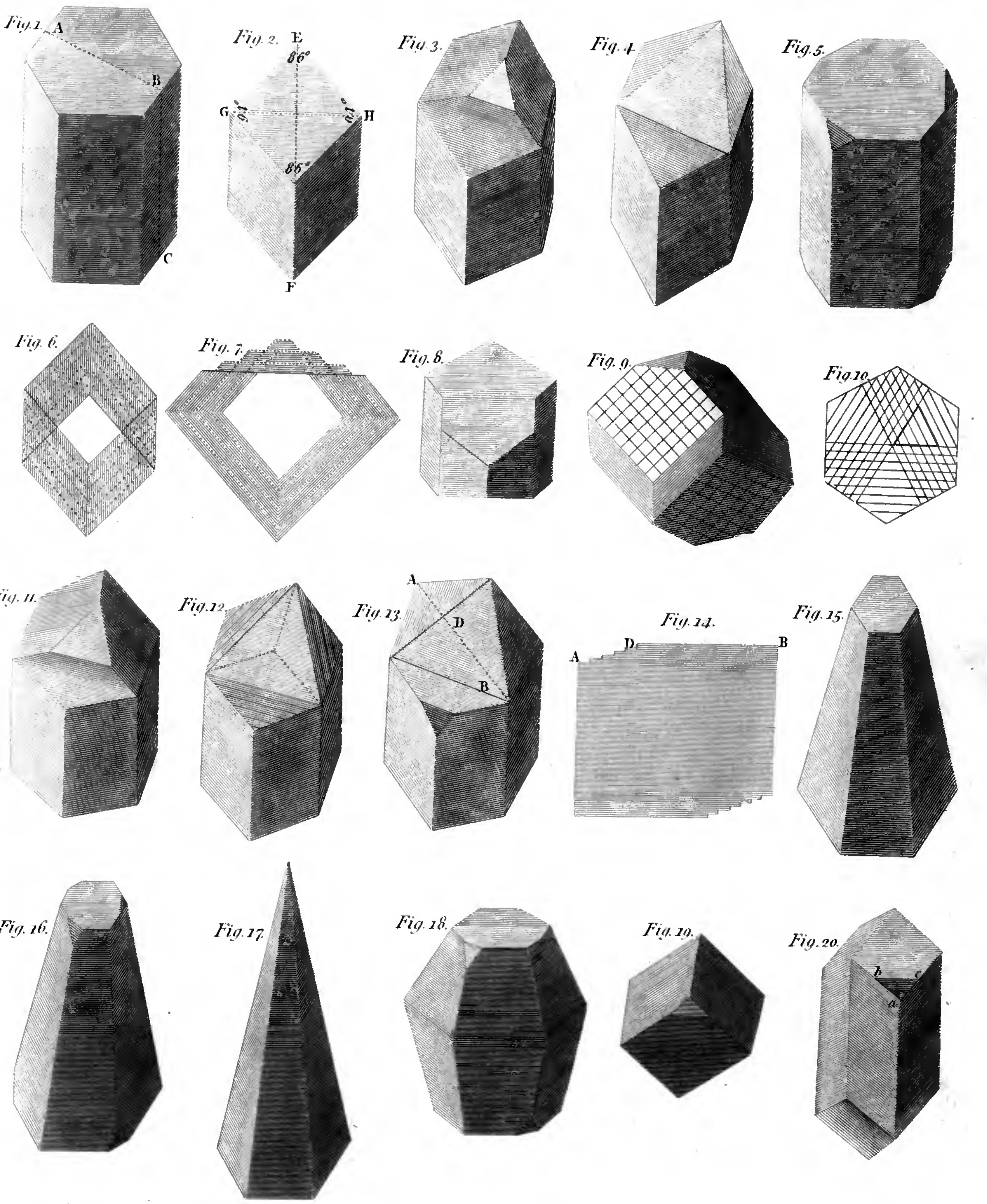


Fig. 1.
A
B
C
Fig. 2.
E
86°
G
94°
94°
H
86°
F
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.
Fig. 7.
Fig. 8.
Fig. 9.
Fig. 10.
Fig. 11.
Fig. 12.
Fig. 13.
A
D
B
Fig. 14.
A
D
B
Fig. 15.
Fig. 16.
Fig. 17.
Fig. 18.
Fig. 19.
Fig. 20.
b
c
a

INDEX

TO THE

FIRST AND SECOND VOLUMES.

D.

Descotils,

G.

H.

I.

J.

K.

Petrol

Q.

R.

Wadstrom,

ERRATA IN VOL. II.

Page.	Line.	
29	3	from the bottom, for *as*, read *aa*
46	13	after *page* 424, inſert *Vol. I.* The plate of *Apparatus* ſhould be numbered II.
123	18	for *periodical*, read *ſynodic*
	19	for 1 *d.* 8 *h.* 26 $\frac{6}{10}$ *m.* read 1 *d.* 18 *h.* 28 *m.* 36 *s.*
124	37	for *not quite* +37′, read *upwards of* — 30″
—	40	for 97½ *day's uſe*, read 117 *and* 97½ *day's uſe reſpectively*
—	*ult.*	for × read *of.*
125	11	What is ſaid of *the firſt and ſecond ſatellites*, reſpecting the annual error, is exactly true, only with reſpect to *the ſecond:*—the error of *the firſt* being *minus*, and in a ſmaller proportion.
264	—	In ſome copies, the figures *in the note* have ſlipped, and give an imperfect impreſſion. Read *the amount of Gold coined between the years* 1762 *and* 1772 *both incluſive, was* 8,157,203*l.* 15*s.* 6*d. and between* 1782 *and* 1792, *both incluſive, was* 19,675,666*l.* 14*s.* 6*d. and between* 1773 *and* 1777, *both incluſive, was* 195591,833*l.* 15*s.* And in the fourth line from the bottom, read *reckoned at* ½ *(half) per cent.*
275	8	read *the pipe A B F E*
439	8	from the bottom for *ſlate* read *plate*
488	14	from the bottom, eraſe the full ſtop after the word *poliſhed*, and add a full ſtop after *length*
490	7	for *offered*, read *afforded*
—	3	from the bottom for K B read K D
491	12	read *interior* part of the tube
—	3	from the bottom for D D F C read D B F C
558	6	for *Biringuccius* read *Beringuccius.*