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THE PHILOSOPHICAL MAGAZINE.

NUMBER XXV.

JUNE 1800.

ILLUSTRATED WITH

THE FOLLOWING ENGRAVINGS BY MR. LOWRY:

1. A Plate illustrating an unusual horizontal Refraction of the Air.
2. Apparatus employed by Mr. HOWARD for trying the Effects of fulminating Mercury.

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THE FOLLOWING ENGRAVINGS BY MR. LOWRY:

1. A Plate to illustrate the Theory of the Evening and Morning Dew.
2. The Vespertilio Plicatus.
3. A Mercurial Air-holder and Breathing Machine.

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COMMERCE.

NUMBER XXVII.

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ILLUSTRATED WITH

THE FOLLOWING ENGRAVINGS BY MR. LOWRY:

1. An improved Distilling and Rectifying Apparatus for saving Fuel.
2. A Machine by which the Strength of Horses may be employed to knead Paste for Bread.

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SEPTEMBER 1800.

ILLUSTRATED WITH

THE FOLLOWING ENGRAVINGS BY MR. LOWRY:

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2. A Meteorological Chart to illustrate the Influence of the Sun and Moon in producing a periodical Variation in the Barometer.
3. The grand antique Bacchanalian Vase, late in the Possession of Lord Cawdor, now at Woburn Abbey; and, Apparatus employed by Dr. Herschel in his Experiments on the different Refrangibility of the Rays of Light and Heat.

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Engd. by Mackenzie from an Original Picture.

Jos de Beauchamp

Astronomer.

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BY ALEXANDER TILLOCH,

MEMBER OF THE LONDON PHILOSOPHICAL SOCIETY, ETC. ETC.

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

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THE

PHILOSOPHICAL MAGAZINE.

JUNE 1800.

I. *Refearches refpecting the Compofition of Enamel.* By C. Clouet, *Affociate of the French National Inftitute**.

WHITE ENAMEL.

WHITE enamel, either for earthen-ware, or the purpofe of being applied on metals, is compofed in the following manner: You firft calcine a mixture of lead and tin, which may be varied in the following proportions; *viz.* for 100 parts of lead, 15, 20, 30, and even 40 of tin. A mixture of lead and tin calcines very eafily in contact with the air. As foon as this mixture is brought to a red heat, nearly a cherry colour, it burns like charcoal, and is calcined very fpeedily. The compofition which calcines beft, is that which in 100 pounds of lead contains from 20 to 25 of tin. The tin here meant is pure tin. In proportion as the calcination is effected, you muft take out the calcined part, and continue to oxydate the reft until the whole has become pulverulent. As fome fmall particles always efcape calcination, you muft expofe to the fire a fecond time the oxyd obtained in order to calcine it completely; which may be eafily known by its ceafing to fparkle; that is to fay, when you no longer fee any parts burn like coal, and when the whole appears of an uniform colour. When the proportion of tin exceeds 25 or 30, a ftronger fire is neceffary to produce the calcination. In a word, by varying the degrees of heat you will be able

* From the *Annales de Chimie.*

to discover that best suited to the mixture on which you operate.

A hundred parts of the calx above mentioned, which in the French potteries is called *calcine*, is generally taken with 100 parts of sand. From 25 to 30 pounds of sea-salt, or muriat of soda, are added: the whole is well mixed together, and it is fused in the bottom of a furnace in which potter's ware is baked. This matter is generally placed on sand, on lime quenched in the open air, or on ashes. The bottom of the mass is in general badly fused. This, however, does not prevent the matter, after it has been pounded, and applied on the articles, from becoming exceedingly white and hard in the furnace. When taken from the furnace it is not white; it is even often very black: in general it is marbled with black, gray, and white.

This process is that generally used in potteries. In the compositions destined for earthen-ware, the proportion of 25 parts of tin to 100 of lead is never exceeded: for common earthen-ware, the manufacturers are even satisfied with 15 of tin to 100 of lead. It may be easily seen, that if you wish to obtain an enamel whiter and more fusible, you must diminish the quantity of sand; but there is no necessity for augmenting that of the sea-salt, or muriat of soda: as the whiteness and opacity depend on the quantity of tin, you may use *calcine*, which contains 25 or 30 *per cent.* For example, 100 of such calcine, 60 of sand, and 25 of marine salt, give a composition exceedingly fusible.

But it is to be observed, that it is necessary to employ some further manipulations when you wish to have enamel proper for being applied on metal, and are desirous to give it all the perfection of which it is susceptible. In that case, you do not employ crude sand, but calcine it, in a strong heat, with a quarter of its weight of marine salt, either in a small quantity in a crucible, or on a large scale in a potter's furnace. If you wish to have a very fusible enamel, you may even add minium, or lead calcined by the former operation, and nearly as much sea-salt, that is to say, a fourth. You then obtain a white mass half fused and porous, which you pulverise, and employ in the composition of enamel instead of sand, and in the

the fame proportions as fand: you may even diminifh the quantity of this matter to 50 *per cent.* if you are defirous to obtain an enamel very fufible. This will depend alfo on the *calcine* employed; for that which is moft charged with tin is the leaft fufible.

When you wifh to have fluxes for the colours, you employ the fame compofitions before mentioned, except that you put little or no tin into the lead. In the latter cafe you muft generally employ minium. This flux is good for certain colours, but not for all. There are fome which become tarnifhed by fluxes, that contain the oxyds of lead. In that cafe, you muft make fluxes without oxyd of lead. Nitre and borax are generally ufed for making this glafs, but you add no calx of tin. The following are thofe which I have tried:

Three parts of filiceous fand, one of chalk, and three of calcined borax, give a matter proper to be ufed as a flux for purples, blues, and other delicate colours.

Three parts of white or flint glafs, one of calcined borax, a quarter of a part of nitre, one of the white oxyd of antimony made with nitre well wafhed, give an exceedingly white enamel, which may ferve alfo as a flux for purple, and particularly for blue.

Sixty parts of enamel fand or lefs, thirty of alum, thirty-five of fea-falt, and a hundred of minium, or any other oxyd of lead, give a white enamel when the fluxes do not predominate too much, and a gelatinous glafs when a great deal of fluxes has been added. This glafs is good for red, and the enamel may be applied to all kinds of clay capable of fuftaining a ftrong heat.

It is of great importance to remark, and to know, that the fand employed for enamel muft not be fand which contains only filex: fand of that kind alone is of no ufe. The fand proper for this purpofe is that which contains talc with filex. To make a fand proper for enamel and the fluxes of colours, &c. there muft be nearly one part of talc and three of filiceous fand.

What appears to me moft effential in regard to the fuccefs of enamel, is the choice of fand. It is very poffible to compofe pose this sand by art; and though I have not decomposed it, I have found by synthesis, that three parts of siliceous sand and one of talc form an excellent sand for enamel. From this it may be readily seen, that, to compose with facility sand for enamel, nothing is necessary but to determine, by a good analysis, the quantity of talc. This sand may be procured in places where earthen-ware is made. It may be easily known; for, besides the siliceous sand, which forms the greatest part of it, you may observe in it talcky particles in great abundance; and, to be good, it must contain nearly a quarter. When it does not contain a sufficient quantity, the enamel it produces fuses with more difficulty, and does not become smooth; it remains granulated and pitted. There are certainly some combinations of earth which may produce very good fluxes, either for enamel or for transparent colours. It might be attended with advantage to try some of these combinations. Ponderous earth (barytes) and lime fuse very well together: by adding a little silex, or a little magnesia, it is probable that an excellent matter might be produced. If this glass, composed of lime and barytes only, had sufficient solidity to resist the air and weak acids, there would be no necessity perhaps to add silex; but if the marine salt, as I am inclined to think, ought also to enter into the composition of this kind of glass, silex ought likewise to form a part of it. The experiments on this head, for the sake of trial, may be varied different ways. When the glass destined to serve as flux for colours is employed, it is customary, in order that they may be rendered more fusible, to add a little nitre and borax. The common borax of the shops contains an excess of soda, which, in my opinion, it would be of benefit to saturate with the nitric acid. I think also that the flux might be rebaked with the dose of nitre and borax, or of nitric borax, which might be added before being employed. It is only to colours such as purple and the oxyd of cobalt that nitre and borax are added.

I have tried to find a substitute for marine salt in the composition of white enamel. Potash produced only an ugly and ill fused gray mass, which acquired no lustre in the furnace; nitre produced a green mass, but exceedingly friable; sulphat

ſulphat of potaſh produced very nearly the ſame effect, only the maſs was a little whiter: but neither of theſe enamels was worth any thing. I did not try pure ſoda: I have, however, heard common ſoda extolled; but as it contains a great deal of marine ſalt, it muſt undoubtedly be on account of this ſalt that it produces a good effect. Pure ſoda may nevertheleſs be tried, either alone or with marine ſalt; it perhaps might produce no bad effect with potaſh.

I have tried alſo a mixture of equal parts of lime and argil, to which I added one part of ſilex, and likewiſe without ſilex; but this mixture did not ſupply the place of talcky ſand. This ſand is not in general found in grains; it exhibits itſelf moſt commonly under the form of a ſtone, ſuch as free-ſtone; but ſome of it is found alſo in grains.

We ſhould be much deceived in making white enamel were we to employ the oxyds of tin and lead ſeparately, as I have read in all the authors I could find who treat on the art of pottery. None of them ſay what they ought reſpecting enamel, nor even reſpecting the compoſition or nature of the earth proper for bearing an enamel.

It is eſſential that the lead and tin for making the oxyd deſtined to produce white enamel, ſhould be fuſed and mixed together before they are calcined; and if you wiſh that the enamel ſhould immediately acquire its full whiteneſs, it will be requiſite that the calcination ſhould be complete.

Biſmuth might perhaps be employed as a ſubſtitute for the lead, and it is not improbable that it would give a good product. Biſmuth alſo might be mixed with the lead in the following manner; *viz.* one part of lead, one of biſmuth, and one of tin: or other proportions might be employed; but I have not tried any others. As the oxyd of biſmuth, however, is exceedingly fuſible, I think it might be admitted, with great advantage, into certain fluxes. I have not tried what might be produced by the white calx of zinc, nor by that of tin, made by diſſolving i in the nitric acid or by detonation with nitre. A mixture of lead and tin, detonated with nitre, would be uſeful. Though the white calx of regulus of antimony made by nitre, and well waſhed, (diaphoretic antimony,) produces a very beautiful white enamel when

fuſed

fused with three parts of white glass, (which contains neither lead nor other metallic oxyds,) and one of glass of borax, with a half or fourth part of nitre; yet this calx, so white when mixed with the composition of enamel, made with enamel sand and the combined oxyd of lead and tin, instead of increasing, tarnishes the whiteness, and only gives a blueish enamel of a livid colour *. Perhaps enamels, completely made and mixed together in the first instance, would not produce the same effect; but this I never tried. I have, however, employed this composition as a flux for colours, which, applied afterwards on the enamel of earthen-ware, preserved its beauty. I put some of this pure enamel also over that of earthen-ware, and I think it preserved its whiteness.

The principal quality of good enamel, and that which renders it fit for being applied on baked earthen-ware, or on metals, is the facility with which it acquires lustre by a moderate heat, (a cherry-red heat, more or less, according to the nature of the enamel,) without entering into complete fusion †. Enamels applied to earthen-ware and metals possess this quality. They do not enter into complete fusion; they assume only the state of paste, but of a paste exceedingly firm; and yet when baked one might say that they had been completely fused.

There are two methods of painting on enamel: on raw or on baked enamel. Both these methods are employed, or may be employed, for the same object. Solid colours, capable of sustaining the fire necessary for baking the enamel ground, may be applied in the form of fused enamel on that which is raw, and the artist may afterwards finish with the tender colours. The colours applied on the raw material do not require any flux; there is one, even, to which silex must be added, that is, the calx of copper, which gives a very

* Antimony, employed in any manner as a glazing for earthen-ware, would be more dangerous than lead; even the latter should, if possible, be discarded.—EDIT.

† The ingenious author has omitted another principal quality. It ought never to contain such a portion of deliquescing salts, as to endanger its being afterwards injured by water. This takes place oftener than is generally suspected.—EDIT.

beautiful

beautiful green: but when you wish to employ it on the raw material, you must mix with it about two parts of its weight of silex, and bring the mixture into combination by means of heat. You afterwards pulverise the mass you have thus obtained, in order to employ it.

To obtain good white enamel, it is of great importance that the lead and tin should be very pure. If these metals contain copper or antimony, as is often the case, the enamel will not be beautiful: iron is the least hurtful.

OF COLOURED ENAMELS.

All the colours may be produced by the metallic oxyds. These colours are more or less fused in the fire, according as they adhere with more or less strength to their oxygen. All metals which readily lose their oxygen cannot endure a great degree of heat, and are unfit for being employed on the raw material.

Purple.

This colour is the oxyd of gold, which may be prepared different ways; as by precipitating, by means of a muriatic solution of tin, a nitro-muriatic solution of gold much diluted in water. The least quantity possible of the solution of tin will be sufficient to form this precipitate. The solution of tin must be added gradually until you observe the purple colour begin to appear: you then stop; and having suffered the colour to be deposited, you put it into an earthen vessel to dry slowly*. The different solutions of gold, in whatever manner precipitated, provided the gold is precipitated in the state of an oxyd, give always a purple colour, which will be more beautiful in proportion to the purity of the oxyd; but neither the copper nor silver, with which gold is generally found allayed, injure this colour in a sensible manner: it is changed, however, by iron. The gold precipitate which gives the most beautiful purple is certainly fulminating gold, which loses that property when mixed with fluxes. Purple is an abundant colour; it is capable of bearing a great deal of flux, and in a small quantity communicates its colour to a great deal of matter. It appears that saline fluxes are better suited

* The colour is always more beautiful, if the precipitate is ground with the flux before it has become dry.—EDIT.

to it than those in which there are metallic calces. Those, therefore, which have been made with silex, chalk, and borax, or white glass, borax, and a little white oxyd of antimony, with a little nitre, as I have already mentioned, ought to be employed with it. Purple will bear from four to twenty parts of flux, and even more, according to the shade required. Painters in enamel employ generally for purple a flux which they call brilliant white. This flux appears to be a semi-opake enamel, which has been drawn into tubes, and afterwards blown into a ball at an enameller's lamp. These bulbs are afterwards broken in such a manner that the flux is found in small scales, which appear like the fragments of small hollow spheres. Enamel painters mix this flux with a little nitre and borax. This matter, which produces a very good effect, I employed, without attempting to decompose it. It may be a very fusible common white enamel which has been blown into that form. It is to be remarked, that purple will not bear a strong heat.

Red.

We have no metallic oxyd capable of giving directly a fused red; that is to say, we have no metallic calces which, entering into fusion, and combining under the form of transparent glass with fluxes or glass, give directly a red colour. To obtain this colour it must be compounded different ways, as follows:—Take two parts, or two parts and a half (you may, however, take only one part,) of sulphat of iron and of sulphat of alumine; fuse them together in their water of crystallisation, and take care to mix them well together. Continue to heat them to complete dryness; then increase the fire so as to bring the mixture to a red heat. The last operation must be performed in a reverberating furnace. Keep the mixture red until it has every where assumed a beautiful red colour, which you may ascertain by taking out a little of it from time to time, and suffering it to cool in the air. You may then see whether the matter is sufficiently red: to judge of this it must be left to cool, because while hot it appears black. The red oxyds of iron give a red colour; but this colour is exceedingly fugitive; for, as soon as the oxyd of iron enters into fusion, the portion of oxygen which gives it its red colour leaves it, and it becomes black,

yellow,

yellow, or greenish. To preserve, therefore, the red colour of this oxyd in the fire, it must be prevented from vitrifying, and abandoning its oxygen; which may be accomplished by the method I have indicated. I have tried a variety of different substances to give it this fixity, but none of them succeeded except alum. The doses of alum and sulphat of iron may be varied. The more alum you add, the paler will be the colour. Three parts of alum to one of sulphat of iron give a colour which approaches flesh colour. It is alum also which gives this colour the property of becoming fixed at a very strong heat. This colour may be employed on raw enamel: it has much more fixity than the purple, but not so much as the blue of cobalt. It may be washed to carry off the superfluous saline matter, but it may be employed also without edulcoration; in that state it is even more fixed and more beautiful. It does not require much flux; the flux which appeared to me to be best suited to it, is composed of alum, minium, marine salt, and enamel sand. This flux must be compounded in such a manner as to render it sufficiently fusible for its object: from two to three parts of it are mixed with the colour. In general, three parts of flux are used for one of colour: but this dose may, and ought to be varied according to the nature of the colour and the shade of it required. Red calx of iron alone, when it enters into fusion with glass, gives a colour which seems to be black; but if the colour be diluted with a sufficient quantity of glass, it at last becomes of a transparent yellow. Thus, the colour really produced by calx of iron combined with glass is a yellow colour, but which being accumulated becomes so dark that it appears black. In the process above given for making the red colour, the oxyd of iron does not fuse: and this is the essential point; for, if this colour is carried in the fire to vitrification, it becomes black, or yellowish, and disappears if the coat be thin, and the oxyd of iron present be only in a small quantity.

Yellow.

Though yellow may be obtained in a direct manner, compound yellows are preferred; because they are more certain in their effect, and more easily applied than the yellow, which may be directly obtained from silver. The compound yellows are obtained in consequence of the same principles as the red colour of iron. For this purpose we employ metallic oxyds, the vitrification of which must be prevented by mixing with them other substances, such as refractory earths, or metallic oxyds difficult to be fused.

The metallic calces which form the bases of the yellow colours are generally those of lead; as minium, the white calx of lead, or litharge, the white calx of antimony, called diaphoretic antimony: that called crocus metallorum is also employed. This regulus, pulverised and mixed with white oxyd, gives likewise a yellow. The following are the different compositions used: one part of the white oxyd of antimony, one of the white oxyd of lead (or two or three); these doses are exceedingly variable; one part of alum, and one of sal-ammoniac. When these matters have been all pulverised, and mixed well together, they are put in a vessel over a fire sufficient to sublimate and decompose the sal-ammoniac; and when the matter has assumed a yellow colour, the operation is finished*. The calces of lead mixed in a small quantity either with silex or alumine, also with the pure calx of tin, exceedingly white, give likewise yellows. One part of the oxyd of lead is added to two, three, or four of the other substances above mentioned. In these different compositions for yellow, you may use also oxyd of iron, either pure, or that kind which has been prepared with alum and vitriol of iron: you will then obtain different shades of yellow. From what has been said, you may vary these compositions of yellow as much as you please. Yellows require so little flux, that one or two parts, in general, to one of the colour are sufficient; saline fluxes are improper for them, and especially those which contain nitre. They must be used with fluxes composed of enamel sand, oxyd of lead, and borax, without marine salt.

A yellow may be obtained also directly from silver. All these mixtures may be varied, and you may try others. For this purpose you may use sulphat of silver, or any oxyd of that metal mixed with alumine or silex, or even with both,

* This in colour-shops is called Naples yellow.

in

in equal quantities. The whole muft be gently heated until the yellow colour appear, and the matter is to be employed with the fluxes pointed out for yellows. Yellow of filver, like purple, cannot endure a ftrong heat: a nitric folution of filver may be precipitated by the ammoniacal phofphat of foda, and you will obtain a yellow precipitate, which may be ufed to paint in that colour with fluxes, which ought then to be a little harder.

Befides the methods above mentioned, the beft manner of employing the oxyd of filver is, in my opinion, to employ it pure: in that cafe, you do not paint, but ftain. It will be fufficient, then, to lay a light coating on the place which you wifh to ftain yellow, and to heat the article gently to give it the colour. You muft not employ too ftrong a heat: the degree will eafily be found by practice. When the article has been fufficiently heated, you take it from the fire and feparate the coating of oxyd, which will be found reduced to a regulus: you will then obferve the place which it occupied tinged of a beautiful yellow colour, without thicknefs. It is chiefly on tranfparent glafs that this procefs fucceeds beft. Very fine filver filings produce the fame effect: but what feemed to me to fucceed beft in this cafe, was fulphat of filver, well ground up with a little water, that it may be extended very fmooth. From what has been faid, it may readily be feen that this yellow muft not be employed like other colours; that it muft not be applied till the reft have been fufed; for, as it is exceedingly fufible, and ready to change, it would be injured by the other colours; and as the coating of filver which is reduced muft be removed, the fluxes would fix it, and prevent the poffibility of its being afterwards feparated. Working on glafs is not attended with this inconvenience, becaufe the filver-yellow is applied on the oppofite fide to that on which the other colours are laid.

Green.

Green is obtained directly from the oxyd of copper. All the oxyds of copper are good: they require little flux, which even muft not be too fufible: one part or two of flux will be fufficient for one oxyd. This colour agrees with all the fluxes, the faline as well as the metallic; which tends to vary

a little

a little the ſhades. I have already pointed out the method of employing theſe oxyds on raw enamel: were not that method only followed in this caſe, the oxyd of copper would extend, and ſpread itſelf; like a cloud, beyond the limits preſcribed for it.

A mixture of yellow and blue is alſo uſed to produce green. Thoſe who paint figures or portraits employ glaſs compoſed in this manner; but thoſe who paint glazed veſſels, either earthen-ware or porcelain, employ in general copper green.

Independently of the beautiful green colour produced by oxydated copper, it produces alſo a very beautiful red colour; but I do not know that it is employed on enamel. This beautiful red colour, produced by copper, is exceedingly fugitive. The oxyd of copper gives red only when it contains very little oxygen, and approaches near to the ſtate of a regulus. Notwithſtanding the difficulty of employing this oxyd for a red colour, a method has been found to ſtain tranſparent glaſs with different ſhades of a very beautiful red colour by means of calx of copper. The proceſs is as follows: You do not employ the calx of copper pure, but add to it calx of iron, which for that purpoſe muſt not be too much calcined; you add alſo a very ſmall quantity of calx of copper to the maſs of glaſs which you are deſirous of tingeing. This glaſs at firſt muſt have only a very ſlight tinge of green, inclining to yellow. When the glaſs has that colour you make it paſs to red, and even a very dark red, by mixing with it red tartar in powder, and even tallow. You muſt mix this matter well in the glaſs, and it will aſſume a very dark red colour. The glaſs ſwells up very much by this addition. Before it is worked it muſt be ſuffered to ſettle, and become compact; but as ſoon as it has fully aſſumed the colour it muſt be immediately worked, for the colour does not remain long, and even often diſappears while working; but it may be reſtored by heating the glaſs at the flame of a lamp. It is exceedingly difficult to make this colour well; but when it ſucceeds it is very beautiful, and has a great deal of ſplendour. By employing the calx of copper alone for the proceſſes above mentioned you will obtain, when you ſucceed well, a red ſimilar to the moſt beautiful carmine. The calx of iron changes the

the red into vermilion, according to the quantity added. If we had certain proceſſes for making this colour, we ſhould obtain all the ſhades of red from pure red to orange, by uſing, in different proportions, the oxyd of copper and that of iron. The calx of copper fuſes argil more eaſily than ſilex: the caſe is the ſame with calx of iron. If you fuſe two or three parts of argil with one of the oxyd of copper, and if the heat be ſufficient, you will obtain a very opake enamel, and of a vermilion red colour: the oxyd of copper paſſes from red to green through yellow; ſo that the enamel of copper, which becomes red at a ſtrong heat, may be yellow with a weaker heat. The ſame effect may be produced by deoxydating copper in different degrees: this will be effected according as the heat is more or leſs violent. The above compoſition might, I think, be employed to give a vermilion red colour to porcelain. The heat of the porcelain furnace ought to be of ſufficient ſtrength to produce the proper effect.

The calx of iron fuſed alſo with argil in the ſame proportions as the calx of copper, gives a very beautiful black. Theſe proportions may, however, be varied.

Blue.

Blue is obtained from the oxyd of cobalt. It is the moſt fixed of all colours, and becomes equally beautiful with a weak as with a ſtrong heat. The blue produced by cobalt is more beautiful the purer it is, and the more it is oxydated. Arſenic does not hurt it. The ſaline fluxes which contain nitre are thoſe beſt ſuited to it: you add a little alſo when you employ that flux which contains a little calcined borax or glaſs of borax, though you may employ it alſo with that flux alone.

But the flux which, according to my experiments, gives to cobalt blue the greateſt ſplendour and beauty, is that compoſed of white glaſs (which contains no metallic calx), of borax, nitre, and diaphoretic antimony well waſhed. When this glaſs is made for the purpoſe of being employed as a flux for blue, you may add leſs of the white oxyd of antimony: a ſixth of the whole will be ſufficient.

Violet.

Violet.

Black calx of manganese, employed with saline fluxes, gives a very beautiful violet. By varying the fluxes, the shade of the colour may also be varied: it is very fixed as long as it retains its oxygen. The oxyd of manganese may produce different colours; but for that purpose it will be necessary that we should be able to fix its oxygen in it in different proportions. How to effect this, as far as I know, has never yet been discovered.

These are all the colours obtained from metals. From this it is evident that something still remains to be discovered. We do not know what might be produced by the oxyds of platina, tungsten, molybdena, and nickel: all these oxyds are still to be tried; each of them must produce a colour, and perhaps red, which is obtained neither directly nor with facility from any of the metallic substances formerly known and hitherto employed.

GENERAL REMARKS.

Those who paint on enamel, on earthen-ware, porcelain, &c. must regulate the fusibility of the colours by the most tender of those employed, as for example, the purple. When the degree which is best suited to purple has been found, the other less fusible colours may be so regulated, (by additions of flux,) when it is necessary to fuse all the colours at the same time, and at the same degree of heat.

You may paint also in enamel without flux; but all the colours do not equally stand the heat which must be employed. If the enamel, however, on which you paint be very fusible, they may all penetrate it. This manner of painting gives no thickness of colour; on the contrary, the colours sink into the enamel at the places where the tints are strongest. To make them penetrate, and give them lustre, a pretty strong fire will be necessary to soften the enamel and bring it to a state of fusion. This method cannot be practised but on enamel composed with sand, which I call enamel sand, as already mentioned. It may be readily seen, also, that the colours and enamel capable of enduring the greateſt heat, will be the moſt ſolid, and the leaſt liable to be changed by the air. An account of the method of employing and baking enamel may be found in various works, and may be learned alſo by ſeeing the operations of enamellers.

II. *On a new fulminating Mercury.* By EDWARD HOWARD, *Eſq. F.R.S.**

SECTION I.

THE mercurial preparations which fulminate, when mixed with ſulphur, and gradually expoſed to a gentle heat, are well known to chemiſts: they were diſcovered, and have been fully deſcribed, by Mr. Bayen †.

MM. Brugnatelli and Van Mons have likewiſe produced fulminations by concuſſion, as well with nitrat of mercury and phoſphorus, as with phoſphorus and moſt other nitrats ‡. Cinnabar, likewiſe, is amongſt the ſubſtances which, according to MM. Fourcroy and Vauquelin, detonate by concuſſion with oxymuriat of potaſh §.

Mr. Ameilon had, according to Mr. Berthollet, obſerved, that the precipitate obtained from nitrat of mercury by oxalic acid fuſes with a hiſſing noiſe ‖.

SECTION

* From *Tranſactions of the Royal Society of London for* 1800.

† *Opuſcules Chimiques de Bayen*, Tom. I. p. 346, and note in p. 344.

‡ *Annales de Chimie*, Tom. XXVII. p. 74 and 79.

§ *Ibid.* Tom. XXI. p. 238.

‖ This fact has been miſrepreſented in the introduction to a work entitled The Chemical Principles of the Metallic Arts, by W. Richardſon, Surgeon, F. A. S. Sc. (page lvii.) The author, ſpeaking of the acid of ſorrel, ſays, " Klaproth of Berlin precipitated a nitrous ſolution of mercury with acid of wood-ſorrel neutraliſed with vegetable alkali. The white precipitate, well waſhed and dried, produced a fulminating noiſe not inferior to that of fulminating gold. Acid of ſugar, perfectly neutraliſed by vegetable alkali, produced the ſame precipitate, which, on expoſure to heat, exhibited the ſame fulminating power." I muſt confeſs, I have not been able to produce any ſuch fulmination. Mr. Richardſon has moreover given this ſuppoſed diſcovery to Mr. Klaproth; whereas Mr. Berthollet, when quoting the fact to which I ſuppoſe Mr. Richardſon intended to allude, obſerves, " Qu'on avoit déjà donné le nom d'argent fulminant

Section II.

But mercury, and moſt, if not all its oxyds, may, by treatment with nitric acid and alcohol, be converted into a whitiſh cryſtalliſed powder, poſſeſſing all the inflammable properties of gunpowder, as well as many peculiar to itſelf.

I was led to this diſcovery by a late aſſertion, that hydrogen is the baſis of the muriatic acid: it induced me to attempt to combine different ſubſtances with hydrogen and oxygen. With this view, I mixed ſuch ſubſtances with alcohol and nitric acid, as I thought might (by prediſpoſing affinity) favour, as well as attract, an acid combination, of the hydrogen of the one, and the oxygen of the other. The pure red oxyd of mercury appeared not unfit for this purpoſe; it was therefore intermixed with alcohol, and upon both nitric acid was affuſed. The acid did not act upon the alcohol ſo immediately as when theſe fluids are alone mixed together, but firſt gradually diſſolved the oxyd: however, after ſome minutes had elapſed, a ſmell of ether was perceptible, and a white denſe ſmoke, much reſembling that from the *liquor fumans* of Libavius, was emitted with ebullition. The mixture then threw down a dark-coloured precipitate, which by degrees became nearly white. This precipitate I ſeparated by filtration; and, obſerving it to be cryſtalliſed in ſmall acicular cryſtals, of a ſaline taſte, and alſo finding a part of the mercury volatiliſed in the white fumes, I muſt acknowledge I was not altogether without hopes that muriatic acid had been formed, and united to the mercurial oxyd. I therefore, for obvious reaſons, poured ſulphuric acid upon the dried cryſtalline maſs; when a violent efferveſcence enſued, and, to my great aſtoniſhment, an exploſion took place.

The ſingularity of this exploſion induced me to repeat the proceſs

fulminant au précipité du nitrate d'argent par l'acide oxalique, dans lequel M. Klaproth avoit découvert la propriété de fuſer avec vivacité lorſqu'on l'expoſe à la chaleur. M. Ameilon avoit auſſi, depuis longtems, fait connoître que l'acide oxalique communiquoit cette propriété au mercure, quoique moins fortement qu'à l'argent; mais cet effet (he continues) eſt fort éloigné de celui qu'on déſigne par la fulmination."—*Annales de Chimie*, Tom. I. p. 57.

procefs feveral times; and, finding that I always obtained the fame kind of powder, I prepared a quantity of it, and was led to make the feries of experiments which I fhall have the honour to relate in this paper.

SECTION III.

I firft attempted to make the mercurial powder fulminate by concuffion; and for that purpofe laid about a grain of it upon a cold anvil, and ftruck it with a hammer, likewife cold: it detonated flightly, not being, as I fuppofe, ftruck with a flat blow; for, upon ufing three or four grains, a very ftunning difagreeable noife was produced, and the faces both of the hammer and the anvil were much indented.

Half a grain, or a grain, if quite dry, is as much as ought to be ufed on fuch an occafion.

The fhock of an electrical battery, fent through five or fix grains of the powder, produces a very fimilar effect: it feems, indeed, that a ftrong electrical fhock generally acts on fulminating fubftances like the blow of a hammer. Meffrs. Fourcroy and Vauquelin found this to be the cafe with all their mixtures of oxymuriat of potafh *.

To afcertain at what temperature the mercurial powder explodes, two or three grains of it were floated on oil, in a capfule of leaf tin; the bulb of a Fahrenheit's thermometer was made juft to touch the furface of the oil, which was then gradually heated till the powder exploded, as the mercury of the thermometer reached the 368th degree.

SECTION IV.

Defirous of comparing the ftrength of the mercurial compound with that of gunpowder, I made the following experiment, in the prefence of my friend Mr. Abernethy.

Finding that the powder could be fired by flint and fteel, without a difagreeable noife, a common gunpowder proof, capable of containing eleven grains of fine gunpowder, was filled with it, and fired in the ufual way: the report was fharp, but not loud. The perfon who held the inftrument in his hand felt no recoil; but the explofion laid open the

* *Annales de Chimie*, Tom. XXI. p. 239.

upper part of the barrel nearly from the touch-hole to the muzzle, and ſtruck off the hand of the regiſter, the ſurface of which was evenly indented to the depth of 0,1 of an inch, as if it had received the impreſſion of a punch.

The inſtrument uſed in this experiment being familiarly known, it is therefore ſcarcely neceſſary to deſcribe it; ſuffice it to ſay, that it was of braſs, mounted with a ſpring regiſter, the moveable hand of which cloſed up the muzzle, to receive and graduate the violence of the exploſion. The barrel was half an inch in caliber, and nearly half an inch thick, except where a ſpring of the lock impaired half its thickneſs.

Section V.

A gun belonging to Mr. Keir, an ingenious artiſt of Camden-town, was next charged with 17 grains of the mercurial powder, and a leaden bullet. A block of wood was placed at about eight yards from the muzzle to receive the ball, and the gun was fired by a fuſe. No recoil ſeemed to have taken place; as the barrel was not moved from its poſition, although it was in no ways confined. The report was feeble: the bullet, Mr. Keir conceived, from the impreſſion made upon the wood, had been projected with about half the force it would have been by an ordinary charge, or 68 grains, of the beſt gunpowder. We therefore recharged the gun with 34 grains of the mercurial powder; and, as the great ſtrength of the piece removed any apprehenſion of danger, Mr. Keir fired it from his ſhoulder, aiming at the ſame block of wood. The report was like the firſt in Section IV, ſharp, but not louder than might have been expected from a charge of gunpowder. Fortunately, Mr. Keir was not hurt, but the gun was burſt in an extraordinary manner. The breech was what is called a patent one, of the beſt forged iron, conſiſting of a chamber 0,4 of an inch thick all round, and 0,4 of an inch in caliber; it was torn open and flawed in many directions, and the gold touch-hole driven out. The barrel, into which the breech was ſcrewed, was 0,5 of an inch thick; it was ſplit by a ſingle crack three inches long, but this did not appear to me to be the immediate effect of the exploſion. I think the ſcrew of

 the

the breech, being ſuddenly enlarged, acted as a wedge upon the barrel. The ball miſſed the block of wood and ſtruck againſt a wall, which had already been the receptacle of ſo many bullets, that we could not ſatisfy ourſelves about the impreſſion made by this laſt.

Section VI.

As it was pretty plain that no gun could confine a quantity of the mercurial powder ſufficient to project a bullet with a greater force than an ordinary charge of gunpowder, I determined to try its comparative ſtrength in another way.

I procured two blocks of wood, very nearly of the ſame ſize and ſtrength, and bored them with the ſame inſtrument to the ſame depth. The one was charged with half an ounce of the beſt Dartford gunpowder, and the other with half an ounce of the mercurial powder; both were alike buried in ſand, and fired by a train communicating with the powders by a ſmall touch-hole. The block containing the gunpowder was ſimply ſplit into three pieces; that charged with the mercurial powder was burſt in every direction, and the parts immediately contiguous to the powder were abſolutely pounded; yet the whole hung together, whereas the block ſplit by the gunpowder had its parts fairly ſeparated. The ſand ſurrounding the gunpowder was undoubtedly moſt diſturbed: in ſhort, the mercurial powder appeared to have acted with the greateſt energy, but only within certain limits.

Section VII.

The effects of the mercurial powder, in the laſt experiments, made me believe that it might be confined, during its exploſion, in the centre of a hollow glaſs globe. Having therefore provided ſuch a veſſel, 7 inches in diameter, and nearly half an inch thick, mounted with braſs caps, and a ſtop-cock, (ſee Plate I.) I placed ten grains of the mercurial powder on very thin paper, laid an iron wire 149th of an inch thick acroſs the paper, through the midſt of the powder, and, cloſing the paper, tied it faſt at both extremities, with ſilk, to the wire. As the incloſed powder was now attached to the middle of the wire, each end of which was connected

connected with the brafs caps, the packet of powder became, by this difpofition, fixed in the centre of the globe. Such a charge of an electrical battery was then fent along the wire, as a preliminary experiment * had fhown me would, by making the wire red-hot, inflame the powder. The glafs globe withftood the explofion, and of courfe retained whatever gafes were generated: its interior was thinly coated with quickfilver in a very divided ftate. A bent glafs tube was now fcrewed to the ftop-cock of the brafs cap, which being introduced under a glafs jar ftanding in the mercurial bath, the ftop-cock was opened. Three cubical inches of air rufhed out, and a fourth was fet at liberty when the apparatus was removed to the water-tub. The explofion being repeated, and the air all received over water, the quantity did not vary. To avoid an error from change of temperature, the glafs globe was, both before and after the explofion, immerfed in water of the fame temperature. It appears, therefore, that the ten grains of powder produced four cubical inches only of air.

To continue the comparifon between the mercurial powder and gunpowder, ten grains of the beft Dartford gunpowder were in a fimilar manner fet fire to in the glafs globe: it remained entire. The whole of the powder did not explode, for fome complete grains were to be obferved adhering to the interior furface of the glafs. Little need be faid of the nature of the gafes generated during the combuftion of gunpowder: they muft have been carbonic acid gas, fulphureous acid gas, nitrogen gas, and (according to Lavoifier †) perhaps hydrogen gas. As to the quantity of thefe gafes, it is obvious that it could not be afcertained; becaufe the two firft were, at leaft in part, fpeedily abforbed by the alkali of the nitre, left pure after the decompofition of its nitric acid.

SECTION VIII.

From the experiments related in the 4th and 5th fections, in which the gunpowder proof and the gun were burft, it might be inferred, that the aftonifhing force of the mercurial

* With Mr. Cuthbertfon's electrometer.

† See Lavoifier, *Traité elementaire*, p. 527.

powder

powder is to be attributed to the rapidity of its combuſtion; and a train of ſeveral inches in length being conſumed in a ſingle flaſh, it is evident that its combuſtion muſt be rapid. From the experiments of the 6th and 7th ſections, it is ſufficiently plain that this force is reſtrained to a narrow limit; both becauſe the block of wood charged with the mercurial powder was more ſhattered than that charged with the gunpowder, whilſt the ſand ſurrounding it was leaſt diſturbed; and likewiſe becauſe the glaſs globe withſtood the exploſion of ten grains of the powder fixed in its centre: a charge I have twice found ſufficient to deſtroy old piſtol barrels, which were not injured by being fired when full of the beſt gunpowder. It alſo appears, from the laſt experiment, that ten grains of the powder produced by ignition four cubical inches only of air; and it is not to be ſuppoſed that the generation, however rapid, of four cubical inches of air will alone account for the deſcribed force; neither can it be accounted for by the formation of a little water, which, as will hereafter be ſhown, happens at the ſame moment: the quantity formed from ten grains muſt be ſo trifling, that I cannot aſcribe much force to the expanſion of its vapour. The ſudden vaporiſation of a part of the mercury ſeems to me a principal cauſe of this immenſe, yet limited, force; becauſe its limitation may then be explained, as it is well known that mercury eaſily parts with caloric, and requires a temperature of 600 degrees of Fahrenheit to be maintained in the vaporous ſtate. That the mercury is really converted into vapour, by ignition of the powder, may be inferred from the thin coat of divided quickſilver, which, after the exploſion in the glaſs globe, covered its interior ſurface; and likewiſe from the quickſilver with which a tallow candle, or a piece of gold, may be evenly coated, by being held at a ſmall diſtance from the inflamed powder. Theſe facts certainly render it more than probable, although they do not demonſtrate, that the mercury is volatiliſed; becauſe it is not unlikely that many mercurial particles are mechanically impelled againſt the ſurface of the glaſs, the gold, and the tallow.

As

As to the force of dilated mercury, Mr. Baumé relates a remarkable inſtance of it, as follows:

" Un alchymiſte ſe préſenta à Mr. Geoffroy, et l'aſſura qu'il avoit trouvé le moyen de fixer le mercure par une opération fort ſimple. Il fit conſtruire ſix boîtes rondes en fer fort épais, qui entroient les unes dans les autres; la derniere étoit aſſujettie par deux cercles de fer qui ſe croiſoient en angles droits. On avoit mis quelques livres de mercure dans la capacité de la première: on mit cet appareil dans un fourneau aſſez rempli de charbon pour faire rougir à blanc les boîtes de fer; mais, lorſque la chaleur eut pénétré ſuffiſamment le mercure, les boîtes creverent, avec une telle exploſion qu'il ſe fit un bruit épouvantable: des morceaux de boîtes furent lancés avec tant de rapidité, qu'il y en eut qui paſſerent au travers de deux planchers; d'autres firent ſur la muraille des effets ſemblables à ceux des éclats de bombes *."

Had the alchemiſt propoſed to fix water by the ſame apparatus, the neſt of boxes muſt, I ſuppoſe, have likewiſe been ruptured; yet it does not follow that the exploſion would have been ſo tremendous: indeed it is probable that it would not; for, if (as Mr. Kirwan remarked to me) ſubſtances which have the greateſt ſpecific gravity have likewiſe the greateſt attraction of coheſion, the ſuppoſition that the vapour of mercury exceeds in expanſive force the vapour of water, would agree with a poſition of Sir Iſaac Newton, *that thoſe particles recede from one another with the greateſt force, and are moſt difficultly brought together, which upon contact cohere moſt ſtrongly* †.

Section IX.

Before I attempt to inveſtigate the conſtituent principles of this powder, it will be proper to deſcribe the proceſs and manipulations which, from frequent trials, ſeem to me beſt calculated to produce it.

100 grains, or a greater proportional quantity, of quickſilver (not exceeding 500 grains ‡) are to be diſſolved, with heat,

* *Chymie expérimentale et raiſonnée*, Tom. II. p. 393. *Paris*, 8vo. 1773.

† Newton's Optics, p. 372, 4th edition. *London* 1730.

‡ The reaſon of this limitation is not on account of any danger attending

heat, in a meafured ounce and a half of nitric acid *. This folution being poured cold *upon* two meafured ounces of alcohol †, *previoufly* introduced into any convenient glafs veffel, a moderate heat is to be applied until an effervefcence is excited. A white fume then begins to undulate on the furface of the liquor; and the powder will be gradually precipitated, upon the ceffation of action and re-action. The precipitate is to be immediately collected on a filter, well wafhed with diftilled water, and carefully dried in a heat not much exceeding that of a water-bath. The immediate edulcoration of the powder is material, becaufe it is liable to the re-action of the nitric acid; and, whilft any of that acid adheres to it, it is very fubject to the influence of light. Let it alfo be cautioufly remembered, that the mercurial folution is to be poured *upon* the alcohol.

I have recommended quickfilver to be ufed in preference to an oxyd, becaufe it feems to anfwer equally, and is lefs expenfive; otherwife, not only the pure red oxyd, but the red nitrous oxyd, and turpeth, may be fubftituted; neither does it feem effential to attend to the precife fpecific gravity of the acid or the alcohol. The rectified fpirit of wine and the nitrous acid of commerce never failed, with me, to produce a fulminating mercury. It is indeed true, that the powder prepared without attention is produced in different quantities, varies in colour, and probably in ftrength. From analogy, I am difpofed to think the whiteft is the ftrongeft; for it is well known, that black precipitates of mercury approach the neareft to the metallic ftate. The variation in quantity is remarkable; the fmalleft quantity I ever obtained from 100 grains of quickfilver being 120 grains, and the largeft 132 grains. Much depends on very minute circumftances. The greateft product feems to be obtained, when a veffel is ufed which condenfes and caufes moft ether to return into the mother liquor; befides which, care is to be

ing the procefs; but becaufe the quantities of nitric acid and alcohol required for more than 500 grains, would excite a degree of heat detrimental to the preparation.

* Of the fpecific gravity of about 1,3.

† Of the fpecific gravity of about ,849.

had, in applying the requifite heat, that a fpeedy and not a violent action be effected. 100 grains of an oxyd are not fo productive as 100 grains of quickfilver.

As to the colour, it feems to incline to black, when the action of the acid on the alcohol is moft violent, and *vice versâ*.

Section X.

I need not obferve, that the gafes which were generated during the combuftion of the powder in the glafs globe, were neceffarily mixed with atmofpheric air; the facility with which the electric fluid paffes through a vacuum made fuch a mixture unavoidable.

The cubical inch of gas received over water was not readily abforbed by it; and, as it foon extinguifhed a taper, without becoming red, or being itfelf inflamed, barytes water was let up to the three cubical inches received over mercury, when a carbonat of barytes was immediately precipitated.

The refidue of feveral explofions, after the carbonic acid had been feparated, was found, by the teft of nitrous gas, to contain nitrogen or azotic gas; which does not proceed from any decompofition of atmofpheric air, becaufe the powder may be made to explode under the exhaufted receiver of an air-pump. It is therefore manifeft, that the gafes generated during the combuftion of the fulminating mercury confift of carbonic acid and nitrogen gafes.

Section XI.

The principal re-agents which decompofe the mercurial powder are, the nitric, the fulphuric, and the muriatic acids. The nitric changes the whole into nitrous gas, carbonic acid gas, acetous acid, and nitrat of mercury. I refolved it into thefe different principles, by diftilling it pneumatically with nitric acid: this acid, upon the application of heat, foon diffolved the powder, and extricated a quantity of gas, which was found, by well known tefts, to be nitrous gas mixed with carbonic acid gas. The diftillation was carried on until gas no longer came over. The liquor of the retort was then mixed with the liquor collected in the receiver, and the whole faturated with potafh; which precipitated the mercury in a yellowish-brown powder, nearly as it would have done from a solution of nitrat of mercury. This precipitate was separated by a filter, and the filtrated liquor evaporated to a dry salt, which was washed with alcohol. A portion of the salt being re-fused by this menstruum, it was separated by filtration, and recognised, by all its properties, to be nitrat of potash. The alcoholic liquor was likewise evaporated to a dry salt, which, upon the affusion of a little concentrate sulphuric acid, emitted acetous acid, contaminated with a feeble smell of nitrous acid, owing to the solubility of a small portion of the nitre in the alcohol.

Section XII.

The sulphuric acid acts upon the powder in a remarkable manner, as already has been noticed. A very concentrate acid produces an explosion nearly at the instant of contact, on account, I presume, of the sudden and copious disengagement of caloric from a portion of the powder which is decomposed by the acid. An acid somewhat less concentrate likewise extricates a considerable quantity of caloric, with a good deal of gas; but, as it effects a complete decomposition, it causes no explosion. An acid diluted with an equal quantity of water, by the aid of a little heat, separates the gas so much less rapidly, that it may with safety be collected in a pneumatic apparatus. But, whatever be the density of the acid, (provided no explosion be produced,) there remains in the sulphuric liquor, after the separation of the gas, a white uninflammable and uncrystallised powder, mixed with some minute globules of quicksilver.

To estimate the quantity, and observe the nature, of this uninflammable substance, I treated 100 grains of the fulminating mercury with sulphuric acid a little diluted. The gas being separated, I decanted off the liquor as it became clear, and freed the insoluble powder from acid, by edulcoration with distilled water; after which I dried it, and found it weighed only 84 grains; consequently had lost 16 grains of its original weight. Suspecting, from the operation of the nitric acid in the former experiment, that these 84 grains (with the exception of the quicksilver globules) were oxalat

 of

of mercury, I digested them in nitrat of lime, and found my suspicion just. The mercury of the oxalat united to the nitric acid, and the oxalic acid to the lime. A new insoluble compound was formed; it weighed, when washed and dry, 48,5 grains. Carbonat of potash separated the lime, and formed oxalat of potash, capable of precipitating lime-water and muriat of lime; although it had been depurated from excess of alkali, and from carbonic acid, by a previous addition of acetous acid. That the mercury of the oxalat in the 84 grains, had united to the nitric acid of the nitrat of lime, was proved by dropping muriatic acid into the liquor from which the substance demonstrated to be oxalat of lime had been separated; for a copious precipitation of calomel instantly ensued.

The sulphuric liquor, decanted from the oxalat of mercury, was now added to that with which it was edulcorated, and the whole saturated with carbonat of potash. As effervescence ceased, a cloudiness and precipitation followed; and the precipitate, being collected, washed, and dried, weighed 3,4 grains: it appeared to be a carbonat of mercury. Upon evaporating a portion of the saturated sulphuric liquor, I found nothing but sulphat of potash; nor had it any metallic taste. There then remains, without allowing for the weight of the carbonic acid united to the 3,4 grains, a deficit from the 100 grains of mercurial powder, of 12,6 grains, which I ascribe to the gas separated by the action of the sulphuric acid. To ascertain the quantity, and examine the nature, of the gas so separated, I introduced, into a very small tubulated retort, 50 grains of the mercurial powder, and poured upon it three drams, by measure, of sulphuric acid, diluted with an equal quantity of water, and extricated the gas with the assistance of a gentle heat. I first received it over quicksilver, the surface of which, during the operation, partially covered itself with a little black powder *.

The gas, by different trials, amounted from 28 to 31 cubical inches; it at first appeared to be nothing but carbonic acid, as it precipitated barytes water, and extinguished a taper, without being itself inflamed, or becoming red. But, upon

* I cannot account for this appearance.

upon letting up to it liquid cauſtic ammoniac, there was a reſidue of from 5 to 7 inches of a peculiar inflammable gas, which burnt with a greeniſh blue flame. When I made uſe of the water-tub, I obtained, from the ſame materials, from 25 to 27 inches only of gas, although the average quantity of the peculiar inflammable gas was likewiſe from 5 to 7 inches; therefore, the difference of the aggregate product, over the two fluids, muſt have ariſen from the abſorption, by the water, of a part of the carbonic acid in its naſcent ſtate. The variation of the quantity of the inflammable gas, when powder from the ſame parcel is uſed, ſeems to depend upon the acid being a little more or leſs dilute.

With reſpect to the nature of the peculiar inflammable gas, it is plain to me, from the reaſons I ſhall immediately adduce, that it is no other than the gas (in a pure ſtate) into which the nitrous etheriſed gas can be reſolved, by treatment with dilute ſulphuric acid.

The Dutch chemiſts have ſhown *, that the nitrous etheriſed gas can be reſolved into nitrous gas, by expoſure to concentrate ſulphuric acid; and that, by uſing a dilute inſtead of a concentrate acid, a gas is obtained which enlarges the flame of a burning taper, ſo much like the gaſeous oxyd of azot, that they miſtook it for that ſubſtance, until they diſcovered that it was permanent over water, refuſed to detonate with hydrogen, and that the fallacious appearance was owing to a mixture of nitrous gas with an inflammable gas.

The inflammable gas ſeparated from the powder anſwers to the deſcription of the gas which at firſt deceived the Dutch chemiſts: 1ſt, in being permanent over water; 2dly, refuſing to detonate with hydrogen; and, 3dly, having the appearance of the gaſeous oxyd of azot when mixed with nitrous gas.

The gas ſeparable by the ſame acid, from nitrous etheriſed gas, and from the mercurial powder, have therefore the ſame properties. Every chemiſt would thence conclude, that the nitrous etheriſed gas is a conſtituent part of the powder, had the inflammable and nitrous gas, inſtead of the inflam-

* *Journal de Phyſique*, p. 250. October, 1794.

mable

mable and carbonic acid gas, been the mixed product extricated from it by dilute sulphuric acid.

It however appears to me, that nitrous gas was really produced by the action of the dilute sulphuric acid; and that, when produced, it united to an excess of oxygen present in the oxalat of mercury.

To explain how this change might happen, I must premise, that my experiments have shown me, that oxalat of mercury can exist in two, if not in three states.

1st, By the discovery of Mr. Ameilon already quoted, the precipitate obtained by oxalic acid, from nitrat of mercury, fuses with a hissing noise. This precipitate is an oxalat of mercury, seemingly with excess of oxygen. Mercury dissolved in sulphuric acid and precipitated by oxalic acid, and also the pure red oxyd of mercury digested with oxalic acid, give oxalats in the same state.

2dly, Acetat of mercury precipitated by oxalic acid, although a true oxalat is formed, has no kind of inflammability. I consider it as an oxalat with less oxygen than those above mentioned.

3dly, A solution of nitrat of mercury boiled with dulcified spirit of nitre, gives an oxalat more inflammable than any other: perhaps it contains most oxygen.

The oxalat of mercury remaining from the powder in the sulphuric liquor, is not only always in the same state as that precipitated from acetat of mercury, entirely devoid of inflammability, but contains globules of quicksilver; consequently, it must have parted with even more than its excess of oxygen; and, if nitrous gas was present, it would of course seize at least a portion of that oxygen. It is true, that globules of quicksilver may seem incompatible with nitrous acid; but the quantity of the one may not correspond with that of the other, or the dilution of the acid may destroy its action.

As to the presence of the carbonic acid, it must have arisen either from a complete * decomposition of a part of the oxa-

* Inflammable oxalat of mercury, made to fuse in a retort connected with the quicksilver tub, gives out carbonic acid gas.

lat;

lat; or, admitting the nitrous etherifed gas to be a conftituent principle of the powder, from a portion of the oxygen, not taken up by the nitrous gas, being united with the carbon of the etherifed gas.

Section XIII.

The dilute muriatic acid, digefted with the mercurial powder, diffolves a portion of it without extricating any notable quantity of gas. The diffolution evaporated to a dry falt, taftes like corrofive fublimate; and the portion which the acid does not take up, is left in the ftate of an uninflammable oxalat.

Section XIV.

Thefe effects all tend to eftablifh the exiftence of the nitrous etherifed gas as a conftituent part of the powder, and likewife corroborate the explanation I have ventured to give of the action of the fulphuric acid. Moreover, a meafured ounce and a half of nitrous acid, holding 100 grains of mercury in folution, and two meafured ounces of alcohol, yield 90 cubical inches only of gas; whereas, without the intervention of mercury, they yield 210 inches. Upon the whole, I truft it will be thought reafonable to conclude, that the mercurial powder is compofed of the nitrous etherifed gas, and of oxalat of mercury with excefs of oxygen.

1ft, Becaufe the nitric acid converts the mercurial powder entirely into nitrous gas, carbonic acid gas, acetous acid, and nitrat of mercury.

2dly, Becaufe the dilute fulphuric acid refolves it into an uninflammable oxalat of mercury, and feparates from it a gas refembling that into which the fame acid refolves the nitrous etherifed gas.

3dly, Becaufe an uninflammable oxalat is likewife left, after the muriatic acid has converted a part of it into fublimate.

4thly, Becaufe it cannot be formed by boiling nitrat of mercury in dulcified fpirit of nitre, although a very inflammable oxalat is by this means produced.

5thly, Becaufe the difference of the product of gas, from the fame meafures of alcohol and nitrous acid, with and without mercury in folution, is not trifling: and,

6thly,

6thly, Becaufe nitrogen gas was generated during its combuftion in the glafs globe.

Should my conclufions be thought warranted by the reafons I have adduced, the theory of the combuftion of the mercurial powder will be obvious to every chemift. The hydrogen of the oxalic acid, and of the etherifed gas, is firft united to the oxygen of the oxalat, forming water *; the carbon is faturated with oxygen, forming carbonic acid gas; and a part, if not the whole, of the nitrogen of the etherifed gas is feparated in the ftate of nitrogen gas; both which laft gafes, it may be recollected, were after the explofion prefent in the glafs globe. The mercury is revived, and, I prefume, thrown into vapour; as may well be imagined, from the immenfe quantity of caloric extricated, by adding concentrate fulphuric acid to the mercurial powder.

I will not venture to ftate, with accuracy, in what proportions its conftituent principles are combined. The affinities I have brought into play are complicated, and the conftitution of the fubftances I have to deal with not fully known. But, to make round numbers, I will refume the ftatement, that 100 grains of the mercurial powder loft 16 grains of its original weight by treatment with dilute fulphuric acid: 84 grains of mercurial oxalat, mixed with a few minute globules of quickfilver, remained undiffolved in the acid. The fulphuric liquor was faturated with carbonat of potafh, and yielded 3,4 grains of carbonat of mercury. If 1,4 grain fhould be thought a proper allowance for the weight of carbonic acid in the 3,4 grains, I will make that deduction, and add the remaining two grains to the 84 grains of mercurial oxalat and quickfilver; I fhall then have,

of oxalat and mercury - - -	86 grains
and a deficit, to be afcribed to the nitrous etherifed gas and excefs of oxygen -	14
	100

It may perhaps be proper to proceed ftill further, and recur to the 48,5 grains, feparated by nitrat of lime from the 84 grains

* Drops of water were obferved on the internal furface of the globe the day after feveral explofions had been produced in its centre.

grains of mercurial oxalat and globules of quickfilver, in the 11th fection. Thefe 48,5 grains were proved to be chiefly oxalat of lime; but they likewife contained a minute infeparable quantity of mercury, almoft in the ftate of quickfilver, formerly part of the 84 grains from which they were feparated. Had the 48,5 grains been pure calcareous oxalat, the quantity of pure oxalic acid in them would, according to Bergmann *, be 23,28 grains. Hence, by omitting the two grains of mercury in the 3,4 grains of carbonat, 100 grains of the mercurial powder might have been faid to contain, of pure oxalic acid 23,28 grains; of mercury 62,72 grains; and of nitrous etherifed gas and excefs of oxygen 14 grains. But, as the 48,5 grains were not pure oxalat, inafmuch as they contained the mercury they received from the 84 grains, from which they were generated by the nitrat of lime, fome allowance muft be made for the mercury fucceffively intermixed with the 84 grains and the 48,5 grains.

In order to make correfponding numbers, and allow for unavoidable errors, I fhall eftimate the quantity of that mercury to have amounted to two grains, which I muft of courfe deduct from the 23,28 grains of oxalic acid. I fhall then have the following ftatement:

That 100 grains of the fulminating mercury ought to contain,

of pure oxalic acid - - - - -		21,28 grains,
of mercury formerly united to the oxalic acid	60,72	
of mercury diffolved in the fulphuric liquor	2	
and of mercury left in the fulphuric liquor after the feparation of the gafes -	2	
total of mercury -		64,72
of nitrous etherifed gas and excefs of oxygen		14,
		100.

Since 100 grains of the powder feem to contain 64,72 grains of mercury, it will be immediately inquired, what becomes of 100 grains of quickfilver, when treated as directed,

* Bergmann, *de Acido Sacchari.* Opufcula. Tom. I. § 6. p. 248. *Leipzig*, 1788.

in the defcription of the procefs for preparing the fulminating mercury.

It has been ftated (in fection 9,) that 100 grains of quickfilver produce, under different circumftances, from 120 to 132 grains of mercurial powder; and if 100 grains of this powder contain 64,72 grains, 120 grains, or 132 grains muft, by parity of reafoning, contain 78,06 grains, or 85,47 grains; therefore, 13,34 grains, or 20,75 grains, more of the 100 grains are immediately accounted for; becaufe 64,72 grains + 13,34 grains = 78,06, and 64,72 grains + 20,75 grains = 85,47 grains. The remaining deficiency of 21,94 grains, or 14,53 grains, which, with the 78,06 grains, or 85,47 grains, would complete the original 100, of quickfilver, remains partly in the liquor from which the powder is feparated, and is partly volatilifed in the white denfe fumes, which in the beginning of this paper I compared to the *liquor fumans* of Libavius. The mercury cannot, in either inftance, be obtained in a form immediately indicative of its quantity; and a feries of experiments to afcertain the quantities in which many different fubftances can combine with mercury, is not my prefent object. After obferving, that the mercury left in the refiduary liquor can be precipitated in a very fubtle dark powder, by carbonat of potafh, I fhall content myfelf with examining the nature of the white fumes.

Section XV.

It is clear that thefe white fumes contain mercury: they may be wholly condenfed in a range of Woulfe's apparatus, charged with a folution of muriat of ammoniac. When the operation is over, a white powder is feen floating with ether on the faline liquor, which, if the bottles are agitated, is entirely diffolved. After the mixture has been boiled, or for fome time expofed to the atmofphere, it yields to cauftic ammoniac a precipitate, in all refpects fimilar to that which is feparated by cauftic ammoniac from corrofive fublimate.

I would infer from thefe facts, that the white denfe fumes confift of mercury, or perhaps oxyd of mercury, united to the nitrous etherifed gas; and that, when the muriat of ammoniac containing them is expofed to the atmofphere, or is

 boiled,

boiled, the gas ſeparates from the mercury; and the exceſs of nitrous acid, which always comes over with nitrous ether, decompoſes the ammoniacal muriat, and forms corroſive mercurial muriat or ſublimate. This theory is corroborated by comparing the quantity of gas eſtimated to be contained in the fulminating mercury, with the quantities of gas yielded from alcohol and nitrous acid, with and without mercury in ſolution; not to mention that more ether, as well as more gas, is produced without the intervention of mercury; and that, according to the Dutch chemiſts, the product of ether is always in the inverſe ratio to the product of nitrous etheriſed gas. Should a further proof be thought neceſſary, of the exiſtence of the nitrous etheriſed gas in the fulminating mercury, as well as in the white denſe fumes, it may be added, that if a mixture of alcohol and nitrous acid holding mercury in ſolution be ſo dilute, and expoſed to a temperature ſo low, that neither ether nor nitrous etheriſed gas are produced, the fulminating mercury, or the white fumes, will never be generated; for, under ſuch circumſtances, the mercury is precipitated chiefly in the ſtate of an inflammable oxalat. Further, when we conſider the different ſubſtances formed by an union of nitrous acid and alcohol, we are ſo far acquainted with all, except the ether and the nitrous etheriſed gas, as to create a preſumption, that no others are capable of volatiliſing mercury, at the very low temperature in which the white fumes exiſt, ſince during ſome minutes they are permanent over water of 40° Fahrenheit.

[To be concluded in the next Number.]

III. *On the Origin and Progreſs of the Manufacture of Pig-Iron with Pit-Coal; and Compariſon of the Value and Effects of Pit-Coal, Wood, and Peat-Char.* By Mr. DAVID MUSHET.

AFTER the former communications, relative chiefly to the manufacturing of pig-iron with pit-coal, it may not be amiſs to make a few general obſervations as to the origin of the proceſs, the different modes of conducting it, and, by drawing a compariſon between the reſults obtained at various

 periods,

periods, to prove the flow but fteady progrefs of improvement. A comparifon betwixt the effects produced by the ufe of charred wood and pit-coal in the blaft-furnace will alfo afford fubject for a few reflections.

It would appear that, towards the clofe of queen Elizabeth's reign, blaft-furnaces had been conftructed of fize fufficient to produce, with ores and the charcoal of wood, from 2 to 3 tons *per* day, or from 15 to 21 tons of pig-iron *per* week. Such great produces in iron at this early period were, however, confined to fituations where there was abundance of water, and where water-wheels and leathern bellows of confiderable magnitude were ufed. The more common modes of operation were confined to furnaces of an inferior fize, where air was fupplied by means of bellows excited by cattle or the labour of men.

As the manufacture of cannon, mortars, &c. was at this period confiderable, and as pit-coal had not yet been applied to any branch of the manufacturing of iron, it is probable that thefe articles would be caft from the large blaft-furnaces at once; the flame of wood not being well calculated for heating the large reverberating furnace where fuch heavy pieces of ordnance are now caft, and where feveral tons will be melted in one furnace by the flame of pit-coal; the non-application of pit-coal, either to the fmelting of ores or melting of iron, would greatly retard the improvement of the fmaller branches of the cafting department. The difficulty with which moft Englifh charcoal pig-iron melts, and its almoft immediate tendency to be converted into malleable iron in an air-furnace, would, in every attempt to improvement, prefent an infurmountable obftacle.

This would appear, amongft others of inferior note, to be the chief reafon why the improvements of cafting of every denomination have only kept pace with the original invention and improvement of caft iron with pit-coal char.

From the time of the invention by Dudley till about 60 years ago, a period of 120 years, the manufacture feems to have fallen afleep: and it is only during the laft 40 years that we can note a rapid improvement in the fabrication of caftings. The univerfal application of pig-iron, in almoft every

every poſſible ſhape, to the purpoſes of manufacture and the neceſſaries of life, wherein taſte, beauty, and durability are frequently united, belongs entirely to the preſent improved ſtate of the fabrication of pig-iron with the char of pit-coal.

In the early period above alluded to, by much the greateſt proportion of pig-iron was converted into bar-iron by means of the refinery fire, and, in many of the ſmall works, the pig-iron was *natured*, or malleabiliſed, before it was drawn from the furnace. The iron-maſters in general conſidered their own period as that of the higheſt pitch of advancement in the iron buſineſs. The erection of the great blaſt, capable, by powerful exertions, to make 21 tons weekly; the partial ſubſtitution of the refinery fire for the purpoſe of making malleable iron; and the abandonment of the hand and foot blaſt-bloweries where the other erections could be procured, were grounds ſufficient for the manufacturer to look with complacence upon the revolutions which had imperceptibly crept in, and kept pace with his practice.

The advantages which individuals derived from the manufacture of iron had induced many to engage in it. The buſineſs in point of extent ſeemed only limited to the ſupply of wood. New erections, for want of a proper ſupply of materials, became impracticable: thoſe already engaged were more anxious to preſerve their ſupply, however much circumſcribed, than to liſten to innovation, which, by ſubſtituting pit-coal for the charcoal of wood, would give to the new eſtabliſhing manufacturer a great ſuperiority in the market. It was alſo highly probable that many of the iron-works then eſtabliſhed were at a conſiderable diſtance from pit-coal, the univerſal introduction of which would have proved fatal to their intereſts. Under ſuch unfavourable circumſtances the diſcovery, or rather the practicability, of making pig-iron with pit-coal, we find announced by Simon Sturtevant, Eſq. in the year 1612, who, upon application, was favoured with a patent from king James for the excluſive manufacture of iron with pit-coal in all its branches for thirty-one years. In return, the ſaid Simon Sturtevant bound himſelf to publiſh his diſcoveries, which afterwards appeared in quarto under the title of his *Metallica*.

It

It is uncertain from what reaſons, but Mr. Sturtevant failed in the execution of his diſcoveries upon a large ſcale, and was obliged next year to render up his letters of monopoly.

The ſecond adventurer in this line we find to have been John Ravenſon, Eſq. who, like Sturtevant, was ſucceſsful in obtaining a patent for the new manufacture, but, like him alſo, was inadequate to the completion of it upon an extenſive ſcale. Ravenſon was alſo enjoined to publiſh his diſcoveries under the title of his *Metallica*, printed for Thomas Thorp *anno* 1613. Several other adventurers ſtepped forth, all of whom had the mortification of reſigning their patents without having contributed to the ſucceſs of the arduous undertaking.

In 1619 Dudley obtained his patent, and declared, that although he made only at the rate of three tons of pig-iron weekly, he made it with profit. The diſcovery was perfected at his father's works at Penſent in Worceſterſhire. This gentleman's ſucceſs in the various manufactures of iron with pit-coal had united not only all the proprietors of the charcoal iron trade, but many new adventurers, who wiſhed to ſhare in the emoluments of the new diſcovery. Their intereſt was ſo powerful as to limit Dudley's patent from 31 to 14 years. During the moſt of this period he continued to manufacture pig and bar-iron, and various caſtings, all of which he ſold much lower than the charcoal manufacturers. In the article of caſtings alone he muſt have had greatly the ſtart of the charcoal foundries, as the quality of carbonated coke pig-iron is far ſuperior to that of the charcoal iron of this country for the general purpoſes of caſting.

The ſuperior genius of Dudley was not always an object of paſſive indifference in the narrow eſtimation of the long eſtabliſhed manufacturers. The envy occaſioned by his uncommon ſucceſs produced at laſt a ſpirit of combination, which terminated in a hoſtile attack upon his devoted works. His improved bellows, furnace, forge, &c. all fell a prey to a lawleſs banditti, betwixt whom and its furious leaders no ſhades of diſtinction were viſible, but thoſe of avarice, ignorance, and the moſt contemptible prejudice.

To

To evade the mode of operation discovered by Dudley, or to introduce the making of pig-iron with pit-coal to greater advantage, a new plan was adopted by Captain Buck, Major Wildman, and others, in the forest of Dean, where they erected large air-furnaces, into which they introduced large clay pots, resembling those used at glass-houses, filled with various proportions of the necessary mixture of ores and charcoal. The furnaces were heated by the flame of pit-coal, and it was expected that, by tapping the pots below, the separated materials would flow out. This rude process was found entirely impracticable; the heat was inadequate to perfect separation, the pots cracked, and in a short time the process was abandoned altogether.

The misfortunes which successively befel the unfortunate Dudley, arising from rivalship in the iron business and his attachment to the royal cause during the civil wars, prevented his improvements from being closely followed up. The refusal of a new patent after the restoration prevented him from again entering into the business with his usual enterprise. From that period till about the year 1740, nothing of importance was done in the manufacture of coke pig-iron. The application of the steam-engine for raising and compressing air, no longer confined the manufacturer to local situations. Larger furnaces, with a proportionate quantity of blast, were introduced. Among the first effects, from 8 to 10 tons of pig-iron were produced weekly. Ever since, the weekly quantity has in general been increasing. The produce being considerably dependent upon the quantity of air used for reduction, it is now so well understood, that at some works the blowing-machine is calculated to produce frequently 40 tons of melting pig-iron *per* week at each furnace. At some iron-works in Wales, where oxygenated crude iron is manufactured purposely for converting into bar-iron, there are several instances of a furnace producing 70, 71, and 72 tons of metal weekly. This astonishing quantity forms a most striking contrast with the early exertions of Dudley, who conceived three tons a profitable produce, and whose greatest exertions never exceeded 7 tons of pig-iron weekly.

After this slight sketch of the progress of manufacturing pig-

pig-iron with pit-coal, it may be gratifying to make a few obſervations upon the proceſs conducted with the charcoal of wood. The ſuperior purity of the carbonaceous matter in wood, and its greater degree of inflammability, render this operation more ſimple than that performed with pit-coal. The former properly admitted of a ſmall furnace being uſed; the latter required a much leſs degree of blaſt to purify the ore, and give out ſatisfactory reſults both in the quantity and quality of the metal. Few charcoal furnaces exceeded the height of 20 feet, and many of them were from 12 to 15. A very ſmall column of blaſt was neceſſary to excite ignition, and produce the reduction of the materials. Lancaſhire and Cumberland ores were chiefly in uſe; their ſuperior richneſs in iron rendering them ſoon metalliſed when in contact with ignited charcoal of wood: 12 to 24 hours were ſufficient for this purpoſe, according to the ſize of the furnace and the quality of the pig-iron wiſhed. Not ſo in the manufacturing of coke pig-iron at preſent: the inferior quantity of iron which is contained in iron-ſtones, the impurity of the carbonaceous matter in pit-coal, eſtabliſhes a much leſs degree of affinity betwixt the metal and the principle of its reduction. The oxygen of the iron-ſtone is longer in being removed: this requires an additional period of contact. To procure this, the furnace muſt be heightened to 35 or 40 feet, and the deſcent of the materials protracted to three days.

It will be proper to exhibit the comparative effects produced by the char of pit-coal and that of wood. The following particulars will ſerve as data to make a calculation of the relative effects of the two different fuels with a charcoal furnace of 26 or 27 feet high, 9 feet wide at the boſhes, and blown by two 1¾ inch pipes placed along ſide of each other at the tuyere of the furnace. To make forge-pigs with this furnace, the following proportions have by experience been found requiſite for each charge:

2 ſacks of charcoal of 112 lb. each - -	224 lb.
7 meaſures of well dried Lancaſhire ore, each 112 lb.	784
Raw iron-ſtone - - - -	56
Limeſtone - - - - -	14

In

In 24 hours 18 of the above charges would have been confumed, and nearly 3 tons of forge-pigs produced.

The total quantity of char ufed for the quantity would be -	$2 \times 18 =$	36 cwt.
The total quantity of iron ore -	$7 \times 18 =$	126
——— of iron-ftone	$\frac{1}{2} \times 18 =$	9
——— of limeftone -	$\frac{1}{8} \times 18 =$	$2\frac{1}{2}$

We find therefore that 12 cwt. or 1344 lb. of the charcoal of wood, produces of forge pig-iron 2240 lb. or 1 ton; and that 126 cwt. of Lancafhire ore + 9 cwt. of iron-ftone, yielded of metal 60 cwt. This quantity, by an eafy mode of calculation, will be found to be at the rate of 44·4 *per* cwt.

When carbonated crude iron was produced, the charge was, 200 cwt. of wood char, 5 cwt. of ore, $\frac{1}{2}$ cwt. of iron-ftone, 14 lb. of limeftone.

The weekly quantity was always diminifhed in proportion to the reduction of the quantity of ore ufed. In 24 hours the quantity of the pig-iron produced, averaged 48 cwt. of carbonated crude iron. As this is the quality of metal which ferves as the bafis of the calculation in Number XX. of the Philofophical Magazine, it will be the moft proper ftandard to compare the widely different effects produced by wood and pit-coal. In that Number a table is given of the quantities of crude iron each variety of cokes produce, and which, for the fake of immediate comparifon, I fhall here again infert.

1 ton, or 2240 lb. clod-coal cokes produces of carbonated iron - -	1040 lb.
1 ton, or 2240 lb. fplint-coal cokes -	840
——— 2240 mixed coals - -	702
1 ton, or 2240 lb. of charcoal of wood, according to the proportions furnifhed above, will produce of carbonated iron -	2986
1 ton, or 2240 lb. of the fame, will produce of oxygenated crude iron for forge-pigs	3718; and
1 ton of carbonated pig-iron will require of the cokes of clod-coal - -	4824.6 lb.
——— fplint-coal -	5973·3
——— mixed -	7147·5
——— wood charcoal -	1680

 From

From this comparative view it is found that charcoal of wood produces triple effects, or carbonates three times the quantity of crude iron that the clod-coal cokes do; $3\frac{1}{2}$ times as much as the ſplint; and $4\frac{1}{2}$ times as much as a mixture of free and ſplint.

The next conſideration is the price of the two fuels. Charcoal of wood, about forty years ago, ſold at 2*s.* 6*d.* *per* ſack of 1 cwt. or 50*s.* *per* ton. 1 ton of good ſplint-coal cokes will be prepared on many of the preſent iron-work banks, labour included, at 11*s.* and at ſome places ſo low as 10*s.* At preſent the price of charcoal is upwards of 4*l.* *per* ton. So that at this period, although the effects of wood are $3\frac{1}{2}$ times thoſe of ſplint-coal cokes, yet the price of one ton of charcoal wood will purchaſe eight tons of cokes; the quantity of the former limited to, and produced from, land which might be better applied to the purpoſes of agriculture; the latter found in immenſe fields, and in tracts of country which are always augmented in value by the development of their mineral treaſure.

A charcoal blaſt-furnace which ſmelts the whole year round, and occaſionally makes forge-pigs and carbonated iron to the amount of 1000 tons annually, will conſume 14,000 ſacks of charcoal; which may be eſtimated at 1 cwt. *per* ſack, or 700 tons, or 1,568,000 lb. This divided by 18.75 lb. the pounds in a cubic foot, gives for the quantity of timber in cubical feet, 83.626. This is going upon a former calculation, and is ſuppoſing the wood to ſhrink but little during the proceſs of charring. In the preſent ſtate of the woods which are attached to iron-works, one acre will not yield more than 1200 cubical feet of timber. To enſure the annual ſupply, $\frac{83626}{1200} = 69.69$ acres of land would require every year to be cleared, or nearly 1400 acres would be requiſite to form, with proper care, an unfailing ſource of ſupply; and at the rate of 4*l.* *per* ton, the fuel would coſt 2,800*l.*

Let this be compared with a blaſt-furnace manufacturing the ſame quantity and quality of coke pig-iron. The average quantity for each ton will be nearly ſix tons of ſplint, or, as they loſe 50 *per cent.* in charring, 3 tons of cokes

× 1000 tons of pigs = 3000 tons of cokes, which at the higheſt price ſtated, 11 *s. per* ton, amounts to 1,650 *l.* or leſs than the charcoal, 1,150 *l.* 6000 tons of ſplint-coals will be eaſily procured from *one* acre of meaſurement where the ſeam meaſures 4½ to 5 feet thick. An obſervation ſtrikes us forcibly here, that, great as the conſumption of wood is, a ſufficient extent of country can from time to time replace any given quantity. No facts, however, which have hitherto come under our obſervation, warrant us to ſuppoſe the reformation of pit-coal.

I cannot cloſe this paper without bringing forward to notice a ſubſtance which has hitherto remained in a comparatively uſeleſs ſtate to ſociety. We have ſeen how many extenſive foreſts of excellent land have been cleared of their natural timber; how many thouſand acres reſtored to agricultural purpoſes, to ſupply the wants of the increaſing population of our country by the immediate neceſſities of the iron-works; while at the ſame time individuals received ample recompenſe for their neceſſary labour. May we not again look forward and truſt, that the rapid ſtrides which ſcience in our times is making towards the perfection of manufactures, will deviſe a method which will enable the manufacturer of iron to uſe peat and turf in his manipulations, either alone or as a mixture? Many ſchemes have been tried to introduce theſe cheap fuels to advantage. Mixtures of charcoal and well dried peats ſmelt and carbonate a conſiderable portion of metal. The weekly produce, however, ſeldom exceeded eight or ten tons; the extreme inflammability of the peat rendered it impracticable to uſe a blaſt ſufficiently compreſſed to produce a quantity without riſking the furnace. When peats were uſed alone, the produce diminiſhed ſo much as not to defray the labour. But the quality of the metal was always ſuperior, and nothing was wanting but a preparation of the fuel in ſuch a manner as to ſtand a heavier diſcharge of air.

Peats have alſo been uſed for the refining of caſt into bar-iron with various degrees of ſucceſs, none of them ſufficiently important in their advantages to conſtitute a permanent ſubſtitute for pit coal or the charcoal of wood. The qualities of peat-ground, or rather of the moſs from which peats are cut, are as various as thofe of pit-coal. That which feems to have undergone the moft complete decompofition, to have become denfe and compactly united in all its parts, forms the hardeft and moft durable peat. The blacker the colour, the more carbonaceous is the fuel, and the firmer is the char it produces. In mofs-grounds where the decompofition is not complete, the peat abounds more with earths and bog-iron ore. The remains of the vegetables are more entire, and its tendency to confolidation in burning is lefs. I have expofed fuch peat as this to a long drying heat, and found 100 parts of it, when thus dried, to be compofed of,

Water, hydrogen, and fulphur - -	72.8
Carbon - - - -	15.1
Oxyd of iron, with a flight mixture of alkaline falts - - - -	12.1
	100 parts.

The fuperior quality of peat or turf hardens by long keeping, and becomes lefs fragile than common pit-coal, and poffeffes alfo confiderable weight. The following may be confidered as an analyfis of what is commonly found in Lanarkfhire:

Water, hydrogen, and fulphur - -	72.6
Carbon - - - -	25.2
Brownifh magnetic afh, very light, and confiderably alkaline - - -	2.2
	100 parts.

The above quality of peats forms, by careful ignition, a char as firm as fome varieties of wood: and it is further thought poffible, that the mode of charring may be fo varied as, by caufing a part of the carbon to diffolve in the hydrogen, to form a bituminous cement for the whole, and render the char equal in compactnefs to that of wood.

The expenfe of cafting peats is the next object which ought here to be confidered. A man will with eafe caft 4000 of an ordinary fize *per* day. For this quantity are required two carriers, commonly women, and one dyker *.

* A perfon employed to pile up the peats to dry.

The

The peat, when thoroughly dried, will weigh 3 pounds; the product, therefore, will be $3 \times 4000 = 12{,}000$ lb. upwards of 5 tons. The total expenſe of this quantity for this ſhare of labour would be,

The man's wages - -	2*s.* 6*d.*
Two women ditto, 1*s.* each	2*s.* 0*d.*
Girl for dyking - -	0*s.* 6*d.*
Or equal to 1*s. per* ton.	5*s.* 0*d.*

Suppoſe that with additional labour, and the trouble of drying, a ſum equal to the firſt was incurred, ſtill we find that peats may be furniſhed in great plenty at 2*s. per* ton. By the former analyſis it appears that 4 tons would do more than make one ton of peat char, containing a portion of vegetable carbon ſuperior even to that contained in oak. This quantity of char would only coſt at moſt 8*s. per* ton, beſides the labour of charring, which, by proper erection and execution, would not coſt more than 2*s.* 6*d.* making in all 10*s.* 6*d.* The charcoal wood in this country at preſent ſells at the enormous price of 4*l.* 6*s.* 8*d. per* ton, and is ſcarce in point of quantity. Should it ever happen that peat moſs ſhall become a ſubſtitute for pit-coal, or obtain a preference from local advantages and cheapneſs by the manufacturers of iron, we ſhall then ſee a happy commencement to the moſt rational and moſt likely way of regenerating immenſe heaths of country, and of rendering up large tracts of ſurface, which for ages have remained uſeleſs to the wants of man; this way conquering one of the moſt pernicious evils which the progreſs of agriculture ever encountered.

It will, however, moſt likely be found, that a procedure upon ancient modes of manufacture already eſtabliſhed, will be productive of no greater ſucceſs than has hitherto been experienced. It is the buſineſs of the manufacturer to apply his knowledge of the principle of his proceſs to the exigency of the ſtructure and component parts of one or all of his materials. The general introduction of peat or turf for the manufacture of iron in preferable ſituations in our age, ought not to excite the ſame degree of wonder that Dudley's invention

invention with pit-coal did in the last century. Thus, it is very possible that the bare attempt will meet with as keen reprobation, dictated by the same narrow, selfish, and unenlightened spirit which marked that period, and which has hitherto clouded the genius of our manufacturing class.

IV. *Advantageous Method of separating the whole of the tartarous Acid from crude Tartar. By M.* Lowitz; *abridged by Dr.* Van Mons, *of Brussels**.

Mr. LOWITZ showed, in the year 1786, that a tartarous acid might be obtained by charcoal dust from crude tartar, as pure as that obtained from white tartar; but as the tartrite of potash, which is separated in this operation, is too strongly united to the colouring matter of the tartar to be able to get rid of it, the loss of this salt, necessarily occasioned by the process, destroyed all the advantage derived from it. Means, indeed, had been found to decompose entirely the acidulous tartrite of potash by the means of quicklime; but this process is accompanied with great inconveniencies, which M. Goettling attempted to obviate by proposing to decompose the tartrite by the acetite of the same earth.

This process, however, of Goettling cannot be employed but with white tartar, on account of the impossibility of depriving of its colour the acetite of potash which results from it. This induced M. Lowitz to endeavour to discover another method of decomposition.

This method consists in mixing together 15 lb. of crude tartar with 4lb. of chalk, and adding gradually 200 lb. of cold water. After the effervescence has ceased, the mixture must be heated; and at the moment when it boils, you throw into it sometimes a little tartar, and sometimes a little chalk, until no more effervescence is excited: you then filter the liquid, and precipitate it by means of a solution of the muriat of lime. You then decant the coloured liquid, and wash the united precipitates until they have been deprived of all the

* From the *Annales de Chimie*, No. 101.

separable

feparable part of their colour, and until they become infipid. This tartrite of lime is then decompofed by eight pounds of fulphuric acid diluted with as much water; and the acid feparated, is again diluted with 50 or 60 pounds of that liquid. It is neceffary that the acid fhould contain a flight excefs of fulphuric acid. The liquor is then feparated from the fulphat of lime; and after it has been mixed with from four to fix pounds of charcoal duft, it is evaporated to the point of cryftallifation by being kept in a continual ftate of ebullition. When the liquor has cooled, you feparate from it a fmall remnant of the fulphat of lime which has been depofited, and you fubject it to infenfible evaporation to make it cryftallife.

M. Lowitz mentions a mode of afcertaining the juft proportion of the free fulphuric acid which the liquor ought to contain in order to prevent a portion of the tartrite of lime from remaining diffolved in the ftate of acidulous tartrite, which caufes the tartarous acid, inftead of cryftallifing, to confolidate itfelf into a faline mafs, without form, and pulverulent. This method confifts in diluting in an ounce of water half a dram (gros) of the acid folution, and adding from 10 to 15 drops of the liquid acetite of lead. On dropping nitric acid into this mixture, if the liquid immediately becomes tranfparent, and remains fo for feveral hours, it is a proof that there is too little acid: if the liquid does not immediately become clear, there is too much: in the laft place, if, after having become clear, it again grows turbid at the end of a few minutes, the juft point has been attained. This trial ought to be made at the time when the liquor has reached the point of faturation to be expofed to cryftallife.

This operation will furnifh eight pounds of tartarous acid in very beautiful cryftals, and exceedingly white. To free it from any of the fulphuric acid which may be adhering to it, nothing will be neceffary but to throw over it a little cold water.

We would advife thofe who wifh to follow this procefs, to dilute the fulphuric acid with at leaft four times its weight of water, in order to prevent the de-hydrogenating action of that acid on the tartarous acid; not to dilute the acid with the

the whole quantity of water which the author prescribes, in order that the evaporation may be shortened; not to continue the ebullition when the liquor has acquired a certain degree of concentration, with a view to prevent the de-hydrogenating effect of the heat; and, in the last place, not to add an excess of sulphuric acid till the whole of the sulphat of lime, which this acid in acidulating would render soluble, is separated.

M. Lowitz justly observes, that the salt which he employs to decompose the tartrite of potash is found in all the laboratories of druggists as the remains of other operations, and that it is often thrown away as useless.

The author proposes to draw tartarous acid in the same manner, from tartrite of lime, which is precipitated in great abundance after the first crystallisations of the acidulous tartrite of potash and of soda, and which remains in the mother-water of that salt.

M. Lowitz terminates this memoir with advising to leave in the citric acid, as in the tartarous acid, a slight excess of the sulphuric acid; which is the more necessary in it, as the acidulous citrat of lime is much more soluble than the tartrite of that earth. M. Lowitz assures us that this practice contributes much towards obtaining the citric acid in large and regular crystals.

V. *On the Method of preparing Ether by the Muriatic Acid, or the Marine Ether of the Shops. By* J. B. VAN MONS*.

THE preparation of ether by the muriatic acid is a process which could not be brought to perfection till after the discovery of the oxygenated muriatic acid, and which required to have light thrown upon it by the new chemical doctrine of the French. It is not astonishing, therefore, that the efforts of the old chemists should have often miscarried in the preparation of this liquid.

Indeed, as the supposed radical of the simple muriatic acid, and the unoxygenated combinations of that acid, do not part

* Read in the Medical Society at Bruffels, 30 Nivose, an. 8.

with

with oxygen to any ſubſtance or union of known combuſtible ſubſtances; and, as etherification is an effect which depends on the action of this principle on the ſtate of the hydro-carbonous combination of alcohol, the converſion of the alcoholic liquor into ether could be hoped for only through the intervention of this principle. Ludolph *, Baumé, and others, attempted to make marine ether by means of common muriatic acid; but without ſucceſs, notwithſtanding the concentrated and active ſtate in which they have applied this acid on alcohol †.

Some chemiſts, however, without knowing that ſtate of combination of oxygen with the muriatic acid which conſtitutes the oxygenated acid, (probably with a view to preſent to the alcohol a highly concentrated acid,) employed for making marine ether different oxygenated metallic muriats. Thus, Baſil Valentine, and after him Wenzel, tried with that view the oxygenated muriat of antimony; Neumann, Bayen, de Bormes, the ſame muriat of zinc; Pott, that of arſenic; Rouelle, Courtanvaux, Spielmann, Erxleben, and lately the celebrated Klaproth ‡, that of tin; and others the oxygenated muriat of mercury, the red muriat of iron, &c. and the reſults have been attended with more or leſs ſucceſs; but it was neceſſary, as already ſaid, that the real preparation of this ether ſhould be preceded by the diſcovery of the oxygenated muriatic acid.

The different proceſſes, however, for the preparation of

* (Ludolph) Einleitung in di Chim. 1664. (Baumé) Diſſertation ſur l'Ether, 1767.

† Brugnatelli, no doubt, had heard mention made of the oxygenated acid, when he ſaid, that the muriatic acid with alcohol produced a perfect marine ether: "*l'oſſi muriatico ſomminiſtra coll' alcoole, un perfetto etere di oſſi muriatico.*" Elementi di Chimica, 1796, Vol. II. p. 56.

‡ (Valentin) Tolden, Letztes Teſtament, 1712, p. 218; (Wenzel) Lehre von der Verwandtſchaft, p. 148; (Neumann) Prælect. Chim. Leip. 1737; (Bayen) Mém. de l'Acad. des Sciences 1774, Vol. IV; (De Bormes) Mém. des Savans Etrangers, Vol. VI. p. 612; (Pott) Obſ. Chim. de Acid. Sal. Vinoſ. 1739, p. 117; (Rouelle) Journal des Savans 1759; (Courtanvaux) Mém. des Savans Etrangers, Vol. VI; (Spielmann) Inſtitut. Chim. 1763; (Erxleben) Anfangſgr. der Chem. 1775, p. 272; (Klaproth) Von Crell's Chem. Annal. 1796, Vol. I. p. 29.

marine ether by this acid are ftill imperfect, and, for the moft part, betray inexperience and fervile imitation. Scheele did not fail to apply foon the wonderful properties which he had difcovered in this acid (a difcovery by which he has been immortalifed) to the preparation of the liquid in queftion *. Befides other proceffes, he diftilled muriatic acid from off the oxyd of manganefe; received the oxygenated acid in a veffel containing alcohol; and rectified the liquid, thus impregnated and etherified, by a gentle heat. But thefe operations only furnifhed hints for conducting chemifts to a better procefs; for Hahnemann †, Weftrumb ‡, and Scheele himfelf agree, that by means of the oxygenated acid they obtained rather a vinous oil than real ether.

Pelletier §, and before him Weftrumb, as well as other German chemifts, adopted the procefs in which Scheele prefcribed the diftillation of alcohol from a mixture of the muriat of foda, fulphuric acid, and the oxyd of manganefe: but this manipulation, in the ftate of imperfection in which it has hitherto been performed, either gave a little ether by the action of the fulphuric acid, or furnifhed only the produce of the other proceffes employed by the Swedifh chemift. I fhall enter into fome details refpecting the phenomena which take place in thefe operations.

It is impoffible that this liquid, by the action of the oxygenated muriatic acid on the alcohol, fhould not experience, in a degree proportioned to its principles, that change from which ether refults. This, indeed, is what has been obferved by thofe who put in practice the methods of Scheele and Weftrumb. They firft faw the liquid float on the furface of the water; but when they wifhed to fubmit it to rectification, or when they did not feparate it foon enough from the water impregnated with the oxygenated acid, they found their ether converted into oil, firft liquid and floating on the water, when they feparated it from the alcohol in which it was diffolved; but afterwards becoming thick, and falling to the bottom of

* Koning. Vetenfkaps-Academiens nya Handlingar, 1782, Vol. III. p. 35.

† Phyf. Chem. Abhand, Vol. I. p. 35.

‡ Laborant, in Groffen, Vol. I. p. 236.

§ Mem. de Chimie, Vol. I. p. 39.

the

the ſame liquid. This olefication of ether is a natural effect of the oxygenating action of the acid, when care has not been taken to ſtop this operation immediately after the ether is formed. A like action is exerciſed by the air of the atmoſphere on natural oils.

The oxygenated muriatic acid, which paſſes at the ſame time as the ether, continues to act on that liquid, and makes it undergo a modification of compoſition that brings it to the ſtate of oil, to which it had before nearly approached. This effect, as I have ſaid, takes place when the ether is left for ſome time mixed with the acid it has carried along with it, or when it is rectified without ſeparating it from that acid. It may readily be conjectured, that the continuation of the ſame labour ought to make the liquid oil paſs to the ſtate of thick oil, and that it would bring the latter to the ſtate of greaſe, and, conſequently, to that of wax. The ſame thing takes place with the olefying gas of the Dutch chemiſts, which from ethereous gas transforms itſelf, by the re-action of the oxygenated muriatic acid gas on the ſtate of the combination of its principles, into an oil, firſt liquid and floating on the water, but which afterwards becomes inſpiſſated to olefication when you add only a few bubbles of the latter gas in exceſs. When this exceſs of oxygenated gas was conſiderable, I have often ſeen the ethereous oil converted into a real white greaſe, opake, and of the conſiſtence of half melted tallow. I informed the public, more than ten years ago, that the oxygenated muriatic acid gas, kept over oil of *colſat* (coleſeed), converted it into a waxy matter, taking from it its ſmell and colour, and rendering it perfectly white. All theſe effects depend on withdrawing a portion of the hydrogen; ſo that ether is oil, plus a certain portion of hydrogen; oil is greaſe, plus that proportion, &c. Ether made by the ſulphuric acid undergoes, but more ſlowly, the ſame transformation into oil, in the like manner as that made by the nitric acid, which experiences this change almoſt as ſpeedily as muriatic ether does. Nothing is ſo eaſy to be conceived as the de-hydrogenation, at all temperatures, effected by condenſed oxygen.

Thoſe who have obſerved this ready olefication of marine ether muſt have given a preference, the motives of which are not difficult to be diſcovered, to a certain proceſs which preſcribes that the alcohol ſhould be mixed with the ſulphuric acid before this acid is poured over the two other ingredients, muriat of ſoda and oxyd of manganeſe. There is formed by the aid of the heat which is excited in this mixture, as Fourcroy and Vauquelin obſerved, a quantity of ether by the ſulphuric acid, which firſt paſſes in the diſtillation, and which ſuffers itſelf to be ſeparated with more certainty, being leſs liable to be attacked by the oxydating principle than marine ether. This ſulphuric ether muſt have often been conſidered by inexperienced judges as real muriatic ether; from which, however, it differs by a ſmell and taſte peculiar to itſelf.

In this ſtate of the ſcience reſpecting the preparation of marine ether, I have thought it my duty to communicate to the Society a proceſs which removes every difficulty in this preparation.

Place in a ſand-bath, gently heated, the retort of Woulfe's apparatus as improved by Lavoiſier, and compoſed only of a balloon and two bottles: introduce into it 1.00 of any weight of the muriat of ſoda, perfectly dry; and in the receiver and the two bottles diſtribute the ſame weight of good alcohol. After having exactly luted the joinings, and furniſhed the laſt bottle with a tube of ſafety, pour upon the ſalt in the retort 0·50 of concentrated ſulphuric acid, and leave the operation to proceed cold for five or ſix hours. Then make a moderate fire, which muſt be augmented by degrees, to bring the bottom of the capſule of the bath to a ſlight degree of red heat. The muriatic acid in the natural ſtate is gaſeous: in that ſtate it paſſes over and diſſolves in the alcohol. It is of benefit in this operation to plunge the tubes of communication to a certain depth in the alcohol, that the gas may experience a condenſation, which greatly contributes to its abſorption. This diſpoſition of the tubes, when the diſengagement is rapid, makes the alcohol ſometimes paſs from the one bottle into the other; in which caſe the poſition of the bottles muſt be changed in ſuch a manner as to make that which contains the greater quantity of liquid to communicate with the receiver. It may readily be conceived that this paſſage of the alcohol from one bottle to another, and even its elevation to ſome centimetres above its level, is neceſſarily the effect of a conſiderable degree of compreſſion experienced by the gas, and which re-acts, or is tranſmitted by the latter to the liquid; for, even by applying to the explanation of the phenomena of a liquid in motion the general principle of mechanics, that the maſſes and velocities are inverſely proportional, it is neverthelefs true, that at reſt the force which raiſes and maintains a liquid above its level, cannot be inferior to the weight of a maſs of that liquid having for height its elevation in the tube, and for baſe the internal diameter of the bottle; or, to make uſe of common language, having for bulk its elevation multiplied by its diameter: but a compreſſing force, to be exerciſed by the gas, ought to be experienced by it; and this compreſſion, by concentrating its parts, diſpoſes it to diſſolve in the alcohol. I have indulged in this ſhort digreſſion, which may appear foreign to my object, on account of its numerous applications in the practice of the chemiſtry of gaſes.

When the whole muriatic acid has paſſed, the liquors in the different bottles are to be united, and poured into the retort, after firſt taking out the ſalt. It forms a muriatic alcohol exceedingly concentrated. You muſt add in the retort 0·20 of the oxyd of manganeſe in very fine powder; and put into the recipient, and the two bottles, a certain quantity of a ſolution of cauſtic potaſh. You then diſtil at a gentle heat, conducted with care. It is here ſeen that the alkaline liquor ſerves to enchain the oxygenated acid which is in exceſs at the formation of the ether, and that it prevents the action which oleſies this liquid. It is chiefly in this practice that the part of my method which preſerves the ether conſiſts.

Notwithſtanding this precaution, it is not poſſible, however, to prevent a greater or leſs portion of the ether from being decompoſed by the acid, which muſt traverſe that liquid to reach the alkaline liquor: beſides, the oxygenated muriatic acid united to the alkali, continues no leſs to act as an oxygenating ſubſtance in its ſtate of muriat; for, properly,

perly, it is the action of the acid, and not that of the oxygen, which the alkali neutraliſes. I have freed the latter entirely from theſe inconveniencies by receiving the ether on liquid ammonia, which is burnt by the oxygen in proportion as it comes in contact with it: but this manipulation is accompanied with too many dangers for me to venture to recommend it in general.

Another method of ſeparating the ether from the oxygenated acid would be, to receive this liquid on water; to remove it in proportion as it floats on the ſurface, by means of a ſyphon or pump; to ſhake it with the alkaline ſolution; and to decant it again on pure water. You then obtain the whole ether, which may be afterwards ſeparated from the water by rectification.

Ether, freed in any manner from its acid, muſt finally be mixed with twice its bulk of water, and rectified at a heat equal to that of Guyton's economical furnace.

A marine ether may be compoſed by one operation if you diſtill, at a boiling heat, a mixture of alcohol and the oxygenated muriat of potaſh in the proportion of 1·00 to 0·25.

It is not neceſſary to recommend to thoſe who are ever ſo little habituated to this kind of labour, to cool, from time to time, in the courſe of the diſtillation, the receiver, by applying to it a wetted ſponge.

VI. *Obſervations upon an unuſual horizontal Refraction of the Air; with Remarks on the Variations to which the lower Parts of the Atmoſphere are ſometimes ſubject. By the Rev.* S. VINCE, *A.M. F.R.S. and Plumian Profeſſor of Aſtronomy and experimental Philoſophy in the Univerſity of Cambridge* *.

THE uncertainty of the refraction of the air near the horizon has long been known to aſtronomers, the mean refraction varying by quantities which cannot be accounted for from the variations of the barometer and thermometer; on

* From the *Tranſactions of the Royal Society for* 1799.

which

which account, altitudes of the heavenly bodies which are not more than 5° or 6° ought never to be made uſe of when any conſequences are to be deduced from them. The cauſe of this uncertainty is probably the great quantities of groſs vapours, and exhalations of various kinds, which are ſuſpended in the air near to the earth's ſurface, and the variations to which they are ſubject; cauſes, of which we have no inſtruments to meaſure the effects which they produce in refracting the rays of light. In general, the courſe of a ray paſſing through the atmoſphere, is that of a curve which is concave towards the earth, the effect of which is to give an apparent elevation to the object; and thus the heavenly bodies appear above the horizon, when they are actually below it; but it will not alter the poſition of their parts in reſpect to the horizon; that is, the image of the higheſt part of the object will be uppermoſt, and the image of the loweſt part will be undermoſt. The figures, however, of the ſun and moon, when near the horizon, will ſuffer a change, in conſequence of the refraction of the under limb being greater than that of the upper; from which they aſſume an elliptical form, the minor axis of which is perpendicular to the horizon, and the major axis parallel to it. But a perpendicular object, ſituated upon the ſurface of the earth, will not have its length altered by refraction, the refraction of the bottom being the ſame as that of the top *. Theſe are the effects which are produced upon bodies at or near the horizon, in the common ſtate of the atmoſphere, by what I ſhall call the *uſual* refraction.

But, beſides the uſual refraction which affects the rays of light, the atmoſphere over the ſea is ſometimes found to be in a ſtate which refracts the rays in ſuch a manner as to produce other images of the object, which we will call an effect from an *unuſual* refraction. In the Philoſophical Tranſactions for 1797, Mr. Huddart has deſcribed ſome effects of this kind, which he has accounted for by ſuppoſing that, from the evaporation of the water, the refractive power of the air is not greateſt at the ſurface of the ſea, but at ſome diſtance above it; and this will ſolve, in a very ſatisfactory

* See my *Complete Syſtem of Aſtronomy*, art. 194.

manner,

manner, all the phænomena which he has obferved. But effects very different from thofe which have been defcribed by Mr. Huddart are fometimes found to take place. Thefe I had an opportunity of obferving at Ramfgate, laft fummer, on Auguft the firft, from about half an hour after four o'clock in the afternoon till between feven and eight. The day had been extremely hot, and the evening was very fultry; the fky was clear, with a few flying clouds. I fhall defcribe the phænomena as I obferved them with a terreftrial telefcope, which magnified between 30 and 40 times; they were vifible, however, to the naked eye. The height of the eye, above the furface of the water, at which moft of the obfervations were made, was about 25 feet; fome of them, however, were made at about 80 feet from the furface; and it did not appear that any of the phænomena were altered from varying the height of the eye, the general effect remaining the fame.

The firft unufual appearance which I obferved, was that which is reprefented in Plate II. fig. 1. Directing my telefcope at random, to examine any objects which might happen to be in view, I faw the top of the mafts of a fhip A, above the horizon, *x y*, of the fea, as fhown in the figure; at the fame time alfo, I difcovered in the field of view, two complete images, B, C, of the fhip in the air, vertical to the fhip itfelf, B being inverted, and C erect, having their hulks joined. The phænomenon was fo ftrange that I requefted a perfon prefent to look into the telefcope and examine what was to be feen in it, who immediately defcribed the two images, as obferved by myfelf; indeed they were fo perfect, that it was impoffible we could differ in our defcription. Upon this, I immediately took a drawing of the relative magnitudes and diftances of the fhip and its images, which, at that time, were as reprefented in the figure, as near as it was poffible for the eye to judge; and it was very eafy to eftimate them to a very confiderable degree of accuracy. As the fhip was receding from the fhore, lefs and lefs of its mafts became vifible; and, continuing my obfervations, in order to difcover whether any, or what variations might take place, I found that, as the fhip defcended, the images B, C afcended; but,

but, as the ſhip did not ſink below the horizon, I had not an opportunity of obſerving at what time, and in what order, the images would have vaniſhed, if the ſhip had ſo diſappeared.

Being deſirous of ſeeing whether the ſame effect was produced upon the other ſhips which were viſible, I directed my teleſcope to another ſhip A, (Fig. 2.) whoſe hulk was juſt in the horizon *xy*; when I obſerved a complete inverted image B, the main-maſt of which juſt touched that of the ſhip itſelf. In this caſe, there was no ſecond image as before. The ſhip A moving upon the horizon, B continued to move with it, without any variation in its appearance.

The next ſhip which I directed my teleſcope to, was ſo far on the other ſide of the horizon *xy*, as juſt to prevent its hulk from being ſeen, as is repreſented by A, (Fig. 3). And here I obſerved only an inverted image of part of the ſhip, the image *y* of the topſail, with the maſt joining that of the ſhip, the image *x* of the top *a* of the other maſt, and the image *z* of the end *c* of the bowſprit, only appearing at that time. Theſe images would ſuddenly appear and diſappear very quickly after each other; firſt appearing below, and running up very rapidly, ſhowing more and leſs of the maſts at different times, as they broke out; reſembling, in the ſwiftneſs of their breaking out, the ſhooting out of a beam of the Aurora borealis. As the ſhip was deſcending on the other ſide of the horizon, I continued my obſervations upon it, in order to diſcover what changes might take place; when I found that, as it continued to deſcend, more of the image gradually appeared, till at laſt the image of the whole ſhip was completed, with their main-maſts touching each other; and, upon the ſhip deſcending lower, the image and the ſhip ſeparated; but I obſerved no ſecond image, as in the firſt caſe: a ſecond image, however, might probably have appeared if the ſhip had continued to deſcend.

Upon moving my teleſcope along the horizon, in order to examine any other ſhips which might be in ſight, I obſerved, juſt at the horizon *xy*, (in Fig. 4.) the top *a* of the maſt of a ſhip; and here an effect was obſerved which had not been before diſcovered; for there was an inverted image B,

 vertical

vertical to a, an erect image C, both of them very perfect and well defined, and an image $v\,w$ of the ſea between them, the water appearing very diſtinctly. As the ſhip was coming up towards the horizon, I continued to obſerve it, in order to diſcover the variations which might follow, and found, that as the ſhip approached the horizon, the image C gradually diſappeared, and at laſt it vaniſhed; after that, the image $v\,w$ of the ſea diſappeared; and during this time the image B deſcended; but the ſhip did not riſe ſo near to the horizon as to bring the main-maſts together. Had I directed my teleſcope to the ſame point of the horizon a little ſooner, I ſhould have ſeen the two images before the ſhip itſelf was viſible. In fact, the images were viſible when the whole ſhip was actually below the horizon; for, from the very ſmall part of the maſt which was at firſt viſible, that part muſt then have been below the horizon, and appeared above it by the uſual refraction; the altitude of a, above the horizon, having then been much leſs than the increaſe of altitude which ariſes from the common horizontal refraction. The diſcovery of ſhips in this manner might, in ſome caſes, be of great importance; and, on ſuch occaſions, it might be worth while to appoint proper perſons to make obſervations for that purpoſe.

The cliffs at Calais being very viſible, I directed my teleſcope towards them, in order to examine whether there was any thing unuſual in their appearance; when I obſerved an image of the cliffs above the cliffs themſelves, together with an image of the ſea ſeparating them, as is repreſented in Fig. 5.; in which, $x\,y$ repreſents the horizon of the ſea, AB the cliffs, $a\,b$ their image, and $v\,w$ the image of the ſea between them: the depth of $a\,b$ was much leſs than that of AB. It is probable, however, that $v\,w$ might not be the image of the ſea immediately adjoining to the cliffs, but a partial elevation of the ſea at ſome diſtance from them; and that the image $v\,w$ might intercept ſome part of the image $a\,b$, which would otherwiſe have been viſible: we muſt not, therefore, conclude, that the image $a\,b$, ſo far as it appeared, was leſs than the correſponding part of the object. From the memorandums which I made at the time of obſervation,

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I do not find that I examined the appearance of the cliff AB, and its image *a b*; which, had there at that time been any ſtriking marks in them, would have determined whether the object and its image were of the ſame magnitude. The image *a b* was, however, erect; the boundaries on the top of AB and *a b* agreeing together. Having examined this for ſome time, and taken a drawing of the appearance, during which I could diſcover no variation, I directed my teleſcope to other objects; and, upon turning it again to the ſame cliffs, after the ſpace of about ſix or ſeven minutes, the images *a b* and *v w* were vaniſhed; but, examining them again ſoon after, the images were again viſible, and in every reſpect the ſame as they appeared before. A ſhort time after, they diſappeared, and did not appear any more.

Soon after the above appearances, I obſerved a ſhip C, with the hulk below the horizon *x y*, paſſing by the ſame cliffs AB; an inverted image D of which appeared againſt the cliffs, as repreſented in Fig. 6. The ſhip was in motion, and remained at the ſame diſtance on the other ſide of the horizon: I continued my obſervations upon it till it had paſſed the cliffs for a conſiderable diſtance, but there was no change of appearance. The cliffs were illuminated by the ſun, and appeared very diſtinctly; but there was no image above, as in the laſt caſe.

Continuing to obſerve the ſame cliffs AB, Fig. 7, I ſoon after diſcovered two partial elevations, *m*, *n*, of the ſea, by the unuſual refraction; they changed their figures a little, and diſappeared in the place where they firſt appeared, and were equally diſtinct in every part.

About this time, I obſerved a very thick fog coming upon the horizon from the other ſide, rolling upon it with a prodigious velocity; curling as it went along, like volumes of ſmoke ſometimes out of a chimney. This appeared ſeveral times. I conclude, therefore, that there was a conſiderable fog on the other ſide of the horizon.

The laſt phænomenon which I obſerved was that which is repreſented in Fig. 8.; where *x y* repreſents the horizon, *a b* two partial elevations of the ſea, meeting at *c*, and conti-

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nued to *d*; *e*, another partial elevation of the sea, of which kind I observed several, some of which moved parallel to the horizon with a very great velocity. I conjecture, therefore, that these appearances were, in part at least, caused by the fog on the other side of the horizon. For, though I did not at the same time see the motion of these images and that of the fog, yet, from memory, I judged the motions to be equal; and they were also in the same direction. A fog which, by producing an unusual refraction, might form these images, would, by its motion, produce a corresponding motion of the images.

I have here described all the different phænomena which I observed from the unusual refraction, of most of which I saw a great many instances. Every ship which I observed on the other side of the horizon of the sea, exhibited phænomena of the kind here described, but not in the same degree. Of two ships which, in different parts, were equally sunk below the horizon, the inverted image of one would but just begin to appear, whilst that of the other would represent nearly the whole of the ship. But this I observed, in general, that as the ship gradually descended below the horizon, more of the image gradually appeared, and it ascended; and the contrary, when the ships were ascending. Upon the horizon, in different parts, one ship would have a complete inverted image; another would have only a partial image; and a third would have no image at all. The images were in general extremely well defined; and frequently appeared as clear and sharp as the ships themselves, and of the same magnitude. Of the ships on this side of the horizon, no phænomena of this kind appeared. There was no fog upon our coast, and the ships in the Downs, and the South Foreland, exhibited no uncommon appearances. The usual refraction at the same time was uncommonly great; for the tide was high, and at the very edge of the water I could see the cliffs at Calais a very considerable height above the horizon; whereas they are frequently not to be seen in clear weather from the high lands about the place. The French coast also appeared both ways, to a much greater distance

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than I ever obferved it at any other time; particularly towards the eaft, on which part alfo the unufual refraction was the ftrongeft.

During the remainder of my ftay at Ramfgate, which was about five weeks, I continued daily to examine all the fhips in fight; but I difcovered no phænomena fimilar to thofe which I have here given a defcription of. The phænomenon of the fhip obferved by Mr. Huddart, differed altogether from thofe above defcribed, as the inverted image which he obferved was below the fhip itfelf. An appearance of this kind I obferved on Auguft the 17th, about half an hour after three o'clock in the afternoon, of which Fig. 9. is a reprefentation. The real fhip is reprefented by A, and the image by B; *e r*, *m v*, the hulks; *s t* the flag, and *w x* its image, juft touching it, with the fea *x y* below. Between the two hulks fome faint dark fpots and lines appeared, but I could not difcover what they were the reprefentatives of. The veffel, at the time of this appearance, was not quite come up to the horizon; and, as it approached it, the image gradually diminifhed, and totally difappeared when the fhip arrived at the horizon.

It remains now, that we inquire into the caufes which might produce the very extraordinary effects which have been above related. From the phænomena, we are immediately led to the nature of the path of the rays of light to produce them; and we may conceive, that the air may poffibly be in fuch a ftate as will account for the unufual tract which they muft have defcribed. For, let bz (Fig. 10.) be the furface of the fea; ab an object; E the place of the eye; arE, bsE, the progrefs of two rays, by the ufual refraction, from the extreme parts of the object to the eye; to thefe curves draw the tangents Ea', Eb', and $a'\,b'$ will be the image of the object, as ufually formed. Now, if we take the cafe reprefented in Fig. 4, let $a''\,b''$ reprefent the inverted image, and $a'''\,b'''$ the erect image; join a''E, a'''E, and b''E, b'''E, and thefe lines muft refpectively be the directions of the rays entering the eye from a and b, in order to produce the images $a''\,b''$ and $a'''\,b'''$; hence, thefe lines muft be tangents at E, to the curves which are defcribed by the

the rays of light; let therefore *anE*, *amE*, *bvE*, *bwE*, be the curves described. We have therefore to assign a cause which may bring rays passing above the rays *arE*, *bsE*, to the eye at E. Now, if there were no variation of the refractive power of the air, a ray of light passing through it would describe a straight line; therefore, the curvature of a ray of light passing through the atmosphere, depends upon the *variation* of the refractive power of the air. If, therefore, we suppose the air lying above *arE*, to vary quicker in its refractive power than the air through which *arE* passes, the curvature of a ray proceeding above that of *arE*, will be greater than the curvature of *arE*; and upon this principle we may conceive that a ray may describe the curve *anE*: and, in like manner, if a quicker variation of refractive power should take place above the curve *anE*, than in that curve, a third ray may describe the curve *amE*. The same may be said for the rays *bvE*, *bwE*, diverging from *b*. The alterations of the refractive power may arise, partly from the variations of its density, and partly from the variations of its moisture; and the passage of the rays through the boundary of the fog may there suffer a very considerable refraction; for, from the motion of the fog, and that of the images above mentioned, I have no doubt that the fog was a very considerable agent in producing the phænomena. When all the causes co-operate, I can easily conceive that they may produce the effects which I have described. If the cause should not operate in the tract of air through which the curves *anE*, *bvE* pass, but should operate in the tract through which *amE*, *bwE* pass, an erect image would be visible, but there would be no inverted image; and, should it operate in the latter case, but not in the former, there would be only an inverted image.

As the phænomena are very curious, and extraordinary in their nature, and have not, that I know of, been before observed, I have thought proper to lay a description of them, with all the attending circumstances, before the Royal Society. They appear to be of considerable importance, as they lead us to a knowledge of those changes to which the lower parts of the atmosphere are sometimes subject. If, when

these

these phænomena appear, a vessel, furnished with a barometer, thermometer, and hygrometer, below, and also at the top of the mast, were sent out to pass below the horizon and return again; and an observer at land, having like instruments, were to note, at certain intervals, the situation and figure of the images, it might throw further light upon this subject, and lead to useful discoveries respecting the state of the atmosphere, from a conjunction of the causes which affect these instruments.

VII. *Case of Tetanus cured by Wine. Communicated in a Letter to Dr.* JAMES GREGORY, *Professor of the Practice of Physic in the University of Edinburgh, by Dr.* DAVID HOSACK, *Professor of Botany and Materia Medica in Columbia College* *.

THE treatment of lock-jaw by the use of tonic remedies, has been long since sanctioned by the successful practice of Dr. Cochran of Nevis, (see Med. Commentaries, Vol. III.) Dr. Wright of Jamaica, (see London Med. Obs. and Inq. Vol. VI.) Dr. Rush of Philadelphia, (see American Philos. Transactions,) and Dr. Currie of Liverpool, (see Med. Mem. of London, Vol. III.) but the same treatment, in other hands, has not been equally successful, insomuch that it is still considered as one of those diseases which generally baffle medical prescription. To what causes is this failure to be ascribed? Not to any defect in the principle upon which those remedies have been prescribed: the cases which have been recorded of the success of tonic medicines are too numerous to admit a doubt, that the proximate cause of lock-jaw consists in an exhausted state of the sensorial power from violent irritation applied to the nervous system: stimulants and tonics are therefore the remedies which are best calculated to restore this lost energy. Their failure I have considered to proceed from the complicated and inert manner in which they have usually been administered.

In the greater number of cases which I have either wit-

* From the *American Medical Repository*, Vol. III. No. 1.

nessed

neffed in practice, or remember to have feen defcribed, the bark, wine, cold-bath, and, in many inftances, opium, mufk, and mercury, have been exhibited at the fame time: I muft except one cafe, recorded by Dr. Currie, in the Memoirs of the Medical Society of London, in which, the patient rejecting the mixture of bark and wine, the bark was omitted, and the wine was employed alone, which ultimately effected a cure; but in this cafe, opium, mercury, and the cold and warm bath, had been previoufly employed, but to no purpofe.

This complicated mode of practice cannot but be prejudicial in any difeafe; in lock-jaw it muft efpecially prove injurious, by haraffing the patient, and by offending the ftomach with the difcordant and naufeous mixture of the remedies above mentioned: if, therefore, a practice more fimple, and, at the fame time, more efficacious can be devifed, it is certainly a defideratum in the treatment of this formidable and fatal difeafe.

Having, in a variety of difeafes, attended with great exhauftion of the vital powers, employed wine alone, with fuccefs, without the ufe of thofe remedies which are ufually prefcribed in this condition of the body, I long fince refolved to give it a trial in lock-jaw.

In January 1798, a merchant of this city, while engaged in opening a box of goods, ftruck the infide of his right hand upon a nail; the fkin was confiderably torn, but the wound did not appear to extend beneath the integuments. In twenty-four hours his hand became painful, and fwelled, attended with great heat and rednefs, which fpread over the wrift. He immediately applied a poultice of bread and milk to the part affected. In forty-eight hours the pain extended the whole length of his arm, and produced fome uneafinefs about his throat, efpecially in the act of chewing and fwallowing. He became alarmed, and applied to me for advice. I found him in great pain; but being free from fever, I directed him to have recourfe to wine; to take a large wine-glafs full every hour until his pain was removed; and, in addition to the ufe of wine, to apply a comprefs, wet with fpirits, to the wound. When he had taken to the amount

of

of four glaſſes, he felt himſelf very ſenſibly relieved; and by the occaſional uſe of the wine for twenty-four hours, his pains entirely left him; the ſwelling ſubſided, and in a few days the wound was healed, without any unpleaſant appearances.

What would have been the progreſs and termination of this caſe, had it been left to itſelf, is uncertain; but the immediate good effects of the treatment preſcribed, encouraged me to make trial of the ſame remedy in a caſe where the diſeaſe might appear in a more formidable ſhape.

On Tueſday, March 13, 1798, about one o'clock P. M. I was called to viſit a mulatto ſervant woman of John Harrington, Eſq. of this city. I was informed that about an hour before, while engaged in waſhing clothes, ſhe had pricked herſelf with a pin in the wriſt of her right arm. The part at which the pin entered was upon the inſide of the wriſt, immediately over the connection of the radius with the carpus.

The pin was inſtantly removed, and, finding no inconvenience from the accident, ſhe returned to her employment. In a ſhort time ſhe felt a great degree of ſoreneſs in the part which had been injured, with pain ſhooting occaſionally to the arm, ſhoulder, and neck. Theſe ſymptoms, in a few minutes, were ſucceeded by ſtiffneſs about the throat, difficulty of ſwallowing, ſome interruption of her ſpeech, and, at length, a locked ſtate of the jaws, attended with a ſpaſmodic contraction of the muſcles at the back part of the neck, and occaſional ſubſultus tendinum, with ſome coldneſs of her extremities. In this ſituation I found her.

She was naturally of a delicate and irritable habit of body, and had been much ſubject to hyſterical complaints and fits of fainting, which were ſometimes induced by the moſt trifling cauſes. Her irritability of habit was alſo at this time probably increaſed, having but three months before borne a child, which ſhe was then ſuckling.

Although I have been long ſince convinced of the inſufficiency of opium in the cure of this diſeaſe, in the hurry of the moment I gave her about ſixty drops of laudanum in a ſmall quantity of wine. Her jaws being cloſely locked, it was with great difficulty adminiſtered. In a few minutes after ſwallowing the laudanum ſhe ſickened at the ſtomach, and vomited violently, complaining at the ſame time of great pain and diſtreſs at the pit of her ſtomach. The anodyne draught was entirely rejected; but, upon a moment's reflection, I did not regret this circumſtance, as the diſeaſe aſſumed a very decided character, and I had made up my mind to rely upon the effects of wine alone, without the aſſiſtance of any other remedy: accordingly, about two o'clock, I directed a large wine-glaſs full of Madeira wine (the glaſs containing about two ounces,) to be given punctually every hour, and a cup of ſago, or panado, with wine, to be given, from time to time, as her nouriſhment. At this time another phyſician, who had alſo been called upon at the time of the accident, arrived. I related to him what had been done, and the mode of treatment which I directed for the patient. This gentleman having had frequent opportunities of ſeeing this diſeaſe, and having frequently witneſſed the failure of the ordinary mode of treatment, he at once, with great candour, acceded to the plan propoſed, and, in addition to the uſe of wine, propoſed the application of cauſtic to the part which had been wounded. Accordingly the wound was freely pencilled with the lunar cauſtic, and afterwards covered with a poultice of bread and milk, with the view to obtain ſuppuration as ſoon as poſſible.

The wine was adminiſtered with great fidelity, by the mother of the patient, until about five o'clock the next morning. She had ſome ſlight convulſions in the courſe of the afternoon, but they were more of an hyſterical ſort, induced by her great anxiety of mind, than to be aſcribed to the diſeaſe itſelf. Generally ſpeaking, there had been a very manifeſt abatement in all her ſymptoms, and ſhe had paſſed a more comfortable night than could have been expected. At five o'clock on Wedneſday morning, her miſtreſs, alarmed at the quantity of wine ſhe had taken, deſiſted from its further uſe. From this time appearances became more unfavourable, and at eight o'clock her jaws, which had been relaxed during the plentiful uſe of wine, again became ſtiff and cloſed. We ſaw her at nine, and immediately gave her about half a pint of wine, and ordered it to be adminiftered as before. At one her fymptoms were greatly changed; we found her fitting up in bed, eating fmall portions of roafted oyfters, which fhe had called for. At this time her jaws were almoft in their natural ftate. She had taken her wine punctually as directed, but experienced no inconvenience from it whatever, although in health fhe had not been accuftomed to its ufe. Her pulfes were ftill fmall and feeble, without any excitement from the ufe of wine. The heat of body remained at its natural ftandard, but not at all increafed. The pain in her head was abated, but without any appearance of fuppuration. Finding this mode of treatment to agree fo well with her, we directed it to be continued. We faw her again in the evening: her fymptoms ftill continued favourable, without the fmalleft febrile action from the ufe of wine. Having had no difcharge from her bowels fince her illnefs, an injection was adminiftered; which remedy was afterwards employed, from time to time, in the courfe of her difeafe, whenever the ftate of her bowels required it. The wine was continued through the night: fhe flept, altogether, about three hours in the courfe of the night, and took freely of her panado.

Thurfday morning at nine o'clock her complaints appeared to be, in a great meafure, fubdued; infomuch that we did not think it neceffary to vifit her again until late in the evening, and directed the wine to be given at longer intervals, and the quantity to be leffened.

She remained in a very comfortable condition until the afternoon: the pain in her hand returned with violence, extending to her arm and neck as before; her jaws were again clofed; the rigidity of the mufcles at the back of her neck returned; her mind became greatly agitated; fhe again complained of diftrefs at the pit of her ftomach; fhe fainted, and had feveral flight convulfions. Being called at that time, I gave her, with fome difficulty, about half a pint of wine, and ordered a warm poultice to be immediately boiled. When prepared, I poured upon the furface of it half an ounce of laudanum, and applied it to the wound. Her fymptoms were in a ſhort time allayed: I left her, directing the wine to be continued as before, a large wine-glaſs full every hour.

We ſaw her again at nine in the evening. She remained tranquil: her jaws were leſs firmly cloſed, but the pain in her hand was not altogether removed. Although ſhe had taken the wine punctually as directed, it had not produced the leaſt apparent excitement. Having had no diſcharge from her bowels for the laſt twenty-four hours, an injection was adminiſtered. The anodyne poultice was renewed; and, in addition to this application, we directed her arm to be bathed with laudanum occaſionally through the night.

Friday morning we found ſhe had paſſed a more comfortable night than the laſt; had taken her wine every hour; her jaws were perfectly relaxed; the pain in her hand had greatly abated, and ſhe was enabled to extend her fingers at pleaſure, which ſhe could not do before. Her pulſes and ſkin were natural; her appetite unimpaired; her mind compoſed, without any inconvenience from the wine. We directed her remedies to be all continued as before, fearing leſt any alteration might ſubject her to a return of her complaints.

In the evening we obſerved the wine had exhilarated her ſpirits; ſhe became very talkative; her pulſes became full, and free from all tenſion; her ſkin was ſomewhat heated, and all complaints removed except the wound at the wriſt, which exhibited a healthy appearance, and was entirely free from pain, but without any ſign of ſuppuration.

We directed the wine to be adminiſtered through the night, but in ſmaller quantities and at longer intervals, unleſs her complaints ſhould return and demand a continuance of it as before.

Saturday morning we were informed ſhe had ſlept the greater part of the night, and had taken but a ſmall quantity of wine: her ſymptoms being, in all reſpects, favourable, the wine was diſcontinued, except a ſmall quantity mixed with nouriſhment. A dreſſing of ſimple ointment was applied to the wound. From that time ſhe remained free from any return of her complaints, and has ſince been in perfect health.

Upon calculating the quantity of wine which ſhe had taken, it amounted to three gallons.

VIII. *On*

VIII. *On the Expansion of Water during Congelation. Communicated in a Letter from* STEPHEN DICKSON, *M.D. to Dr.* MITCHILL *.

IT is well known that water, in the act of freezing, expands with considerable force. By calculations instituted on some experiments of the Florentine academicians, this force appears to be so great, as, in a spherule of water only one inch in diameter, to be superior to a resistance of thirteen tons and a half.

Some ingenious experiments on this subject were made, a few years ago, in Quebec, during a very cold season, by Major Williams. He exposed to intense cold, water inclosed in an iron bomb-shell: the fuze was expelled to a considerable distance, and a cylinder of crystallised ice was shot forth from the aperture, which, by Dr. Hutton's calculation †, in one instance, amounted to upwards of an eighteenth part of its original bulk.

The philosopher who observes that this is not the uniform effort of specific particles mutually receding from one another, since the specific gravity of ice is greater than that of water, naturally inquires to what cause it is to be attributed. None seems to have a greater appearance of verisimilitude than the suggestion of Dr. Black; *viz.* that, in the moment of the conversion of water into ice, the latent heat set loose enters into the air contiguous to, or combined with, the water, and expands it with such vehemence as to effect the divarication of the ice into irregular masses, and the explosion of resisting bodies. This explanation is countenanced by a minuter attention to the experiments to which it is applied; for we find that ordinary rain-water was employed, and even that the bomb-shells were but "nearly filled."

To put the inquiry, however, to a more rigid test, I procured two exactly equal and similar cylinders of tin, closed

* From the *American Medical Repository*, Vol. III. No. 1.

† Transactions of the Royal Society of Edinburgh, Vol. II.

at

at the extremities, except by a ſmall pipe in each iſſuing from the upper ſurface. One of theſe I filled with pure but ordinary rain-water; the other with water which I had carefully diſtilled. The apertures of both were cloſed alike by well fitted plugs of wood, which I had previouſly boiled in oil. I expoſed them to the open air, on the ſurface of the ſnow, in Quebec, the night of the 5th of January 1799, when the thermometer ſtood at 28 degrees below zero of Fahrenheit's ſcale. About twenty minutes afterwards, I found both the cylinders inclining to the horizon at an angle of about thirteen degrees; being ſo far rent from their baſes, which yet reſted on the ſnow. The intermediate ſpace in each was filled with rude maſſes of ice, which, on being examined with the microſcope, (in a room,) appeared to be compoſed of cryſtals, chiefly in the form of parallelopipeds and truncated pyramids. The whole of the appearances, in both caſes, were exactly ſimilar. I afterwards repeated theſe experiments, without any variety in the reſult.

From hence we perceive evidently that Dr. Black's ſolution is inadequate to account for theſe remarkable phenomena. At the ſame time I think it impoſſible not to admit the original principles upon which his ingenious rationale reſts. It is impoſſible that water can be converted into ice without the evolution of one-tenth of the whole quantity of its ſpecific fire, which is equal to 146 degrees of Fahrenheit's thermometer. Now, we know that water can be converted into vapour, under particular circumſtances, by much inferior temperature.—*Quære.* Muſt not part of the water have been converted into vapour in the very proceſs of congelation; and may not this cauſe be adequate to the production of the moſt violent expanſive force of froſt?

Quebec, January 24, 1799.

IX. *The*

IX. *The Theory of Explosions. Communicated in a Letter from Mr.* Blanchet *to Dr.* Mitchill, *Professor of Chemistry, Natural History, and Agriculture, in Columbia College, in the State of New-York.*

THE cause of explosions, in general, which, at first view, appears to be simple and easy to comprehend, is, nevertheless, beset with many difficulties; and it is with much reluctance that I have ventured to discuss this subject.

In order to avoid a tiresome recital, I shall decline to state the opinions of philosophers upon this subject. My design is to explain the immediate cause of certain phenomena, which seem hitherto to have eluded their research. The phenomena, which seem to be exceptions from general principles, because they are presented under an irregular and peculiar form, are included in the following questions:—What is the immediate cause of the bursting of vessels containing water in the state of congelation? In what manner does lightning rend and shiver a tree into so many pieces? Whence arise volcanic explosions and earthquakes? These inquiries form the subject of my letter.

As to the immediate cause of the bursting of vessels filled with congealing water, the celebrated Dr. Black, according to the observation of Dr. Dickson, (see Medical Repository, Vol. III. p. 35.) seems, in some degree, to have understood it, when he supposes, "that in the moment of the conversion of water into ice, the latent heat of the water, then set at liberty, enters into the air which is contiguous to its surface, or combined with the water, and dilates it with such force that it produces a separation of the ice into an irregular form, and a bursting of the containing and resisting vessels." But this explanation, however ingenious, is not entirely satisfactory. Besides the caloric extricated from freezing water, which may dilate the air combined with it, there is another agent which escaped Dr. Black's attention; that is, the sudden detachment of atmospheric air which is found lodged in the interstices of the particles of the water.

* From the *American Medical Repository*, Vol. III. No. 3.

The

The most simple principles of natural knowledge teach us that atmospheric air cannot pass from its permanently aëriform state to that of fluid or solid, by any degree of cold with which we are acquainted. Admitting this to be true, there is no longer any ground of surprise in seeing the bursting of bodies which resist the force of freezing water. The air, a fluid of permanent elasticity, being compressed, and forced to give way to the particles of water, which cold, or the attractive power of the atoms of bodies, draws violently together, it follows that the ice, into which the water is converted, must break asunder, in order to afford a passage to this aëriform substance, which cold can neither condense nor make solid, and must rend and shiver, with a cracking noise, the vessels no longer able to hold it.

This theory is rendered probable, and strengthened, by a fact which I have observed myself, *viz.* that the fats, such as hog's fat, &c. settle considerably down in the vessels in which they are condensed. I have also noticed cavities in the interior parts of such masses of fat. Now, this shrinking of volume, and these cavities in fat, undoubtedly, can only arise from the extrication of atmospheric air in the process of condensation. Independently of this fact, which every body is ready to attest, there is not a Canadian peasant who is ignorant that the cavities found in a mass of ice were formed by air, and contain it. These facts, therefore, prove that, in the congelation of water, the atmospheric air which exists in the interstices of the particles of water is forced to detach, or set itself at liberty, as soon as cold converts them into ice.

On this principle it is easy to explain the rending and splitting of trees, with a cracking noise, during the frosts of winter; the bursting of stones in the moment of the conversion of the water contained in them into ice; and the breaking, as well as the loss of the primitive perpendicularity of walls which had been built during the autumnal season.

According to the Academy *Del Cimento*, it appears to be demonstratively true, that frozen water occupies more bulk than fluid water. This opinion seems to clash with all fact and analogy: for we consider it as ascertained, that the specific gravity of ice is greater than that of water; that is to say,

that

that a cubic foot of frozen water contains more watery particles than a cubic foot of fluid water, ſince, in the former caſe, the particles are undeniably more cloſely approximated and compreſſed than in the latter. Now, is it poſſible that frozen water, whoſe particles have been condenſed, occupies more ſpace than water in the ſtate of fluidity? I have often ſeen water freezing in open veſſels, ſuch as the copper veſſels of kitchens, &c., and I have always remarked, towards the middle of the veſſel, a riſing, divided by chinks, which doubtleſs had been formed by the eſcape of atmoſpheric air during the congelation of the water. It is this phenomenon which has probably led to a wrong concluſion. Beſides, the hog's fat, which, like water, becomes ſolid by cold, or by the abſence of caloric, is diminiſhed in bulk by condenſation; and, if the air which eſcapes in this proceſs produce no burſting, it is becauſe the fat is always ſet to cool in open veſſels. Upon what ground, then, could an exception be made to the general law? But I now come to explain the effect produced by lightning when it ſtrikes a tree, &c.

No perſon, ſo far as I know, ſeems yet to have accounted for the ſhattering of trees by the ſtroke of lightning. A moment's reflection, however, ſuffices to diſcover and explain the cauſe. As it is only by a careful analyſis of facts that we can deduce juſt concluſions, I proceed to examine what happens in the moment of the lightning ſtriking a tree, and it will be ſeen whether this ladder will enable us to reach to the cauſe of this phenomenon.

Since the beautiful experiments of Lavoiſier, of Fourcroy, &c. have demonſtrated that caloric is the cauſe of the repulſive power of the atoms of bodies, there is no doubt that the more the atoms of a body are removed from one another, the greater is the capacity of ſuch a body to contain caloric, or the more caloric it contains. It is alſo inconteſtably proved, that the bodies which are conſtantly in the ſtate of gas, in the common temperature of the atmoſphere, contain a great quantity of caloric, and in proportion to their ſpecific capacity for holding it. So likewiſe the electric fluid, or what might, with more propriety, be called the electric gas, muſt contain a prodigious quantity of caloric, ſince this element always exists in the state of gas, and the more this gas is accumulated, the more the amount of specific heat must also be augmented.

This being premised, whenever the lightning (which is only an accumulation of electric gas, discharged from one cloud to pass to another containing a less quantity) happens, in its course, to strike a tree, &c. the tree becomes so suddenly overcharged with caloric, that the air and other constituent parts of it, which are disposed rapidly to combine with heat, undergo expansion, and thereby shiver the tree, in order to force a passage, and to set themselves at liberty. I cannot compare this phenomenon to any thing better than to the discharge of a cannon or of a musket; for every body knows that the discharge of a cannon is owing to the sudden expansion of the constituent parts of the powder by the introduction of caloric.

In like manner, I have every reason to believe that the violent rending of a tree, by lightning or the electric gas, is owing to a similar cause; that is, a sudden accumulation of caloric, which expands, with violence, the air, &c. lodged in the interstices of that body. This opinion will be readily embraced, if it be considered that the electric gas melts the metals. If, indeed, the metals be suddenly melted by this gas, or rather by the caloric which it contains, is it not thereby demonstrated that such a degree of heat is sufficient immediately to set fire to all combustible* bodies, and so far to expand the air contained in the tree as to cause the rending of its parts?

If animals which are killed by a stroke of lightning, or by the accumulation of the electric gas, receive no apparent injury, such as laceration of their parts, as in the case of the tree, their death must be owing to a decomposition produced in their bodies. This opinion, which has never been attended to, shall be fully developed and discussed in another work. I proceed, therefore, to the third part of my subject;

* The experiment of Messrs. Lavoisier and Meunier, in which they caused an electric spark to pass through a balloon which contained oxygen and hydrogen, to produce combustion in order to form water, proves how large a quantity of caloric is contained in the electric gas.

to

to wit, to inquire concerning the immediate cause of the eruption of volcanoes and of earthquakes.

Whatever may be the agent which kindles and inflames volcanic substances, whether it be the electric gas which communicates its fire to those most inflammable substances, such as sulphur, &c. or whether it be the subterranean heat, which, in making its escape, sets fire to those substances, still it is certain that atmospheric air must be found in great quantities in the interior of volcanoes. For, since the ever memorable discoveries of Lavoisier, of Priestley, of Scheele, and of Ingenhouz, on combustion, it is admitted as a demonstrable truth, that the burning of combustible substances is owing to oxygen; that is to say, it is fixed and absorbed by the body in combustion, while the caloric and the light, which are combined with it, escape, in order to form the flame, the light, and the heat. In this manner atmospheric air is one of the essential agents in supporting the fire of a volcano.

These things being premised, as soon as a volcano is kindled and set on fire, the atmospheric air which feeds it, as well as the bases of sulphur, of water, &c. which are also converted to the state of gases by the evolution or accumulation of heat, are expanded, and break out with a violence and impetuosity not to be restrained by the force of opposing bodies, and thereby give birth to earthquakes, &c.

This theory is remarkably countenanced by the common and uniform series of occurrences which follows the eruption of a volcano. In the first place, as soon as it is kindled, there appears a thick smoke; soon after, when the air, &c. become expanded, stones of enormous bulk are thrown out to great distances into the aërial regions, accompanied with earthquake, and agitation of all the surrounding country. As soon as the superfluous air is discharged and set free, there succeeds a bright flame, which seems to restore serenity to the abyss, which again, after a short time, vomits forth a shower of fiery and destructive matters.

To recapitulate: 1. The bursting of vessels in the freezing of water is owing to the escape of atmospherical air, which also separates the ice into irregular masses. Nothing is more improbable than the expansion or increased repellency of the

 particles

particles of water in the proceſs of congelation. This gives an anſwer to the queſtion of Dr. Dickſon, (ſee Med. Repoſ. Vol. III. p. 34.) 2. The ſhivering of a tree by lightning is cauſed by heat expanding the air, &c. which it contains. 3. The eruption of volcanoes and earthquakes is likewiſe produced by the expanſion of air. In a word, every exploſion is owing to the ſudden extrication of ſome matter incapable of confinement, or to the expanſion of air in a reſiſting body.

I am, with much eſteem and conſideration, Sir,

Your moſt humble ſervant,

F. BLANCHET.

Eſſex County (*N. J.*), *Aug.* 15, 1799.

X. *Letter from Dr.* VAN MONS, *of Bruſſels, to* J. C. DELAMETHERIE, *on the conſtituent Principles of fixed Alkalies, &c.* *

CRAANER, an apothecary at Amſterdam, has juſt made an experiment from which it would appear that fixed alkalies contain carbon. He introduced potaſh or ſoda, beſprinkled with water, under a bell filled with oxygen gas. The gas was abſorbed, and the alkali became effervefcent. Beſides this, an alkali which has been treated ſeveral times with this gas, or which has been ſuffered at ſeveral times to become carbonated in pure air or atmoſpheric air, is found exhauſted of carbon, and can be carbonated no longer. This fact may explain why alkalies are ſo ſpeedily converted into alkaline carbonates even at elevations in the atmoſphere which the carbonic acid never reaches.

I long ago publiſhed that potaſh is formed during the combuſtion of hydro-carbon, and during the fermentation of the juice of grapes, by the intervention of the azot of the atmoſphere When theſe operations take place in pure oxygen, you obtain neither potaſh nor acidulous tartrite of that alkali, and the wine becomes ſo acid that it cannot be drunk. It is poſſible that fixed alkali is an azoto-carbon, or carbon of azot.

* From the *Journal de Phyſique* for Floreal, an. 8.

I, how-

I, however, formerly made an experiment which is contrary to that of Craaner; I treated in a retort, at a red heat, with iron, a mixture of cauſtic potaſh and red oxyd of mercury, and obtained only oxygen gas, nitric acid, and water. The alkali was totally diſperſed. Another experiment, which I have juſt made, is equally unfavourable to it: I pounded oxygenated muriat of potaſh with cryſtalliſed cauſtic potaſh, and poured the mixture, which had become liquid, into a bottle: I corked the bottle very cloſely, and at the end of three days poured over it muriatic acid, but I obſerved no effervеſcence. As carbon is not combuſtible in the air but at a red heat, if this principle enters into the compoſition of alkalies, it muſt be united in them with hydrogen, unleſs the azot ſerves as the medium of its union with the oxygen.

Brugnatelli has ſeparated butter from cream without the aid of oxygen. He poured into one part of cream four parts of warm water, and made the mixture paſs through a filter of cloth into a cloſe veſſel, and in which were extended ſeveral other filters of the like kind formed of coarſe cloth. At the end of twenty-four hours the ſerous part was found imbibed by the cloth, and the butter completely ſeparated.

Rouppe and Bicker have deſcribed, in the firſt volume of the New Memoirs of the Society of Rotterdam, a new diaſtatometer, which ſeems to anſwer the different purpoſes of ſuch inſtruments. Mention will be made of it in the Annales.

A ſpirit made from the refuſe of the beet-root, and conſidered as ſuperior to French brandy, has been already ſold at Berlin. The houſe of Claude has alſo diſtributed ſamples of arrack made from the ſame ſubſtance, which has exactly the taſte and ſtrength of foreign arrack.

The memoir of Crell on the radical of the boracic acid has been publiſhed ſome time. He treated that acid with oxygenated muriatic acid, and obtained carbon.

Trommſdorff has diſcovered a new earth, which he calls *aguſtine*, from its property of forming, with acids, ſalts deſtitute of taſte.

XI. *Curſory*

XI. *Curſory View of ſome of the late Diſcoveries in Science.*

[Continued from Page 314 of the laſt Volume.]

CRYSTALLOGRAPHY.

FOURCROY, in treating urine, obſerved a phenomenon of great importance to cryſtallography. It is known that cubes of ſal-ammoniac and octaedra of marine ſalt have been found in urine. But whence happens it that marine ſalt cryſtalliſes in octaedra, and not in cubes, and that ſal-ammoniac cryſtalliſes in cubes, and not in octaedra? Fourcroy is of opinion that theſe forms are owing to the preſence of *ur.e.* This he proves by the following experiment:

Marine ſalt, or muriat of ſoda, exceedingly pure and in perfect cubes, having nothing in common with human urine, to which it never belonged, ſince it aroſe from ſpring-water evaporated, was diſſolved, with an equal part of cryſtalliſed *ur.e*, in five times its weight of diſtilled water. The ſolution was put into a porcelain capſule, which was covered with paper to prevent the acceſs of foreign bodies, and was left to ſpontaneous evaporation. In a few weeks there were formed octaedral cryſtals exceedingly regular, and of a reddiſh brown colour.

Sal-ammoniac, or muriat of ammonia, treated in the ſame manner, cryſtalliſed in cubes, though it cryſtalliſes in general in octaedra.

"It is a fact then, fully proved," ſays he, "that *ur.e*, diſſolved in the ſame water as the two ſalts above mentioned, modifies and reverſes their natural form by combining with each of them, and by penetrating the laminæ of their cryſtals. To it, therefore, is due the octaedral form aſſumed by marine ſalt, with which human urine has been ſaturated."

Haüy has deſcribed various cryſtals; ſuch as thoſe of cinnabar or ſulphurated mercury, of ſulphat of ſtrontian, and of the arragonite or calcareous ſpar cryſtalliſed in hexagonal priſms, which is found in Arragon.

VOLCANOES.

Kirwan has publiſhed an excellent memoir on the primitive ſtate of the globe, and the cataſtrophe which ſucceeded it.

 He

He is of opinion with me *, that the globe was formed by cryſtalliſation. In ſpeaking of mountains, he ſays, that volcanic mountains, ſuch as Veſuvius, Ætna, &c. exiſted anterior to the eruption of volcanoes, and that ſubterranean fires were kindled up in their bowels.

G. A. Deluc has attacked this latter part of the opinion of Kirwan. He thinks that the foci of ſubterranean fires are at a great depth, and that volcanic mountains, ſuch as Veſuvius, Ætna, the Lipari iſlands, &c. were formed chiefly from matters thrown up by volcanoes and accumulated in the neighbourhood.

Breiſlak has given a phyſical topography of Campania. He firſt diſcovered there, in 1793, an extinguiſhed volcano, which ſeems to have been much more conſiderable than Veſuvius, and to approach near to Ætna. It is called Rocca-Montfina. He ſuppoſes with Gioeni, that the firſt origin of Veſuvius was at the bottom of the ſea. He obſerved a ſource of petroleum at the bottom of the ſea oppoſite to Mount Veſuvius, and at the diſtance of ſomewhat leſs than a mile from the ſhore. When drops of this ſubſtance riſe to the ſurface of the water, they form on it ſpots perfectly round, of from three to four inches in diameter, and of a yellowiſh-brown colour. A ſpring of petroleum near Veſuvius might afford ſome aſſiſtance to a framer of ſyſtems. By combining this phenomenon with other ſprings of petroleum in the neighbourhood of the Appenines, and with the foſſil coal of Benevento and Giſone, which nothing prevents us from ſuppoſing to be extended below the earth, we may figure to ourſelves under Veſuvius an immenſe reſervoir of bitumen, kindled by an electric ſhock, or by ſome other unknown cauſe.

The combuſtion will continue as long as the maſs of the reſervoir is not conſumed, and may be renewed whenever a new cauſe of inflammation acts again upon a new quantity of bitumen.

France at preſent is ſubject to earthquakes, which, if we are to judge by the conſiderable number of extinguiſhed volcanoes found there, muſt formerly have been very common.

* This is extracted from the *Journal de Phyſique*, of which Delametheric is editor.

In

In the month of January laſt year, a ſlight ſhock of an earthquake was experienced in the whole of the weſt of France, from Rouen to Bourdeaux. The ſame diſtrict experienced alſo a violent agitation in 1755, at the time of the earthquake which deſtroyed Liſbon. No extinguiſhed volcanoes, however, are known in that diſtrict except in the neighbourhood of Treguier.

On the 29th of Prairial there was a terrible earthquake at Acapulco: whole diſtricts were convulſed, and the port was filled up.

Salmon has given a beautiful memoir on the origin of volcanic baſaltes. Some, among whom are the greater part of the French naturaliſts, have conſidered them as the production of fuſion by fire. Others, ſuch as the celebrated Werner, are of opinion that baſaltes has been depoſited by water. Salmon undertakes to reconcile the two hypotheſes, and aſſerts, that they have been produced by an aquoſo-igneous liquefaction. Water reduced to vapour, for example, in Papin's digeſter, acquires a great degree of heat, capable of fuſing various ſubſtances which require a ſtrong heat to be reduced to that ſtate. All volcanic vapours and all ſpiracles contain a great quantity of water.

He is of opinion that ſeveral ſubſtances contained in baſaltes, ſuch as feld-ſpar, augite, hornblende, zeolithes, and mica, have been encloſed in it accidentally while it was liquid. But he thinks that there are ſeveral others, ſuch as leucites, which have been fuſed with the baſaltes, which have afterwards cryſtalliſed apart, and been ſeparated from the maſs by different laws of affinity.

Buch entertains the ſame opinion. He has no doubt that leucite has cryſtalliſed in the maſs of the baſaltes at the moment of its liquidity. The portions of hornblende or of baſaltes, found in the centre of ſeveral cryſtals of leucite, ſeem to theſe naturaliſts a convincing proof of their opinion.

[To be continued.]

NEW

NEW PUBLICATIONS.

An Essay on the Analysis of Mineral Waters. By RICHARD KIRWAN, *Esq. F.R.S. &c.*

IN this excellent treatise Mr. Kirwan enumerates the different mineralising substances which have been found in mineral waters; explains by what tests the presence of those mineralisers, whether simple or in a state of combination, is to be detected in the waters in which they exist; and treats, with eminent skill and accuracy, of the methods of process by which the tests are to be the most successfully employed in the analysis of the waters. Tables are subjoined of the quantities of real acid in mineral acids of different densities; of the quantities of acid absorbed by different bases; of the quantity of each basis absorbed by each acid; of the proportion of ingredients in neutral salts; of the length of a column of common air at different barometrical heights and different temperatures. Genius, patience, and accuracy of investigation, and an extensive acquaintance with the labours of other chemists at home and abroad, are eminently conspicuous in this useful work. It has likewise the merit of being better written, as a literary composition, than most English books upon chemical or physical science that have fallen into our hands.

The Clinical Guide; or, A concise View of the leading Facts in the Nature and Treatment of Diseases. By WILLIAM NISBET, *M.D. &c.* 3 Vols. 12mo. Johnson, Saint Paul's Church-yard.

This comprehensive and well written work, the production of Dr. Nisbet, of Pickering-place, St. James's-street, includes, in these three volumes, a view of the present practice of medicine, surgery, and midwifery. A fourth volume, on the diseases of children, is, we believe, to be soon added, to complete the author's design. A work, uniting, more correctly, a larger proportion of rational theory with sound practical information concerning the subjects of which it treats, will not be easily named. We need scarcely recommend it; for the public already know, and highly approve its merits.

INTELLIGENCE,

AND

MISCELLANEOUS ARTICLES.

FRENCH NATIONAL INSTITUTE.

C. COULOMB read in the Inftitute on the 26th of May, a curious memoir on the refiftance of fluids when the movement is exceedingly flow, and he has found that it is then merely as the velocity, inftead of being as the fquare of the velocity. For his experiments he employed the force of torfion, of which he firft formed an idea. He complains that Mr. Cavendifh, in the Philofophical Tranfactions for laft year, afcribes this invention to an Englifhman who never publifhed any thing on the fubject.

LALANDE.

ASTRONOMY.

The obfervations of the Arabs in the tenth century are extremely valuable. We were acquainted with only three, when I difcovered among the manufcripts of my former mafter, Jofeph Delifle, an Arabic copy of a part of the work of Ibn Iunis, where there are a great many. But the original was at Leyden; and we have long folicited the Batavian government to intruft us with it. At length, on the 26th of May, our ambaffador brought to the Inftitute this valuable manufcript, containing 400 pages in quarto fmall characters; and we hope that not only a tranflation of it will be printed, but that part alfo of the Arabic original which is interefting to aftronomy. C. Chauffin has already tranflated that which I procured for him.

LALANDE.

THE CONNECTICUT ACADEMY OF ARTS AND SCIENCES.

A number of literary gentlemen in Connecticut have lately affociated for the purpofe of encouraging philofophical refearches, reſearches, and, particularly, for developing the natural hiſtory of that State. The aſſociation has aſſumed the title of *The Connecticut Academy of Arts and Sciences.* The general meeting of the Academy, for the election of officers, is to be held, annually, at New-Haven, on the fourth Tueſday of October; and the other meetings are to be on the fourth Tueſdays of December, February, April, June, and Auguſt, at New-Haven.

CHEMISTRY.

The following articles of chemical news were communicated to Samuel L. Mitchill, M. D. Profeſſor of Chemiſtry in Columbia College, New-York, by James Woodhouſe, M.D. Profeſſor of Chemiſtry in the Univerſity of Pennſylvania, in a letter dated Auguſt 22, 1799:

1. *Of the non-Action of the Nitric Acid on Silver, Copper, and Tin.*

Having occaſion to make a ſolution of ſilver in the nitric acid, ſeveral thin pieces of ſilver were digeſted forty-eight hours, in a ſmall quantity of the moſt pure and concentrated acid, prepared by diſtilling ſtrong ſulphuric acid on nitre, from which the water of cryſtalliſation had been thrown off by means of heat, and the metal was not diſſolved. The temperature of the air varied between 75 and 90 degrees of Fahrenheit's thermometer.

This phenomenon was contrary to what ought to have taken place, according to the chemiſts of all nations, who declare that the nitric acid diſſolves ſilver with the utmoſt rapidity.

Suppoſing that the non-action of the acid was owing to the metal being in ſmall maſſes, the filings of ſilver were tried, but no ſolution took place in the ſpace of two days. Having then added a ſmall quantity of water to the acid, the ſilver was diſſolved in a few minutes.

Repeating this experiment upon copper, the ſame effect happened.

Nitric acid was poured upon copper, and no action was produced; but, uppon the addition of water, ſolution imme-

 diately

diately commenced, and oxygenous and nitrous air was diſcharged; the latter holding a portion of the copper in ſolution, as appeared by immerſing a lighted taper in the nitrous acid gas, when it burned with an enlarged vivid and blue flame. The flame of the taper was frequently blown out, and rekindled by dipping it into the air.

Some concentrated nitric acid was alſo poured upon tin foil, when it remained in a quieſcent ſtate for the ſpace of one week; but, upon the addition of water, the whole was inſtantaneouſly converted into a white oxyd, with the production of a high degree of heat.

The errors of chemiſts, in regard to the action of nitric acid upon tin, will be ſeen more clearly by extracting what has been ſaid upon the ſubject.

Chaptal tells us the nitric acid devours tin, that the decompoſition is ſpeedy, and that the metal is inſtantly precipitated in the form of a white oxyd. The ſame author ſays, Mr. Baumé even pretends that the nitric acid does not diſſolve tin; but Kunckel and the famous Rouelle have maintained the contrary.

Fourcroy declares that tin decompoſes nitric acid, even in the cold, with amazing rapidity, and that this is one of the moſt aſtoniſhingly rapid ſolutions in all chemiſtry.

From what has been ſaid, it appears that Mr. Baumé is right, and that Fourcroy, Chaptal, Rouelle, and Kunckel, uſed an acid diluted with water.

In what manner does water act in theſe experiments?

Dr. Prieſtley ſuppoſes that no air can be produced without water, and that it is neceſſary to the conſtitution of every kind of air: but this throws little light upon the ſubject, and it does not account for the manner in which water acts in promoting the ſolution of ſilver, copper, and tin, in the nitric acid; and nitrous air may be obtained from zinc and biſmuth by the acid, however concentrated.

It may be ſuppoſed that the water merely produces heat by uniting with the acid, and ſo diſſolves the metals; but this is not the caſe; for, if the acid is diluted with water, and ſtands until it is cool, it will ſpeedily diſſolve them.

It

It is a common thing with the teachers of chemiſtry to fold up a portion of the dry nitrat of copper in tin foil, and to let it remain for ſome time in contact with the tin, to ſhow that it will not act upon the metal in a dry ſtate. The tin foil is then unfolded, and a little water is added to the nitrat of copper, and it is again encloſed in the tin; when a violent action enſues, accompanied with ſparks of fire, and a diſcharge of nitrous air.

The intention of this experiment is to ſhow that bodies do not act upon each other in a dry ſtate—*corpora non agunt niſi ſoluta.* But from the experiments which have been related, of the non-action of the nitric acid on tin, the explanation of what takes place muſt be ſought for in the action of the water on the nitric acid of the nitrat of copper.

Some writers have taken notice of the production of ammoniac, when nitrous acid is added to copper and tin. As the concentrated acid has no action on theſe metals, the ammoniac muſt be produced by the hydrogen of the water uniting with the azot of the nitric acid, while its oxygen, and that of the water, unites to the tin and copper, and converts them into oxyds.

Having related theſe facts, the language of chemiſts in future ought to be—The nitric acid has no action on ſilver, copper, and tin; but, if water be added to the acid, ſolution ſpeedily takes place.

Dr. Hope has taken notice of the non-action of the nitric acid on ſtrontian earth; and Mr. Leonhardi tells us, that it quickly deſtroys wool and ſilk, but that linen may remain immerſed in a bottle of the ſtrong acid a whole day without injury.

2. *Of the Difference in the Quantity of Ammoniac obtained from Bones, by diſtilling with and without a Lute.*

A quantity of the bones of horſes and cows were placed in a diſtilling apparatus, formed of iron, which communicated with the worm of a refrigeratory, to which a large glaſs receiver was annexed. Upon applying a high degree of heat, three ounces of volatile alkaline ſpirit, impregnated with the black

black animal oil of Dippel, were obtained in three hours. The receiver was cloſely luted to the worm, and the air in it was perfectly tranſparent. Upon taking away a part of the lute, in ſuch a manner as to permit the air of the atmoſphere to enter the receiver, it became immediately filled with a thick brown yellow cloud of ſmoke.

Having made a variety of comparative experiments, to determine the difference in the quantity of the product, by diſtilling with and without the lute, it was found that five times as much of the volatile alkaline ſpirit could be obtained by carrying on the diſtillation without the lute, as could be procured, in the ſame ſpace of time, with the application of the lute.

Lavoiſier ſuppoſes, that when ammoniac is obtained from animal ſubſtances, the hydrogen and the azot of theſe bodies unite together, and form the volatile alkali; but it appears, from what has been ſaid, that the azotic air of the atmoſphere enters into the worm of the refrigeratory, joins the hydrogen of the bones, and ſo forms the ammoniac.

Manufacturers of the volatile ſpirit of ſal ammoniac may take ſome valuable hints from theſe experiments.

3. *Of Putrid Urine expoſed to the Froſt.*

A quart of the moſt putrid urine, and of as yellow a colour as gamboge, was expoſed two nights to intenſe cold, when it became perfectly ſweet, and was as colourleſs as rock water.

May not this wonderful change be attributed to the agency of the oxygen gas of the cold atmoſpheric air?

The acid of citrons not only neutraliſes the volatile alkali of putrid ſubſtances, but completely deſtroys the nauſeous ſmell which exiſts independent of the ammoniac. The ſulphuric and muriatic acids have no ſuch effect. Does the oxygen of the citric acid act here likewiſe? Lowitz, a Ruſſian chemiſt, ſuppoſes that charcoal neutraliſes the putrid effluvia of animal bodies; but, in my opinion, it acts mechanically, in preventing the putrid particles of matter from flying into the air.

4. Of

4. *Of Starch prepared from the Fruit of the Æsculus Pavia, or Horse Chestnut.*

In the 29th number of the Repertory of Arts, there is an account of a patent, obtained by Lord William Murray, for making ſtarch from the fruit of the æſculus hippocaſtanum. A writer in the London Monthly Magazine for 1798 ſays, he has repeatedly, and in various ways, endeavoured to make ſtarch of the fruit, but always unſucceſsfully; for it turns to a yellow colour.

The fruit of our æſculus pavia is much larger than that of the æſculus hippocaſtanum, and is of a white colour: that of the hippocaſtanum is yellow.

A ſingle nut, dried, weighed half an ounce and twenty-five grains, and yielded forty-four grains of fine ſtarch.

I prepared half a pound of this ſtarch from the nuts of the æſculus pavia, and have kept it two years, and the white colour is no way impaired. It is ſuperior to the fineſt Poland ſtarch, and has been uſed, by ſeveral ladies, to ſtarch various articles of dreſs, without imparting any yellow colour to them.

The method of preparing it is, to take off the ſhells from the nuts with a knife; grate them in a veſſel of water which will hold the fine particles of the ſtarch ſuſpended; when they are to be decanted into another veſſel, which muſt remain at reſt until the ſtarch ſubſides to the bottom. The water is then to be poured off, and freſh is to be added, and the ſtarch is to be well ſtirred about in it; when it muſt be again permitted to ſubſide. The water is then to be thrown away, and the ſtarch is to be dried in the ſun.

The water of the firſt waſhing holds a poiſonous matter in ſolution, which, when evaporated to the conſiſtence of an extract, and mixed with dough, will intoxicate and ſwell the bellies of ſmall fiſhes.

5. *Of the Compoſition of Flores Martiales and Ens Veneris.*

The French chemiſts ſay that the flores martiales are compoſed of ſal ammoniac, coloured by an oxyd of iron; and

8 that

that ens veneris is ſal ammoniac, coloured by an oxyd of copper: but I can eaſily prove that the one conſiſts of ſal ammoniac and the muriat of iron, and the other of ſal ammoniac and the muriat of copper.

1ſt, When iron or copper is added to ſal ammoniac, the ſalt is decompoſed, the iron or copper unites to the muriatic acid and forms a muriat of iron or copper, which, when expoſed to heat in cloſe veſſels, ſublimes with the ſal ammoniac, while the volatile alkali is ſet at liberty.

2dly, Flores martiales, or ens veneris, may be made by ſubliming ſal ammoniac with the muriat of iron or copper, prepared by adding the muriatic acid to theſe metals.

3dly, When flores martiales, or ens veneris, are diſſolved in water, and ſuffered to cryſtalliſe, the muriat of iron and copper will be obtained ſeparate from the ſal ammoniac.

6. *Of Aromatic Oils, obtained from the Pellicle which envelops the Seeds of the Laurus Saſſafras and Laurus Benzoin.*

The method of obtaining theſe oils is, to boil the pellicle which ſurrounds the ſeeds of the ſaſſafras and benjamin-tree in water; when they float upon its ſurface, from which they may be ſkimmed with a ſpoon.

That of the ſaſſafras differs materially from the oil obtained from the bark of the root of this tree. Its aroma is different; it is much lighter, and it congeals in a higher degree of heat.

The oil of the benzoin-tree is a delightful aromatic, is very inflammable, and might be uſed as a ſpice in food, and in all thoſe diſeaſes in which the aromatic oils are uſeful. It has been tried with ſucceſs, as an external application, in a ſevere caſe of chronic rheumatiſm. One half pound of the pellicle of the ſeeds will yield ſeveral ounce meaſures of oil.

7. *Of the Eudiometer.*

The eudiometer is an uſeleſs inſtrument in aſcertaining the purity of atmoſpheric air.

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1ſt, Becauſe nitrous air can never be obtained of the ſame degree of ſtrength.

2dly, When one meaſure of nitrous air is added to one meaſure of atmoſpheric air, the abſorption will be great or ſmall, according to the time the air remains over the water, or is agitated in it.

Having made ſeven trials with this inſtrument, with the ſame atmoſpheric air, I obtained a diminution, at the firſt experiment, of,

	0	60
2d,	0	56
3d,	0	95
4th,	0	87
5th,	0	90
6th,	0	93
7th,	0	100

From this it is evident that Dr. Davidſon was deceived in ſuppoſing that the air of Martinique was much purer than the air of Europe, and that the error lay in his inſtrument. The nitrous air which I uſed was procured from nitric acid diluted with water and copper.

8. *Of the Baſe of the Muriatic Acid.*

Mr. William Lambe, in an eſſay in the fifth volume of the Mancheſter Memoirs, has attempted to prove that ſulphurated hydrogen is the baſe of the muriatic acid. He obtained ox. muriatic gas by dropping ſulphuric acid upon the reſiduum left after evaporating water which had been impregnated with hepatic gas in which iron and mangeneſe had been digeſted. I have performed this experiment, and the reſult is exactly as ſtated by Mr. Lambe.

Two drachms of the filings of bar-iron were placed in twenty-two ounce meaſures of diſtilled water, which had been impregnated with ſulphurated hydrogen gas in Nooth's apparatus. In five days twelve ounce meaſures of inflammable air eſcaped from the water. Six ounces of the clear fluid evaporated to dryneſs, left a reſiduum, conſiſting of dephlogiſticated muriat of iron, which attracted the moiſture of

 the

the atmoſphere. A few drops of ſulphuric acid let fall upon it, produced an efferveſcence, and white clouds of ox. muriatic gas eſcaped, as was very evident from the ſmell, and from the teſts generally uſed to detect the preſence of this gas.

The diſcovery of Mr. Lambe is the beſt which has been made for many years.

METHOD OF RESTORING WRITING WHICH HAS BEEN EFFACED BY OXYGENATED MURIATIC ACID.

As the art of effacing writing by means of this acid might give riſe to acts of fraud, the following letter from the Secretary-general of the French Miniſter of the Interior has been lately publiſhed in the official paper the Moniteur: it is addreſſed to the editor:—" C. Guietand, apothecary of the Grand Hoſpital of Humanity, the ci-devant Hotel-Dieu, has diſcovered the means of reviving writing effaced by oxygenated muriatic acid *. His proceſs has been examined by chemiſts, who have found, that it is eaſy, as well as ſpeedy in its effect, and that it deſerves to be known.

" The miniſter, therefore, has determined that it ſhall be made known by being printed. I herewith tranſmit you an account of the proceſs; and I requeſt that you will inſert it in your next number."

" *May* 26, 1800. (Signed) F. DESPORTES."

The ſulphure of ammonia and the pruſſiat of potaſh are the two ſubſtances which C. Guitand prefers for reviving writing effaced by oxygenated muriatic acid. The diſagreeable ſmell of the ſulphure of ammonia will, no doubt, prevent many perſons from employing it: it has, however, this advantage, that it does not ſtain the paper, and that it makes the writing re-appear when ſimply expoſed to its vapour in a cloſe veſſel or under a bell-glaſs. But to produce this effect more ſpeedily, it will be beſt to pour a few drops of it into water, and to dip the paper in it: the effaced writing ſoon re-appears of a dark brown colour, and exceedingly legible.

* The proceſs for effacing writing by this acid may be ſeen in the Philoſophical Magazine, Vol. II. p. 28.

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The pruſſiat of potaſh gives a blue colour to the writing, whether that effaced, or that which has been ſubſtituted for it; which is ſufficient to detect fraud of this kind: it gives to the paper, at the ſame time, a ſlight tint of blue. To employ it, you put into a baſon, or any other deep veſſel, and of a ſuitable ſize, as much water as may be ſufficient for immerſing the leaves of paper which you wiſh to ſubject to examination. You then add about half a thimble-full of the pruſſiat; and, when it has been well mixed with the water, you immerſe in the liquid a leaf of paper. When it has imbibed the liquid well, a few drops of ſulphuric or any other acid, poured into the mixture in ſuch a manner as to render it ſlightly acid, will be ſufficient to make the writing reappear. It is well known that the ſame ſubſtance poſſeſſes the property of reviving old writing if employed in the like manner.

BOTANY.

On ſome mountains in South Pruſſia a plant was diſcovered in autumn laſt which yields a fine beautiful ſilk. It grows on dry mountains, covered with buſhes, to a height of three or four feet; has roots of the ſhape of quitch-graſs; heart-ſhaped leaves, of a light-green colour; a ſoft ſtalk, of the thickneſs of a quill, covered with a firm ſilky baſt; and a fruit ſimilar to that of the ſilk-plant of Syria, with the difference only, that the pods which contain the ſeed and ſilk-wool are more pointed and ſmaller. The ſilk cannot be diſtinguiſhed from that of Syria.

A NEW VOLCANIC ISLAND.

A letter from the celebrated naturaliſt Pallas, dated the Tauride, announces, that on the 5th of September laſt an iſland ſuddenly made its appearance in the ſea of Azof, at the diſtance of 150 toiſes from the ſhore. This phenomenon was preceded by a noiſe like that of thunder, and accompanied with an eruption of ſmoke and flames, the exploſion of which was as loud as that of a large piece of ordnance: at the ſame time a violent ſhock of an earthquake was experienced from Cuban as far as Catrinodan.

VACCINE INOCULATION.

An eſtabliſhment has lately been formed at Paris for the vaccine inoculation. "A trial of the vaccine inoculation," ſays a letter from Paris, dated June 3, "was begun here yeſterday. Thirty children were inoculated with matter received from London, and according to the directions given by Dr. Pearſon. The matter was tranſmitted hither in phials filled with hydrogen gas cloſed by mercury, and covered with a piece of bladder. The greater part of theſe children were inoculated with the matter on lancets, which Dr. Pearſon recommended as moſt efficacious; ſome of them with threads, applied by means of veſicatories; and a ſmall number with matter collected on glaſs. A daily report will be given of theſe trials, which are made at Vaugirard, the houſe of C. Colon, at which the members of the medical committee are to meet every day at one in the afternoon."

The following letter on this ſubject from Mr. Stromeyer, of Hanover, to Mr. Hannehmann, dated 24th March laſt, contains ſome further information reſpecting the ſtate of vaccine inoculation on the Continent:

"This year we have inoculated forty perſons, as well with the vaccine matter received of Dr. Pearſon, as with that from Dr. Jenner; all of whom underwent the diſeaſe properly.

"Betwixt the London and Glouceſter vaccine matter, it appears to me there ſubſiſts an eſſential difference. The London matter produces frequently an eruption of ſmall pimples, but they diſappear within a day or two at furtheſt. Dr. Pearſon calls theſe eruptions *puſtules*. The Glouceſter matter has never produced this effect here; but it frequently *occaſioned ulcerations of the inoculated part, of a tedious and long duration, which the former matter never did*; on account of which I now only make uſe of Dr. Pearſon's vaccine matter.

"The nettle-fever-like eruptions I have obſerved ſeveral times, but *never that ſort of eruption, repeatedly noticed in London, which ſo much reſembles the ſmall-pox*.

"Of all thoſe I have inoculated, none of them were ſeverely ill during the diſeaſe. Their general ſymptoms were, a ſlight fever, depreffion of fpirits, and a pale countenance. In a few inftances, in which the inoculated part attained to a due degree of maturity, and where a fwelling of the axillary glands was obferved, no figns of any illnefs exifted, at leaft no fymptoms could be obferved which proved the infection to have acted upon the whole fyftem. Thefe I fhall re-inoculate. Should the refult be as before, would it be fair to fuppofe them fecured againft fmall-pox infection?

" The moft of our phyficians here exclaim againft the vaccine inoculation; and their only weak and almoft refuted argument is—Are people thus fecured for all their lifetime againft the fmall-pox? Neverthelefs I have the fatisfaction to fee a partiality for it difplayed by the greater part of the public; and I have already inoculated many noblemen's children, as well as thofe of other very refpectable inhabitants of Hanover; and, without doubt, I fhall always have fubjects fufficient for continuing this inoculation.

" On the ninth, tenth, or eleventh day, when the inoculated puftule is filled with lymph, and the furrounding inflammation is complete, I then ufually open it, collect the lymph on cotton thread, and immediately lay fome more thread upon it, which is removed the following day: all thefe ferve for future inoculations. After this period I have hitherto defifted to repeat collecting more matter from fuch a puftule, but wifh to know how long it might be continued.

" I fhall efteem it a great favour if you will lay thefe refults and queftions before Dr. Pearfon, and beg he will have the goodnefs to anfwer them.

" In Langenhagen, a village near Hanover, I have inoculated the clergyman's children along with four others; which induced many farmers and peafants to have their children alfo inoculated.

" From Halle, Halberftadt, and many other places, I have received applications for vaccine matter; which proves that the continent has many adherents to the vaccine inoculation as well as England.

" Dr. Ballhorn, an intimate and long acquaintance of mine, affifted me in moft of thefe operations, in order that he might vifit viſit the inoculated patients when my time would not admit of it.

"In Nos. 15 and 16 of the Hanoverian Magazine we have publiſhed our laſt year's inoculations, and we intend continuing the ſame from time to time. With Dr. De Carro, whom Dr. Pearſon knows, and who occupies himſelf greatly with this inoculation, we correſpond.

"Only one of our this year's patients has been inoculated for the ſmall-pox: a puſtule was produced upon the inoculated part, accompanied with a ſlight ſurrounding inflammation, but no other effects whatever."

NEW VOYAGE OF DISCOVERY.

A new voyage of diſcovery to the South Sea has been lately projected by the French. The National Inſtitute have obtained two corvettes for that purpoſe, which are now fitting out at Havre. They are to follow the route purſued by Dentrecaſteaux, and to continue the reſearches made during the late expedition into thoſe ſeas in order to determine ſeveral important points of geography. They will explore the ſouth-eaſt coaſts of New Holland, and bring back ſuch vegetable productions as are likely to anſwer the purpoſes of cultivation in Europe. The command of this new enterpriſe is entruſted to Captain Baudin, who has already diſtinguiſhed himſelf by his nautical talents and knowledge of the natural ſciences, and who has enriched the Muſeum of Natural Hiſtory at Paris with various curious objects which he collected during a former voyage. C. Quenot, an able navigator and obſerver; and C. Ciccolini, a ci-devant knight of Malta, who has long applied to aſtronomical purſuits in the College de France, are the aſtronomers propoſed for this expedition.

DISCOVERIES IN AFRICA.

The following particulars have been received reſpecting the expedition of Horneman to the interior parts of Africa: —In the month of September 1798, he ſet out from Cairo, where he was expoſed to great danger from the plague, and proceeded on his journey with the caravan bound to Fezzan. In the oaſis of Seewah he found Brown's obſervations re-

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ſpecting the ſmall ancient buildings there, confirmed. He traced out the ground plan of the walls by which they were ſurrounded, in ſuch a manner, that it appears highly probable that theſe ruins are of great antiquity, and perhaps belonged to the temple of Jupiter Ammon. During the courſe of their journey, after leaving this place, the caravan was ſtopped by about a hundred Arabs on horſeback, who inſiſted on Horneman and his fellow-traveller, a native of Cologne, who, after reſiding long in Egypt, had become a Mahometan, being delivered up to them, on a pretence of their being French ſpies; but they ſaved themſelves by repeating Arabic prayers from the Coran. In forty-one days the caravan arrived at Murſok. Here Horneman's fellow-traveller died, and he himſelf was attacked by an endemial fever, which prevented him from continuing his journey with the caravan to Soudan. He therefore reſolved, as ſoon as he was recovered, to remain at Tripoli till the departure of the next caravan for Soudan, and in the mean time to arrange his papers, to make copies of them, and to tranſmit them to Europe by an Engliſh veſſel. In the month of December he intended to return to Murſok, and to wait there for the departure of the next caravan to Fezzan in the ſpring. It appears by his letters, that, as he is now well acquainted with the languages and manners of the Mahometans, he entertains great hopes of being able to accompliſh his expedition in a ſucceſsful manner.

BREWING.

One of the foreign journals announces, that Profeſſor Scherer, of Vienna, has diſcovered that beet-roots which have been ſubjected to the preſs, and deprived of the greater part of their juice, after being dried, and treated in the ſame manner as grain intended to be made into malt, may be employed in the brewing of beer. A fact of a ſimilar kind, which may furniſh ſome uſeful economical hints, is, that very good brandy may be made from gooſeberries: a gentleman of our acquaintance has now in his poſſeſſion ſome brandy procured from the huſks of gooſeberries left in making gooſeberry wine, which is little inferior to the beſt French brandy.

LETTER

LETTER TO THE EDITOR.

"SIR, *Clifton, June* 8, 1800.

"By a number of lately received communications, I find that much greater activity has been lately exerted in trying the effect of nitric acid in the venereal disease than I supposed. I had abandoned every idea of publishing another separate collection on this subject; but so many curious facts have been ascertained by my correspondents, and the perfect and permanent antisiphylitic virtue of the acid, when properly managed, so entirely established, that I cannot hesitate to forward the papers, with which I have been favoured, to the press. They will be found to contain interesting observations on other subjects.

"The printing of Mr. Davy's researches on nitrous (gaseous) oxyd will be finished this week. The volume will contain ample details on the effects produced by this gas when respired.

"I am, Sir, yours very respectfully,

"*Mr. Tilloch.* THOMAS BEDDOES."

DEATHS.

At Madrid, in January last, J. Bapt. Munoz, well known by his History of the New World, which has been translated into different languages. This work is not yet completed.—On the 20th of January, at Paris, J. B. Le Roy, Member of the National Institute, Fellow of the Royal Society of London, and of the Society of Philadelphia.—At Harderwyck, in Holland, on the 1st of February, in the 32d year of his age, Dr. C. P. Schacht, professor of medicine, botany, chemistry, and natural history.—At Milan, in prison, during the course of the same month, C. Barletti, formerly professor of natural philosophy at Pavia, and author of several works on natural philosophy and chemistry.—At Pavia, in March, Prisciani, professor of physiology, and author of several works on that subject. He has left to the university his collection of preparations respecting comparative anatomy, all made by himself, and which amount to several thousand articles.

THE

PHILOSOPHICAL MAGAZINE.

JULY 1800.

I. *An Account of ſome Experiments on the Fecundation of Vegetables. In a Letter from* THOMAS ANDREW KNIGHT, *Eſq. to the Right Hon. Sir* JOSEPH BANKS, *K. B. P. R. S.**

THE reſult of ſome experiments which I have amuſed myſelf in making on plants, appearing to me to be intereſting to the naturaliſt, by proving the exiſtence of ſuperfœtation in the vegetable world, and being likely to conduce to ſome improvements in agriculture, I have taken the liberty to communicate them to you.

The breeders of animals have very long entertained an opinion, that conſiderable advantages are obtained by breeding from males and females not related to each other. Though this opinion has lately been controverted, the number of its oppoſers has gradually diminiſhed; and I can ſpeak from my own obſervation and experience, that animals degenerate, in ſize at leaſt, on the ſame paſture, and in other reſpects under the ſame management, when this proceſs of croſſing the breed is neglected.

The cloſe analogy between the animal and vegetable world, and the ſexual ſyſtem equally pervading both, induced me to ſuppoſe, that ſimilar means might be productive of ſimilar effects in each; and the event has, I think, fully juſtified this opinion. The principal object I had in view, was to obtain new and improved varieties of the apple, to ſupply the

* From *Tranſactions of the Royal Society of London for* 1799.

place of those which have become diseased and unproductive, by having been cultivated beyond the period which nature appears to have assigned to their existence. But, as I foresaw that several years must elapse before the success or failure of this process could possibly be ascertained, I wished, in the interval, to see what would be its effects on annual plants. Amongst these, none appeared so well calculated to answer my purpose as the common pea; not only because I could obtain many varieties of this plant, of different forms, sizes, and colours; but also, because the structure of its blossom, by preventing the ingress of insects and adventitious farina, has rendered its varieties remarkably permanent. I had a kind growing in my garden, which, having been long cultivated in the same soil, had ceased to be productive, and did not appear to recover the whole of its former vigour, when removed to a soil of a somewhat different quality; on this, my first experiment, in 1787, was made. Having opened a dozen of its immature blossoms, I destroyed the male parts, taking great care not to injure the female ones; and, a few days afterwards, when the blossoms appeared mature, I introduced the farina of a very large and luxuriant gray pea into one half of the blossoms, leaving the other half as they were. The pods of each grew equally well; but I soon perceived that in those into whose blossoms the farina had not been introduced, the seeds remained nearly as they were before the blossoms expanded, and in that state they withered. Those in the other pods attained maturity, but were not in any sensible degree different from those afforded by other plants of the same variety; owing, I imagine, to the external covering of the seed (as I have found in other plants) being furnished entirely by the female. In the succeeding spring, the difference, however, became extremely obvious; for the plants from them rose with excessive luxuriance, and the colour of their leaves and stems clearly indicated that they had all exchanged their whiteness for the colour of the male parent: the seeds produced in autumn were dark gray. By introducing the farina of another white variety, (or, in some instances, by simple culture,) I found this colour was easily discharged, and a numerous variety of new kinds produced,

produced, many of which were, in ſize, and in every other reſpect, much ſuperior to the original white kind, and grew with exceſſive luxuriance, ſome of them attaining the height of more than twelve feet. I had frequent occaſion to obſerve in this plant a ſtronger tendency to produce purple bloſſoms; and coloured ſeeds, than white ones: for, when I introduced the farina of a purple bloſſom into a white one, the whole of the ſeeds in the ſucceeding year became coloured; but, when I endeavoured to diſcharge this colour, by reverſing the proceſs, a part only of them afforded plants with white bloſſoms; this part ſometimes occupying one end of the pod, and being at other times irregularly intermixed with thoſe which, when ſown, retained their colour. It may perhaps be ſuppoſed, that ſomething might depend on the quantity of farina employed; but I never could diſcover, in this, or in any other experiment in which ſuperfœtation did not take place, that the largeſt or ſmalleſt quantity of farina afforded any difference in the effect produced.

The diſſimilarity I obſerved in the offspring afforded by different kinds of farina, in theſe experiments, pointed out to me an eaſy method of aſcertaining whether ſuperfœtation (the exiſtence of which has been admitted amongſt animals) could alſo take place in the vegetable world. For, as the offspring of a white pea is always white, unleſs the farina of a coloured kind be introduced into the bloſſom, and, as the colour of the gray one is always transferred to its offspring, though the female be white, it readily occurred to me, that if the farina of both were mingled, or applied at the ſame moment, the offspring of each could be eaſily diſtinguiſhed.

My firſt experiment was not altogether ſucceſsful; for the offspring of five pods (the whole which eſcaped the birds) received their colour from the coloured male. There was, however, a ſtrong reſemblance to the other male, in the growth and character of more than one of the plants; and the ſeeds of ſeveral, in the autumn, very cloſely reſembled it in every thing but colour. In this experiment I uſed the farina of a white pea, which poſſeſſed the remarkable property of ſhrivelling exceſſively when ripe; and, in the ſecond year, I obtained white ſeeds, from the gray ones above mentioned, perfectly ſimilar to it. I am ſtrongly diſpoſed to believe, that the ſeeds were here of common parentage; but I do not conceive myſelf to be in poſſeſſion of facts ſufficient to enable me to ſpeak with deciſion on this queſtion.

If, however, the female afford the firſt organiſed atom, and the farina act only as a ſtimulus, it appears to me by no means impoſſible, that the exploſion of two veſicles of farina, at the ſame moment, (taken from different plants,) may afford ſeeds (as I have ſuppoſed) of common parentage; and, as I am unable to diſcover any ſource of inaccuracy in this experiment, I muſt believe this to have happened.

Another ſpecies of ſuperfœtation (if I have juſtly applied that term to a proceſs in which one ſeed appears to have been the offspring of two males) has occurred to me ſo often, as to remove all poſſibility of doubt as to its exiſtence. In 1797, that year after I had ſeen the reſult of the laſt-mentioned experiment, having prepared a great many white bloſſoms, I introduced the farina of a white and that of a gray pea, nearly at the ſame moment, into each; and as, in the laſt year, the character of the coloured male had prevailed, I uſed its farina more ſparingly than that of the white one; and now almoſt every pod afforded plants of different colours. The majority, however, were white; but the characters of the two kinds were not ſufficiently diſtinct to allow me to judge with preciſion whether any of the ſeeds produced were of common parentage or not. In the laſt year I was more fortunate: having prepared bloſſoms of the little early frame pea, I introduced its own farina, and immediately afterwards that of a very large and late gray kind, and I ſowed the ſeeds thus obtained in the end of the laſt ſummer. Many of them retained the colour and character of the ſmall early pea, not in the ſlighteſt degree altered, and bloſſomed before they were eighteen inches high; whilſt others, (taken from the ſame pods,) whoſe colour was changed, grew to the height of more than four feet, and were killed by the froſt, before any bloſſoms appeared.

It is evident, that in theſe inſtances ſuperfœtation took place; and it is equally evident, that the ſeeds were not all of common parentage. Should ſubſequent experience evince that a ſingle plant may be the offspring of two males, the analogy between animal and vegetable nature may induce ſome curious conjecture relative to the proceſs of generation in the animal world.

In the courſe of the preceding experiments, I could never obſerve that the character, either of the male or female, in this plant, at all preponderated in the offspring; but, as this point appeared intereſting, I made a few trials to aſcertain it. And as the foregoing obſervations had occurred in experiments made principally to obtain new and improved varieties of the pea, for garden culture, I choſe, for a ſimilar purpoſe, the more hardy varieties uſually ſown in the fields. By introducing the farina of the largeſt and moſt luxuriant kinds into the bloſſoms of the moſt diminutive, and by reverſing this proceſs, I found that the powers of the male and female, in their effects on the offspring, are exactly equal. The vigour of the growth, the ſize of the ſeeds produced, and the ſeaſon of maturity, were the ſame, though the one was a very early, and the other a late variety. I had, in this experiment, a ſtriking inſtance of the ſtimulative effects of croſſing the breeds; for the ſmalleſt variety, whoſe height rarely exceeded two feet, was increaſed to ſix feet; whilſt the height of the large and luxuriant kind was very little diminiſhed. By this proceſs, it is evident, that any number of new varieties may be obtained; and it is highly probable, that many of theſe will be found better calculated to correct the defects of different ſoils and ſituations, than any we have at preſent; for I imagine that all we now poſſeſs, have in a great meaſure been the produce of accident; and it will rarely happen, in this or any other caſe, that accident has done all that art will be found able to accompliſh.

The ſucceſs of my endeavours to produce improved varieties of the pea, induced me to try ſome experiments on wheat; but theſe did not ſucceed to my expectations. I readily obtained as many varieties as I wiſhed, by merely ſowing the different kinds together; for the ſtructure of the bloſſom of this plant (unlike that of the pea) freely admits the ingreſs of adventitious farina, and is thence very liable to ſport in varieties. Some of thoſe I obtained were excel-

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lent; others very bad; and none of them permanent. By ſeparating the beſt varieties, a moſt abundant crop was produced; but its quality was not quite equal to the quantity, and all the diſcarded varieties again made their appearance. It appeared to me an extraordinary circumſtance, that in the years 1795 and 1796, when almoſt the whole crop of corn in the iſland was blighted, the varieties thus obtained, and theſe only, eſcaped, in this neighbourhood, though ſown in ſeveral different ſoils and ſituations.

My ſucceſs on the apple (as far as long experience and attention have enabled me to judge from the cultivated appearance of trees which have not yet borne fruit) has been fully equal to my hopes. But, as the improvement of this fruit was the firſt object of my attention, no probable means of improvement, either from ſoil or aſpect, were neglected. The plants, however, which I obtained from my efforts to unite the good qualities of two kinds of apple, ſeem to poſſeſs the greateſt health and luxuriance of growth, as well as the moſt promiſing appearance in other reſpects. In ſome of theſe, the character of the male appears to prevail; in others, that of the female; and in others, both appear blended, or neither is diſtinguiſhable. Theſe variations, which were often obſervable in the ſeeds taken from a ſingle apple, evidently ariſe from the want of permanence in the character of this fruit when raiſed from ſeed.

The reſults of ſimilar experiments on another fruit, the grape, were nearly the ſame as of thoſe on the apple, except that, by mingling the farina of a black and a white grape, juſt as the bloſſoms of the latter were expanding, I ſometimes obtained plants, from the ſame berry, ſo diſſimilar, that I had good reaſon to believe them the produce of ſuperfœtation. By taking off the cups, and deſtroying the immature male parts, (as in the pea,) I perfectly ſucceeded in combining the characters of different varieties of this fruit, as far as the changes of form, and autumnal tints, in the leaves of the offspring, will allow me to judge.

Many experiments, of the ſame kind, were tried on other plants; but it is ſufficient to ſay, that all tended to evince, that improved varieties of every fruit and eſculent plant may be be obtained by this procefs, and that nature intended that a fexual intercourfe fhould take place between neighbouring plants of the fame fpecies. The probability of this will, I think, be apparent, when we take a view of the variety of methods which nature has taken to difperfe the farina, even of thofe plants in which it has placed the male and female parts within the fame empalement. It is often fcattered by an elaftic exertion of the filaments which fupport it, on the firft opening of the bloffom; and its exceffive lightnefs renders it capable of being carried to a great diftance by the wind. Its pofition within the bloffom is generally well adapted to place it on the bodies of infects; and the villous coat of the numerous family of bees, is not lefs well calculated to carry it. I have frequently obferved, with great pleafure, the difperfion of the farina of fome of the graffes when the fun had juft rifen in a dewy morning. It feemed to be impelled from the plant with confiderable force; and, being blue, was eafily vifible, and very ftrongly refembled, in appearance, the explofion of a grain of gunpowder. An examination of the ftructure of the bloffoms of many plants, will immediately point out, that nature has fomething more in view, than that its own proper males fhould fecundate each bloffom; for the means it employs are always thofe beft calculated to anfwer the intended purpofe. But the farina is often fo placed, that it can never reach the fummit of the pointal, unlefs by adventitious means; and many trials have convinced me, that it has no action on any other part of it. In promoting this fexual intercourfe between neighbouring plants of the fame fpecies, nature appears to me to have an important purpofe in view; for, independent of its ftimulative power, this intercourfe certainly tends to confine, within more narrow limits, thofe variations which accidental richnefs or poverty of foil ufually produces. It may be objected, by thofe who admit the exiftence of vegetable mules, that, under this extenfive intercourfe, thefe muft have been more numerous; but my total want of fuccefs, in many endeavours, to produce a fingle mule plant, makes me much difpofed to believe that hybrid plants have been miftaken for mules; and to doubt

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(with all the deference I feel for the opinions of Linnæus and his illustrious followers) whether nature ever did, or ever will, permit the production of such a monster. The existence of numerous mules in the animal world, between kindred species, is allowed; but nature has here guarded against their production, by impelling every animal to seek its proper mate; and, amongst the feathered tribe, when, from perversion of appetite, sexual intercourse takes place between those of distinct genera *, it has, in some instances at least, rendered the death of the female the inevitable consequence. But, in the vegetable world, there is not any thing to direct the male to its proper female: its farina is carried, by winds and insects, to plants of every different genus and species; and it therefore appears to me, (as vegetable mules certainly are not common,) that nature has not permitted them to exist at all.

I cannot dismiss this subject without expressing my regret, that those who have made the science of botany their study, should have considered the improvement of those vegetables which, in their cultivated state, afford the largest portion of subsistence to mankind and other animals, as little connected with the object of their pursuit. Hence it has happened, that whilst much attention has been paid to the improvement of every species of useful animal, the most valuable esculent plants have been almost wholly neglected. But, when the extent of the benefit which would arise to the agriculture of the country, from the possession of varieties of plants which, with the same extent of soil and labour, would afford even a small increase of produce, is considered, this this subject appears of no inconsiderable importance. The improvement of animals is attended with much expense, and the improved kinds necessarily extend themselves slowly; but a single bushel of improved wheat or peas, may in ten years be made to afford seed enough to supply the whole island; and a single apple, or other fruit-tree, may within the same time be extended to every garden in it. These considerations have been the cause of my addressing the fore-

* This is said to be the case with the drake and the hen.

going

going obſervations to you at this time; for it was much my wiſh to have aſcertained, before I wrote to you, whether in any inſtance a ſingle plant can be the offspring of two male parents. The deciſion of that queſtion muſt of neceſſity have occupied two years, and muſt therefore be left to the teſt of future experiment.

II. *Chemical Experiments and Obſervations on the Preparation of Sugar from vegetable Productions found in Europe. By* Sigismund Frederick Hermbstadt *.

THE chemical examination of vegetable ſubſtances, and the reſearches made reſpecting their admixture and component parts, furniſh ſufficient proofs that the Eaſt and Weſt Indies are not the only countries which nature has bleſſed with ſaccharine plants. Saccharine matter is diffuſed in ſuch abundance throughout the vegetable kingdom, that nothing is neceſſary but perſeverance to diſcover thoſe individual links of the grand chain from which it may be extracted in the greateſt plenty, in the pureſt manner, and at the cheapeſt rate.

Of all the plants hitherto examined for this purpoſe. There is none, without doubt, that approaches ſo near to the ſugar-cane as the maple-tree, particularly the ſugar-maple *acer ſaccharinum*, and the ſilver-maple *acer daſycarpum*. Theſe trees have been employed in North America more than half a century for making ſugar, and during the laſt eight years with very great ſucceſs. I have learned by my own experiments, purpoſely made on various kinds of the maple, and on a pretty large ſcale, for three years, ſince the winter of 1796, not only that they may all be employed with greater or leſs advantage for the production of ſugar, but that the ſugar and ſilver maples which grow in Germany, and even on indifferent ſoil, when their juice is boiled, will produce ſugar fully equal to the raw or moſcavado ſugar of the Weſt Indies, and at ſo cheap a rate, that a pound will not coſt

* From *Schriften der Geſellſchaft naturforſchender Freunde zu Berlin*, Vol. II.

more than four-pence or five-pence. But it may be readily ſeen, that when this labour can be undertaken on a large ſcale, in which caſe one workman, during the proceſs of tapping, might be able to attend 500 trees, and if the liquor could be boiled with coals or turf, a pound of ſuch ſugar would not coſt nearly ſo much. The proceſs of boiling the juice is ſo ſimple that it may be undertaken by every farmer. As a proof of this I ſhall obſerve, that Count Von Podevils, of Guſon, having brought his huntſman to me in order that he might be preſent at the boiling of maple juice, and to learn the method, he ſoon after tried the proceſs himſelf on his maſter's eſtate, and ſent him a pretty large portion of maple ſugar as a ſpecimen. But it appears, from various experiments, that the ſugar and ſilver maples are the propereſt for this purpoſe. As the aſh-leaved maple, *acer negundo*; common maple, *acer campeſtre*; Norway maple, *acer plantoides*; great maple or baſtard ſycamore, *acer pſeudo-platanus*; and all the other ſpecies, are leſs abundant in juice, and as the juice contains leſs ſaccharine matter, the manufacturing of maple ſugar can be expected to become advantageous only when ſufficient plantations of the ſilver and ſugar maples have been formed, and have come to maturity, which will require a period of from twenty-five to thirty years *.

In

* I cannot omit taking this opportunity to refute ſome ridiculous ideas which have been entertained in regard to the maple. Sometimes it is ſaid that it is apt to be killed by the ſeverity of the froſt in winter; ſometimes that it dies after being tapped; and ſometimes that it is deſtroyed by caterpillars. In regard to the firſt objection, I ſhall refer to the maple plantations of Count Veltheim at Harbke. The greater part of the ſugar and ſilver maples there are above thirty years old; they withſtood the cold winters of the years 1776, 1788, and 1793; and will ſtill continue to thrive, for the leaſt trace of corruption, not even in the branches, can be obſerved in them. Muſt not theſe trees have been long ago deſtroyed, if they are ſo liable to ſuffer from the effects of froſt? In regard to the ſecond objection, that alſo may be refuted by experience. The maples at Harbke have been tapped two years ſucceſſively without one of them being hurt; on the contrary, they produced bloſſoms and ſeeds the following ſpring. This winter Count Veltheim cauſed them to be tapped, for the third time. In the like manner, ſeveral trunks of the *acer pſeudo-platanus*

In order, therefore, that Germany, and in particular the Pruſſian ſtates, may not be ſo long deprived of an opportunity of eſtabliſhing manufactories of ſugar from indigenous productions, it would be of great benefit if, among the many ſaccharine plants which nature has beſtowed on Germany, one were diſcovered capable of producing a ſubſtitute for Weſt India ſugar, until plantations of the maple-tree can be formed. This conſideration induced me, while employed in reſearches reſpecting maple ſugar, to begin a ſeries of experiments, the object of which was, to examine what the old and modern chemiſts had done on this ſubject, and to enlarge their diſcoveries by my own. The reſult of my experiments was as follows:

1. *Experiment on Turkiſh Wheat in order to procure Sugar from it.*

Turkiſh wheat, (*zea mays*,) according to Mr. Von Juſti *, contains a great deal of real ſugar in the knots of the young ſtalks. Mr. Jacquin, of Vienna †, frequently ſucceeded in preparing ſugar from the ſtalks of Turkiſh wheat; and the ſame thing is aſſerted by Mr. Marabelli in a treatiſe which he publiſhed on this ſubject. The manufacturing of ſugar from the ſtalks of Turkiſh wheat, and eſpecially ſuch as grows in a marſhy ſoil, has been tried alſo in Italy on a large ſcale; but it has been found that the ſugar procured from this production is too dear, when the price of it is compared with

platanus and *acer plantoides*, in the foreſts of Prince Henry of Pruſſia at Rheinſberg, were tapped laſt winter (1798) without one of them ſuſtaining the leaſt injury. In order to try how far it was poſſible to kill a maple-tree by tapping, I cauſed two trunks to be bored with twenty-four holes each, and when the ſap had ceaſed flowing, the holes not only were left open, but eighteen deep wounds were made in each tree with an axe, and all the branches were lopped off; yet both theſe trees remained ſound, and next ſummer all the wounds had cloſed up of themſelves. The caſe is not the ſame with the birch, which, when tapped, and deprived of a great deal of ſap, immediately dies. The third objection is ſo ridiculous, and ſo contrary to experience, that nothing needs be ſaid on the ſubject.—H.

* See his *Oeconom. Schriften*, Part I. p. 397, and Part II. p. 191.

† Crell's *Chem. Annalen* for 1794, Vol. I. p. 96.

that of raw ſugar. As my principal object was to be convinced of this by my own obſervation, I made the following experiments:

I ſowed for this purpoſe, in the ſummer of the year 1796, on a pretty good, but ſomewhat marſhy ſoil, a certain quantity of Turkiſh wheat. When the young plants were about ſix inches in height, the leaves, on being chewed, had a ſweetiſh taſte, but the ſtalks, particularly at the joints, had a real ſaccharine taſte. Theſe young plants were cut cloſe to the ground, and, after they had been freed from the leaves and dirt adhering to them, ten pounds of them were bruiſed in a ſtone mortar, and the juice expreſſed. This juice, which weighed three pounds, and which had a ſweet but herbaceous taſte, was clarified with the white of an egg; and the liquor, which was now of a bright yellow wine colour, and which had loſt almoſt entirely its unpleaſant taſte, was inſpiſſated to the conſiſtence of ſyrup, eight ounces of which would have afforded an agreeable ſyrup fit for uſe.

2. *Examination of the Ears of Turkiſh Wheat.*

As the young ears of Turkiſh wheat, when they are juſt beginning to be formed, have a very agreeable ſaccharine taſte, theſe alſo were ſubjected to examination. Ten pounds of them, freed from the ſurrounding leaves, being bruiſed in a ſtone mortar, and then preſſed, gave four pounds of a milky juice, which could not be perfectly clarified by the white of an egg. When evaporated ſlowly to the conſiſtence of ſyrup, I obtained from the above quantity of juice nine ounces of a brown well taſted ſyrup; which, however, was diſtinguiſhed by a greater abundance of ſlimy matter than the former.

3. *Examination of the Stalks of Turkiſh Wheat at a later Period of Growth.*

Twenty pounds of the ſtalks of this plant at a more advanced period of growth being chopped, were bruiſed in a ſtone mortar with a little water, and the juice was expreſſed. This juice, which had an unpleaſant herbaceous and ſome-

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what ſharp taſte, when clarified as before, with the white of an egg, and then inſpiſſated, gave twelve ounces of ſyrup, which had a diſagreeable ſaline taſte, and might be conſidered rather as a vegetable extract than a ſyrup.

4. *Experiment made to obtain dry Sugar from Turkiſh Wheat.*

To try how far it was poſſible to obtain cryſtalliſed ſugar from this plant, a quantity of ſyrup prepared from the young ſtalks, and another procured from the ears, were each diſſolved by newly made lime-water, and then boiled ſlowly, during which proceſs a great deal of impurities were thrown up. When the liquors were ſtrained through a woollen cloth, each of them was inſpiſſated ſeparately to the conſiſtence of thick ſyrup, and ſuffered to remain for eight months in a glaſs in a place moderately warm. At the end of that time I found ſmall cryſtals of real ſugar ſhot up in them; but it was with great difficulty they could be ſeparated from the fluid part, the quantity of which was far greater. Each of the ſyrups was therefore inſpiſſated, at a gentle heat, to complete dryneſs, and the dry maſs was then digeſted with ſix parts of rectified ſpirit of wine. The liquor, ſtill warm, being ſpeedily ſtrained through a piece of linen as the undiſſolved ſlimy parts remained behind, real ſugar of a yellowiſh colour cryſtalliſed from the ſpirituous ſolution on cooling. The alcohol was now drawn off from the remaining liquor, and more ſugar was obtained from the reſiduum by ſlow evaporation. I procured in the whole from the ſyrup produced by the young ſtalks, two ounces; and from that produced by the young ears, one and a half ounce of ſugar.

By this it is ſufficiently proved that real ſugar may be obtained both from the young freſh ſtalks and from the young ears of Turkiſh wheat. The ſeparation of it, however, from the gummy and other parts with which it is combined, is attended with ſo much difficulty, and the produce in general is ſo ſmall, that a pound of raw ſugar of this kind will not coſt leſs than a dollar; ſo that, in an economical point of view, no advantage can be expected from the preparation of ſugar in this manner.

5. *Experiment made to obtain Sugar from Siberian Cow-parſnip.*

The Ruſſian or Kamtſchatſdale cow-parſnip, *Heracleum ſphondylium* Linn. *Heracleum Sebiricum* has been long known as a ſaccharine plant. According to Steller, it is the moſt abundant in ſaccharine matter next to the ſugar-cane.* The Ruſſians call it ſweet-herb, and the inhabitants of Kamtſchatka *katſch.* According to Gmelin, it is in nothing different from common bear's breech; but according to others, it forms a peculiar ſpecies *Sphondylium Panaces.* The natives of Kamtſchatka gather the ſtems and large ſtalks of the leaves, and having freed them from the leaves, ſcrape the external rind with muſcle-ſhells, and then leave them to dry in the ſun.

The natives of Kamtſchatka generally chew theſe dried ſtalks in order to extract from them the ſaccharine matter. When the moiſture of the juice has been evaporated in the ſun, the ſurface of the ſtem becomes covered with a white, ſaccharine, mealy ſubſtance, which is ſeparated by ſhaking the ſtalks in leathern bags, and which is then preſerved as powder ſugar. Forty pounds of dried ſtems produce, however, ſcarcely a quarter of a pound of this ſugar, which therefore is ſcarce. The ſtems and roots of this plant are employed alſo by the natives of Kamtſchatka for preparing a kind of brandy †.

As I did not find the ſtems of this plant nearly ſo abundant in ſaccharine matter as thoſe growing in Siberia are ſaid to be, I collected about four pounds of the roots in the end of harveſt: they had a ſweetiſh taſte, ſomething like that of parſnips. I ſcraped off the rind and then dried them, but I could obſerve on them no ſaccharine incruſtation. I therefore raſped the whole four pounds, kneaded the raſpings with water, and expreſſed the juice, which had a ſweetiſh and ſomewhat ſharp taſte. I then boiled it with the white of an egg, by which it was clarified; and having inſpiſſated the liquor, it produced ſix ounces of a brown and not unpleaſant

* Steller's *Reiſen nach Kamtſchatka,* p. 84.
† Gmelin *Flora Sebirica,* Vol. I. p. 214.

ſyrup;

fyrup, in which, after the end of three months, a kind of brown granulated fugar had cryftallifed. This fugar, however, was not free from a foreign tafte. That fugar can be obtained from this plant is therefore proved, but, in an economical point of view, neither the fugar nor the fyrup it produces would be advantageous, as both would be too expenfive. It is not improbable that this plant in our climate is lefs abundant in fugar than that found in Kamtfchatka.

6. Experiment with the Juice of Grapes in order to obtain Sugar.

The liquor obtained from perfectly ripe grapes, evidently fhows, by its fweet tafte, that it contains abundance of faccharine matter, but mixed with a great deal of mucilage. In order that I might try how far it was poffible to procure from it real fugar, or at leaft fyrup, fit for domeftic purpofes, I undertook the following experiment:—Eight Berlin quarts of juice obtained from perfectly ripe and fweet grapes, by merely fuffering it to diftil fpontaneoufly, was mixed with the white of an egg, then boiled and clarified, and afterwards filtered. The clear filtered liquor being evaporated, gave three pounds of a fyrup not unpleafant, but which had a fomewhat fourifh tafte. To free it from its acidity it was diffolved in lime-water, till it appeared by reagents that it contained no more free acid. The liquor was then once more clarified with the white of an egg, and again evaporated, by which means I obtained a very pleafant fyrup, but notwithftanding all my labour I was not able to procure from it cryftallifed fugar. This fyrup, therefore, while a hogfhead of new wine cofts fifteen dollars, would be too expenfive to be ufed for domeftic purpofes.

7. Experiments with the Juice of the White and Black Birch to obtain Sugar from it.

Different opinions have been entertained refpecting the poffibility of obtaining fugar from the juice of the birch. As fome have afferted that fugar may be procured from this liquor, while others have maintained the contrary, I was defirous to determine the queftion by my own experience.

According

According to Stahlhammer's experiments *, eight quarts of the juice of the common white birch *betula alba*, drawn off in the ſpring, gave a ſmall quantity of ſyrup, weaker than that obtained from the maple, but ſuperior to the common brown ſyrup. We are told, on the other hand, by Kalm †, that a great deal of ſugar is prepared from the juice of the black American birch *Betula nigra carpinifolia*, called alſo the ſugar-birch, but that this ſugar is not altogether ſo ſweet as the maple ſugar. By the kindneſs of Count Von Veltheim, at Harbke, I was enabled to obtain a ſufficient quantity of the juice of both theſe kinds of birch, with which I made the following experiments:

The trunks of fifty white birches, of from eight to ten inches in diameter, being pierced in the month of April, which is the moſt favourable period, gave in the courſe of four days 140 Berlin quarts of ſap, which by inſpiſſation produced two and a half pounds of brown ſyrup, which had an unpleaſant taſte, and from which no cryſtalliſable ſugar could be obtained.

On the other hand, ten trunks of the black birch which had been tapped, gave in the courſe of ten days fifty quarts of ſap, which produced by inſpiſſation one pound and a half of very good ſyrup; which, however, was far inferior to that obtained from the maple, though it was better than common ſyrup, and therefore I am induced to believe that Stahlhammer for his experiments muſt have employed the black birch. In the courſe of four months a conſiderable portion of ſugar had cryſtalliſed from the juice of this birch. It is therefore proved that real ſugar may be obtained from the ſap of the black birch, but inferior in quality to that of the maple-tree, and much more expenſive.

8. *Experiments with the White Beet* Beta cicla alba.

The late Margraf made various experiments, above fifty years ago, with ſeveral kinds of beet-root, in order to try how far it was poſſible to procure ſugar from them, and according to his own account he ſucceeded in obtaining from

* *Tranſactions of the Swediſh Academy of Sciences*, Vol. XXXV. p. 335.
† *Ibid.* Vol. XIII. p. 151.

the

the *Beta cicla* not only good ſyrup, but ſugar, fit for domeſtic purpoſes. Two pounds of freſh roots gave on drying half a pound; and from this quantity he produced, after extracting the juice by means of rectified ſpirit of wine, half an ounce of good ſugar.

In conſequence of theſe experiments I took 130 roots, which weighed ſixty-eight pounds, and having bruiſed them and expreſſed the juice, I poured tepid water over the reſiduum, expreſſed the juice again, and obtained altogether twenty quarts of juice. This juice I evaporated to one-third, then added to it twenty quarts of freſh lime-water, and boiled the whole for half an hour. The clear liquor, which was of a yellow wine colour, being filtered when cool, and then inſpiſſated, gave ſix pounds of a pleaſant browniſh-yellow tranſparent ſyrup. A part of this ſyrup I poured into a ſomewhat deep glaſs evaporating veſſel, in the inſide of which I placed ſome glaſs rods, and ſuffered the liquor to evaporate in a moderately warm place for ſixteen weeks, at the end of which I found the glaſs rods covered with cryſtals of ſugar, ſome of which were as large as lentils, and others as peas, and which had a perfect reſemblance to yellow ſugar-candy. As the quantity of theſe beets which I employed coſt half-a-crown, and as the reſt of the expenſe amounted only to about eightpence, a pound of ſuch ſyrup would coſt no more than ſixpence or ſeven-pence, which ſufficiently proves that it might be uſed as a ſubſtitute for common ſugar ſyrup. Sugar alſo fit for domeſtic purpoſes might be made from it; and though it would be inferior to maple ſugar, and though the proceſs for obtaining it would be ſomewhat tedious, it might be uſed with advantage when we conſider the high price of common ſugar.

[To be concluded in the next Number.]

III. *Obſervations on the Evening and Morning Dew; in a Letter addreſſed to* C. HASSENFRATZ, *of the Polytechnic School. By* C. A. PRIEUR *.

AS you requeſt me to give you in writing the ſubſtance of what I ſaid a few days ago reſpecting the phenomena of the evening and morning dew, I ſhall endeavour to gratify your wiſhes; being as deſirous as you are, that ſome utility may reſult from it to the young citizens whom you inſtruct.

The evening and morning dew occur to us ſo frequently, and in ſo many climates, that there is reaſon to be aſtoniſhed that philoſophers ſhould have hitherto paid ſo little attention to theſe phenomena, and been contented with the vague explanations given on the ſubject. In the year 1788, I often had occaſion to ride out in the morning and evening; and being thus expoſed to the impreſſions of the dew, I reflected particularly on the circumſtances of theſe phenomena, which have been as yet ſo little examined. I well knew that moiſture depoſited on bodies placed in the open air at the time of ſunſet, was not the ſame as that ſeen afterwards on theſe bodies at the period of ſunriſe: that conſequently there was an interruption in the phenomenon; an evaporation of the dew or water which appeared in the evening, and a new production of water in the morning. I was acquainted alſo with this partial explanation in regard to the evening dew, *viz.* that the moiſture ſeen in the evening ariſes from the air being no longer able to retain the water which it held in ſolution during the day. But whence that wind which always proceeds from the quarter where the ſun is, and which always accompanies this precipitation of water? And beſides, how happens it, that when the ſun is about to appear above the horizon, and even after it has been ſomewhat heated by his preſence, there ſhould be occaſioned a greater cold, a ſtronger wind, and a more abundant precipitation of water than in the evening? To ſolve this difficulty I had recourſe to thoſe fundamental principles by the help of which Monge

* From *Journal de l'Ecole Polytechnique*, Vol. II.

has

has explained, in fo ingenious a manner, the greater part of the phenomena of meteorology; and I foon conceived that they were fufficient for the object in queftion.

The principles from which we muft fet out, and which it is here proper to call to mind, are the three following:

1ft, The air, every thing elfe being equal, diffolves more water in proportion as it is denfer, that is to fay, more compreffed.

2d, It diffolves a greater quantity alfo in proportion as its temperature is more elevated.

3d, The denfity or fpecific gravity of air which holds water in folution, is lefs than the fpecific gravity of pure air, the preffure and temperature being equal; and this fpecific gravity is lefs according as the quantity of water diffolved in the air is greater.

It muft, in the next place, be admitted, that the changes of preffure and weight which may take place in fome columns of the atmofphere, muft neceffarily difturb its equilibrium, and produce in it movements or currents.

Thefe bafes being laid down, let us fuppofe for a moment, to render our examination eafier, that the earth, deprived of its rotary motion, is in an immoveable ftate before the fun; and let us fet afide alfo the confideration of all local influence, or in general all caufes that tend to perturb the regularity of effects. In this ftate of things what will take place in our atmofphere? The air expofed to the rays of the fun will be heated, and particularly that part of it near the earth, on account of its greater denfity and the reverberation of the planet. The air thus heated will acquire a greater diffolving power, and will indeed diffolve a great deal of water if there be any in contact with it. It will take up water therefore from the fea, from lakes, rivers, and ponds, and from refervoirs, the furface of which is expofed to its action: it will even abforb part of the moifture with which the earth may be impregnated.

Let us now confider what will take place in a vertical column affumed in the atmofphere, and firft in one lying directly under the fun. The air being heated at its lower part,

 and

and becoming ſpecifically lighter by diſſolving water, will riſe and be replaced by the adjacent air. A current, therefore, will be eſtabliſhed in the column from the bottom upwards. Were this column inſulated from the neighbouring columns, as it would be if contained in a vertical tube, in proportion as the inferior air charged with water roſe, the upper air would deſcend; would become charged with water in its turn; and would aſcend, and be continually replaced without interruption. Beſides, the air holding water in ſolution, when it attained to a certain height, and experiencing there a colder temperature with a leſs degree of preſſure, would become ſuper-ſaturated, and would abandon its exceſs of water. There would thus be formed a fog or cloud, which might continue to aſcend in conſequence of the movement it had acquired, but which, when accumulated to a certain point, would fall down in rain. It may readily be conceived that this tranſlation of air from a lower to a higher, and from a higher to a lower ſituation, would be performed by reciprocal infiltration, or by a current in both directions regularly maintained.

But things are not altogether ſo in ſuch a vertical column, becauſe in reality it is not ſeparated from the reſt. The latter is ſubject to the ſame effects, only that they are leſs in proportion as theſe columns are removed from the direction of the ſun: the heat, ſolution of water, and aſcenſional force, go on decreaſing the further they are diſtant from that direction. If the ſurface of the earth, therefore, be ſuppoſed a plane, it will be neceſſary to repreſent the air which riſes as a ſort of cone, with its ſummit directed towards the heating luminary; and on the other hand, as the abſolute gravity of each column is increaſed by the whole of the water it diſſolves, the equilibrium requires that there ſhould be a diſcharge from each, into the lateral ones; a circumſtance which muſt evidently be effected where there is the leaſt preſſure. Thus, on the ſame ſuppoſition of the earth being a plane, we ſhould ſee the ſuperior air deſcend and force itſelf along the ſides of the cone above mentioned, producing, by this oblique motion at the ſurface of the earth, a current proceeding in every direction from the quarter of the ſun;

and

and this current would be increafed by the vacuum formed at thofe places where the air, fufficiently elevated and cooled, precipitated its fuperabundant water.

We need only to modify this image a very little to make it correfpond with the contour of our globe. The conical furface will be converted into a fpherical fegment inclofing the enlightened portion of the earth, and according to the curvature of which, the before-mentioned currents will be eftablifhed. This curvature will be feen in the annexed figure, (Plate III.) which reprefents the globe of the earth placed in the centre of a circular ftratum fuppofed to be occupied by the atmofphere. The circumference of the earth contains twenty-four divifions, from which arife as many perpendiculars to its furface, in order to give an idea of the variations of the phenomena from hour to hour. The fun, placed towards S, in the prolongation of the line TS, is fuppofed to have his whole mafs united in his centre, which can do no hurt to the object which we have here in view. In the laft place, the curve ABC, traced out at random becaufe its law is unknown, will be fufficient to give an idea of the effects which we are defirous of illuftrating.

Such, then, will be the refult of the circumftances fuppofed: Water diffolved by the inferior air in the part expofed to the fun; an afcending movement of the air in that part; defcending currents diverging from all fides, and prolonging themfelves over the earth; thefe currents carrying with them water which they have taken from the heated columns that rife, and which are forced off in a lateral direction; this water thus carried away precipitated, either becaufe the air, coming from the upper regions, is too cold to keep it diffolved; or on account of the diminution of preffure arifing from the weight of the columns being lefs in proportion to their diftance from that immediately under the fun; or in confequence of the mixture of currents with the atmofpheric air, which they traverfe at the furface of the earth when that air is colder; and, in the laft place, the earth, and all bodies which the currents touch, moiftened by the water precipitated.

Every one muft here perceive the evening and morning dew

dew, with the wind and the cold which accompany them; but a few remarks are ſtill neceſſary to complete the deſcription. We ſhall firſt obſerve, that the deſcending currents are prolonged until the reſiſtance of the air which they traverſe has entirely deſtroyed their motion. Beſides, we ſhall ſee directly under the ſun a circular ſpace very much heated, and which does not exhibit to the inhabitants of that part of the earth the phenomenon we are attempting to deſcribe. Leaving this part, in proportion as the ſun appears in a more oblique poſition, we ſhall arrive at a region of leſs heat, where we ſhall begin to perceive a wind coming from the quarter of the ſun, accompanied with a precipitation of water. This region forms a ring around the circular ſpace before mentioned. In a word, by removing from the internal edge of this ring, the wind is found ſtronger, the cold more ſenſible, and the precipitation of water more abundant; effects which ſtill decreaſe at a certain diſtance, and ceaſe entirely on that part of the earth diametrically oppoſite to the ſun.

Thus, according to the hypotheſis of the immobility of our globe in regard to the ſun, there would be on that planet a very extenſive region continually ſubject to the phenomenon of morning and evening dew. Either of theſe expreſſions will denote the phenomenon according as the obſerver ſuppoſes himſelf placed to the eaſt or weſt of the ſun, ſince the one takes place at the riſing of that luminary, and the other at his ſetting.

Let us now return to the real ſtate of things, and reſtore to the earth its diurnal rotation. In that caſe it will ſucceſſively preſent different parts of its ſurface to the phenomena above deſcribed. Thoſe who inhabit that part where the ſun is deſcending towards the horizon, will ſoon ſee the evening dew appear, accompanied with a freſhening eaſt wind; they will ſee theſe phenomena increaſe more and more until after ſunſet, when the effect will be diminiſhed, and at laſt ceaſe altogether. During the night the moiſture depoſited by the evening dew will be evaporated, if the atmoſphere be not already too much charged with water, and no traces of it will remain. Next morning at break of day, the phenomenon

menon will be renewed on the eaſt ſide of the obſerver, with the ſame circumſtances as the preceding evening, and with the ſame gradations and the ſame effect when the ſun is ſtill below the horizon; but with this very remarkable difference, that the effects will be much ſtronger than thoſe of the evening: there will therefore be more wind, more moiſture depoſited, and a more ſenſible cold. The reaſon of this is, that in the evening the precipitation of water, the wind, and the cold, which accompany it, muſt be diminiſhed, becauſe the whole takes place in the neighbourhood and by the mixture of air, which the ſun has heated during the day; while in the morning the coolneſs of the night-air leaves or gives to the phenomenon a little more effect.

We ſee alſo that in the two temperate zones, where there is a greater difference between the temperature of ſummer and winter, and where the length of the days and nights vary a great deal, the effects of the evening and morning dew will be varied and irregular. In ſummer, if the air has diſſolved more water during the day, the diſtillation of dew takes place in air ſtrongly heated, and that of the morning dew in air which the ſhort duration of the night has cooled only to a certain point: in that caſe, the ſolution of water is conſiderable, and the precipitation of dew leſſened. In winter, on the other hand, the cauſe of the ſolution is leſs, but that of precipitation greater. Local circumſtances, and particularly the neighbourhood of water, muſt alſo have an influence on theſe effects. Fine weather increaſes them, and renders them more ſenſible, but cloudy weather leſſens or annuls them.

Under the torrid zone a greater equality prevails between the days and the nights, ſince at the equator they conſiſt of twelve hours each throughout the year. The difference of temperature in ſummer and winter is leſs; that of the day in compariſon of the night is greater than in our latitude, the heat of the day is far more intenſe, and the ſky is almoſt always ſerene and unclouded. It ought thence to follow, that the evening and morning dew will be more ſenſible in ſuch a ſcorched climate than in any other part of the earth. This has indeed been obſerved by travellers. In Egypt, Turkey,

 Aſia,

Aſia, the Antilles, and Mexico, and on board ſhips which navigate between the tropics, evening and morning dew have been obſerved in ſuch abundance, that bodies expoſed to them ſeemed as wet as if they had been drenched with rain.

But a conſequence of very great importance, which I think may be drawn from theſe phenomena, is, that they muſt have a great influence on the production and duration of the trade-winds; for the air of the torrid zone being excited to motion almoſt regularly every day, in two oppoſite directions, by forces very different in quantity, muſt be impelled to aſſume and to preſerve a movement in the direction of the ſtrongeſt, which is here the eaſt wind, or that which brings with it the dew of the morning. This cauſe muſt be more effectual, as it acts in the inferior part of the atmoſphere, and as, by affecting the denſeſt part of the air, it is more capable of eſtabliſhing a current in the whole maſs.

Under the frigid zone, where the ſun, particularly during winter, ſcarcely riſes above the horizon in the courſe of the whole day, the precipitation of water will be conſiderable on account of the coldneſs of the climate; the country will be involved in a thick fog, which will be ſcarcely diſperſed on the approach of ſummer; and when it takes place in ſummer, it will extend a great way into the temperate zone. Hence, therefore, we obſerve in our own country, during that ſeaſon, conſiderable fogs, which are often ſo thick as to ſuffer only a faint light to penetrate through them even at noon.

It would be curious to exhibit here a ſeries of correct obſervations on the circumſtances which accompany the evening and morning dew, at different times of the year, and in different countries; on the hour at which theſe phenomena begin, and on the ſuſpenſion or irregularity by which they are affected in different local ſituations; but if the generality of the cauſe to which they are here aſcribed can excite the attention of philoſophers, the intereſting taſk which I have pointed out will no doubt be ſoon accompliſhed.

IV. *Proceſs*

IV. *Process employed by Professor* LAMPADIUS *for extracting Sugar from White Beet-root**.

IN the month of January 1799, Professor Lampadius, having learned that Mr. Achard had extracted sugar from the white beet, *Beta cicla* Linn., resolved to try an experiment of the like kind, and from a quintal of beet-roots obtained four pounds of sugar by the following process:—He took a hundred pounds of the roots, peeled them, and having freed them from the ligneous parts, rasped them and expressed the juice: the quantity of the juice obtained was 44 pounds. He put the juice into a copper pan, brought some charcoal to a state of ignition in the fire, broke it into small fragments of the size of a pea, throwing aside the dust, which would have coloured the sugar, and put forty-four ounces of it into the juice. He then boiled the whole for a quarter of an hour, strained it through a piece of flannel cloth, and, after being filtered, placed it again over the fire to bring it to the consistence of syrup. It is on this operation that the success of the crystallisation chiefly depends. When the juice had acquired the proper consistence, he put it into a cool place and left it there for the space of fifteen days: a crystallisation was produced, and at the end of that time the juice had become like thick syrup, and of a brown colour. On taking up a little of it between the fingers, the crystals were felt in the form of small grains. Professor Lampadius then strained the syrup through a piece of linen cloth, squeezing it very closely. The crystals which remained behind were put into lime-water mixed with a pound of ox's blood, the whole was placed over the fire, and during the ebullition care was taken to skim the mixture well. It was afterwards filtered through a piece of flannel, and left to crystallise. At the end of forty-eight hours, being thrown upon a linen cloth, the crystals which remained were not of so brown a colour as those first obtained, and appeared to be of a larger size. They were again boiled in lime-water with a pound of milk; the matter which floated at the surface was skimmed off, and the crystals obtained

* From the *Journal de Physique*, Prairial, an. 8.

were blue. They were thrown into a mould after being moiſtened, and produced a loaf of pretty white ſugar, weighing four pounds, and in taſte perfectly ſimilar to that of common ſugar.

In February, Profeſſor Lampadius made a ſecond trial: the juice in this caſe was not altogether ſo white as the former, and he obtained from it only three pounds and a half of ſugar. From the different remains and ſyrups which he procured by filtration, he obtained each time ſeven pounds of a kind of rum or very ſtrong ſpirit. Encouraged by this ſucceſs, Profeſſor Lampadius and a rich citizen of Freyberg named Kanitzki, have eſtabliſhed a large manufactory of this kind of ſugar. The firſt reſults of their undertaking have been already publiſhed.

V. *On a new fulminating Mercury.* By EDWARD HOWARD, *Eſq. F.R.S.*

[Concluded from Page 35.]

SECTION XVI.

HITHERTO, as much only has been ſaid of the gas, which is ſeparated from the mercurial powder by dilute ſulphuric acid, as was neceſſary to identify it with that into which the ſame acid can reſolve the nitrous etheriſed gas; I have further to ſpeak of its peculiarity *.

The characteriſtic properties of the inflammable gas, ſeem to me to be the following:

1ſt, It does not diminiſh in volume, either with oxygen or nitrous gas.

2dly, It will not explode with oxygen by the electric ſhock in a cloſe veſſel.

3dly, It burns like hydro-carbonat, but with a bluiſh green flame. And,

4thly, It is permanent over water. (Section XII.)

* It muſt be firſt noticed, that it is never pure when obtained from the nitrous etheriſed gas; nor am I aware how it is to be purified, unleſs the nitrous gas could be taken from it without being converted into nitrous acid; for, by that acid, it would probably be itſelf converted into nitrous gas.

It

It is of courſe either not formed, or is convertible into nitrous gas, by the concentrate nitric and muriatic acids; becauſe, by thoſe acids, no inflammable gas was extricated from the powder.

Should this inflammable gas prove not to be a hydro-carbonat, I ſhall be diſpoſed to conclude that it has nitrogen for its baſis; indeed, I am at this moment inclined to that opinion, becauſe I find that Dr. Prieſtley, during his experiments on his dephlogiſticated nitrous air, once produced a gas which ſeems to have reſembled this inflammable gas, both in the mode of burning, and in the colour of the flame.

After the termination of the common ſolution of iron in ſpirit of nitre, he uſed heat, and got, ſays he*, "ſuch a kind of air as I had brought nitrous air to be, by expoſing it to iron, or liver of ſulphur; for, on the firſt trial, a candle burned in it with a much enlarged flame. At another time, the application of a candle to air produced in this manner, was attended with a real though not a loud exploſion; and, immediately after this, a *greeniſh coloured flame deſcended from the top to the bottom of the veſſel* in which the air was contained. In the next produce of air, from the ſame proceſs, *the flame deſcended blue* and very rapid, from the top to the bottom of the veſſel."

Theſe greeniſh and blue coloured flames, deſcending from the top to the bottom of the veſſel, are preciſely deſcriptive of the inflammable gas ſeparated from the powder. If it can be produced with certainty by the repetition of Dr. Prieſtley's experiments, or ſhould it by any means be got pure from the nitrous etheriſed gas, my curioſity will excite me to make it the object of future reſearch; otherwiſe, I muſt confeſs, I ſhall feel more diſpoſed to proſecute other chemical ſubjects: for, having reaſon to think that the denſity of the acid made a variation in the product of this gas, and having never found that *any* acid, however denſe, produced an immediate exploſion, I once poured ſix drams of concentrate acid upon fifty grains of the powder. An exploſion, nearly at the inſtant of contact, was effected: I was wounded ſeverely, and

* Prieſtley on Air, Vol. II. p. 58. Birmingham, 1790.

moſt of my apparatus deſtroyed. A quantity, moreover, of the gas I had previouſly prepared, was loſt by the inadvertency of a perſon who went into my laboratory, whilſt I was confined by the conſequences of this diſcouraging accident. But, ſhould any one be deſirous of giving the gas a further examination, I again repeat, that as far as I am enabled to judge, it may with ſafety be prepared, by pouring three drams of ſulphuric acid, diluted with the ſame quantity of water, upon 50 grains of the powder, and then applying the flame of a candle until gas begins to be extricated. The only attempt I have made to decompoſe it, was by expoſing it to copper and ammoniac; which, during ſeveral weeks, did not effect the leaſt alteration.

Section XVII.

I will now conclude, by obſerving, that the fulminating mercury ſeems to be characteriſed by the following properties:

It takes fire at the temperature of 368 Fahrenheit; explodes by friction *, by flint and ſteel, and by being thrown into concentrate ſulphuric acid. It is equally inflammable under the exhauſted receiver of an air-pump, as ſurrounded by atmoſpheric air; and it detonates loudly, both by the blow of a hammer, and by a ſtrong electrical ſhock.

Notwithſtanding the compoſition of fulminating ſilver, and of fulminating gold, differ eſſentially from that of fulminating mercury, all three have ſome ſimilar qualities. In tremendous effects, ſilver undoubtedly ſtands firſt, and gold perhaps the laſt. The effects of the mercurial powder and of gunpowder admit of little compariſon. The one exerts, within certain limits, an almoſt inconceivable force: its agents ſeem to be gas and caloric, very ſuddenly ſet at liberty, and both mercury and water thrown into vapour. The other diſplays a more extended but inferior power: gas and caloric are, comparatively ſpeaking, liberated by degrees; and water, according to Count Rumford, is thrown into vapour †.

Hence

* Conſequently it ſhould not be incloſed in a bottle with a glaſs ſtopper.

† See Philoſophical Tranſactions for the year 1797, p. 222.

The

Hence it ſeems that the fulminating mercury, from the limitation of its ſphere of action, can ſeldom if ever be applied to mining; and, from the immenſity of its initial force, cannot be uſed in fire-arms, unleſs in caſes where it becomes an object to deſtroy them; perhaps, where it is the practice to ſpike cannon, it may be of ſervice, becauſe, I apprehend, it may be uſed in ſuch a manner as to burſt cannon without diſperſing any ſplinters.

The inflammation of fulminating mercury by concuſſion, offers nothing more novel or remarkable, than the inflammation, by concuſſion, of many other ſubſtances. The theory of ſuch inflammations has been long ſince expoſed by the celebrated Mr. Berthollet, and confirmed by Meſſieurs Fourcroy and Vauquelin: yet, I muſt confeſs, I am at a loſs to underſtand, why a ſmall quantity of mercurial powder, made to detonate by the hammer, or the electric ſhock, ſhould produce a report ſo much louder than when it is inflamed by a match, or by flint and ſteel. It might at firſt be imagined, that the loudneſs of the report could be accounted for, by ſuppoſing the inſtant of the inflammation, and that of the powder's confinement between the hammer and anvil, to be preciſely the ſame; but, when the electrical ſhock is ſent through or over a few grains of the powder, merely laid on ivory, and a loud report is the conſequence, I can form no idea of what cauſes ſuch a report.

The operation by which the powder is prepared, is perhaps one of the moſt beautiful and ſurpriſing in chemiſtry; and it is not a little intereſting to conſider the affinities which are brought into play. The ſuperabundant nitrous acid of the mercurial ſolution, muſt firſt act on the alcohol, and generate ether, nitrous etheriſed gas, and oxalic acid. The mercury unites to the two laſt in their naſcent ſtate, and relinquiſhes freſh nitrous acid, to act upon any unaltered alcohol. The oxalic acid, although a prediſpoſing affinity ſeems exerted

The hard black ſubſtance mentioned by the Count as remaining after the combuſtion of gunpowder, muſt, I believe, have been an alkaline ſulphuret, mixed chiefly with ſulphit and carbonat of potaſh. The conjecture that it is white when firſt formed, is certainly juſt, as my experiment with the glaſs globe evinced.

in favour of its quantity, is evidently not formed faſt enough to retain all the mercury; otherwiſe, no white fumes, during a conſiderable period of the operation, but fulminating mercury alone, would be produced.

Should any doubt ſtill be entertained of the exiſtence of the affinities which have been called prediſpoſing or conſpiring, a proof that ſuch affinities really exiſt, will, I think, be afforded, by comparing the quantity of oxalic acid, which can be generated from given meaſures of nitrous acid and alcohol, with the intervention of mercury, and the intervention of other metals. For inſtance, when two meaſured ounces of alcohol are treated with a ſolution of 100 grains of nickel in a meaſured ounce and a half of nitrous acid, little or no precipitate is produced; yet, by the addition of oxalic acid to the reſiduary liquor, a quantity of oxalat of nickel, after ſome repoſe, is depoſited. Copper affords another illuſtration: 100 grains of copper, diſſolved in a meaſured ounce and a half of nitrous acid, and treated with alcohol, yielded me about 18 grains only of oxalat; although cupreous oxalat was plentifully generated, by dropping oxalic acid into the reſiduary liquor. About 21 grains of pure oxalic acid ſeem to be produced, from the ſame materials, when 100 grains of mercury are interpoſed. (See Section XIV.) Beſides, according to the Dutch paper, more than once referred to, acetous acid is the principal reſidue after the preparation of nitrous ether. How can we explain the formation of a greater quantity of oxalic acid, from the ſame materials, with the intervention of 100 grains of mercury, than with the intervention of 100 grains of copper, otherwiſe than by the notion of conſpiring affinities, ſo analogous to what we ſee in other phænomena of nature?

I have attempted, without ſucceſs, to communicate fulminating properties, by means of alcohol, to gold, platina, antimony, tin, copper, iron, lead, zinc, nickel, biſmuth, cobalt, arſenic, and mangeneſe; but I have not yet ſufficiently varied my experiments, to enable me ſpeak with abſolute certainty. Silver, when 20 grains of it were treated with nearly the ſame proportions of nitrous acid and alcohol as 100 grains

of

of mercury, yielded, at the end of the operation, about three grains of a gray precipitate, which fulminated with extreme violence. Mr. Cruickſhank had the goodneſs to repeat the experiment: he diſſolved 40 grains of ſilver in two ounces of the ſtrongeſt nitrous acid diluted with an equal quantity of water, and obtained (by means of two ounces of alcohol), 60 grains of a very white powder, which fulminated like the gray precipitate above deſcribed. It probably combines with the ſame principles as the mercury, and of courſe differs from Mr. Berthollet's fulminating ſilver, alluded to in page 230. I obſerve that a white precipitate is always produced in the firſt inſtance, and that it may be preſerved, by adding water, as ſoon as it is formed; otherwiſe, when the mother liquor is abundant, it often becomes gray, and is re-diſſolved.

P. S. Since the preceding pages were written, I have been permitted, by the Right Honourable Lord Howe, Lieutenant General of the Ordnance, to make the following trials of the mercurial powder, at Woolwich, in conjunction with Colonel Blomefield and Mr. Cruickſhank *.

Experiment 1. From the manner in which the ſcrew of the gun-breech, mentioned in Section V. had acted on the barrel, it was imagined, that by burſting an iron caſe, exactly fitted to the bore of a cannon, its ſudden enlargement might make many flaws, and ſplit the piece, without diſperſing any ſplinters. In conformity to this opinion, a caſt iron caſe was conſtructed, with a cylindrical chamber, of equal length and diameter, calculated to hold $3\frac{1}{4}$th ounces troy of the mercurial powder. The caſe, being firmly ſcrewed together, was charged through its vent-hole, and introduced into a twelve-pounder carronade, the bore of which it exactly fitted. The powder was then inflamed, with proper precautions. The gun remained entire, but the caſe divided: the portion forming the upper ſurface of the chamber was expelled in one maſs; that adjoining the breech, which conſtituted the reſt of the chamber, was cracked in every direction, and in part crumbled; yet it was ſo wedged into ſome indentations

* It is with pleaſure I take this opportunity of acknowledging the civil attention I received from the different officers.

which

which the explosion had made in the sides of the piece, that the fragments were not removed without great labour.

Exp. 2. Another cast iron case was prepared, of the same size as the former, with a chamber also cylindrical, but wrought in a transverse direction, and of a greater length than diameter; the thickness of metal at each extremity not being more than a quarter of an inch. This case was filled with nearly five ounces troy of the mercurial powder, and placed in the same carronade. Three twelve-pound shot were next introduced, and brought into close contact with the upper surface of the case, as well as with each other. The gun a second time withstood the explosion: the case was divided, across the middle of the chamber, into two equal parts; that adjoining the breech was, as in the former experiment, much flawed, and left immoveable; that nearest to the muzzle was also much flawed, but driven out with the shot. All the three shot were broken; the two lower being divided into several pieces, and the upper one cracked through the centre.

The report was so feeble, in both experiments, that an inattentive person, I am confident, would not have heard it at the distance of two hundred yards.

Exp. 3. It was found so difficult to extract the fragments of the case remaining in the carronade after the last experiment, that a channel was drilled through them to the vent-hole of the piece. It was then charged with six ounces troy of the mercurial powder, made up as a cartridge, which did not occupy above one-half of the diameter of the bore. A wad was placed over the powder, dry sand superadded, to fill all vacuities, and the gun filled to the muzzle with two twelve-pound shot. A block of wood was set at a small distance, to receive the impression of the shot, and the powder was inflamed as usual. The carronade still resisted. One of the shot was split into two pieces; and the block of wood was driven to a considerable distance, but not penetrated by the shot above the depth of one inch. The report was somewhat louder than the former ones. In all three instances, a considerable recoil evidently took place. I presume, therefore, that in the first experiment related in the fifth section, there

there muſt have been a recoil, though too trifling to be obſerved; and, in the inſtances where the gun and the proof were burſt, it was not ſo much to be expected.

Exp. 4. Finding that the carronade, from the great comparative ſize of its bore to that of its length, required a larger quantity of mercurial powder to burſt it than we were provided with, we charged a half-pounder ſwivel with an ounce and a half avoirdupois of the mercurial powder, (the ſervice charge of gunpowder being three ounces,) and a half-pound ſhot between two wads. The piece was deſtroyed from the trunnions to the breech, and its fragments thrown thirty or forty yards. The ball penetrated five inches into a block of wood ſtanding at about a yard from the muzzle of the gun; the part of the ſwivel not broken, was ſcarce, if at all, moved from its original poſition.

Exp. 5. One ounce avoirdupois of the mercurial powder, incloſed in paper, was placed in the centre of a ſhell 4,4 inches in diameter, and the vacant ſpace filled with dry ſand.

The ſhell burſt by the exploſion of the powder, and the fragments were thrown to a conſiderable diſtance. The charge of gunpowder employed to burſt ſhells of this diameter, is five ounces avoirdupois.

Exp. 6. A ſea grenade, 3,5 inches diameter, charged like the ſhell in the laſt experiment, was burſt into numerous fragments by $\frac{1}{4}$ of an ounce avoirdupois of the mercurial powder. The fragments were projected with but little force, and only to the diſtance of eight or ten yards. The charge of gunpowder required for grenades of this ſize, is three ounces.

Exp. 7. A ſea grenade, of the ſame diameter as the laſt mentioned, and charged in the like manner, with $\frac{1}{8}$ of an ounce avoirdupois, or $57\frac{1}{2}$ grains, of the mercurial powder, was ſplit into two equal pieces, which were not thrown ten inches aſunder.

The report in the four laſt experiments was very ſharp, but not loud in proportion.

It ſeems, from the manner in which the ſwivel was burſt, in the fourth experiment, that a ſmaller charge would have been ſufficient for the purpoſe. We may therefore infer,

 both

both from this inſtance and from the ſecond experiment made with the gun, in Section V, that any piece of ordnance might be deſtroyed by employing a quantity of the mercurial powder equal in weight to one-half of the ſervice charge of gunpowder; and, from the ſeventh and laſt experiment, we may alſo conclude that it would be poſſible ſo to proportion the charge of mercurial powder to the ſize of different cannons, as to burſt them without diſperſing any ſplinters. But the great danger attending the uſe of fulminating mercury, on account of the facility with which it explodes, will probably prevent its being employed for that purpoſe.

In addition to the other ſingular properties of the fulminating mercury, it may be obſerved, that two ounces inflamed in the open air, ſeem to produce a report much louder than when the ſame quantity is exploded in a gun capable of reſiſting its action. Mr. Cruickſhank, who made ſome of the powder by my proceſs, remarked that it would not inflame gunpowder. In conſequence of which, we ſpread a mixture of coarſe and fine grained gunpowder upon a parcel of the mercurial powder; and, after the inflammation of the latter, we collected moſt, if not all, of the grains of gunpowder. Can this extraordinary fact be explained by the rapidity of the combuſtion of fulminating mercury? or is it to be ſuppoſed, (as gunpowder will not explode at the temperature at which mercury is thrown into vapour,) that ſufficient caloric is not extricated during this combuſtion?

From the late opportunity I have had of converſing with Mr. Cruickſhank, I find that he has made many accurate experiments on gunpowder; and he has permitted me to ſtate, "that the matter which remains after the exploſion of gunpowder, conſiſts of potaſh united with a ſmall proportion of carbonic acid, ſulphat of potaſh, a very ſmall quantity of ſulphuret of potaſh, and unconſumed charcoal. That 100 grains of good gunpowder yield about 53 grains of this reſiduum, of which three are charcoal. That it is extremely deliqueſcent, and when expoſed to the air, ſoon abſorbs moiſture ſufficient to diſſolve a part of the alkali; in conſequence of which, the charcoal becomes expoſed, and the whole aſſumes a black, or very dark colour." Mr. Cruickſhank

ſhank likewiſe informs me, that after the combuſtion of good gunpowder under mercury, no water is ever perceptible.

References to the Figures of the Glaſs Globe, &c. mentioned in Section VII. (See Plate VIII.)

A, a ball or globe of glaſs, nearly half an inch thick, and ſeven inches in diameter. It has two necks, on which are cemented the braſs caps B, C, each being perforated with a female ſcrew to receive the male ones D, E: through the former a ſmall hole is drilled; the latter is furniſhed with a perforated ſtud or ſhank G. By means of a leather collar H, the neck C can be air-tightly cloſed. When a portion of the powder is to be exploded, it muſt be placed on a piece of paper, and a ſmall wire laid acroſs the paper, through the midſt of the powder: the paper being then cloſed, is to be tied at each end to the wire, with a ſilken thread, as ſhown at I. One end of this wire is to be faſtened to the end of the ſhank G, and the ſcrew D inſerted to half its length into the braſs cap B; the other end of the wire, *a*, by means of the needle K, is to be drawn through the hole F. The ſcrew E being now fixed in its place, and the wire drawn tight, it is to be ſecured by puſhing the irregular wooden plug L into the aperture of the ſcrew D, taking care to leave a paſſage for air. The ſtop-cock M, the ſection of which is ſhown at N, is now to be ſcrewed on to the part D, which is made air-tight by the leather collar *b*. The glaſs tube O is bent, that it may more conveniently be introduced under the receiver of a pneumatic apparatus. P ſhows the manner of connecting the glaſs tube with the ſtock-cock.

VI. *Comparative Analyſis of Human Bones, and thoſe of different Animals. By* C. Merat-Guillot, *Apothecary at Auxerre* *.

BUFFON ſays, "We ought to collect objects of every kind, to compare them, to ſtudy their nature, and from their combined relations to deduce that information which may

* From the *Annales de Chimie*, No. 100.

enable us to acquire a clear conception of them, and to increase our knowledge respecting them *." Fourcroy observes, in his Elements of Chemistry, under the head Bone, that "We cannot refuse believing that the bones of man and of quadrupeds are of a different nature from the soft and flexible bones of fishes, reptiles, and, above all, the corneous skeletons of insects, as well as from the calcareous testaceous covering of worms which have shells."

Being desirous to ascertain the truth of what had been advanced by C. Fourcroy, and guided by what is prescribed by Buffon in the treatise above mentioned, I undertook a labour, superior, no doubt, to my ability, and in which it is possible I may have been deceived, but which I submit to the judgment of those who are masters in the art. I was desirous of procuring all the objects which it was necessary should be subjected to comparison, such as a series of human bones, from which I might trace out the progress of ossification; but as I could not, and wishing to know at least the difference between products of animal bones at an early age, and those of the same animal at an advanced age, I employed for that purpose the bones of oxen and calves. I wanted, to complete my experiments in this analysis, the bones of carnivorous animals, the skeletons of insects, &c. When I am able to procure them, I shall resume my labours.

Being desirous to comprehend in this analysis, hair, corneous substances, and the bristles of different animals, I remarked, when treating the first of these substances with caustic soda, by means of heat, that there was produced a considerable disengagement of ammonia; that in pouring into this solution the muriatic acid, there was disengaged sulphurated hydrogen, and that a blackish precipitate, but in very small quantity, was thrown down. Having boiled this liquor, there was still disengaged a great deal of sulphurated hydrogen, from which I conjectured that it contained sulphur; and this fact I wished to ascertain. With that view, I put a silver spoon into the liquid, and again boiled it; the spoon became exceedingly black, and I thence concluded that the hair contained sulphur. As this labour presented

* *Discours de la Manière d'Etudier et de Traiter l'Histoire Naturelle*, p. 16.

 peculiar

peculiar reſults, which cannot be compared with the analyſis here given, I intend, having already collected notes on that ſubject, to make a particular analyſis of this ſubſtance compared with that of other ſubſtances of an analogous nature.

It is to be obſerved that I always operated on 100 parts of each ſubſtance, and that all the products, the quantities of which I determined, had been previouſly dried as much as poſſible.

Names of the Subſtances.	Proportions of Gelatinous Matter.	Proportions of the Phoſphat of Lime.	Proportions of the Carbonat of Lime.	Loſs.
Human bones taken from a burying-ground	16	67	1·5	15·5
Dry human bones which had not been in the earth	23	63	2	2
Bones of an ox -	3	93	2	2
—— of a calf -	25	54	traces of it	21
—— of a horſe	9	67·5	1·25	22·25
Teeth of a horſe	12	85·5	0·25	2·25
—— of an elephant, or ivory	24	64	0·1	11·15
Bones of a ſheep	16	70	0·5	13·5
—— of the elk	1·5	90	1	7·5
Horns of the ſtag	27	57·5	1	14·5
Bones of a ſwine	17	52	1	30
—— of the hare	9	85	1	5
—— of a hen -	6	72	1·5	20·5
Egg-ſhells - -	3	2	72	23
Bones of the pike	12	64	1	23
—— of the carp	6	45	0·5	48·5
—— of the viper	21·5	60·5	0·5	17·5
Lobſter ſhells - -	18	14	40	28
Mother-of-pearl	2·5	0	66	31·5
Crab's eyes - -	2	12	60	26
White coral - -	1·5	0	50	48·5
Red ditto - -	0·5	0	53·5	46
Articulated coralline - - -	7·5 *	0	49	43·5
Cuttle-fiſh bones	8	0	68	24

Being deſirous to aſcertain whence aroſe the loſs experienced in each analyſis, I ſubjected to a ſtrong heat 100 parts

* The product which I place under the head of Gelatinous Matter, is not entirely compoſed of that ſubſtance. In my opinion it contains a portion of a ſubſtance analogous in its nature to that which forms the polypiers known under the name of *lithophytes*.

of

of crabs eyes: they decreased 22 parts, and for that reason, I presume that this loss might be ascribed to the water which is found, at least in the gelatinous matter, since it is dried as much as possible; but as there still remains four parts, I ascribe the loss to a portion of the gelatinous matter which is found dissolved in the liquid employed for the analysis, and to a small quantity of a saline substance which I did not examine, and which may be dissolved in it. Should this essay be of any utility, the end I proposed will be answered *.

VII. *Method of determining the Quantity of Sulphur and Iron contained in Yellow Copper Ore. By* B. G. SAGE, *Director of the first School of Mines* †.

THE colour of this ore is a bright yellow inclining to that of gold; on the surface of it there is often observed reddish, violet, purple, and greenish blue varying shades, on which account it has been called peacock's-tail or pigeon's-neck copper ore. The form of this yellow copper ore is the equilateral tetraedron and its varieties. Though it is rare to find these crystals regular, the ore, in a compact, irregular, and sometimes lamellated mass, is very common; some of it is met with in the schistous impressions of fish found at Eisleben and Mansfeld.

I have said, in the second volume of my Mineralogy, that the yellow copper ore always contains iron and sulphur in greater or less quantity: the proportions I have been able to determine by the following experiments. The yellow copper ore which I employed had no matrix. Having pulverised it in a copper mortar, four hundred grains of it were exposed to torrefaction in a test, but, as it was too much heated, the ore became fused, and produced a yellow compact brilliant mass, which had a violet appearance in some parts of its fracture. This ore was not in the state of slag, that is to say, in a black mass, as is the case when torrefied on a large scale.

* This analysis agrees pretty well with that of Mr. Hatchett already given in this Magazine, Nos. 21 and 24.

† From the *Journal de Physique*, Prairial, an. 8.

Having

Having roasted four hundred grains of the ore, at a heat proper for keeping it red, in a test, four hours were required to disengage all the sulphur. There remained a blackish brown powder composed of calx of iron and copper. A magnet plunged into this mixed calx, came out covered with particles of iron.

The yellow copper ore sustains no sensible diminution of its weight by torrefaction; the sulphur, however, burns, and is exhaled in sulphureous acid; but the metals becoming calcined, increase in weight by the acid and water which they absorb: the sulphur furnishes acid, and the air decomposed furnishes water.

I determined the quantity of iron contained in the yellow copper ore, by putting a quintal of it, after being torrefied, to digest cold in volatile alkali. I added of it to this calx till it was no longer coloured blue: the iron remained at the bottom of the vessel under the form of a blackish powder, which, when washed and dried, was susceptible of being attracted by the magnet. This iron was equal in weight to one-half of the ore which I had put to digest with the volatile alkali.

I disengaged the sulphur from the yellow copper ore by distilling it with two parts of concentrated vitriolic acid: there passed sulphureous acid, and at the same time the sulphur was sublimated of a citron-yellow colour, and condensed in the neck of the retort. When detached, washed, and weighed, it amounted to a fifth part of the ore. This proportion is the same as that of the sulphur, which serves to mineralise antimony, bismuth, and cobalt.

The residuum of the distillation of the yellow copper ore and vitriolic acid having been washed, filtered, and evaporated, produced in the first instance martial vitriol (or sulphat of iron) in beautiful rhombs: it had a blueish tint, arising from the copper.

The yellow copper ore may be decomposed by the nitric acid at twenty-six degrees, which dissolves with effervescence the copper and the iron, and assumes a beautiful green colour. Nitrous acid must be repeatedly digested over the copper ore till it no longer become coloured, and until the sulphur

ſulphur remain at the bottom of the matraſs under the form of a white powder, which ſometimes retains a little iron.

Having reduced the yellow copper ore with three parts of black flux and a fifth of charcoal, it produced *per* quintal thirty pounds of friable copper, in part ſubject to be attracted by the magnet. It appears by this reduction that the ore yields *per* quintal,

Copper	-	-	40
Iron	-	-	40
Sulphur	-	-	20

It is the fixed alkali of the black flux which diſſolves the copper; ſo that if the reduction of this metal were attempted without adding charcoal, there would be danger of not obtaining the button.

VIII. *Method of determining the Quantity of Acid of Sugar contained in Spirit of Wine. By* B. G. SAGE, *Director of the firſt School of Mines* *.

URBAN HIERNE, a Swediſh chemiſt, firſt ſhowed, that when ſpirit of wine is decompoſed by nitrous acid, a ſalt is obtained as reſiduum on cooling. It was reſerved, however, for Scheele to make known that this ſalt is of the ſame nature as the acid of ſugar †.

I long conſidered ſpirit of wine ‡ as an acid ſoap, ſince the ether and the oil of wine which compoſe it, are miſcible with water. The acid of ſugar is the medium of union, and is found in it in a pretty conſiderable proportion. I was not

* From the *Journal de Phyſique*, Prairial, an. 8.

† The acid of ſugar is known in the new nomenclature under the name of *oxalic acid*, but why not ſpeak French? Why not leave it the name of acid of ſugar, ſince it is in that ſalt that this acid is moſt abundant, as ſixteen parts of ſugar produce ten parts of concrete acid?

‡ Rectified ſpirit of wine is called, but improperly, by the neologiſt chemiſts, *alcohol*. This term, ſays the dictionary of Trevoux, is derived from the Arabic word *kol*, which ſignifies to render ſubtle, to diminiſh, to reduce to an impalpable powder. It cannot therefore be applicable but to ſolid bodies. Philoſophers ought to employ in their language more preciſion and correctneſs.

able,

able, however, to determine it till after a great number and variety of experiments. That which anſwered the purpoſe beſt was as follows:—I put into a glaſs retort one part of ſpirit of wine, and three parts of nitrous acid at thirty-two degrees, adapted the retort to a ſeries of balloons, which I did not lute, and expoſed to a ſmall degree of heat the ſand-bath in which I had placed the retort. At about the thirtieth degree of Reaumur, the mixture was decompoſed; an efferveſcence, accompanied with large bubbles and red vapours of nitrous gas was produced; and at the ſame time the ether paſſed with an exploſion into the receiver, and perfumed the laboratory. It is neceſſary in this experiment that the retort ſhould be ſufficiently large to contain twelve times the quantity of the mixture introduced into it.

The ether being thus diſengaged, the fire was kept up under the ſand-bath, and the diſtillation was continued, until no more than about a thirty-ſecond part of the mixture remained. The ſand-bath was then ſuffered to cool, and I found at the bottom of the retort beautiful cryſtals of the acid of ſugar, in tetraedral priſms, under a little water ſlightly acid.

From ſixteen ounces of rectified ſpirit of wine, I obtained one ounce, one dram, and twenty-four grains of concrete acid of ſugar. When it is in combination with ether and the eſſential oil of wine, it neutraliſes their odour as well as the oils which render it volatile. What appears remarkable is, that it burns with them, ſince the combuſtion of pure ſpirit of wine, which takes place in the open air, leaves nothing.

Though the boiling heat is neceſſary for diſengaging from wine the inflammable ſpirit, it nevertheleſs exiſts in it. In my opinion it is engaged there by a portion of tartar which is found in the reſiduum of the diſtillation of the wine. The heat acts alſo, in all probability, on a glutinous matter ſimilar to that found in the dregs of wine: after it is ſeparated and inſpiſſated, the ſpirit of wine is diſengaged and volatiliſed.

When a mixture of equal parts of ſpirit of wine and concentrated vitriolic acid is diſtilled, the eſſential oil of the ſpirit of wine is reſiniſied and charred, the ether is liberated from

 its

its confinement. This kind of essential oil, surcharged with inflammable matter, is inalterable by acids, and may be considered as inflammable gas in the form of a fluid, of which it has indeed all the properties.

IX. *Comparative View of some dangerous Diseases, supposed to be occasioned by Insects, which prevail in Sweden, Russia, Siberia, and the adjacent Countries* *.

DR. SOLANDER was the first person who observed, with the eye of a philosopher, a disease known in West Bothnia and the neighbouring parts of Lapland under the name of *skät* or *skütz* †. This disease attacks those only who travel or labour in the open air; and in the above districts it is considered as the effect of the witchcraft of the Laplanders. The common people believe that the Laplanders cause balls of the hair of their cattle to fly through the air, and that the hairs which fall from these balls penetrate the bodies of men and animals, according to the pleasure of the witch or sorcerer, and occasion their death with excruciating pain.

Except in East and West Bothnia, Torneo, Kemi, and other parts on the gulph of Bothnia, this disease is exceedingly rare in Sweden. In those parts, however, it is generally known, and attacks some of the inhabitants every year. Linnæus conjectured that this disease was occasioned by a small kind of worm, one of which, in a dried state, had been sent to him from Kemi ‡; but he concluded, very erroneously, that the disease might be of the same nature as that prevalent among the horses in Holland, and known under the name of the *vyvel* or *feyfel*. This is now proved to be a mistake. Dr. Solander, however, has placed it beyond all doubt, that a small worm which penetrates into the flesh of living animals, is the occasion of those dangerous and mortal tumours

* From *Neue Nordische Beyträge*, by Professor Pallas, Vol. I.

† *Furia infernalis*, Dan. Chr. Solander in *Nov. Acta Upsaliensia*, Vol. I. p. 44.

‡ *Amœnitates Academicæ*, Vol. III. p. 322.

with

with which healthy people are often unexpectedly attacked in the before-mentioned districts. According to his observations, the extensive fens in the northern part of Bothnia are the principal seat of this destructive insect, and the tumours it produces have been remarked chiefly by the inhabitants of these districts or those who travel through them. In men, this disease generally attacks those parts of the body which are exposed to the air, such as the hands, arms, neck, &c. A prick like that of a needle is first felt, after which a black speck is seen in the place; and a violent itching ensues. This is soon after followed with extreme pain, a red spot is produced, and the neighbouring parts are inflamed. As the disease increases, the parts become more irritated and painful, and the patients are seized with an inflammatory fever, accompanied with fits of fainting and delirium, which often carries them off in the course of a few days, and sometimes of a few hours. Some, however, get over this fever, but are affected in the injured part with malignant and tedious sores, which often remain incurable during their whole lives.

Dr. Solander was himself an eye-witness of a case of this kind, and he mentions others which were related to him by persons worthy of credit. The greater part of them occurred in the spring and summer, though instances of this disease have been observed in winter. When the worm has penetrated into the flesh, it is often impossible to save the patient even by cutting out the affected part; but it sometimes happens that the small animal which is the cause of the evil may be extracted with a pair of pincers, or the teeth, if the proper means be employed immediately after the puncture is felt. When this is done by cutting, the patient, if the worm has penetrated to a great depth, is exposed to the most excruciating pain. Old women, who perform the operation of cutting the flesh or extracting the worm with a variety of superstitious practices, are accustomed to apply birch-tar to the wound, and then to bind it up. In one case, the worm was extracted by the accidental application of curdled milk or new cheese, and the patient cured in the course of six hours. This remedy was afterwards repeated with success. When the patient dies, the destructive insect, it is said, comes forth of its own accord from the mortified part.

But in whatever manner it may come forth, it has always the appearance of a very tender small worm, of the size of a hair, about half an inch in length, of a flesh or whitish yellow colour, and often black at the extremities. On each side it is armed with a single row of uncommonly tender bristles standing backwards, which enable it to penetrate the flesh better; and in all probability these bristles occasion that acute pain which is felt, and prevent it from being extracted. No rings or indentations can be observed on it; and, except the bristles, it has a great resemblance to the hair-worm.

This is the singular animal which Linnæus has admitted into his system under the very proper name of the *Furia infernalis*. It is however hardly possible to suppose, with this naturalist, that the above worm falls down from the atmosphere; for it will be difficult to explain by what power it can raise itself into the air. But the wonder will cease if we suppose that this worm may be borne aloft on bits of straw or the leaves of trees, and may then be conveyed by the winds till it at length falls down on the body of some animal.

In different parts of the Russian empire, particularly in dry summers, and in open marshy districts, dangerous diseases of the like kind have been observed, but essentially different from that of Sweden, though in all probability produced by a similar cause.

The elder Gmelin was the first person who remarked this disease in Siberia. He mentions it in his travels *, and gives an account of its symptoms as well as of the remedies employed to cure it. In order that the effects of the Russian disease may be compared with those of the Swedish fury, I shall here give an account of the former according to the latest observations on the subject †. The open level country which extends from the Urali mountains along the river Ui, or the so called Ui line, the whole district of Ischimi and Barabyni as far as it belongs to Russia along the Irtisch, and

* Part IV. p. 143.

† Pallas *Reise durch verschiedene provinzen des Russischen reichs*, Part II. p. 308.

even

even the districts between the Jaik and the Irtisch, but particularly the low, marshy, and saline fens of these immense plains which are dried up every year, and situations of the like kind near the rivers and brackish lakes from the banks of which the water retires during the droughty weather, are the places where this disease breaks out every year, and prevails more or less both among men and cattle. In these countries, the horses and other smooth-haired animals never go out with safety in summer to pasture. For this reason the inhabitants of the banks of the Irtisch, where this disease is most dangerous, drive their horses in summer to the high pastures, at a distance from the river, where in that season cool winds prevail, and where this kind of plague is never observed to make its appearance. In the northern and eastern parts of Siberia, and also in the mountains, it is not known. This disease, which is distinguished in the above districts by the name of *morowasa jaswa*, or plague, is most common in the summer months, when the sultry south winds prevail. As soon as the winds and the weather become cold, no traces of it are to be seen; but if such a change of temperature does not take place, the disease often continues till autumn, both among men and horses. Smooth-haired cattle also, and camels, the skins of which in summer are almost destitute of hair, are subject it. If we now reflect, that people who reside much in the house escape this scourge, and that it attacks those only who are exposed to the open air in the fields and meadows; that it chiefly affects those parts which are naked or thinly covered, and that it appears most frequently in horses in the belly, where the hair is thin; that sheep, by whose thick wool the disease seems, as it were, to be checked, never suffer from it, and that the smoke of the fires which the natives kindle, protects, in some measure, their cattle; we shall naturally be led to conclude, when the symptoms also are taken into consideration, that something must penetrate imperceptibly into the body from without, and by its poisonous quality produce inflammatory tumours and ulcers. It is probable, also, that it is something endowed with life, which is conveyed through the air to the marshy plains, where it attacks animals, and forces its way into their flesh. But whatever

ever it may be, the nature of it has not yet been determined by certain observation. The puncture of flies and gnats, especially when they have fed upon carrion, is in those districts of a poisonous and inflammatory nature: but these insects are too perceptible, and their effects so sensible, that we cannot ascribe to them a disease which at first is not attended with any remarkable pain, and in this respect it is particularly distinguished from the Swedish.

The symptoms of this disease, as generally observed, are as follows:—Persons in perfect good health, of all ages and of both sexes, unexpectedly feel, while walking in the fields, an itching accompanied with a small hard tumour, which at first they are inclined to consider as the effect of the puncture of a gnat or fly. It, however, speedily increases in size and hardness, and before much attention has been paid to it, has proceeded to such length, that a needle may be stuck into the hard swollen part without the patient being sensible of pain, unless the needle is made to reach the sound flesh. In the centre of the tumour there is then seen, for the most part, a red or blueish point like the prick of an insect, and in which, if proper means be not soon applied, mortification or gangrene ensues, and spreads itself to the neighbouring parts. In the first stage of the disease, the patient has no internal affections; but as the inflammation of the tumour increases (which, however, is not attended with much pain), head-ache, dejection, and internal uneasiness take place: but it is not improbable that the fear of danger may contribute to promote these symptoms. Among cattle attacked by this disease, uneasiness and dejection are not observed till it has attained to the utmost degree of its violence.

Cattle attacked by this disease generally die, partly because the tumours are observed too late, and partly because the common people, being negligent, do not give themselves the trouble to apply a cure; on the other hand, among the human species, a cure seldom fails if the proper means have been employed in time. The cure generally used by the common people, as mentioned in Gmelin's Travels, is, to run a long needle through the tumours in different directions, to rub into the punctures a mixture of sal-ammoniac and

and tobacco, and to forbid the patients the use of cold liquors and of certain kinds of food. This cure, however, has been rendered still simpler, and various other means more effectual have been devised. On the Irtisch, a strong lixivium of wormwood ashes is employed, and a decoction of tobacco and sal-ammoniac, or alum, without scarification. On the Ui line, a warm plaster made of pulverised nightshade, sal-ammoniac, yeast, and oatmeal, is said to be an infallible remedy; but the common people still adhere to the old painful method of cure, which is used even by some surgeons. In other places the common people consider the application of a living frog to the tumours as of service; but I have no authority for warranting its good effects. The other means, as is well known by long experience, are proper for destroying vermin, and therefore favour the conjecture before mentioned respecting the primary cause of this disease.

In the extensive low-lands on the Wolga, from Saratof towards Astracan, the cattle, and particularly horses, when they are driven out to pasture too soon after the water with which the fields have been overflown has run off, frequently die in consequence of hard inflammatory tumours which break out on the breast and belly, and which, without doubt, are of the same nature as the above-described Siberian disease. The Calmucs call these tumours *mohmo*, but, as appears, confound under this name other maladies, accompanied with tumours, which attack cattle. This much, however, is certain, that on the Wolga the tumours here described are prevalent among the human species; but they are most common in dry summers; and the men, who are daily exposed to the open air, are more subject to them than the women, who constantly reside in their smoky huts, or never remove to a great distance from them. Among the Calmucs on the Wolga, the Songari, who were acquainted with this disease in their former country, the Altai mountains, recommend, as a remedy for it, to burn on the tumour a bit of Chinese incense, which consumes in the same manner as tinder, and then to thrust into the black point which appears in the middle of it, a copper or iron needle, till it penetrate to the sound flesh. This remedy, however, must be applied before the expiration of the second day, else it will be ineffectual. The Torgatese are accustomed to stimulate and scarify these tumours with the petrified tooth of a shark, which they call a snake's horn. I have been informed in a letter from a person who was an eye witness, that tumours of this kind in a horse, dissected after death, were found to consist of soft watery matter. Horses often die on the day when the tumours appear *. Among the Calmucs it is customary to burn all the cattle which die of the so called *mobmo*, but the poor are seldom so nice, they only cut out the flesh around the tumours, and eat the remainder without any ceremony. It is said that the blood of those who feed on such carrion becomes corrupted, and that they are subject to running sores, called *chattigä*, which are often infectious. But that this observation is applicable rather to other diseases among cattle, than to the tumours in question, I conclude from this circumstance, that the Tartars say, that the *mobmo* of horses, which, as is well known, are most subject to external tumours, are not always infectious; those, however, of cows and goats are more dangerous, so that not only the flesh of these animals, when eaten inadvertently, is infectious, but even their skins if a person lies or sits upon them. According to some late observations which have been communicated to me, purgatives administered to those attacked with these tumours are pernicious: on the other hand, strengthening spirituous remedies are found to be serviceable if used before the appearance of lethargic affections, which in those countries seldom fail to accompany this disease, and which sometimes continue two or three days. The Calmucs, as I am informed, are inclined to ascribe the *mobmo* tumours to the poisonous puncture or filth of an insect, and therefore to an external cause.

[To be concluded in the next Number.]

* Pallas *Sammlungen historischer nachrichten über die Mongolischen volkerschaften*, Part I. p. 161.

X. *Descrip-*

X. *Description of the Vespertilio plicatus.* By FRANCIS BUCHANNAN, *M. D. A. L. S.**

VESPERTILIO PLICATUS †, tailed; the noſtrils round, ſimple perforations; the upper lip very large, and folded; the ears as large as the head, folded, and half pendulous. Inhabits old houſes at Puttahaut in Bengal.

From its teeth this bat can only be miſtaken for the *Cephalotes*; but the deſcription of that ſpecies will not apply to this.

From the point of the noſe to the root of the tail three inches; from the extremity of one wing to that of the other twelve inches.

The wings, and naked parts of the body, are ſoot-coloured. The hair is mixed with aſh-colour, and is paler below than on the back.

The head is large, thick at the ſhoulders, and tapers gradually to the ſnout; which is blunt, terminates in a heart-ſhaped margin, and projects far beyond the lower jaw. It is moſtly naked; but has ſeveral long, erect briſtles. The noſtrils are ſmall circular holes, remote from each other, and placed under the margin of the ſnout. The upper lip at the ſides hangs over the under jaw, and at each ſide is deeply wrinkled with ſeven or eight vertical folds. The ears are large, blunt, wrinkled, and ſomewhat pendulous. From being bent in ſeveral folds, they at firſt ſight appear to be thick and fleſhy. They approach very near at their inſertion on the brow, and are naked, except on a ſharp ſinus towards the hinder part of the head. On their edge near the tips are five or ſix ſmall warts. There are no internal auricles. The eyes are in two ſmall ſlits above the angles of the mouth, and are almoſt covered by the ears. There are two ſtrong tuſks in each jaw. In the upper jaw there are two conical ſharp fore-teeth, half as long as the tuſks. Below, in place of theſe, there are only two ſmall points, ſcarcely projecting from the gums. The grinders are a little removed from the tuſks, and

* From the *Tranſactions of the Linnæan Society*, Vol. V.

† Section F of Ker's Tranſlation of Gmelin's Syſt. Nat.

are in each fide of each jaw five or fix in number. In the lower jaw each grinder has two fharp points; in the upper jaw each, except the firft pair, has three points.

The neck is very fhort, and fo covered with hair as to be fcarcely obfervable. The fhoulders are high and round, with a deep cavity between them. The body at the fhoulders is much wider than at the haunches. The buttocks are bare.

The tail is naked, round, and blunt: it is turned up at the end. A ftrong hooked claw in place of the thumb. No carpus nor metacarpus. Four long fingers ferve to diftend the membrane of the wing. The hind-feet have five diftinct toes, with fmall fharp claws. The membrane of the wings joins the hinder legs and tail, but it is not nearly fo long as the latter. A broad hairy lift furrounds all round the body, and covers the bottom of the membrane.

The drawing and defcription were taken from a male. I could obferve no nipples.

The natives of Bengal have only two names for all the fpecies of bats found in their country. The large bats, which neftle on trees, and live chiefly on fruit, they call *Badur:* thofe which, like the one above defcribed, inhabit caverns and old buildings, and live chiefly on infects, they call *Chamcheeka.*

Plate IV. reprefents the *Vefpertilio plicatus* of nearly one-half its natural fize.

XI. *Account of a Cavern difcovered on the North-Weft Side of the Mendip Hills, in Somerfetfhire.* By George Smith Gibbes, *M.B. F.L.S.**

PERHAPS the following account of a cavern which I vifited fome time fince may be acceptable, as we there fee the procefs going on, which Nature employs to enclofe foreign fubftances in the harder rocks.

At the bottom of a deep ravine on the north-weft fide of the Mendip hills, in Somerfetfhire, near the little village of Berrington, there has been difcovered a cavern of confiderable

* From the *Tranfactions of the Linnæan Society,* Vol. V.

extent,

extent, in which was found a great collection of human bones.

As I have obferved in this cavern many circumftances which appear curious to me, I beg leave to mention them, as I do not believe there is another place in the kingdom where the different ftages (if I may be allowed the expreffion) of bones incorporating with limeftone rocks can be fo well feen. From the top and fides there is a continual dripping of water, which being loaded with a large quantity of calcareous earth, depofits a white kind of pafte on moft parts of the cavern. Many of the bones are incrufted with this cement, and a large proportion of them are actually fixed in the folid rock. I fuppofe, therefore, that this fubftance, which at firft is in a ftate refembling mortar, by lofing its water, hardens into a firm and folid ftone. I had an opportunity of examining the procefs in every part. Had the cavern not been difcovered, and thefe depofited fubftances not been removed, I do not doubt that the whole excavation would, in no great length of time, have been completely filled up. The water was ftill bringing frefh quantities of calcareous earth, and the bones were in fome places completely incorporated with the folid rock. Every degree of intermediate folidity was plainly difcernible. There were feveral nodules of ftone, each of which contained a perfect human fkull. The fubftance which is depofited from the water effervefces with acids, and has, in fhort, every character of limeftone. At the further end of this very curious cavern, where the height is about fifteen feet, there depends a moft beautiful ftalactite, perfectly conical, which, when the cavern was firft difcovered, reached within an inch of a cone of the fame kind which rifes from the floor. By fome accident a fmall part of the ftalactite was broken off; but Nature is now bufy in repairing an injury which had been done to one of the pretticft productions of her mineral kingdom. Had thefe two cones met, a beautiful column would have been formed of nearly fifteen feet in height. On ftriking this ftalactite, a found is produced fimilar to that of a bell, which may be heard at a confiderable diftance beyond the mouth of the cavern.

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I examined the bones with considerable attention, and I found that there was adhering to the surface of many of them, a substance which resembled the spermaceti I have before described in the Philosophical Transactions for the years 1794 and 1795.

I have to add, that this cavern was discovered about two years ago by accident, and that no satisfactory reason has been given for this singular accumulation of human bones.

XI. *Description of a Mercurial Air-holder, suggested by an Inspection of Mr. Watt's Machine for containing Factitious Airs.* *By* WILLIAM CLAYFIELD *.

SEVERAL modes of counteracting the pressure of a decreasing column of mercury having been thought of, in conjunction with Mr. W. Cox, the following was at last adopted as the most simple and effectual:

Plate V. Fig. 1. represents a section of the machine, which consists of a strong glass cylinder A, cemented to one of the same kind B, fitted to the solid block C, into which the glass tube D is cemented for conveying air into the moveable receiver E.

The brass axis F, fig. 2, having a double bearing at *a, a*, is terminated at one end by the wheel G, the circumference of which is equal to the depth of the receiver, so that it may be drawn to the surface of the mercury, by the cord *b*, in one revolution; to the other end is fitted the wheel H, over which the balance-cord *c*, runs in an opposite direction in the spiral groove *e*: a front view of the wheel H is shown at Fig. 3.

Having loaded the receiver with the weight I, something heavier than may be necessary to force it through the mer-

* From an ingenious work lately published entitled, *Researches Chemical and Philosophical, chiefly concerning nitrous Oxyd or dephlogisticated nitrous Air, and its Respiration*; *by Humphrey Davy, Superintendent of the Medical Pneumatic Institution.*—This apparatus was first described in the Third Part of Dr. Beddoes's Considerations, but it has since received some improvements.

cury;

cury, it is balanced by the ſmall weight K, which hangs from that part of the ſpiral where the radius is equal to that of the wheel G; from this point the radius of the ſpiral muſt be increaſed in ſuch proportion, that in every part of its circuit, the weight K may be an exact counterpoiſe to the air-holder. In this way, ſo little friction will be produced, that merely plunging the lower orifice of the tube D under mercury contained in the ſmall veſſel L, will be ſufficient to overcome every reſiſtance, and to force the gas diſcharged from the beak of a retort into the receiver, where, whatever may be its quantity, it will be ſubjected to a preſſure exactly correſponding to that of the atmoſphere. The edge of the wheel H being graduated, the balance-cord *c* may be made to indicate its volume.

Should it at any time be neceſſary to reduce the preſſure to the medium ſtandard of the barometer, it may eaſily be done by graduating the lower end of the tube D, and adding to the weights I or K, as may be found neceſſary; the ſurface of the mercury in the tube pointing out the increaſe or diminution.

The concavity at the top of the internal cylinder is intended to contain any liquid it may be thought proper to expoſe to the action of the gas.

The upper orifice *f*, with its ground-ſtopper, is particularly uſeful in conveying air from the retort *g*, with its curved neck, into the receiver, without its paſſing through the tube D. In all caſes where a rapid extrication of gas is expected, the retort *g* ſhould be firmly luted to the orifice, and the weight I removed from the top of the receiver; this, by diminiſhing the preſſure, will admit the gas to expand freely in the air-holder at the inſtant of its formation, and prevent an exploſion of the veſſels. The ſame caution muſt be obſerved whenever any inflammation of gas is produced by the electric ſpark.

The air may be readily transferred through water, or even mercury, by the tube *h*, Fig. 1.

To prevent an abſorption of mercury in caſe of a condenſation taking place in the retort made uſe of for generating air, Mr. Davy has applied the ſtop-cock *i*, to which the neck is firmly luted. This ſtop-cock is likewiſe of great ſervice in ſaturating water with acid or alkaline gaſes, which may be effected by luting one end of the tube *k* to the ſtop-cock and plunging the other into the fluid in the ſmall veſſel *l*, cemented at top, and terminating in the bent funnel *m*; the tube *b* having been previouſly removed, and the lower orifice of the tube D either ſunk to a conſiderable depth in mercury, or cloſed with a ground-ſtopper. The bend of the funnel *m* may be accurately cloſed by the introduction of a few lines of mercury.

The application of the ſtop-cock *n*, has enabled Mr. Davy to perform ſome experiments on reſpiration with conſiderable accuracy.

XII. *Curſory View of ſome of the late Diſcoveries in Science,*

[Continued from Page 80.]

FOSSILS.

G. A. DELUC has examined the lenticular numiſmal, and the belemnite. He conſiders the lenticular as the bones of a kind of ſepia. Naturaliſts in general conſider it as a kind of cornu ammonis.

He has the ſame opinion reſpecting the belemnite. "It is extremely probable," ſays he, "that this foſſil has been the ſoft bone of a fiſh."

The numiſmal is found in Europe, in Egypt, in India near the Ganges, and in Bengal.

He found at Mount-Salève a petrified buccinum ſimilar to thoſe found at Ermenonville.

He obſerved phollades in the columns of the temple of Serapis near Pozzuolo: they are in a part of the column, raiſed at preſent twenty-ſeven feet above the level of the waters of the ſea. He ſuppoſes that this temple has been thrown into the ſea by an earthquake; that the phollades then penetrated into the columns; and that a new earthquake conveyed them back to the place where they now ſtand.

FAUJAS has begun to give a deſcription of the foſſils found in the mountain of Saint-Pierre, near Maeſtricht. They

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confift of different fhells, and a great number of bones. Whole jaw-bones, more than four feet in length, have been found in that mountain. They are fuppofed to have belonged to crocodiles. He has defcribed alfo fome fhells which appear to have belonged to the common tortoife, the *tortue franche* of Lacepede, or the *teftudo mydas* of Linnæus. This further confirms, that the remains of animals ftill exifting are found among foffils.

Denis Montfort has publifhed a memoir on a particular foffil kind of the cornu ammonis, found in the environs of Rouen and in other places. It is not of a fpiral form like the common cornua ammonis, but fhaped like a buccinum.

Villars has found foffil wood on the mountain of Laus, in the canton of Pifans, at the height of 2537 feet, that is to to fay, 932 feet above the moft elevated line at which trees grow at prefent. The trees of which foffil remains are found, are the trembling poplar, birch, and common larch.

GEOLOGY.

Bertrand has given a new explanation of his geologic fyftem. He ftill fuppofes that the globe of the earth was originally a frozen mafs, and that along with motion it received light, liquidity, heat, and life.

Jens Esmarck, a learned mineralogift, has travelled through the Bannat, Tranfylvania, and Hungary, countries exceedingly rich in metallic veins. He has defcribed thefe veins with accuracy, and explained the formation of them according to the theory of Werner, who, as is well known, thinks that fuch veins were originally fiffures, afterwards filled up with minerals. But fome facts related by the author feem to fhow that this fyftem cannot be maintained, for at Chemnitz there are veins of from 100 to 114 feet in thicknefs: at Kremnitz, the principal vein is 656 feet in thicknefs. The author endeavours to fhow that thefe veins arife from the union of more than twenty fmaller veins divided by partitions of the rock.

Humboldt, by the voyage which he has undertaken round the globe, will greatly enlarge our geological knowledge, becaufe because he generalises his observations. The following are those which he has transmitted from Cumana respecting the famous Peak de Teyde, called commonly the Peak of Teneriff:—"The Peak of Teyde, which rises to the height of 1904 toises, is an enormous basaltic mountain that seems to rest on dense and secondary calcareous stone. The latter is of the same kind as that found with a great deal of flint at Cape Non in Africa, at Cadiz, in the Channel, in Provence, and on which the basaltes of Saint-Loup near Agde, and that of Portugal, rest. It is thus seen with what uniformity the globe is constructed. The Azores, the Canaries, and the Cape Verd isles, appear to be a continuation of the basaltic rocks of Lisbon."

"The waves thus carry with them, and cast on shore in Africa and Teneriff, granites, syenites, and granitic micaceous schist, which I have seen at Saint-Gothard. It is to be supposed that the high ridge of Mount Atlas, which extends from the East towards the coasts of Morocco, consists of such rocks."

"The basaltes of which the Peak is constructed, is not only basaltes containing foliaceous and crystallised olivin and volcanic chrysolite, but particularly towards the top there are strata of the *porphyroschiefer* of Werner, or of another kind of porphyry with an obsiadian base. The *porphyroschiefer* is foliated, sonorous, semi-transparent at the edges, has a very hard green base, and contains crystals of vitreous feld-spar.

[To be continued.]

XIII. *Observations on Ants, and on the Poison of these Insects; with some Hints for destroying them.* By M. Amoureux *jun. M. D.**

THE provident ant, which has been so often quoted as an example of foresight and economy, does not, as has been believed, store up provision; it is only laborious, and collects, indiscriminately, things that are eatable and others that are not; the former to supply it with food for the moment, and the

* From *Notice des Insectes de la France reputés venimeux.*

the latter to be employed as materials neceſſary for ſupporting ſubterraneous galleries and for giving occupation to labourers; for their ſociety, like that of the bees, conſiſts of males and females, and a neutral ſpecies, or individuals without ſex. It is the latter that are charged with the internal labour, and the care of nouriſhing the young. The labouring ants are diſtinguiſhed from the reſt by being deſtitute of wings; they enjoy alſo a longer life than the males or the females, and they often remain alone with the larvæ in the neſt. Ants continue in a ſtate of torpor during the winter; and it may thence be preſumed that the articles they ſtore up do not conſiſt of food which they wiſh to provide to afford them a ſupply during periods of ſcarcity. Their granaries are not filled with proviſions, ſince they ſtore up, along with grains of corn, bits of ſtraw and all other ſmall articles which they can find. Naturaliſts, therefore, have been miſtaken reſpecting this pretended foreſight, by aſcribing to a laborious inſect a manœuvre of which man alone is capable.

The ant is not accounted venomous, yet it is more ſo than moſt other inſects. There ariſes from its body an acid vapour which is not perceptible but when they are collected in a large number, as in an ant-hill, or when they are heaped up in a box or bottle, &c. For this reaſon ants leave traces behind them, by a ſort of ſcorched appearance, on the graſs and plants over which they paſs and repaſs in going backwards and forwards from their neſts. Gardeners, therefore, have great reaſon to wiſh for the diſcovery of ſome certain means that might free them from theſe troubleſome gueſts, which ſome have ſuppoſed to be uſeful by having ſeen them attack the ſmall inſects which injure the leaves of fruit-trees, and particularly thoſe of the peach. Ants, indeed, are attracted by the melleous liquor ſhed by ſome of theſe inſects, and which moiſtens their bodies, but they do not on that account carry on war againſt them; in concert with them they deſtroy the plants and trees, on which they aſſemble in large bodies.

The ant pinches very ſtrongly with its mouth, which is armed with jaws; it punctures alſo by means of a ſting at its extremity, which is wanting in the males: it can do injury, therefore, three different ways. Swammerdam says that the jaws or forceps of the ant have on each side seven points or small teeth; and Leuenhoek pretends that it emits from its sting small drops of a certain liquor which occasions pain, as is the case with that of the scorpion. Probably it is nothing else than its acid liquor which is caustic.

This insect is very extraordinary, not only on account of its form, but on account of its small size; and being so commonly seen, it has not been examined with sufficient attention. Those who wish to see ants of a gigantic appearance, may inspect the figure given of one by Gundelius Ab Ach, who flattered himself that, in 1687, he had invented a microscope that magnified twice as much as any ever before constructed. The same author has given the figure of a winged ant of the dimensions of eleven inches.

We may consider ourselves happy, that among the small number of species of this insect known in France, which Mr. Geoffroy makes to be only six, there are none of those voracious and destructive kinds so troublesome in the burning climates of Africa, in Egypt, and in South America. A swarm of ants which have lodged in a hollow tree, issue from it on its being in the least shaken, and, like a shower of fire, fall on the unhappy person exposed to their fury. One of our most learned naturalists, going along the banks of the Niger, found himself invested by a sort of red ants which lodge in the icaco-tree. The punctures they made were so venomous, says he, that my face and hands were covered with blisters similar to those arising from burning, the pain of which could be allayed by nothing but a heavy rain, which took place in the evening. The same traveller was pestered in the isle of Gorée by another kind of white ants (*termites*) which gnaw and destroy every thing that comes in their way.

Botanists long ago observed, that when the flowers of succory were thrown into an ant's nest, their blue colour was changed into red, which is, no doubt, common to the greater part of blue flowers. This phenomenon must at first have excited some surprise, and it was explained, no doubt, according to the physical knowledge of the period; but it was afterwards discovered that this change of colour was owing

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to the acid liquor which the ants had ſhed upon it. This acid ſpirit was carefully examined by ſeveral experiments, an account of which was publiſhed in the Philoſophical Tranſactions for the year 1670. M. Tiſcher appears to have been the firſt who aſcertained the nature of this acid, and this diſcovery muſt have appeared the more aſtoniſhing, as it was known that animals in general furniſhed alkali and an urinous ſpirit.

After this, chemiſts haſtened to analyſe ants and the vapours exhaled from them. Homberg extracted from them, by diſtillation, abundance of acid, and ſhowed it to the Academy of Sciences in 1712. Margraf ſoon obtained the ſame acid; but the learned Pruſſian chemiſt conſidered this acid only in regard to its relation and combinations with other chemical ſubſtances, and the ſolutions it is capable of effecting. This acid has a great reſemblance to that of radical vinegar, though it does not reſemble it in every thing: Neuman firſt perceived an odorous eſſential oil, of a peculiar ſmell, which, however, occaſions no burning ſenſation on the tongue. Margraf obtained the ſame ſubtile oil, and another fat oil ſimilar to that of vegetables. M. Thouvenel drew from ants both theſe oils, and a particular extract; a reſult of the moſt complete analyſis that ever had been made of theſe inſects.

The late M. Roux, the editor of the *Journal de Medecine*, paid attention alſo to this object, and by his curious experiments brought to light ſome facts very little known. For example, if an ant-hill be opened, and a living frog be expoſed in it, the animal will die in leſs than four or five minutes, though it has not been bit by the ants. M. Roux, being employed in collecting ants to fill a bottle, perceived in the evening that his fingers were ſomewhat hot, that they ſwelled and became red; next day the epidermis ſeparated from the ſkin as if he had applied a veſicatory, and his fingers became entirely peeled. It appears alſo, by M. Roux's experiments, that the acid vapour of ants is capable of ſuffocating them when they are ſhut up in a bottle. This may ſupply ſome hints for ſtifling their deſtructive colonies by carefully cloſing up the apertures of ant-hills and heaping earth above them. M. Roux mentions alſo an obſervation ſimilar to his own, which was

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communicated by Baron D'Holbach; he made at the fame time fome reflections on the nature of the poifon of ants, refpecting which he would not venture to give a decifive opinion. This prudent modefty in an able chemift may ferve us as a leffon.

Meffrs. Ardwiffon and Oerhne have alfo made a number of curious experiments on this acid, which they confider as a peculiar kind. It has been admitted into the new chemiftry under the name of the *formicine:* it is ftronger than the vitriolic acid *, ferves for different combinations, and an ether may be obtained from it. M. Thouvenel, who prepared from it the formiate of potafh, experienced the fame effects from the volatile acid of ants as M. Roux; for, after having collected fome of thefe infects, large veficles full of ferous liquor were formed at the ends of his fingers. This learned chemift made many experiments to afcertain the nature of the acid of ants, and they all convinced him that it differs from the mineral acids as well as from fermented vegetable acid, and that it has every chemical relation to the microcofmic acid †, &c.

M. Cadet junior, and other chemifts, found that the volatile alkali deftroys ants and neutralifes their acid. This, therefore, may afford the means of being preferved from it, or of checking its action on the fkin. Olive oil produces the fame effect.

Medicine might perhaps derive more advantage than it has hitherto done from the volatile fpirit and oil of ants. A bath of ants has been prefcribed in cafes of the palfy. The water or fpirit of magnanimity, and other pharmaceutic compofitions, are prepared from ants and their products. They might certainly be employed in veficatories.

To the above obfervations I fhall add the following fact, recorded in the Ephemerides of the curious of Nature for the year 1688 ‡, which will prove the efficacy of ants as a revulfive remedy:—A fuller, who had been long tormented with a pain in his head, and tired of taking medicines with-

* The author, no doubt, means that it will decompofe fome of the vitriolic falts: in this fenfe many vegetable acids are ftronger than the vitriolic.—EDIT.

† Phofphoric acid. ‡ Dec. 2, obj. 80.

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out receiving any benefit from them, refolved to difcontinue them, and to bear his fufferings with patience. One day, however, while collecting ants, his hands were excoriated by the acrid liquor of thefe infects, and he was immediately freed from the pain in his head, which never afterwards returned.

XIV. *Experiments refpecting the Influence which Oxygen has on the Germination of Seeds.* *By* Mr. SAUSSURE *junior* *.

MOST philofophers who have examined the nature of atmofpheric air have found, that feeds expofed to water and pure azotic gas do not germinate; that, on the other hand, carbonic acid gas is formed, which mixes with the azotic gas, and increafes the atmofphere of the plant. They obferved alfo, that when oxygen gas was employed inftead of azot, carbonic acid gas was in the like manner produced, but that the atmofphere was then leffened and the oxygen gas abforbed.

Dr. Rollo †, in the courfe of his refearches refpecting the germination of barley, remarked, that the formation of fugar in the grains of barley during the procefs of germination was the immediate confequence of the action of oxygen gas on the grains, and that, in every cafe where this action was fuppreffed, no fugar was formed. From this obfervation, as well as from the formation of carbonic acid gas in azotic and oxygen gas, he concludes, that oxygen gas is in part abforbed by the barley, but that it in part forms with the carbon of the feeds carbonic acid gas. The fugar which is formed immediately after the germination is, according to him, produced by the union of oxygen gas with the flimy vegetable fermentable matter.

If the quantity of the carbonic acid gas formed in this manner be lefs than that of the oxygen gas which has difappeared, it is probable that a part of the latter is abforbed by the grain, and

* From Scherer's *Allgemeines Journal der Chemie*, No. 7, for January 1800. † It fhould be Cruickfhank.

and that another part is employed in the formation of carbonic acid gas with the carbon of the grain. But if the quantity of the carbonic acid gas produced be greater than the quantity of oxygen gas which has disappeared, we can only conclude, that the corn is capable of producing from its own substance, by the union of its carbon with the oxygen gas of the atmosphere, a quantity of carbonic acid gas. If the quantity, however, of the oxygen gas which has disappeared be perfectly equal to that which contributes to the formation of the carbonic acid gas during the progress of the germination, we may thence conclude that the oxygen gas is not absorbed by the grain, but only employed in the formation of the carbonic acid gas. As no one has yet examined which of these three cases actually takes place (it having been generally admitted that the oxygen gas is absorbed by the grain during the progress of germination, without its being however proved), I made, towards the end of the year 1798, the following experiments at the temperature of between + 6 and + 12° of Reaumur in the shade.

Experiment I.

I sowed on a wet sponge 21 peas, which together weighed 62 grains. The sponge was introduced on a small stool under a receiver which contained 13½ cubic inches of atmospheric air purified by lime-water. The aperture of the receiver was closed by means of water, and the latter rose in it to a sufficient height to prevent the escape of the air during the change of the atmosphere.

In eight days these peas had vegetated so much that roots were thrown out, of from three to four lines in length; the air in the receiver, in consequence of the change occasioned by the temperature and pressure, was lessened one-thirteenth part of its original volume. It now occupied 12·55 cubic inches, and lime-water indicated 0·10 of carbonic acid gas. The phosphoric eudiometer gave 0·04 of oxygen gas, or 0·17 less than atmospheric air. A hundred parts of nitrous gas mixed in an equal quantity with the air in the receiver, left a residuum of 188 parts. The same mixture with atmospheric air, left a residuum of 105 parts. If we suppose, with Lavoisier,

Lavoiſier, that atmoſpheric air contains $\frac{27}{100}$, or $\frac{6}{100}$ leſs oxygen gas than is ſhown by the phoſphoric eudiometer, the $13\frac{1}{2}$ cubic inches of atmoſpheric air, which were loſt in this experiment, contained before the peas were introduced 3·64 cubic inches of oxygen gas and 9·86 of azotic gas. The 12·55 cubic inches which remained after germination contained 1·255 cubic inches of carbonic acid gas, 1·255 of oxygen gas, and 9·86 of azotic gas.

If the decreaſe in the volume of the air during the germination ariſes from the carbonic acid gas being abſorbed by the water, which ſhuts the receiver and promotes the germination of the ſeeds, this decreaſe of volume with the oxygen gas which remains in the atmoſphere of the peas, ſhows the quantity of oxygen gas which has been formed in the courſe of the germination. If this quantity of carbonic acid gas is perfectly equal to that which is formed during the experiment by the union of the oxygen gas, which has diſappeared, with the carbon of the ſeeds, it proves, almoſt inconteſtably, that the oxygen of the atmoſphere is not abſorbed by the ſeeds, but has been employed in the formation of carbonic acid gas, a part of which is abſorbed by the water while the other remains in the atmoſphere of the ſeeds.

If we add the decreaſe of the volume of the air during the courſe of the germination, or 0·95 cubic inches to 1·255 cubic inches, the quantity of the carbonic acid gas which remains in the atmoſphere of the ſeeds, we ſhall have 2·205 cubic inches for the ſum of the carbonic acid gas. But this is almoſt the ſame quantity as that which is obtained by the union of the carbon of the ſeeds with the 2·385 cubic inches of oxygen gas which diſappeared in the courſe of the experiment.

It is very probable, therefore, from this experiment, that the decreaſe in the volume of the atmoſphere during the germination depends merely on the water abſorbing a part of the carbonic acid gas, which is formed by the union of the oxygen gas with the carbon of the ſeeds, and that it cannot be aſcribed to the abſorption of the oxygen gas by the ſeeds. To place the truth of this aſſertion in its full light, I examined whether the decreaſe of the atmoſphere of the ſeeds would

would take place if I endeavoured to prevent the carbonic acid gas from being absorbed by the water.

If water be placed in an atmosphere of pure carbonic acid gas, the water never absorbs more of such gas than what is equal to its volume, provided the fluid sustains no other pressure than that of the weight of the atmosphere. The quantity absorbed by the water is less, the greater the quantity of the atmospheric air mixed with the carbonic acid gas. When the carbonic acid gas makes only 0·02 of the atmospheric air, the portion absorbed is not perceptible. This absorption, therefore, may be always rendered imperceptible, either by increasing the volume of atmospheric air which is in contact with the seeds, or by leaving in the receiver only as much water as is absolutely necessary for the purpose of germination.

Experiment II.

Eighteen peas were placed in 11½ cubic inches of atmospheric air, purified by lime-water, under a bell closed with mercury. The fluid rose an inch higher than within the vessel. Into this receiver I transmitted ¼ cubic inch of water in which the peas were to germinate: they floated half in it on the mercury. Ten days after, the peas had grown so much that their roots were from three to four lines in length. I examined the air under the bell, and found that its volume was not sensibly lessened. Lime-water showed 0.09 of carbonic acid, and the phosphoric eudiometer, after deducting the carbonic acid, 0·12 of oxygen gas, and therefore 0·09 less than in atmospheric air. When nitrous gas was mixed in equal parts with this air, there remained a residuum of 132 parts. If we admit, with Lavoisier, that atmospheric air contains about 0·27 oxygen gas, or 0.06 less than is indicated by the phosphoric eudiometer, which in the air of my laboratory gives only 0·21 oxygen gas, it will be found that the air under the bell, before the peas were introduced, contained 3·105 cubic inches oxygen gas and 8·395 of azotic gas; and that after the germination the same volume of air contained 1·88 cubic inches of oxygen gas, 8·395 of azotic gas, and 1·035 of carbonic acid gas; consequently, 1·225 cubic inches of atmospheric oxygen gas were employed to form the basis

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of 1·035 cubic inch of carbonic acid. This refult approaches as near the truth as can be expected in an experiment made with maffes of air fo exceedingly fmall.

Experiment III.

I introduced into a receiver clofed with quickfilver, 40 cubic inches of atmofpheric air purified by lime-water. In this air I placed a conical glafs with fmall flints, which I moiftened with two cubic inches of water, and depofited on them four beans. At the end of fourteen days all the beans had thrown out roots of from four to five lines in length: I then took them out, and tried their atmofphere. No fenfible change in the volume was obferved. Lime-water fhowed 0·10 of carbonic acid gas: the phofphoric eudiometer, after this gas was deducted, contained 0·13 oxygen gas, or 0·08 lefs than atmofpheric air. This mixture with atmofpheric air gave a refiduum equal to 105 parts. According to this refult, we find, if we add to the quantity of oxygen gas indicated by the phofphoric eudiometer 0·06, that during the experiment 3·96 cubic inches of oxygen gas difappeared, and were employed in the formation of four cubic inches of carbonic acid gas. The beans appear, therefore, in the atmofpheric air to have abforbed no oxygen gas.

Experiment IV.

I put 60 grains of barley, with $\frac{1}{3}$ cubic inch of water, into a receiver clofed with quickfilver, and which contained 18 inches of atmofpheric air purified by lime-water. The greater part of the grains of the barley remained adhering to the moiftened fides of the receiver: the reft floated half under water upon the quickfilver. After the feeds had germinated, their atmofphere was increafed, the changes of temperature and the preffure of a quantity imperceptible to the eye being taken into confideration. Lime-water fhowed $\frac{1\cdot5}{1000}$ carbonic acid gas: the phofphoric eudiometer, after deducting the carbonic acid gas, 0·11 of oxygen gas, or 0·10 lefs than atmofpheric air. A hundred parts of nitrous gas, mixed in an equal portion with this air, left a refiduum of 138 parts: the mixture with atmofpheric air gave a refiduum of 105 parts.

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From this experiment it appears, according to the calculation more fully given above, that during the procefs of germination 2·16 cubic inches of oxygen gas difappeared and were employed for the bafis of 2·07 cubic inches of carbonic acid gas, which was found in the atmofphere of the feeds. This refult approaches near to that given by the compofition of this gas.

But to obtain this refult the feeds muft all germinate, and they muft be in immediate contact with the atmofpheric air. When they do not germinate, either becaufe they are placed too clofe to each other, are of a bad quality, or have too much water over them, the quantity of the carbonic acid gas formed will be greater than that produced by the union of the oxygen gas taken from the atmofphere with the carbon of the feeds. Befides, the experiment muft be finifhed before all the oxygen gas contained in the atmofphere of the feeds has been converted into carbonic acid gas; for without this precaution the plants would be injured and decompofed, and a greater quantity of carbonic acid gas would be found in their atmofphere than would otherwife be the cafe.

The formation of fugar in the feeds by the action of foreign oxygen gas is a very fingular phenomenon which I will not venture to explain; I fhall only remark, that from the above experiments the following conclufions may be drawn:

1. The oxygen of the atmofphere is abforbed during the procefs of germination, not by the feeds, as hitherto feems to have been admitted, but is employed merely in forming carbonic acid gas with the carbon of the feeds.

2. The germinating feeds when in contact with the air do not form carbonic acid gas from their own fubftance, but only furnifh a component part of it, *viz.* carbon. They yield, however, from their own fubftance, the oxygen and carbon of the carbonic acid gas, formed when the feeds are in contact with water and pure azotic gas.

NEW

NEW PUBLICATIONS.

Geological Essays. By RICHARD KIRWAN, *Esq.* F.S.S. *Lond. and Edin. President of the Royal Irish Academy, &c. &c., and Inspector-General of his Majesty's Mines in the Kingdom of Ireland.* Bremner, Strand, 1799: 8vo. 502 Pages.

THIS work consists of a Preface, an Introduction, twelve Essays, and a short paper of Notes, following the last of the Essays. But, though its parts be thus arranged without systematic connexion in the formal structure of the composition, it is, in fact, as to its substance, *one whole*; in which mineralogical science is applied to explain all the appearances to which it can have any relation, on the surface or in the interior strata of the earth. In the explanation of these appearances the author endeavours to ascertain from its present state what must have been the various former changes through which the matter of this globe has passed since the æra at which it was made subject to those laws of existence under which we now behold it. The following are the leading truths and opinions which occur in the successive parts of the work.

The Preface mentions the discovery of the distinction between primary and secondary mountains, by Lehman, as one of the most important events in the progress of geological science; exposes the futility of the theory of Buffon, which represented all the primary mountains to have originated out of a state of igneous fusion; names with applause those philosophers who have substituted mineralogical and chemical investigation for hasty conjecture in their attempts to trace the formation of the earth, and the changes which have happened in the arrangement of its strata; professes the intention of this work to be, to vindicate the divine authority of Revelation, by proving the account given by Moses of the origin of the terrestrial globe, to agree, in the most remarkable manner, with the fairest inductions from the present phenomena.

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The Introduction ftates, that, in compofing the following fyftem, the author has been careful "to attribute no effect to any caufe of which the known powers are inadequate to its production," as well as "to alledge no caufe whofe exiftence has not been proved to him by experience or credible teftimony."

In the firft Effay he obferves that the earth could not have affumed the form of a fpheroid compreffed at the poles, if its fuperficial parts had not originally been, to a certain depth, in a foft or liquid ftate; and that the lower ftrata of primitive calcareous rocks being, in proportion to their depth, continually thinner than thofe which reft upon them, equally evince the whole to have been formerly in a foft ftate, and to have yielded to compreffion while they were dried and hardened. He therefore fuppofes the fuperficies of the globe to have been univerfally fluid, its interior compofition folid and cavernous, at the time when the prefent laws began to operate upon it. The fuperficial fluid he conceives to have been heated at leaft to 33° Fahr. Diffolved or fufpended in it were portions of the eight generic earths, of all the known metallic fubftances, of all the fimple falts, and of all inflammable matters folid and liquid. Siliceous earth and iron were of all the folid materials in the mixture the moft copious. Calcareous earth muft even then have exifted, for it is a component part in many primitive rocks. As the fluid mafs was moved; as its heat paffed into new combinations; as the elective attractions of its various materials began reciprocally to operate; the folidifiable materials were precipitated in the order of their affinities, fomewhat difturbed by the mechanical agency, which at the fame time neceffarily affected them. In their precipitation they were univerfally cryftallifed. Where filiceous and argillaceous earths chiefly abounded, quartz, feld-fpar, and mica, were confufedly depofited in ftrata of granite and gneifs. One depofition fucceeded another till vaft uniform blocks were formed. Carbon, iron, and others of the folidifiable materials of the chaotic flood, were unavoidably involved with the fubfiding ftony particles. Where the materials were intermingled in different proportions, other forts of primitive rock, fuch as filiceous fchiftus, filiceous ſiliceous porphyries, jaſpers, &c. were produced. In the confuſed agitation of all parts of the flood, ſulphur and metallic ſubſtances encountering, formed pyrites; while pit-coal was alſo compoſed by the aſſociation of petrol with carbon. After the formation of combuſtible compounds ſo eaſily decompoſable, heat ſufficient for their combuſtion was ſoon afforded from the inceſſant ſolidification of matters which had been hitherto fluid. A decompoſition of water, by means of heated iron, was one of the firſt effects from the riſing intenſity of the general heat. The combuſtible matters were generally inflamed, and many volcanic fires burſt out from the boſom of the abyſs. Gas-azot, gas-oxygen, and afterwards carbonic acid gas, were, in great quantities, evolved from the conflagration, and diffuſed into an atmoſphere ſurrounding the earth and waters. The eruptions of the fires naturally diſordered the depoſitions of the rocky ſtrata, and puſhed up many vertical or inclined elevations through the layers which might otherwiſe have been, in general, nearly horizontal. Such was the origin of the loftieſt primitive mountains, the Alps, the Andes, and thoſe elevated ranges which occupy ſo extenſive a ſpace in the north of Aſia. Plains were formed by the depoſition of ſtony particles from the chaotic flood in the intervals between the mountains, and with a diffuſion too looſe to admit of their being united in cryſtals as they were depoſited. The volcanic eruptions, by which the firſt mountains were elevated, could not but ſcoop out excavations to receive the ſubſiding waters of the ſea. The caverns of the primitive nucleus of the globe were even laid open by thoſe fires; and, into them, the ſuperficial waters ſhrunk, till a conſiderable extent of dry and ſolid ſurface was left bare under the atmoſphere. At a level of between 8,500 and 9000 feet above that at which it now ſtands, the ocean became for a while ſtationary. It was ſoon after furniſhed with fiſhes. The diſintegration of the primary rocks, the depoſition of animal exuviæ, and the continued precipitation of the ſolid matters ſuſpended in the waters of the ſea, then compoſed thoſe ſtrata of rocks which, from their containing animal remains, are denominated *ſecondary*. The earth was about the ſame time clothed with vegetation, and made the

reſidence

residence of the species of animals. The matter of the secondary rocks was, by the known laws of gravitation and resistance, rather fixed in strata upon the primary rocks than precipitated to the bottom of the great abyss. The retreat of the sea from the surface to the interior caverns of the globe, however, still continued; and the dimensions of the habitable earth were thus continually enlarged.—Such is the substance of the first of these Essays, which contains the author's theory of the formation of the earth. He compares this account with that of Moses, and fancies that they perfectly coincide.

The Deluge is the subject of the second Essay. From the shells found on the heights of the primitive mountains; from the remains of animals natives of hot climates discovered in cold ones, which those animals cannot be supposed to have ever voluntarily inhabited; from the dispersion of marine exuviæ on shores far remote from the seas where we now meet with the species of living animals to which they must have belonged; Mr. Kirwan infers, that the general deluge commemorated in the Holy Scriptures must certainly have taken place as Moses relates. That the axis of the earth was originally parallel to the ecliptic, producing in every latitude perpetual spring, is a position of which astronomy has sufficiently demonstrated the falsehood. The nutation of the poles is, every nine years, retrogressive, and never exceeds ten degrees; so that the equator can never have been, by this cause, where the poles are now. The whole strata of this earth are of such nature that they cannot have originally been, as M. De Buffon supposes, in a state of igneous fusion, the heat from which might have long kept up a genial warm temperature even in the regions the nearest to the poles. The phenomena sufficiently evince that the seas and continents of the earth cannot have mutually changed places, as has been supposed by Mr. Edward King and M. De Luc. Mr. Kirwan, therefore, supposes the southern ocean to have been in the general deluge driven, by some unknown means, with mighty force towards the poles. The character of the exotic remains found in the northern regions; the immensity of waters collected in the southern ocean; the traces which

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ſo many countries bear of having ſuffered ſome mighty ſhock from the ſouth; and the very figures of the great continents, ſharpened towards the ſouth, where they are waſhed by the ſouthern ocean, are ſo many grand facts concurring to give probability to that ſuppoſition. The waters from the central abyſſes of the globe might burſt forth to complete the cataſtrophe. In their progreſs northward, and in their retroceſſion, the waters of the deluge muſt have greatly broken, ravaged and tranſpoſed the ſuperficial ſtrata of the earth: many of the mountains were broken down to powder, and other inconſiderable fragments: the beds of the Euxine and Caſpian ſeas might be then ſcooped out: baſaltic rocks and beds of coal might be formed: a confuſion in the arrangement of the exterior ſtrata of ſecondary and primary rocks would be here and there produced: all terreſtrial animals, ſave ſuch as were preſerved by ſupernatural means, muſt have neceſſarily periſhed. Every rational inference from preſent appearances entirely coincides with the Moſaic hiſtory of the deluge.—This is the ſubſtance of the ſecond of theſe Eſſays.

In the third Eſſay it is ſtated, that, beſide effecting ſo many important and immediate changes on the exterior ſtrata of the globe, the deluge, likewiſe, became the remote cauſe of various great convulſions, which were at ſubſequent times to enſue upon its parts at different places of the ſurface: that the total ſeparation of Aſia from America, the coarctation of the Baltic, the opening of the Thracian Boſphorus, and the diſruption of the iſthmus which once divided the Atlantic Ocean from the Mediterranean Sea, the entire ſeparation of Great Britain from the continent, and various other ſimilar alterations are to be numbered among thoſe great changes poſterior to the deluge, for which the deluge had, however, prepared the ſuperficial ſtrata of the globe: that all theſe changes muſt have been accompliſhed chiefly by extraordinary agitations of the waters of the ocean, and at the diſtance of at leaſt three thouſand ſix hundred years from the preſent time.

The fourth Eſſay explains the phenomena of *lapidification.* Stones conſiſt of earths conglomerated. A lump of any earth of which the parts, though cohering, are diviſible merely by the

the nail of the finger, is called but *indurated earth:* if its cohesion firmly resist the action of the nail, it is to be denominated a *stone*. Stones are hard which cannot be easily filed, or cut into pieces. Those are firm which strongly resist percussion. Some stones appear to have been formed by crystallisation arranging all their parts, internal and external, in a regular form. It is effected from either aqueous or igneous solution. All solidifiable matters are susceptible of it. Even those siliceous matters, which are now almost insoluble in water, might be easily soluble in this menstruum in their original state of extreme division, and were probably crystallised by deposition from water. Nay, in spite of whatever Black and others may have affirmed to the contrary, numerous facts may be produced to show that, even still, water may be made to take up a certain proportion of this earth. Some stones appear to have been formed by the mere concretion of particles which in their primary composition are probably of different figures, and which therefore display not in their cohesion the uniform order of crystallisation. Other stones are formed by the cementation of exceedingly small nodules, or even of larger ones, by the deposition of still particles amongst them, and the cohesion of whole masses, partly by mechanical force, and in part by the reciprocal chemical attractions of the different parts. Many of the smaller nodules, which are found imbedded in rocks not of the same nature with themselves, have been formed by an infiltration, in which water or some other fluid carried their particles through the superficial strata, but deposited them after penetrating the inferior layers only to a certain depth. Those stones which we call petrefactions have been formed by the gradual substitution of stony particles into the places which were occupied by animal or vegetable matters that decay while the lapideous deposition takes place. These are all the different modes of lapidification. They are exemplified in all the different fragments, columns, and strata of rock which the earth presents to the observation of men.

In the fifth Essay, the *disintegration*, reducing them into sands and larger fragments is explained, as having been often produced by the absorption of water, and by its congelation

 or

or rarefaction while it was diffused within them. The known external causes of the entire decomposition of stony matters are represented to be only water, carbon, and carbonic acid gas, which enter into new combinations with the saline matters, the sulphur, the ferruginous oxyds, the manganese, the lime, and other constituent parts of the stones with which they come into contact.

Mountains are the subjects of the sixth of these Essays. The author describes the *primitive* mountains as containing within them no animal nor vegetable remains; as composing all the loftiest extremities of the earth; as never covering nor leaning upon those which are called *secondary*; as existing sometimes in strata, but oftener in huge rocks; as variously composed, of granite and granitic compounds, gneiss, schistose mica, siliceous schistus, basanite, jasper, pitchstone, hornblende, hornblende slate, indurated lithomarga, argillite, trap, serpentine and potstone, porphyry, hornslate, schistose porphyry, sandstone, rubble-stone, sarcilite, granular limestone, marlite, gypsum, fluor, topaz-rock. Of each of these sorts of stone he gives, in part, the mineralogical geography. The *secondary* mountains are next described by him, in the same essay, as being either marigenous or alluvial; as containing in their structure various remains of animals and vegetables, terrestrial and aquatic; as always incumbent on primary rocks, or investing them; as stratified or columnar, unigenous or polygenous; as variously composed of limestone, swinestone, oviform limestone, porous limestone, marlites, and calcareous sandstones, chalk, gypsum, argillite, indurated clay, shale, bituminous shale, indurated lithomarga, slate, argillaceous sandstone, porphyry, trap, and basalt; hornblende, argillaceous and calcareous breccias, hornstone, jasper, siliceous breccias and siliceous sandstone. By an ample induction of facts it is proved, that all these different sorts of stone have been found in circumstances in which they could not be but of secondary formation. This is the substance of that which seems to form, properly, the sixth Essay, though the author has not so distinguished it.

In the seventh Essay he describes volcanic mountains as being generally of a conical form, of no exceedingly ancient origin, burning with no extraordinary intensity of internal heat, and not half so numerous as some wildly theoretic mineralogists have supposed them to be.

In that which we should incline to consider as the eighth Essay, Mr. Kirwan endeavours to explain the internal arrangement of mountains. He here relates that their materials are disposed either in irregular heaps or in beds or strata, horizontal, varying from that position by an angle, of from 5° to 40°, or sometimes even almost vertical. Sometimes the strata run in the direction of the declivity of the mountains they compose, and sometimes their course is directly opposite to this. Mountains of primitive limestone are often in irregular piles, but often, too, horizontally stratified. Of secondary calcareous strata, the structure is commonly horizontal. The strata of secondary mountains, for the most part, ascend towards the primary.—These are some of the leading truths unfolded in this Essay.

Coal-mines are the subjects of the ninth Essay. Pit-coal is a compound of bitumen with carbon: its specific gravity is from 1,23 to 1,500. The soils within which beds of coal usually occur, are argillaceous, arenilitic, trappose, or calcareous. These are disposed in strata over the coal, or in alternation with its strata. The thickness of the strata, or seams of coal, varies from half an inch even to 80 feet. Shale, bituminous shale, or sandstone, are the stones which generally form both the *roof* and *floor* to *seams* of pit-coal. Sometimes these seams are in their disposition horizontal. They rise at times to angles of from 25° to 45° with the horizon. It cannot be true that, as has been supposed by Gensanne, pit-coal is nothing but argillaceous matter impregnated with petrol or asphalt; nor that this coal is entirely of vegetable origin; nor that it is formed from the fat and unctuosity of the animals of the ocean. The strata of this mineral seem rather to have been formed in consequence of the disintegration of many of the primitive mountains by the homogeneous union of the petrol and carbon which were by that disintegration liberated out of the composition into which they had originally entered. Coal is therefore to be sought, with

with probability of ſucceſs, only on the ſides of primitive mountains, in the vicinity of mountains of argillaceous porphyry, amidſt ſandſtone and ſhale bearing the impreſſions of vegetables, under alternating ſtrata of limeſtone and ſandſtone, or amidſt ſtrata of trap, or whin with ſandſtone or clay.

Common Salt and its Mines are the ſubjects of the tenth Eſſay. The ſea is here repreſented as owing its ſaltneſs to ſimple ſaline ſubſtances held in ſolution in it ever ſince the creation. The mines of rock-ſalt which have been in different places diſcovered, are ſaid to have all originated from the detention and evaporation of ſea-water. Salt-ſprings and lakes are related to contain and pour forth, from time to time, quantities of ſaline matter, which, when ſubjected to calculation, cannot but, by their magnitude, excite the greateſt aſtoniſhment. Theſe general facts are unfolded amid an exceedingly curious and intereſting diſplay of particulars.

The eleventh Eſſay is on the ſubject of Metallic Mines. It relates, that all the metals muſt have exiſted perfect and uncontaminated in the chaotic flood. Gold is found naturally pure oftener than any of the other metals. Silver is much more frequently mineraliſed by ſulphur, and alloyed with copper, antimony, lead, mercury, tin, or biſmuth. Nature preſents ſtill leſs of pure copper than of pure ſilver: the ores of copper are ſcarcely ever free from ſulphur. Of pure native iron there are few or no ſpecimens, except certain maſſes found in Siberia and Peru, which ſeem to have been originally agglutinated by petrol, and left bare by the diſſolution of the maſſes of other matters which once ſurrounded them. Native biſmuth accompanies the gray and the bright white ores of cobalt. Native arſenic is found only in the veins of primeval mountains: native mercury occurs in clay in mountains of ſecondary formation: ſulphurated ores have been formed at a time when ſulphur as well as metals were in a ſtate of extreme ſubdiviſion in which they were much more ſoluble in fluids than they are at preſent. The cryſtalliſation of metallic ores was the conſequence, in ſome inſtances, of the diminution of the watery menſtruum, in others, of ſlow precipitation. The veins of metals ſeem to have been, for the moſt part, formed by depoſition from water among the rifts and amid the ſtrata of the ſofter rocks. It is in oxyds, in mineraliſation by ſulphur, in mixture with different earths, and in amalgamation with one another, that all the metals are the moſt commonly found.—Such are the moſt general facts ſtated in this Eſſay.

The twelfth Eſſay is wholly controverſial. The late Dr. Hutton, of Edinburgh, had taught, that, from eternity, this globe had continued to paſs through a ſeries of revolutions, in which the land was ſtill deſtroyed by external influences, and, in its ruins, propelled into the ocean; while, on the other hand, internal fires were conſtantly elevating new mountains under the ocean, and conglomerating the materials driven into it; till, at laſt, the ocean changed its bed, that which had been dry land became ſea, that which had been ſea became dry land, and the deſtruction of the new continents, and the working of the volcanic fires under the new ſeas were again renewed. Mr. Kirwan ſoon attacked this theory as atheiſtical and unphiloſophical. Dr. Hutton warmly defended his own ſyſtem. In this concluding eſſay, as throughout the preceding parts of the preſent work, Mr. Kirwan, in reply to Dr. Hutton's defence, demonſtrates, that igneous fuſion cannot have produced thoſe phenomena which the Doctor aſcribes to it; affirms that volcanic fires are comparatively few and weak in their action; and maintains, that the compoſition of the ſuperficial ſtrata of our preſent continents is ſuch as could not poſſibly be formed at the bottom of the ſea.

The notes are few, and only mention ſome facts of which the author was, perhaps, not aware till it was too late to inſert them in the text of the Eſſays.

This analyſis has enumerated the leading general facts and poſitions of the preſent work. But theſe are founded upon a wonderfully extenſive induction of highly curious particular facts, for which the reader will do well to examine the book itſelf.

Theſe Eſſays certainly exhibit a more conſiſtent ſcheme of the facts and analogies of geology than any that had been before

before presented to the world. They are written in a style eminently correct, manly, and elegant. If subsequent geologists shall continue to improve their science as Mr. Kirwan has here done, the world may, perhaps, at some distant future time, see a faultless theory of the earth.

Phytologia; or an Essay on the Principles of Vegetation, &c. By ERASMUS DARWIN, *M. D. 4to.* Johnson, Saint Paul's Church-Yard.

The Botanic Garden and Zoonomia, by Dr. Darwin, are two noble monuments of fancy, industry, and science. In one we have almost the first instance of the happy application of poetry to illustrate and embellish the physical branches of philosophy. The other is, to say the least of it, a vast collection of the most curiously interesting facts, physiological and pathognomical, in the history of human life, as well as of many of the most ingeniously conceived theories which have been proposed to combine those facts into systems, the foundations of practical rules.

In his Phytologia, the same author, with a flow of fancy and sentiment nearly similar to that in his former works, in the same ornate, yet clear, easy, and natural style, in the same characteristic method of assembling facts, deducing inferences, and suggesting analogies, applies himself to explain the physiology of vegetables, to trace the chemical relations between vegetating bodies and the surrounding substances from which they are nourished, and then to deduce a system of rules for the improvement of the art of the gardener and the practice of the husbandman.

The readers of the Botanic Garden may perhaps have supposed, that in the tale of the Loves of the Plants the author intended only to dignify and animate his account of the theory of Linnæus by a happy and beautiful poetic fiction. But they have mistaken his purpose, and his botanical creed, who have thus conceived of them. He was never in his life more in earnest, than in ascribing to vegetables the sensations, the affections, the passions, and even the designs of which we, ordinary mortals, believe animals alone to be capable. In fact, it is one of his favourite opinions, that

plants

plants are truly animals. This opinion he ſtrenuouſly labours, throughout the phyſiological part of this volume, to illuſtrate and to prove. The ſeeds of plants he eaſily ſhows to reſemble the ova of animals. He diſcovers in the ſtructure of vegetables umbilical veſſels perfectly ſimilar to thoſe by which embryon and naſcent animals derive nouriſhment from the parent body, or from that ſtore with which the wiſdom of Nature involves them in the egg. He traces in the vegetable ſtructure, ſyſtems of abſorbents, glands, and blood-veſſels. He regards the leaves as lungs. He diſcovers the organs and the exerciſe, in theſe beings, of the moſt exquiſite ſenſibility. His facts are partly aſſembled from the works of almoſt all former writers on the phyſiology of vegetables, and, in part, the reſults of experiments ingeniouſly contrived and ſkilfully performed by himſelf. He ſuggeſts many pleaſing and ſpecious analogies, and here and there exhibits ſome clear and accurate deductions of new philoſophical truths. If we muſt, with whatever reluctance, own that he has not preſented to our minds ſufficient evidence of that full and perfect ſimilarity which he ſuppoſes between the ſtructure and functions of vegetables and thoſe of animals; it is, however, at the ſame time certain, that he has opened ſeveral fine veins of phyſiological reſearch, and that ſome of his novel poſitions, though not by himſelf ſatisfactorily proved, are ſuch that they may poſſibly receive complete proof hereafter.

In the ſecond part of his work he examines the chemical relations between the organiſed matter of vegetables, and the nutriment from which it is, by the vegetative functions, elaborated. He follows, with reſpect, the authorities of Prieſtley, Ingenhouz, Kirwan, and other eminent writers upon the application of chemiſtry to the ſubjects of gardening and agriculture. He truſts, however, as we ſhould judge, much too implicitly to the facts of Young, Marſhall, and other authors, whoſe publications on agricultural ſubjects, however popular, and however, in ſome reſpects, uſeful, contain few or no facts of ſuch preciſion and authenticity that a philoſopher can prudently take them for data without firſt verifying them by his own obſervations and experi-

 ments,

ments. Dr. Darwin is yet more fuccefsful in explaining the chemical influences of foils, airs, water, temperature, and light, on the growth of plants, than in the eftablifhment of his new theory of their phyfiology. But in this chemical part of his work, he is content to repeat the principles and opinions of others without prefuming to propofe any important new theory of his own.

The rules for the practice of the gardener and hufbandman, which he deduces as the refults from both his chemical and his phyfiological refearches, are partly difperfed through the previous divifions of the work, and partly affembled together towards the clofe. They befpeak ingenuity and good fenfe, but do not fufficiently wear the air of a practical acquaintance with farming, combined with accurate fcience.

Throughout the work are fcattered fome curious difquifitions on fubjects allied, indeed, to the primary defign, but not entering directly and unavoidably into it. The whole will be read with great pleafure by the admirers of Darwin's former writings.

Elements of Chemiftry, &c. By ROBERT HERON. Longman and Rees, Paternofter-Row. 8vo. Twelve Shillings.

This work explains, in nine books, 1. The general nature and hiftory of chemical fcience: 2. The nature, names, and peculiar ufes of the implements for chemical experiments: 3. The chemical properties and relations of thofe fimple and abundant chemical fubftances, caloric, oxygen, hydrogen, and azot: 4. The hiftory of all the acidifiable bafes except the metallic, and of their acid compounds: 5. The hiftory of the earths and alkalies, and of their neutral compounds with acids: 6. The nature and relations of metallic fubftances: 7. The hiftory of bitumens and fome other fubftances too complex and anomalous in their relations to be conveniently introduced into any of the foregoing branches of the arrangement: 8. The chemical hiftory of the compofition of animal and vegetable fubftances: and, 9. The application of chemical fcience to the phenomena of geology, and to the functions of animal and vegetable life, &c. Several articles

of

of appendix are employed in illuſtrating ſome things which had eſcaped the author's previous attention, in ſtating ſome theories which he does not yet preſume to rank as ſcience, and in conteſting ſome of the doctrines of Count Rumford and others.

This work has been favourably received by the public. Nor can we deny that the arrangement is convenient; that all the known principles and facts of chemical ſcience are in this book clearly ſtated; that the author has ſuggeſted many new things *which the diſcoveries of the ableſt foreign chemiſts are daily confirming*; nor, that this book will be found, perhaps, more eaſily intelligible by the mere novice in chemiſtry, than any other compendium of the elements of this ſcience with which we are acquainted.

The following are ſome of the leading new principles contained in it:

That lime is oxygen in a concrete form, and that the fixed alkalies are peculiar modifications of the ſame ſubſtance:

That, contrary to the aſſumptions of Count Rumford, caloric is a condenſible or elaſtic ſubſtance; and that, when fire is elicited by friction, there is an expanſion of condenſed caloric into a ſtate of extraordinary tenuity:

That, when it has been ſaid that water is a non-conductor of caloric, there has been an error, ariſing from the miſtake of a mechanical for a chemical effect, and from the uſe of indiſtinctly expreſſive terms:

That, in every function of organic bodies, the vital energy and the mechanical action of the organs are accompanied with certain chemical changes; and that the ſtudy of theſe chemical changes can alone guide us to the knowledge of a true theory and a rational practice of medicine:

That the electric fluid is, demonſtratively, a compound of caloric and light:

That, in a true ſyſtem of geology, the origin of the earth is to be traced from the homogeneous expanſion of all other matter in caloric, &c. &c.

INTEL-

INTELLIGENCE,

AND

MISCELLANEOUS ARTICLES.

LEARNED SOCIETIES.

ROYAL IRISH ACADEMY, DUBLIN.

THE Royal Iriſh Academy have propoſed to give a premium of one hundred pounds ſterling to the author of the moſt detailed and accurate mineralogical and geological deſcription of the county of Dublin, with ſpecimens of the minerals found at an acceſſible depth.

The Academy alſo propoſes that a gold medal ſhall be given to the author of the beſt eſſay on the origin and progreſs of rhyme.

The Academy will alſo give a gold medal to the author of the beſt attempt to explain the Brehon laws of Ireland.

Eſſays on any of the ſubjects here propoſed will be received in either the Engliſh, French, Italian, or Latin language (if ſent poſt free) directed to the ſecretary, the Rev. Dr. Elrington, at their houſe, 107, Grafton-ſtreet, Dublin, any time previous to the 1ſt of November 1801: each eſſay to be inſcribed with ſome peculiar motto, and accompanied by a ſealed billet, ſuperſcribed with the ſame motto, in which ſhall be written the author's name and addreſs.

IMPERIAL ACADEMY OF THE SEARCHERS INTO NATURE AT ERLANGEN.

In the ſitting of the 23d of January laſt, the following new prize queſtion was propoſed:

To determine, in a ſatisfactory manner, by experiments, what mode of treatment, particularly in regard to temperature and the admiſſion of free air, is the beſt to be adopted in the ſmall-pox in general, and alſo in the inoculated ſmall-pox, and at the different periods of the diſeaſe? Whether diluting the matter with water before inoculation be attended with eſſential advantage; or whether too ſmall a number of puſtules be not by theſe means produced, and the body of the patient rendered liable to new infection? If it be true that rubbing olive-oil into the body be a certain preſervative againſt the plague, whether the ſame means might not be employed to prevent infection from the ſmall-pox? And what other kind of oil, when olive-oil is not to be had, might be ſubſtituted, with moſt advantage, in its ſtead?

The candidates are requeſted to tranſmit their papers, which the Society would prefer in Latin, but which may be written alſo in German or French, with any device at pleaſure, and accompanied with a ſealed note containing the name and reſidence of the author, to the preſident of the Academy at Erlangen before the 1ſt of September 1801. The prize, which is a gold medal weighing twenty ducats, will be adjudged to the author of the beſt paper on the above ſubject on the 5th of January 1802.

ROYAL ACADEMY OF SCIENCES AT BERLIN.

The following papers were read in the Academy in the end of the laſt, and beginning of the preſent, year:

September 19th, 1799, A ſecond diſſertation on the great advantage and different uſes of the maize plant, and on the bread prepared from it; by Profeſſor Bernoulli. 26th, On the origin of human knowledge, by Counſellor Selle.

October 3d, On a paſſage in the Meno of Plato, by Trembley. 10th, Experiments and obſervations made in the courſe of the year: 1ſt, on the uſe and cultivation of the white beet, and its abundance in ſaccharine particles; 2d, on the varieties of this root, which may be employed with moſt advantage in the preparation of ſugar. 17th, A journal of aſtronomical obſervations made at the obſervatory of Berlin ſince 1798, by Profeſſor Bode. 24th, Second diſſertation on the influence of ſigns in the formation of ideas, by Von Caſtillon. 31ſt, Grammatico-critical obſervations on Montaigne, by Baſtide.

November 7th, Obſervations on certain phenomena in regard to the motion of rivers and ſtreams, by Trembley. 14th, On the length of a pendulum to ſwing ſeconds at Berlin, by Profeſſor Burja. 21ſt, Third diſſertation on the impenetrability of light, by Profeſſor Engel. 28th, Second diſſertation on the art of clothing mankind, and on clothing conſidered under different points of view; by Von Goyon.

December 5th, On eatable muſhrooms, by Mayer. 12th, Obſervations on the attractive force of ſpheroids, by Trembley. 19th, On the principles of metaphyſics, by Ancillon.

January 9th, 1800. Second diſſertation on the prize queſtions of the claſs of the ſciences ſince the year 1772, by Baron Von Chambrier. 16th, Phyſical obſervations on the arts of deſign, by Von Burgſdorf. 23d, Obſervations on the calculations reſpecting the duration of marriage, and the number of married perſons; by Trembley. 30th, This being a public ſitting, Merian the director, as perpetual ſecretary, delivered an oration in French. The following papers, alſo, were read: Conſiderations on the principal definitions in the ſciences, by Ancillon: Hiſtorical panegyric on queen Sophia Charlotte of Pruſſia, by Erman: On yeſterday, to-day, and to-morrow, by Baſtide: Further obſervations on dreſs, by Von Goyon.

February 6th, On the proper meaning of the term *a great man*, by Teller. 13th, On the uſe of perukes among the ancients and in the middle ages to the preſent period, and on the origin of the word *peruke*; by Nicolai. 20th, Examination of the red water of a lake near Lubotin in South Pruſſia, by Profeſſor Klaproth. 27th, On different kinds of maize, by Profeſſor Bernoulli.

March 6th, On ſpace, by Baron Von Chambrier. 13th, On the origin of the names of nations, countries, rivers, cities, and families; by the Abbé Denina. 20th, On eatable muſhrooms, ſecond diſſertation. 27th, Profeſſor Bode preſented the fourth number of his Celeſtial Atlas.

April 3d, Diſſertation on free-will, by Von Boufflers. 24th, On the Circenſian games, by Mejerotto.

May 1ſt, Deſcription of a new Eaſt Indian plant called *Piſonia Georgiana*, by Profeſſor Wildenow. 8th, On ſome new mathematical diſcoveries, by Profeſſor Burja.

ELECTORAL ACADEMY OF THE USEFUL SCIENCES AT ERFURT.

In the fitting of January 3d, Count Von Beuft read an extract from a treatife, written by his fon Frederick Auguftus Leopold, Count Von Beuft, of the Academy of Mines at Freyberg, on the amalgamation of filver ore as practifed at the Halfbruck and Ultermuld works near that place. From this paper and the annexed tables, it appears that 79785$\frac{3}{8}$ cwt. of ore, which, according to the common mode of amalgamation, would have given only 31139 pounds 4$\frac{1}{4}$ ounces of filver, produced, by the method employed at Freyberg, 38330 pounds 2$\frac{5}{8}$ ounces; confequently, there was a faving of 1190 pounds 7$\frac{3}{8}$ ounces.

In this treatife the author obferves, that the theory of amalgamation is much indebted to the important experiments of the Spaniard Don Ethovar, who proved, contrary to the opinion of feveral eminent chemifts, that the muriatic acid, in a concentrated ftate, diffolves gold as well as filver when thefe metals are reduced to fmall particles.

Profeffor Trommfdorf read a continuation of his collections towards a chemical knowledge of mineral bodies. This part related chiefly to fome precious ftones from India which he had examined.

ROYAL ACADEMY OF INSCRIPTIONS, THE FINE ARTS, HISTORY, AND ANTIQUITIES, AT STOCKHOLM.

The Academy, in the fitting of April 9, propofed the following prize fubjects for the year 1800.

Hiftory.—A hiftorico-critical differtation on the printed or unprinted works which have been written by individuals of the royal family of Sweden. The prize is a gold medal of the value of twenty-fix ducats.

Foreign Languages.—What are the advantages or difadvantages in regard to the general happinefs of mankind of the prefent as compared with the laft century? The prize is a gold medal of the fame value.

Antiquities.—Examination of the origin, nature, and object of of the military and naval expeditions undertaken by the Swedes about the middle of the twelfth century. The prize is a gold medal of the value of fifteen ducats.

Inſcriptions and Devices.—1. A Latin inſcription for the Exchange at Stockholm. 2. Plan of a medal to commemorate the moſt remarkable events which have taken place in Sweden in the courſe of the preſent century. The prize is a gold medal of the value of twelve ducats.

The papers, &c. of the competitors muſt be tranſmitted to the Academy, in the uſual form, before the 20th of January 1801.

FRENCH BOARD OF LONGITUDE.

The following paper has lately been publiſhed, ſigned by Delambre the preſident, and Lalande the ſecretary, of the French Board of Longitude:

The tables of the moon are of equal importance to aſtronomy and to navigation: the moſt celebrated geometricians have exerted themſelves to illuſtrate the theory on which theſe tables are founded. The moſt aſſiduous labour of aſtronomy is, to obſerve with care all the movements of this planet, without which there would be no real geography, and which furniſh the navigator with the moſt infallible means of aſcertaining the poſition of his veſſel, of directing its courſe, and of arriving in ſafety at any determined point of the globe. In proportion as the Newtonian theory was thoroughly examined and underſtood, and as aſtronomical inſtruments and modes of obſervation were improved, the lunar tables were improved alſo. Mayer, uniting his own obſervations to thoſe of cotemporary geometricians, and ſelecting the moſt accurate obſervations, was enabled to compoſe tables, which having been ſince compared with nearly twelve hundred unpubliſhed obſervations, have been found wonderfully accurate. Maron, under the direction of Dr. Maſkelyn, made a new improvement in them by adding ſeveral equations which had been omitted but pointed out by Mayer, and by modifying the co-efficients of all the reſt.

But, notwithſtanding this care, theſe tables, ſo valuable towards the middle of the century, began to loſe progreſſively fively their correctnefs. Theory, to which recourfe was again had, fhowed the caufe of the error, and the method by which it might be rectified. The papers tranfmitted as anfwers to the queftions propofed by the National Inftitute two years ago, and proclaimed in the public fitting of the 15th of Germinal laft, (April 5th,) have thrown great light on the equations lately invented for the motion of the apogeum and node. But aftronomers at that time had not been invited to turn their attention to all the elements of which the lunar tables are compofed: this labour would have been too much difproportioned to the time propofed for the competition. One fuccefs often creates a defire, and fometimes furnifhes the means of obtaining another. What has been fo happily executed already, proves the poffibility of doing ftill more, and of procuring to aftronomy lunar tables more correct and more lafting. Nothing remains after eftablifhing the epochs, the fecular motion, and their inequalities, but again to difcufs, by comparifon with a great number of accurate obfervations, the precife quantity of the different equations which enter into the calculation of the moon's place. This is the queftion which the French Board of Longitude now propofes to the aftronomers of every country. The conditions to be fulfilled are:

1ft, To difcufs and eftablifh, by comparifon with a great number of good obfervations, the value of the co-efficients of the inequalities of the moon, and to give for the longitude, latitude, and parallax of that planet, formulæ ftill more correct and complete than thofe which have ferved as the foundation of the tables now in ufe.

2d, To conftruct from thefe formulæ tables of fufficient extent and accuracy for calculation.

The prize will be fix thoufand francs (250*l.*) The Board of Longitude does not fix any period for the competition, and will adjudge the prize to the firft perfon who fhall have fulfilled the above conditions.

The competitors will addrefs their papers to the *Bureau des Longitudes, Palais National des Sciences et Arts à Paris.* They muft not be infcribed with the name of the author, but only with a fentence or motto, and may be accompanied with

with a ſealed note, containing, beſides the motto, the name and addreſs of the author. This note will not be opened but in caſe the paper ſhall have been found entitled to the prize. The prize will be paid, without any formality, to the bearer of an order for that purpoſe, which will be delivered by the ſecretary.

FRENCH NATIONAL INSTITUTE.

The following is an account of the labours of the Claſs of the Mathematical and Phyſical Sciences during the third quarter of the year 8, as read by J. B. S. Delambre, ſecretary:

Among the memoirs of which we have to give an account, there are ſome, and theſe form the greater number, that cannot be appreciated but by thoſe who have particularly applied to the objects on which they treat. As they would require all the attention of the moſt experienced mathematicians, they are not very ſuſceptible of abridgement. Such, in general, are all the memoirs on pure analyſis and the higher parts of geometry. We muſt be excuſed, therefore, if we do not take ſo much notice of theſe works as their importance would require. We ſhall content ourſelves with announcing only, by their titles, a memoir of C. Niewport on the immediate integrability of differential equations to any number of variable ones; and two memoirs of Biot containing analytical reſearches, one on partial differential equations, and the other on vibrating ſurfaces.

We ſhall pay a little more attention to a memoir on mechanics, by C. Coulomb, which contains, beſides the analytical part, a ſeries of experiments, of which we can at leaſt give the reſults. The ſubject is, the reſiſtance of fluids when the motion is very ſlow. If a body impels on a fluid, or when it is impelled by the latter with a very ſmall velocity, ſuch, for example, as 0·3 yard, *per* ſecond, it will be found by experiment that the reſiſtance is in proportion to the ſquare of the velocity: but where the motion is exceedingly ſmall, for example, below 0·01 yard *per* ſecond, the reſiſtance is not merely proportioned to the ſquare, but to a complex function of the velocity. Suppoſing the velocity very ſmall, the quantity which repreſents the reſiſtance will alſo be be very little. It becomes very difficult to eſtimate it by the uſual means, and ſtill more difficult to diſcover what belongs to the different terms of the formula. To accompliſh theſe points it would be neceſſary to have a kind of meaſure which might determine, with ſufficient accuracy, the ſmalleſt forces, and afford the means of varying the velocity at pleaſure, in order to render the different terms of the formula prevalent in turns.

All theſe advantages C. Coulomb has found united in the machine with which he meaſures the torſion of a wire ſuſpended in a vertical poſition. He has already derived great advantages from this inſtrument in other branches of phyſics, and he has given in former memoirs theorems reſpecting the force of torſion, or the means of eſtimating it in weight. With this aid he has demonſtrated the real law of reſiſtance where the motion is ſlow; a law which had eſcaped Newton, Bernoulli, and Graveſend. It reſults from his experiments:

1ſt, That where the motion is very ſlow, the reſiſtance is proportional merely to the velocity, and the term depending on the ſquare becomes inſenſible.

2d, That the height of the fluid above the body immerſed in it, does not ſenſibly increaſe the reſiſtance.

3d, That the reſiſtance ariſes only from the coherence of the parts of the fluid with each other, and not from their adheſion to the body immerſed in them.

4th, That the reſiſtance in limpid oil is ſeventeen times and a half greater than in water.

We ſhall omit ſeveral other reſults which will be read with pleaſure in the memoir itſelf, and only add, that the agreement between the different trials is ſo great as muſt aſtoniſh thoſe not acquainted with the accuracy and preciſion of the inſtrument with which C. Coulomb has enriched natural philoſophy.

C. Prony preſented to the Claſs the three firſt parts of a work entitled Mechanical Philoſophy, or, An Analyſis of the different Parts of Equilibrium and Motion.

Since the invention of the new calculus, the province of mechanics has been conſiderably enlarged. A great number of of queſtions beyond the reach of the ancient geometry have been reſolved, and form at preſent ſo vaſt a body of ſcience that the mind, occupied with its developements and demonſtrations, can ſcarcely comprehend the whole. This inſpired C. Prony with the idea of compoſing a methodical table of all the reſults diſengaged from intermediary calculations. This is what he calls *mechanical philoſophy*. He compoſed this work from the leſſons and materials he collected for the Polytechnic School; and his object is to furniſh ſtudents with the means of placing together and arranging the different parts of inſtruction they have received. The author quotes the ſources from which he derived his information, but his work contains a great many things which belong to himſelf, either as to the matter, or manner in which it is preſented; ſuch, for example, as every thing that concerns equilibrium; a method of obtaining the fundamental equations of ſtatics without employing in any manner the theory of momenta; a general demonſtration which ſhows that the theorems reſpecting momenta are only a peculiar enunciation of the principle of virtual velocity; new formulæ for the equilibrium and preſſure of elaſtic fluids, taking into conſideration the variation of dilatibility, and formulæ which may give to the barometer more generality and more certainty in practice than has hitherto been the caſe in determining the height of mountains. The author alſo makes a very important application of his principles to the calculation of the force with which earth preſſes againſt walls. The formulæ of Prony are entirely new, and exceedingly ſimple.

In conſequence of two papers on the motion of the moon, read in the laſt public ſitting of the Inſtitute, and which were deemed worthy of a prize, Laplace has made a new and very happy application of the theories publiſhed in his *Mecanique Celeſte*. M. Burg, one of the two authors who gained the prize, had remarked a periodical equality in the moon's motion in latitude, and had found that the duration of this period was eighteen years. He, however, aſſigned no cauſe for it, nor gave any law. On the contrary, he requeſted both theſe from the author, who has explained in ſo complete a manner the ſecular inequalities of the moon. His expectation has not been deceived. Laplace, in confequence of M. Burg's defire, has examined the queftion, and found that there exifts in the lunar orbit a motion of nutation analogous to that of the terreftrial equator. The extent of this nutation depends on the oblate form of the earth, and may throw new light on that important element, fince the inequality thence refulting varies from one to two, according as the flatnefs is fuppofed to be $\frac{1}{334}$ or $\frac{1}{230}$.

Another equation, depending on the fame theory, affects the moon's longitude, and is of equal importance in enabling us to afcertain the quantity of the earth's flatnefs. As long as the real caufe was unknown, great uncertainty prevailed in regard to the ufe which ought to be made of this equation, and aftronomers omitted it in their calculations. The exiftence of it was proved by obfervations, but it was indicated by no theory. Laplace himfelf, having calculated by the principle of univerfal gravity, carried it only to 2″, that is to fay, to a third of its real value. But fince he has taken into account the nutation of the lunar orbit, he has found that it indicates the flatnefs to be nearly what it is known to be from other phenomena.

To afcertain it ftill better, he made C. Bouvard felect from his paper which gained the prize three months ago, thofe obfervations beft fitted for deciding the queftion propofed by M. Burg. This refearch was attended with complete fuccefs, and 440 decifive obfervations have given 7″ 5 as the co-efficient of the new equation of latitude, which fuppofes a flatnefs of $\frac{1}{314}$. This is nearly what has been given by the variations in the length of the pendulum as obferved in different latitudes. The meafurement of degrees in France and Peru give ftill fomewhat lefs, and therefore it is demonftrated that the flatnefs $\frac{1}{230}$, as found by Newton, fuppofing the earth homogeneous, is by far too much. The uncertainty which may yet remain refpecting a quantity the caufe of fo much difpute, will in future be confined within very narrow limits: three phenomena, of a very different kind, lead us to the fame conclufion.

Thus the moon, which, by her eclipfes, made known to the ancient aftronomers that the figure of the earth is fenfibly round,

round, shows us at present, by continued observation of her motion, the small ellipticity of the terrestrial meridian. But we must observe that this ellipticity, which is below $\frac{1}{300}$, is too little to be perceptible to our senses even if we had an opportunity of seeing the whole shadow of the earth. We know, however, that we see only a very inconsiderable part of it disfigured by the penumbra and inequalities of the lunar disk; so that it would be impossible for the most accurate eye to discover, in so small an arc, the least difference between the circular curve and that of an ellipse so little flattened.

The obliquity of the ecliptic, which causes the different seasons and the difference in the length of the days, is one of the most interesting elements in theoretic and practical astronomy. Every year the astronomers examine it with care, by determining the solstitial altitude of the sun. Mechain and Delambre, the one at the national and the other at his private observatory, have employed in these nice observations the same instruments which they used in measuring the magnitude of the earth. Lalande the nephew, and Burckhardt, have made similar and simultaneous observations at the College de France with instruments of the like kind, but a little larger. These observations, whatever may be the cause, do not seem to be susceptible of the same precision as those which have given the measure of the meridian by means of the stars, but by taking the general result of more than 600 observations, made at Paris both this year and at the summer solstice of the year 7, there will scarcely be a second of uncertainty in regard to the real quantity of this essential element which enters into all our calculations.

Since we have had occasion to mention the measurement of the meridian, we shall here add, that the astronomers by whom it was performed are now employed in printing their observations, that they may submit, to the examination of the learned in all countries, the proofs of a labour which has already received the sanction of the deputies of different nations assembled at Paris last year.

We have received a letter from Ciscar and Pedrayes, the Spanish members of the commission of weights and measures, by which it appears that his Spanish majesty has received

ceived the standards of the metre and kilogramme sent to him by the Institute; that he has given orders for their being preserved with such care as may ensure their authenticity; and that he has charged Lenoir and Fortin to make, with the same accuracy, four other standards of the metre and kilogramme; and that this order has been successfully executed. On this occasion the king of Spain took a pleasure in making himself acquainted with the new metric system, and he declared that he should be glad to see it adopted throughout all Europe. To render it more palatable to the Spanish nation, and to facilitate the comprehension of it to every one, an elementary memoir has been drawn up on the subject, of which we have received some copies. It contains a complete explanation of the new measures, with a Spanish nomenclature, better adapted to the genius of that language, which M. Ciscar proposes to substitute for the Græco-Latin nomenclature adopted by the French republic.

The following account of the labours of the Class of the Moral and Political Sciences during the last quarter, was read by C. Levesque.

C. Anquetil continues to analyse the manuscript and printed memoirs presented to the Academy of Belles Lettres by candidates for the prizes proposed by that Society. He has read the analysis of a memoir on the state of the sciences in France from the death of king Robert to that of Philip the Fair.

G. Gaudin has transmitted to the Class researches on the legislation of Solon and the government of Athens.

C. Mintelle, in a memoir on the best method of writing geographical names, has shown the advantage of adopting the names of places such as they are used in the different countries to which they belong, and, when imperious necessity requires that they should be disfigured in the French language, of writing them twice, first as they are pronounced in French, and then as they are pronounced by the natives of the country.

C. Buache has read a memoir on New Holland, which, he thinks, was known more than a century before the period of the discovery of it by the Dutch. He supports his conjecture

conjecture by the account given by Peritsol, and an old chart of the Indian ocean preserved in the British Museum. However defective this chart may appear, he is of opinion that it may furnish information of great utility in regard to the discoveries that remain to be made in that part of the globe.

C. Veron Forbonnais has given an analysis of the principles of the circulation of provisions, and of the influence which specie has on that circulation.

C. Bouchaud, in historical researches respecting the edicts of the Roman provincial magistrates, has neglected nothing that could be suggested to him by a subject which belongs to the science of history as well as to that of legislation. Each province had its own government and laws, and the Roman republic, after having acquired an immense extent by its conquests, was never able to form the wise design of attaining to unity.

CONSTITUENT PRINCIPLES OF FIXED ALKALIES.

Guyton-Morveau, on the 26th of April, read, in the French National Institute, a memoir on the constituent parts of fixed alkalies. He made his experiments with Desormes, pupil of the Polytechnic School. Their conclusion was, 1st, That potash is composed of lime and hydrogen: 2d, That soda is composed of magnesia and hydrogen. From other experiments they conclude, 3d, That lime is composed of carbon, azot, and hydrogen: 4th, That magnesia is composed of lime and azot; and, consequently, of carbon, hydrogen, and azot.

NEW METHOD OF DESTROYING INSECTS.

Crell has lately published, in his Chemical Annals, the following recipe for destroying earth-fleas, bugs, ants, &c. discovered by C. Catin:—Take black soap, of the best kind, one pound three quarters, the same quantity of flowers of sulphur, mushrooms two pounds, and sixty measures of river or rain-water. Divide the water into two parts, one of which must be poured into a vessel destined for that purpose: suffer the

the soap to dissolve in it, and add the mushrooms after they have been a little pounded. Boil the other half of the water in a kettle, and tie up the sulphur in a bit of rag or piece of fine linen, and suspend from it a sufficient weight in order that it may sink in the water. During the time the water is kept boiling, which must be at least twenty minutes, stir it continually with a stick, and press the bag containing the sulphur, that the latter may be forced out into the water and communicate to it the necessary strength and colour.

When the liquor is taken from the fire, pour it directly into the cask, and stir it round for a considerable time: the process of stirring must be repeated daily till it acquire a fetid smell. Experience has shown that the more fetid the mixture is, its activity is the greater. Each time that the mixture is stirred, the cask must be stopped immediately after. When you wish to use the liquid, nothing is necessary but to sprinkle a little of it on the plants which you are desirous of preserving, or to dip their branches in it. It will be better, however, to make use of a syringe, having at the end a head, an inch or an inch and a half in diameter, pierced with small holes. This instrument may be used for tender plants; when you apply the liquid to trees, a syringe with larger holes must be employed.

Caterpillars, beetles, earth-fleas, bugs, and the tree-lice which infest orange-trees, will be destroyed by the first application of the liquid. Insects which reside below the earth, such as wasps, hornets, ants, &c. require that the liquid should be squirted out gently, and without intermission, that it may better penetrate to their nests. Ants-nests, according to their size, require from two to three measures of liquid, and in many cases it must be applied for twenty-four hours. When the ants assemble in another place, the process must be repeated. Two ounces of *nux vomica* may be added to the mixture and boiled along with the sulphur. This substance, particularly when you wish to destroy ants, will be of great service. When the whole of the liquid in the cask has been used, the residuum must be buried in the earth to prevent domestic animals from eating it.

CEMENT FOR PRESERVING WOOD AND BRICK, &c.

The compofition, or cement, is compofed of the following materials; *viz.* tar, pulverifed coal (charcoal is efteemed the beft), and fine, well flacked lime; the coal and lime to be well mixed together, proportioned at about four-fifths coal and one-fifth lime; the tar to be heated, and, while hot, thickened with the mixture of coal and lime, until it becomes fo hard as that it may be eafily fpread upon the furface of a board, and not run off when hot. Turpentine or pitch will anfwer nearly as well as tar, and plafter of Paris will anfwer inftead of lime; to be ufed in the fame manner, and in about the fame proportions. The cement muft be applied warm, and is found to be ufed eafieft with a trowel.

NEW REMEDIES FOR THE ITCH.

It has been obferved that the workmen employed in the manganefe mines at Macon, in France, are always free from the itch, and that thofe even attacked by that difeafe in the neighbourhood, are accuftomed to work in thefe mines in order to get rid of their malady. The itching, it is faid, in that cafe foon ceafes, the eruption dries up, and the fkin in a few days becomes perfectly clean.

C. Grille having remarked that the clothing of the labourers in thefe mines, and particularly articles of linen, became exceedingly white in the courfe of a little time, expofed feveral pieces of coloured cotton cloth to the air difengaged from the interior of thefe mines, and found, that in the courfe of a few days they had loft a great deal of their colour. This led him to conjecture that manganefe itfelf might be employed as a remedy for the itch. He therefore took fix parts of pulverifed manganefe and fixteen of hog's lard and formed them into an ointment, which he caufed feveral perfons troubled with the itch to rub over their bodies; he adminiftered alfo at the fame time the ufual internal medicines, and the patients in a few days were perfectly cured.

Parmentier, who has made known this obfervation of C. Grille, adds, that in the courfe of his refearches refpecting the means of obviating the unpleafant evaporation which proceeds from common fewers, he obferved that this evaporation

ration preserves those who clean them from certain diseases. These people assert that they never know any thing of the itch; that they are so secure from it, that they can sleep with itchy persons without any danger of infection; and that itchy persons who undertake their labour, get cured in the course of a few days. When these people scratch or cut their flesh, the wounds heal up in twenty-four hours; it is remarked also that they are never troubled with chilblains or tetters, and that they have always a sound smooth skin.

These observations of C. Grille seem to confirm what has been said in regard to employing vital air for extirpating the small-pox.

KLAPROTH.

The eminent merits of Klaproth as a chemical analyser, to repeat the expression of Kirwan, of the most consummate skill and accuracy, are acknowledged on all hands, and too well known to require encomium: it will therefore give our chemical readers great pleasure to hear, that a *translation of his Analytical Essays towards promoting the Chemical Knowledge of Fossils* is now in the press. We understand it is the performance of the translator of *Gren's Principles of Modern Chemistry systematically arranged*, of which we took notice in a former number.

DEATHS.

At Marburg, on the 12th of May, in the 47th year of his age, Leonard John Charles Justi, public professor of philosophy and professor of theology.

At Gottingen, on the 17th of the same month, in the 40th year of his age, of a fit of apoplexy, after suffering many years from hypochondriac affections, Christopher Girtanner, M.D. He was a native of Swisserland, and was the first chemist in Germany who adopted the new chemical doctrine of the French. In 1791, he published at Berlin a work entitled *Neue Chemische Nomenklatur für die Deutsche sprache*; The New Chemical Nomenclature in the German language; and the next year, The Elements of the Antiphlogistic Chemistry. He was the author also of several other works, and of a great many papers on chemistry and other subjects, printed in different journals.

THE

PHILOSOPHICAL MAGAZINE.

AUGUST 1800.

I. *Obſervations on the Straits of Malacca, in regard to Natural Hiſtory, Geography, and Commerce.* By C. HANSEL *.

THE iſland of Pulo Pinang, which lies cloſe to the coaſt of Queda, between the fifth and fixth degree of northern latitude, and which now belongs to the Engliſh Eaſt India company, may ſerve as a proof how well the Engliſh underſtand the art of eſtabliſhing colonies, and bringing them in a ſhort time to a flouriſhing condition. About the year 1784, the Malay king of Queda made a preſent of the whole iſland to Mr. James Light, the captain of a country ſhip trading thither, and the latter ſold it to the Eaſt India company, with the proviſo that he ſhould be appointed governor of it; which was accordingly done, and he remained in that office till the end of the year 1795. The iſland was uninhabited, and entirely covered with wood. In order to erect a few houſes it was neceſſary, therefore, to deſtroy part of the wood, which was effected chiefly by means of fire.

This iſland, which was now called Prince of Wales Iſland, I viſited, for the firſt time, in the year 1793, conſequently nine years after it had been taken poſſeſſion of by the Engliſh. At that period a beautiful town, of a pretty conſiderable extent, and regularly laid out, had been built. A particular part of it was deſtined for the Malays and other Aſiatics, a great number of whom had already eſtabliſhed

* From *Journal fur Fabrik, Manufur, Handlung, und Mode*, October 1799.

themſelves in the place. The market was well ſupplied with fruits, fiſh, and other neceſſaries. A conſiderable trade had alſo begun to be carried on. Ships from Bengal brought hither opium, rice, and cotton, which were bartered for pepper, tin, betel-nuts, and gold-duſt. All the company's ſhips touch here in their voyage to China, as well as the large fleets from Bombay and Madras. The arrival of ſo many veſſels makes this place a ſtaple, where buying, ſelling and bartering are continually going on. The harbour, which is formed by the coaſt of Queda and the eaſt coaſt of the iſland, is deep, and ſecure from every wind. Thoſe who are unacquainted with its geographical ſituation imagine they are entering a river; ſo ſhort is its diſtance from the continent. When viewed from the weſt ſide, the iſland appears to be round, and its mountains may be ſeen at a conſiderable diſtance. It is about thirty-five or thirty-ſix miles in circumference. The preſent inhabitants have applied chiefly to the cultivation of pepper, which is in a thriving condition, and promiſes to turn out very advantageous. As the iſland produces excellent timber for ſhip-building, docks have been formed, and a great many veſſels have already been launched.

A few miles from the town is a foreſt, which, riſing gradually, covers the mountains behind it. From the ſummit of the higheſt of theſe mountains a large ſtream of water projects itſelf more than a hundred feet, with a prodigious roaring noiſe, on the rocks below, where, after being collected in various natural baſons, it forms a ſmall rivulet, and then proceeds through the town, with a winding courſe, to the ſea. One can hardly conceive how pleaſant and refreſhing it is for thoſe relaxed by the heat of the climate to bathe in one of thoſe baſons, as the water is kept in a continual ſtate of coolneſs by the ſhady branches of the trees, which form over them a ſort of arches, and defend them from the rays of the ſun. The coolneſs of theſe groves, the majeſtic roar of the waterfall, and the noiſe of a variety of unknown inſects, make this an agreeable retreat to thoſe who have a taſte for the beauties of nature. In the year 1796, country-houſes were erected amidſt theſe fairy groves, which, in the

the course of time, may be converted into English gardens.

In the year 1789 the king of Queda took it in his head to demand back the island from the English; and, as the latter showed no inclination to comply with his desire, he assembled a considerable army on the coast opposite to the island, in order to lay siege to it in form. That he might inspire his troops with more courage he gave them an entertainment, at which opium was not spared. His Malayan majesty did not fail to get intoxicated along with his warriors; and this being reported to the English by means of their spies, they attacked the Malays in their camp, killed their king, made a great slaughter among them, and thus put an end to the war. A regular stone fort has since been built, in which a garrison is constantly maintained of three battalions of sepoys and Europeans.

At first, ships that touched here were obliged to sail back the same way they had entered; which was attended with this inconvenience, that those vessels destined to proceed further through the Straits of Malacca could not take advantage of a fair wind, because it blows directly into the harbour, and by these means five or six days were often lost. In the year 1791, however, captain Popham * discovered a passage round the south-east end of the island, by which this inconvenience is obviated.

To sail hence for Malacca there are two ways; one of which, for large ships, lies between two dangerous sandbanks, where a continual and rapid current, which runs ither north-west or south-east, requires all the attention of

* Captain Popham is the gentleman who commanded the expedition of the English to Ostend. As far as I remember, he commanded, in the year 1791, a ship named *Il Trusco*, which had been fitted out at Ostend by the English and sailed under the Tuscan flag. During the peace he obtained permission from the Board of Admiralty, being then a lieutenant in the navy, to enter into the merchant service; but as soon as the war broke out, he was again employed on board a ship of war.—H.

Captain Popham superintended last year the embarkation of the Russian troops destined for Holland, and assisted the operations of the British army in that country. He received the honour of knighthood from the emperor Paul, being made a knight of Malta.—EDIT.

the pilot to paſs in ſafety. Nature, however, ſeems as if deſirous to afford here every poſſible aſſiſtance, ſince the Aru iſlands on the weſt ſide, which conſiſt of ſome ſmall uninhabited rocks, ſerve the pilot as good marks to ſteer by. When theſe diſappear, the ſummit of the mountain Parcelar is diſcovered on the eaſt ſide, which muſt be kept in a certain direction, according to the current, in order to paſs without danger. As navigators between theſe ſand-banks have often to ſtruggle with bad weather and contrary winds, it is impoſſible to proceed but by continually heaving the lead, which is indeed attended with danger. The ſecond paſſage lies along the coaſt through the ſmall channel of Calum, which is ſcarcely ſo broad as the Elbe at Dreſden. Nothing more beautiful can be conceived than this paſſage, where veſſels ſometimes approach ſo near the coaſt that the bowſprit is often entangled among the branches of the trees. Birds of the moſt ſhining and variegated colours, unknown to the Europeans, are ſeen fluttering around, while others delight the ear with their ſong. As a ſand-bank in the middle ſhuts this paſſage againſt large ſhips, it is practicable only for thoſe which draw very little water.

When veſſels have reached the northern part, nothing can be more pleaſant than the navigation. They proceed along the coaſt, from which the wind, when it blows from the land, wafts along with it the moſt delicious aromatic odours. The mouths of the ſmall channels, the different windings of the coaſt, the bays which they form, and the land riſing like an amphitheatre, all together form the moſt enchanting proſpect. Nothing is wanting to complete the picture but habitations; for, in an extent of twenty or thirty miles, the only things of the kind that can be diſcovered, even with a ſpying-glaſs, are a few fiſhermen's huts.

At laſt, after weathering a headland, you ſuddenly diſcover the town of Malacca, formerly belonging to the Dutch. It lies at the head of a bay, and, though ſmall, is neat and well built. It is ſurrounded by beautiful gardens and country-ſeats. No place in the world produces more valuable fruits. The ananas here are half an ell in length and a quarter of an ell in diameter. They have an exceedingly ſweet melleous taſte. The mangoſtan, called in India the queen of fruits, is found here in great abundance, ſo that from two to three hundred of them may be purchaſed for a dollar. They grow only in the ſtraits of Malacca, Sunda, Banca, and in the iſland of Java. They are found neither at Madras nor Bengal, nor in the Philippines or China. The uſual fruits of India thrive here better than any where elſe. The bay abounds with well-taſted fiſh, and excellent oyſters, crabs, cray-fiſh, &c. Though this place lies nearly under the line, the climate is exceedingly healthful, and nothing is known here of thoſe fevers and diſeaſes to which other Aſiatic colonies are ſubject. The temperature of the air is cooled by the ſea and land breezes, which alternately prevail. The former begins between eleven and one in the forenoon, and blows very ſtrongly till towards ſix in the evening; at which time, however ſtrong it may be, the land-breeze aſſumes its place, and continues till about eleven the next day. By theſe means an agreeable coolneſs is always preſerved. At Calcutta, in Bengal, the heat is much more intenſe and inſupportable, though it lies at the diſtance of twenty-three degrees from the line; for it is not uncommon there to ſee Fahrenheit's thermometer, in the ſhade, during the months of April and May, at between eighty and ninety degrees. At the interval only when both winds ceaſe, the heat is ſtifling. The reaſon of the land-breeze here being ſo cool is, that, as Malacca is a peninſula, the wind always paſſes over a portion of ſea, whereas the land-breeze on the coaſt of Coromandel paſſes over the burning deſerts of Perſia.

The frequent and exceedingly violent thunder-ſtorms which take place here, contribute alſo very much to moderate the heat. They ariſe moſtly in the north-weſt, and bring with them ſuch coolneſs that I remember having been ſeveral times ſo cold that I was obliged to have recourſe to warmer clothing. The body here, indeed, is far more ſenſible of the ſmalleſt degree of cold than in the European climates, as the pores are kept ſo open by the heat. Theſe ſtorms are highly gratifying to the navigators bound to the Chineſe ſeas; for, as a ſouth wind, except in a very few places, where the ſea and land breezes ſucceed each other regularly,

regularly, prevails in the straits, they are glad to see such storms, which always begin to be formed in the north-west. In the night, however, they cannot be used with so much advantage, on account of the many sand-banks, islands, and even the coast itself, which must be avoided. It has been remarked, that during these storms the magnetic needle is in a continual state of perturbation; and I have heard the captain of an English East Indiaman declare that he has seen, during a violent storm, the north end of the needle point directly south.

The European productions brought to Malacca for the purposes of trade are confined to a very few articles. Rice, opium, white and coloured cottons, form the principal part of them. The Malays bring hither pepper, nutmegs, sago, rotangs, Spanish reeds, and gold-dust. The latter article is first examined by the officers of the company, and then made up into small packets in Chinese paper, bound round with a thread and impressed with a seal containing Malay characters. Each packet contains a catty, and is worth from 460 to 500 piastres. The Spaniards, who go from the Philippines to Madras for the purposes of trade, touch here always in order to exchange their piastres for gold-dust, which they carry with them to the coast of Coromandel, where, according to circumstances, they gain two *per cent.* These packets, when sold, are never opened, but are taken on the seller's report; and no instance is known of any fraud ever having been practised on such occasions. The small Spanish reeds are sold for about eight piastres *per* hundred, and pepper at fifteen piastres *per* pickel of 17½ Spanish pounds. The cotton and opium brought hither are sold mostly to the Dutch, who sell them afterwards to the Malays. The branch of trade, however, by which the inhabitants chiefly live, is supplying the ships which touch here with provisions, fresh water, salted and fresh fish, poultry and fruit. The price of fowls is generally a piastre for eight or ten. Pine-apples are sold at the rate of from four to six *per* piastre. Those who are acquainted with the number of the ships which pass through these straits every year to China, the Philippines, the South Sea, Batavia, Borneo, Coringa, &c. may form some idea of the importance

importance of this trade. The tavern where ſtrangers reſide is generally ſurrounded by a multitude of Malays having for ſale, apes, parrots, cowries, Spaniſh reeds, and various other articles. Among theſe people one may ſee many of their kings, who are diſtinguiſhed by a turban embroidered with gold, and who enter the tavern without receiving any particular marks of honour from the Europeans. The caſe is the ſame with all the petty Indian kings, of whom too high ideas are entertained in Europe, but who in India are looked upon as little better than corporals. It may be readily underſtood that I here ſpeak of the leſſer princes; for the greater ones, ſuch as the kings of Atcheen, Borneo, Pegu, &c. are in general treated with more reſpect.

The Dutch government, formerly eſtabliſhed here, conſidered it as of great importance to clear the ſtraits from the numerous Malay pirates by which it is infeſted. For that purpoſe it maintained a flotilla of cruiſers and flat-bottomed veſſels, which from time to time ſought out the Malays in their places of retreat behind the ſmall iſlands in ſhallow water, where their flat-bottomed veſſels were of excellent ſervice. The Malay prows generally carry a 24-pounder in the bow; have only one maſt, which can be lowered or raiſed at pleaſure; and are furniſhed with a rudder. They never attack a veſſel when there is wind, becauſe in that caſe it can manœuvre and make uſe of its cannon in every direction; but, if a calm take place, and they diſcover a ſhip from their retreats, they then come out and attack the veſſel behind and before, where they are out of reach of the cannon; maſſacre, with their poiſoned daggers called *kris*, all the white men on board; and carry off the blacks as ſlaves. A Dutch captain named Bloem, who commanded one of theſe cruiſers, was a terror to the Malays. Being acquainted with their moſt private retreats, he often ſurpriſed them when they leaſt expected it. They once imagined they ſhould be able to overcome him, and advanced againſt him, during a calm, with fourteen of their prows; but before they got near him a ſmall breeze ſprung up, ſo that he was enabled to direct his veſſel, and to give them ſuch a reception, that five or ſix of the prows were ſunk, and others had their rudders ſhot

 away;

away; ſo that they were obliged, to avoid total deſtruction, to throw themſelves into the ſea, and to ſave themſelves by ſwimming. The reſt betook themſelves to flight; for the Dutch give the Malays no quarter.

Of the laſt inſtance of this man's courage, which coſt him his life, I myſelf was an eye-witneſs. The French frigate La Prudente, of thirty-ſix guns, on board which I was a priſoner, being in the ſtraits of Malacca, about two o'clock on the 17th of July 1795, the man at the maſt-head calling out *Vive la Republique*, announced that he ſaw a ſail which ſeemed to be ſteering towards us. As there was then little or no wind, the two veſſels approached each other very ſlowly, and it was therefore near ſunſet before we could perceive that the ſtrange ſail was a veſſel with two maſts. About ten at night, after I had lain down in my hammock, I was rouſed by the drum beating to quarters. The officers near whoſe birth my hammock was ſlung, immediately ſtarted from theirs, and deſired the *citoyen priſonnier*, meaning me, to do the ſame. The fore-deck was cleared, and every preparation made for battle. I then proceeded to the fore-deck, and obtained permiſſion to remain there. The two veſſels by this time were ſo near that they could ſpeak each other. Our commandant hailed the ſtrange ſail, and aſked to what nation ſhe belonged. The anſwer was, that ſhe was a Dutch ſhip of war.—Strike to the republican frigate La Prudente, and lower your ſails.—Are you really French? ſaid the Dutch captain.—Yes: take in your ſails and drop your anchor, or I'll ſink you.—I have no anchor on board, replied the Dutchman, but plenty of cannon; and at the moment we received a whole broadſide, which we inſtantly returned. Several of the Dutchmen being killed, they then called out that they ſtruck. Having taken poſſeſſion of the Dutch veſſel, and ſhifted the priſoners on board the frigate, we found the former to be the Java, of eighteen 9-pounders, commanded by the beforementioned captain Bloem, who had that day ſailed from Malacca. A ſhot had carried away part of his left breaſt, but he ordered that the colours ſhould not be ſtruck till after his death.

The inhabitants of Malacca are a motley race, conſiſting of of Europeans, creoles, mulattoes, black Christians (called commonly Portuguese), Chinese, and Malays. The Dutch and Germans established here, all have mulatto or creole wives, for I do not know one whose wife is an European. The wife even of the last governor, Abraham Couperus, was a mulatto. The customs, manners, and dress of these ladies, are all in the Malay taste. The dress which they use, both in the house and on public occasions, consists of a silk gown with wide sleeves, which has a great resemblance to a powdering-gown. Their hair is twisted together and fastened on the crown of the head with a silver pin. In the house their favourite female slaves sit at their feet; and when they go out to walk, or pay visits, they accompany them, bearing a small silver box divided into different copartments, which contain certain articles indispensably necessary for a Malay lady. These articles are, betel, areka-nuts, chalk, a pair of small tongs, and a knife for spreading the chalk on the betel-leaves, in which they wrap up small bits of the areka-nut for the purpose of chewing them. The care which they bestow on their domestic economy consists only in seeing the orders which they give to their slaves carried into execution. When a stranger is invited to dinner, he is first introduced into an apartment where he is treated with a pipe of tobacco. Female slaves then make their appearance, one of whom brings him a silver bason for washing the hands, another a vessel containing water, and a third a towel. The company then sit down to table, and are waited on by the female slaves belonging to the master of the house. They are attended also sometimes by musical slaves, who, during the entertainment, perform pieces of music.

Slaves here, both male and female, a few excepted, are treated with great severity. The master of one of the taverns, a German, named Kreis, born in Hohenlohe, is particularly distinguished in this respect. I never entered his house, which I did above ten times, without seeing punishment inflicted on some of his domestics. If the cook had not prepared the dinner according to his taste, he was immediately carried into the back court, when two other slaves appeared with bamboos in their hands, and belaboured him on the posteriors till his master, who walked up and down with his pipe in his mouth, told them to stop. If the slaves did not execute his orders with sufficient severity, he applied to their shoulders a bamboo, which he always on these occasions carried himself. He always made it a rule when any stranger who happened to be in his house interceded for his slaves, to punish them doubly. Females slaves even were not excepted from this correction, which was inflicted in the most indecent manner. The Dutch government not only authorised this severity, but even put it in practice itself. A poor slave belonging to the same Kreis had conceived an attachment to a female slave who resided in the neighbouring house, and belonged to one Adrian Koek. The lovers could only visit each other in the night-time, and for this purpose the former was obliged to clamber over the roof of a penthouse. This nocturnal intercourse being discovered, Koek complained to the fiscal, and requested that the slave might be punished. Kreis, therefore, was obliged to give up his slave, and the sentence was, that he should receive 500 blows with a bamboo, to be inflicted publicly. A ring was then put round each of his legs, between which an iron bar was fixed, so that when he walked he was obliged to describe an arch with each foot, and in this state was sent back to his master.

On the other hand, I must observe that it is hardly possible, by the severest punishment, to restrain the profligacy and villainy of these men. What idea must we entertain of a man, who, having been severely punished for drunkenness, will, the very next day, take the key of the cellar from his master's closet and get so intoxicated as to be unable to stir from the spot? To a circumstance of this kind I have myself been a witness. The punishment, indeed, was cruel; for the culprit was made fast by the neck, middle, and legs, to iron rings fastened in the earth, and belaboured till the blood gushed from every part of his body. I was a witness also to a more striking instance, which shows how little impression good treatment makes on these uncivilised people. An opulent Englishman, who had resided some time at Malacca, taking a fancy to one of Kreis's slaves, purchased him

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for the ſum of 300 piaſtres. This ſlave was treated by his new maſter like a free man; he received money from him and permiſſion to go abroad on aſking leave, provided he remained to take care of the houſe in his maſter's abſence. But this ſlave, unaccuſtomed to freedom, notwithſtanding every threat and admonition, when he got out of the houſe, would not return in the courſe of the whole day, and at laſt, after being eight days in his new ſervice, he ſtole from his maſter a piece of money. His maſter endeavoured to make him confeſs the theft, with a promiſe of forgiveneſs for this firſt offence, but without ſucceſs. This circumſtance being told to Kreis, he conſidered it as a good opportunity of exculpating himſelf from the charge of cruelty brought againſt him by ſtrangers, as he imagined he could now prove that mild treatment to ſuch men would be entirely loſt. He offered alſo, not only to take back the ſlave, but to force him to confeſs. As the Engliſhman well knew that no one but his ſlave could be the thief, he delivered him over to Kreis, who tied him to a ladder, and then cauſed the uſual puniſhment to be inflicted. When he had received two hundred blows, his body was bent, and in that condition he was thrown into a hole till the next morning, when the puniſhment was renewed. He ſtill continued to hold out for ſome time, but Kreis having threatened to continue the puniſhment till he ſhould confeſs, he at length acknowledged the theft, but would not tell what he had done with the money. On the puniſhment being once more applied, he confeſſed that he had ſold the piece of money for a ſixteenth part of its worth to a Chineſe; who was immediately ſent for, and obliged to give it up. After this he remained in the ſervice of Kreis, who declared that he had never after any occaſion of complaint againſt him.

By what I have here ſaid, I do not mean to juſtify the cruelties exerciſed in this country, I only thought it neceſſary to ſhow, by ſome examples, how cautious people ought to be in forming opinions on this ſubject. The profligacy of theſe people is undoubtedly to be aſcribed to their maſters, who do not pay the leaſt attention to the formation of their moral character. They allow them to grow up like cattle, without taking care to give them any idea of religion, or of other knowledge, except what is immediately neceſſary to their maſters.

Thoſe who poſſeſs a whole family of ſlaves, never ſell any individual of that family, becauſe there have been numerous inſtances in ſuch caſes of the father's or mother's dying through grief, of becoming melancholy, or of deſtroying themſelves. The names given to theſe ſlaves are generally thoſe of the months in which they have been born or purchaſed. The commerce by ſea with the Malays is an armed commerce. All ſhips engaged in this trade carry a conſiderable number of guns, and from ten to twenty ſepoys or black ſoldiers. When a ſhip comes to anchor in any of the Malay ports, a beam is laid acroſs the deck before the mainmaſt to ſerve as a barrier between the buyers and the ſellers, and ſtrict watch is at the ſame time kept by the ſepoys, with their arms loaded and their bayonets fixed. If a Malay have any particular buſineſs to tranſact with the captain that requires longer time, he is admitted into the cabin in the after-part of the ſhip, but he muſt firſt ſuffer himſelf to be ſearched, to ſee whether he has about him a knife or any kind of weapons. Neglect of this precaution has occaſioned the loſs of many lives as well as veſſels. The treacherous Malay can never entertain any friendſhip for the Europeans; even if they ſhould live in habits of intimacy with him for a dozen or twenty years, when a favourable opportunity occurs of promoting his own intereſt, he will make no ceremony of doing it by ſacrificing his old friend. I could here give many inſtances of this kind, but I am convinced by what I have ſeen that the Europeans often give occaſion to this villainy by their own conduct. Being once on board a large Bombay ſhip, bound from Manilla to Madras, we diſcovered in the ſtraits of Malacca five large Malay veſſels, which, on diſcovering us, anchored between us and the land, except two, which were further out at ſea. As it was nearly calm, and the ſhip made little way, the captain, in a fit of intoxication, ſent one of his officers on board them to aſk whence they came, and whither they were bound. Their anſwer was, that they came from Rio, were bound to Ma-
lacca

lacca, and were laden with rotangs. This, however, did not fatisfy the captain, and he immediately began to fire among them. Being much alarmed at this conduct, they cut their cables and rowed with all their might towards the land, where they were in danger of running on fhore. In the mean time we paffed the other two, which being without us were not able to efcape, but, very fortunately, our captain did not obferve them on account of his ideas being deranged, and becaufe his attention was directed to the other fide. As foon as we had paffed, all the Malays on board bent themfelves three times with their foreheads towards the deck, which in all probability was by way of returning thanks to Providence for the danger they had efcaped.

In the ftraits there are a great many iflands which abound with excellent frefh water; but it cannot be at all times ufed, as the Malays poifon the fprings: to fuch length do thefe men proceed in their hatred towards the Europeans. When fhips enter the ftraits, they are generally vifited by fmall Malay boats, which bring fifh and tortoifes for fale. The tortoifes are of that kind called the green tortoife, which is three feet in length, about two in breadth, and weighs from two to three hundred pounds. A Spanifh dollar, a fmall quantity of rice, and a bottle of brandy, are in general the price of a tortoife and a certain number of fifh. The flefh of thefe tortoifes is well tafted and much like that of veal, but difficult of digeftion. The eggs, on the other hand, a hundred of which are fometimes found in one animal, are a great delicacy, and made into foup. The greateft advantage to feamen in purchafing thefe tortoifes is, that they live a long time; we kept many of them alive from three to four weeks on the deck without any nourifhment, and without any other care than throwing fea-water over them in the morning and evening. The whole ftraits are a real paradife interfperfed with a multitude of fmall iflands. Thofe who vifit them cannot help wifhing that they could fpend their whole lives on them, but while the Malays exift they muft be uninhabitable for Europeans.

II. *Chemical*

II. *Chemical Experiments and Obfervations on the Preparation of Sugar from vegetable Productions found in Europe. By* SIGISMUND FREDERICK HERMBSTADT.

[Concluded from Page 113.]

9. *EXPERIMENTS with the* Beta vulgaris altiffima, *the* Beta cicla altiffima *of Jacquin, in order to procure Sugar from it.*

This beet, like the former, is very common, fo that both kinds are ufed in Thuringia, and other parts of Germany, as food for the cattle. It is more juicy than the former, and has an exceedingly fweet tafte. Thofe which I employed for my experiments, grew at Schönberg, near Berlin, on an eftate belonging to M. Nöldechen, member of the privy council. On clofe examination, a great difference was obferved in them; they were all covered on the outfide with a red rind, but in the infide fome of them were totally white; others confifted of red and white rings, and fome of them of light red and dark red rings; many of them were very thick and tuberous, and others were almoft as thin and long as the red beet. Whether this arofe from their being of different kinds, or from the difference of the foil in which they grew, I cannot determine. Thofe which I employed for my experiments, had grown in the fame foil.

I took 112 of thefe beets, which weighed 125 pounds, and, having freed them from the tops and rind, grated them down, and when a portion of the juice had flowed off fpontaneoufly, expofed them to ftrong preffure. I obtained from them twenty-four quarts of very fweet violet-coloured juice. In confequence of what I had been taught by repeated experience, I firft boiled it, without any addition, till a third part only remained, and at the fame time a great quantity of albumen was feparated from it. When the liquor had cooled, it was ftrained through a piece of woollen cloth, and being then mixed with twenty-four quarts of lime-water, was boiled for half an hour. At the end of that period it appeared as a clear liquor of a wine-yellow colour, and

and a great quantity of impure matter had been thrown up as scum. I again suffered it to cool, strained it through flannel, and, being inspissated, I obtained eight pounds of syrup.

A part of this syrup I poured into a deep evaporating dish, into which I put some glass rods, and at the end of two months obtained, adhering to the rods, real sugar, similar to brown sugar candy. As this syrup was much more pleasant than common syrup, and exceedingly cheap, it might be used as a substitute for sugar, like that procured from the white beet.

In continuing my experiments on the beet, I endeavoured to ascertain the actual quantity of sugar that might be procured from a determined quantity of them. With that view I weighed three pounds twelve ounces of syrup obtained by the second experiment, poured it into a conical vessel of tinned copper, and exposed it to slow evaporation at the temperature of from 65° to 70° of Reaumur. In the course of eight hours there was formed on the surface a crystalline crust of granulated sugar, which at the end of twenty-four hours was nearly four lines in thickness: having forced it down to the bottom of the syrup, in two days a new crust was formed, which was also forced down; and this operation I continued till a tough uncrystallised pellicle only appeared on the remainder of the syrup: this induced me to conclude that all the crystallisable sugar was now separated from the liquid, which was shown also by its being less sweet and more slimy. I now put all the crystallised sugar, together with the fluid syrup, into a small sugar mould, which I kept for eight days at the temperature of 30° of Reaumur, by which means the syrup was entirely drained off, and the sugar remained almost in a dry state. The whole of this operation employed thirty-six days. The raw sugar I obtained was still a little moist in the inside, but it did not deliquesce in the course of three weeks, during which I kept it in an open vessel, and it weighed two pounds eight ounces: the syrup which drained from it weighed forty-eight ounces; consequently, from the three pounds twelve ounces of syrup employed, twenty ounces of aqueous particles had evaporated.

rated. From this experiment it appears, that a bushel of these beets, without any peculiar care being employed in the cultivation of them, will give 5¹ pounds of brown raw sugar, and 1⅞ pound of not ill-tasted syrup, which, if not used as syrup, might be used with advantage for distilling spirits. I am now employed in refining my raw sugar, and in determining how much fine sugar and how much syrup will be produced from it.

10. *Experiments with the red Beet, the* Beta rubra, Beta ciela rubra *of Jacquin, to obtain Sugar from it.*

That it is possible to obtain sugar also from the common red beets has been already proved by Margraf. I subjected them, therefore, to the like treatment as the preceding kinds, and obtained from a bushel of red beets 6½ pounds of syrup of a not unpleasant taste, but very inferior to that obtained from the white beet and the *beta vulgaris altissima*. I did not try to obtain sugar from it. Margraf, from a pound of fresh red beets, obtained eight ounces of dry sugar, and from thirty-two ounces of dry roots, about two and a half drams.

11. *Experiments with Carrots,* Daucus carotta *Linn.*

When carrots are cut in slices and boiled with water, you obtain a sweet liquor, which by evaporation produces a sweetish juice, which the country-people either spread upon bread or use to sweeten different articles of food. Having freed a bushel of carrots from the thin outer rind, I rasped them and expressed the juice. This liquor was then boiled, clarified with the white of an egg, and inspissated to the consistence of syrup. I obtained six pounds and a quarter of a not unpleasant syrup, much inferior, however, to that obtained from the white beet and the *beta vulgaris altissima*. I found it impossible to separate real sugar from it: rectified spirit of wine extracted from it a substance which had a great similarity to manna.

12. *Experiments with common Turnips,* Brassica rapa, *to obtain Sugar from them.*

I took twelve turnips and rasped them, after having freed them from the tops and outer rind. The rasped matter had an an agreeable, ſweet, and ſomewhat ſharp taſte, and, being properly expreſſed, gave a colourleſs ſweet juice. After clarifying it with the white of an egg, I ſtrained the clear liquor through a piece of woollen cloth, inſpiſſated it to the proper conſiſtence, and obtained an agreeable ſyrup of an inferior quality to that procured from the white beet, and *beta vulgaris altiſſima*; but equal to common ſyrup. By another experiment, I learned that a buſhel containing 125 roots, weighing 116 pounds, is capable of producing eight pounds of ſyrup. At the end of twelve weeks I found cryſtals of ſugar ſhot up in it; but they were exceedingly brown, and could be ſeparated only with difficulty from the remaining maſs.

13. *Experiments with the Cabbage Turnip,* Braſſica napobraſſica.

The juicy nature of theſe turnips, as well as the agreeable ſweet taſte of their juice, induced me to try whether I could obtain from them ſugar and ſyrup. Having peeled and raſped ſixty of them, which weighed 123 pounds, I obtained from them, by expreſſion, about twenty-two quarts of colourleſs juice, which was agreeably ſweet, but had at the ſame time a ſharp radiſh-like taſte. I boiled it twice; by which means a great deal of flaky matter ſeparated from it, and the liquor became perfectly clear. I filtered it after it had cooled, added to it twenty quarts of freſh lime-water, and boiled it ſlowly. While boiling, the ſharp radiſhy matter was evaporated, and I at length obtained ten pounds of tranſparent browniſh-yellow ſyrup, which had a perfect reſemblance to that obtained from the black birch, but which had the ſame foreign taſte. In order to procure ſugar from it, I put a portion of it into a veſſel in which I placed glaſs rods, ſuffered it to evaporate ſlowly, and at the end of ſome weeks found ſmall cryſtals of ſugar adhering to the rods, but the quantity I was unable to determine. As this turnip is dearer than the white beet and the *beta vulgaris altiſſima*, and gives a leſs pleaſant ſyrup even than common turnips, it muſt in this reſpect be conſidered as inferior to theſe three kinds.

14. *Experiments with the Skirret,* Sium Sisarum.

Margraf obtained from a pound of fresh skirret-roots eighteen ounces of dry sugar; and half a pound of dry roots gave him three drams of real sugar. The object of my experiments was to obtain merely a syrup fit for domestic purposes; but, on account of the many farinaceous particles which these roots contain, the preparation of dry sugar from them, as Margraf remarks, was attended with many difficulties. As they cannot well be rasped, on account of their smallness, a bushel of them, which weighed twenty-six pounds, was pounded in a stone mortar with the addition of a little water, and the juice expressed: the residuum was washed in cold water, and, being again expressed, I obtained a turbid sweet liquor. I suffered it to remain eight days in a cool place, and found that a great part of the farinaceous matter had deposited itself, and that the liquor was become clear and transparent. Being clarified with the white of an egg, and then inspissated, I obtained five pounds of an agreeable syrup of a bright brown colour. From this it appears that these roots are too dear to obtain from them a cheap syrup.

15. *Experiments with Parsnips,* Pastinaca sativa Linn.

The peculiar sweetness of this woody root induced me to subject it to a similar examination, though Margraf had before observed that little sugar was to be obtained from it. With this view I treated, in the same manner as the preceding, a bushel of the roots, which weighed twenty-four pounds, and obtained $5\frac{1}{2}$ pounds of an agreeably tasted syrup, but which had not entirely lost its natural taste. Had my other occupations admitted, I should have endeavoured to ascertain the proportional quantities of sugar contained in determinate quantities of these substances; but as this has been impossible, I must reserve them for another opportunity.

III. *Account of a Series of Experiments, undertaken with the View of decomposing the Muriatic Acid.* By Mr. William Henry *.

MODERN chemiſtry, notwithſtanding its rapid advancement during the few laſt years, ſtill preſents to its cultivators ſeveral intereſting objects, both of analytic and ſynthetic inquiry. Among the former, the decompoſition of the muriatic and of certain other acids holds a diſtinguiſhed place; for our curioſity reſpecting the nature of theſe bodies is ſtrongly excited by the influence which the diſcovery would have on the general doctrines of chemical ſcience, as well as on the explanation of individual facts. The theory of the formation of acids, for example, one of the moſt important parts of the new ſyſtem of chemiſtry, muſt be regarded as incomplete, and liable to ſubverſion, till the individual acids now alluded to have been reſolved into their conſtituent principles. To the beſt of my knowledge, however, we are not in poſſeſſion of a ſingle fact that gives the ſmalleſt inſight into the conſtitution of the muriatic acid; and the attempts to effect its analyſis can only therefore be directed by the analogy of the decompoſition of other bodies, which, from ſimilarity of character, are arranged in the ſame claſs.

One of the firſt objects in the analyſis of a compound body ſhould be, its complete ſeparation from all other ſubſtances, which, by their preſence, may tend to introduce uncertainty into the reſults of the proceſſes that are employed. But it is ſeldom that a ſimplicity ſo deſirable can be obtained in the objects of chemical reſearch; for, agreeably to a known law of affinity, the laſt portions of any ſubſtance are ſeparated with peculiar difficulty; the force of attraction appearing to increaſe as we recede from the point of ſaturation. In a liquid ſtate, the muriatic acid is a totally unfit ſubject for analytic experiment; for, in the ſtrongeſt form under which it can be procured, it ſtill contains a large proportion of water. This watery portion, beſides the complexity which

* From *Tranſactions of the Royal Society of London for* 1800.

 it

it introduces into the results of experiments, prevents any combustible substance that may be applied, from acting on the truly acid part; because that class of bodies, having less difficulty in attracting oxygen from water than from the acid, will necessarily take it from the former source. The state of gas, therefore, is the only one in which the muriatic acid can become a proper object of analysis.

In the series of experiments on this gas, which I am now about to describe, I employed the electric fluid, as an agent much preferable to artificial heat. This mode of operating enables us to confine accurately the gases submitted to experiment; the phænomena that occur during the process may be distinctly observed; and the comparison of the products, with the original gases, may be instituted with great exactness. The action of the electric fluid itself, as a decomponent, is extremely powerful; for it is capable of separating from each other, the constituent parts of water, of the nitric and sulphuric acids, of the volatile alkali, of nitrous gas, and of several other bodies whose components are strongly united. I began, therefore, with examining attentively the effects of the electric muriatic acid gas without admixture *.

Section I.

On the Effects of Electricity on Muriatic Acid Gas.

When strong electrical shocks were passed through a portion of muriatic acid gas, confined in a glass tube over mercury, the following appearances took place. The bulk of the gas, after 20 or 30 shocks, was considerably diminished; and a white deposit appeared on the inner surface of the tube, which considerably obscured its transparency. In some instances, both the contraction and deposit were much more remarkable than in others. The gas which issued from muriat

* The gases submitted to the action of electricity, in the following experiments, were confined in straight glass tubes of various diameters, armed at the sealed end with a conductor of gold or of platina, but generally of the latter metal. The shocks were as strong as could be given without breaking the tubes, which, notwithstanding every precaution, were often shattered by the force of the explosion. Each measure of gas is equal to the bulk occupied by a grain of mercury.

riat of soda, soon after the affusion of sulphuric acid, and while the charge was yet warm, exhibited these appearances in an eminent degree. Of this gas, 307 measures were reduced, by 20 shocks, to 227, or were contracted nearly one-fourth. Gas from the same materials, after they had continued working for some hours, was diminished, by similar treatment, only about a twelfth. These effects, therefore, it seemed probable, depended in some measure on the presence of moisture; and I accordingly found, that muriatic acid gas, after more than a week's exposure to muriat of lime, brought into contact with it immediately after cooling from a state of fusion, was scarcely diminished at all; and that the deposit, though it still occurred, was less copious in quantity. This deposit was not, like corrosive sublimat, soluble in water; but had every property of the less saturated salt, calomel.

The mercury by which the muriatic acid was confined was therefore evidently oxydated; and to the combination of a part of the gas with the oxyd thus produced, the diminution of bulk was doubtless to be ascribed. But it was uncertain from whence this oxygen was derived. It might either result from the decomposition of the acid gas, or of the water chemically combined with it. The following experiments were therefore made to determine this point.

Experiment 1. Through 1457 measures of muriatic acid gas 300 electrical shocks were passed. There remained, after the admission of water, 100 measures of permanent gas, (or not quite seven from each hundred of the original gas,) which, on trial, appeared to be purely hydrogenous.

Exper. 2. Of the gas dried by muriat of lime, 176 measures received 120 shocks. The residue of hydrogenous gas amounted to 11 measures, or rather more than 6 *per cent.*

These experiments, and other similar ones, made on comparative portions of muriatic acid gas in its recent state, and after exposure to muriat of lime, convinced me that it was impossible, by this method, wholly to deprive the muriatic gas of water. The recent gas, however, when electrified in smaller quantity than in experiment 1, gave a larger proportion of hydrogenous gas; which shows, that some portion of its

its moisture was removed by exposure to muriat of lime. In order, if possible, to procure the gas perfectly dry, another mode of preparing it was resorted to. Alum and common salt were first well calcined, separately, to expel their water of crystallisation, and, being then mixed, were distilled together in an earthen retort. The gas proceeding from these materials was received over dry mercury; but, though only the last portion that came over was reserved for experiment, it still, after the usual electrisation, afforded a product of hydrogenous gas.

In the course of the preceding experiments, I observed that the diminution of the muriatic acid gas stopped always at a certain point, beyond which it could not be carried by continuing the shocks. Gas also, which had been thus treated, when transferred to another tube, and again electrified, did not exhibit any further deposit. It became interesting, therefore, to know whether the production of hydrogenous gas had a similar limitation; because the decision of this question would go far towards ascertaining its source. If the evolved hydrogenous gas arose from the decomposition of the acid, it might be expected to be produced as long as any acid remained undecomposed. But if water were the origin of this gas, it would cease to be evolved, when the whole of the water contained in the gas had been resolved into its constituent principles.

Experiments 3 and 4. Into two separate tubes I passed known quantities of muriatic acid gas. Through the one portion 200 discharges were taken, and through the other 400. On comparing the quantities of hydrogenous gas produced, it proved to bear exactly the same proportion, in each tube, to the gas originally submitted to experiment. Hence it may be inferred, that the hydrogenous gas, evolved by electrifying the muriatic acid, has its origin, not from the acid, but from the water which is intimately attached to it. The agency of the electric fluid appears also, from the following experiments, to be exerted, not only in disuniting the elements of water, but in promoting the union of the evolved oxygen with muriatic acid.

Exper. 5. A mixture of common air and muriatic acid

 gas,

gas, in the proportion of 143 of the former to 116 of the latter, was rapidly diminiſhed by electrical ſhocks; 30 of which reduced the whole to 111 *. The remainder conſiſted of muriatic acid and azot gaſes, with a ſmall proportion of oxygenous gas. The depoſit formed on the tube was of the ſame kind as before, but much more abundant.

Exper. 6. The ſame appearances were occaſioned, much more remarkably, by electrifying muriatic acid with oxygenous gas; and the contraction continued till the mercury roſe ſo as to touch the extremity of the platina conductor. At each exploſion, a denſe white cloud was ſeen in the tube, which ſoon ſettled on its inner ſurface, and was of exactly the ſame chemical compoſition as the one already deſcribed. Nitrous gas and muriatic gas, when electriſied together, underwent a ſimilar change.

In order to aſcertain whether the mercury by which the gaſes were confined, in the above experiments, had any influence on their reſults, they were repeated in an inſtrument made, purpoſely for the occaſion, by Mr. Cuthbertſon, of London. It conſiſted of a glaſs tube, ground at each end, with the view of receiving two ſtoppers, each perforated with platina wire, which projected into the cavity of the tube. When the ſtoppers were in their places, the extremities of the wires were at the diſtance of about half an inch; and, by properly diſpoſing the apparatus, electrical ſhocks might be paſſed, through any gas or mixture of gaſes, with the contact only of glaſs and platina.

Exper. 7. In this tube I electrified the muriatic acid gas, and then admitted to it an infuſion of litmus. The ſudden deſtruction of its colour evinced the formation of oxygenated muriatic acid. Not the ſmalleſt depoſit appeared on the tube.

Experiments 8 and 9. The ſame phænomenon took place when an infuſion of litmus was brought into contact with a mixture of common air and muriatic acid, and of oxygenous

* This experiment ſuggeſts an additional reaſon to that already given for the greater diminution of the firſt than of the ſubſequent portions of the muriatic acid gas; for the former may be preſumed to have been much more adulterated, than the latter, with the atmoſpherical air of the veſſels.

gas

gas and muriatic acid, after electrisation in this instrument; oxygenated muriatic acid being produced in both cases.

The above facts prove that the combination of oxygen with muriatic acid, in these experiments, is not occasioned by a pre-disposing affinity in the mercury to combine with oxygenated muriatic acid; but that the electric fluid serves actually as an intermedium in combining the muriatic acid with oxygen.

From the relation of these experiments it appears, that not the smallest progress had been made by them towards the decomposition of the muriatic acid. I resolved, therefore, to attempt its analysis, in a similar manner, with the aid of combustible gases.

Section II.

Effects of electrifying the Muriatic Acid Gas with inflammable Substances.

In a memoir read before the Royal Society, and inserted in their Transactions for 1797, I have shown that, when electrical shocks are passed repeatedly through a confined portion of carbonated hydrogenous gas, the water held in solution by the gas is decomposed by the carbon, which forms a constituent part of it; that carbonic acid is formed; and an addition made, of hydrogenous gas. Hence the bulk of the carbonated hydrogen gas is considerably enlarged by this process, which shows, by its results, that the affinity of carbon for oxygen is rendered much more powerful and efficient by the electric fluid. I have since found that other oxygenated substances are decomposed by electrifying them with carbonated hydrogen gas. Nitrous gas, for example, is speedily destroyed by this process, and carbonic acid and azotic gases are obtained.

Every attempt to decompose the muriatic acid must be founded on the presumption that it is an oxygenated substance; and those bodies promise to be the most successful agents, that possess a strong affinity for oxygen. Now, of all known bodies, charcoal most strongly attracts oxygen; and I have, therefore, repeatedly attempted the destruction of this acid, by passing it over red-hot charcoal. But, in a

series

ſeries of experiments, which I made ſome time ſince, with this view, in conjunction with Mr. Rupp, we ſoon found reaſon to be diſſatisfied with the difficulty and uncertainty of this proceſs. An immenſe production of hydrogenous gas took place; but it was not eaſy to determine whether it had its origin from real acid or from water. Our experiments, however, though inſufficient to furniſh deciſive proof, induced us to believe that it had the latter origin.

It next occurred to me, that the comparative affinities of the muriatic radical, whatever it may be, and of charcoal, for oxygen, would be elegantly and ſatisfactorily aſcertained, by electrifying together the carbonated hydrogen and muriatic gaſes. If the muriatic acid be capable of decompoſition by carbon, it might be expected to be deſtroyed by this proceſs; and the exact quantity of acid decompoſed, and the nature and quantity of the products, would thus be eaſily determined. I electrified, therefore, the muriatic acid and carbonated hydrogen gaſes, with the moſt ſcrupulous attention to the phænomena and reſults. That the electric fluid might not be miſapplied in decompoſing the water of the carbonated hydrogen gas, it was kept more than a week, before uſe, over quicklime, introduced to it while yet hot.

Exper. 10. Of this carbonated hydrogenous gas, 186 meaſures were expanded, by 130 ſhocks, to 211; that is, the gas was increaſed to about 1-8th its bulk.

Exper. 11. Of the ſame gas, 84 meaſures were mixed with 116 of muriatic acid gas, dried by muriat of lime. By 120 ſhocks, the mixture was a little dilated. After the admiſſion of a drop or two of water, there remained 91 meaſures; *i. e.* the addition of permanent gas was 7 meaſures, or about as much as might have been expected from the muriatic gas alone.

Exper. 12. Eighty-three meaſures of dry carbonated hydrogenous gas, with 89 of muriatic acid gas, received 200 ſhocks. The permanent reſidue, after the admiſſion of water, was 101 meaſures: the addition, therefore, amounted to 18. Of the added 18, ſix may be accounted for by the decompoſition of the water of the muriatic gas, and 10 by that of the carbonated hydrogenous gas. There remain, there-

 fore,

fore, only two measures that can be supposed to be produced from the muriatic acid gas; a quantity too small to afford grounds for supposing them to arise from decomposed acid.

Exper. 13. Dry carbonated hydrogenous gas 132 measures,
mixed with dry muriatic gas - 108

240

by 200 shocks, expanded to - 268

Part of this gas was then transferred to another tube, and the proportion of permanent gas ascertained. Through the remainder, 150 additional shocks were passed, before the amount of the gas thus evolved was determined. In both, it bore exactly the same proportion to the original gas; which shows that, by continuing the electrisation, no further effects were produced.

[To be continued.]

IV. *New Process for Tinning Copper and other Vessels in a durable Manner. By* M. BUSCHENDORF, *of Leipsic* *.

THAT copper and brass vessels cannot be used with safety in cooking victuals or for holding articles of food, and particularly those which contain acids, is well known. It is also well known that the tinning applied in the usual manner is not durable, being soon worn away by cleaning, and on that account must be frequently renewed. Some, therefore, have proposed enamelling for kitchen utensils of copper; which, indeed, would answer exceedingly well, and be much safer for the health than impure tin mixed with lead, often employed for tinning: but, unfortunately, enamel is too dear, and readily breaks when the vessel receives the least blow; which cannot always be avoided †.

* From *Journal fur Fabrik, Manufactur, und Handlung,* October 1799.

† Articles that would come high when made singly, may be afforded at a low rate when manufactured on an extensive scale. Cooking utensils lined with a vitrified glazing, are now commonly sold in many shops in London, and at a moderate price. It would be as reasonable to object to the use of earthen-ware or china, because they may be broken by blows, as to make this an objection against the use of glazed kettles.— EDIT.

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The following proceſs for tinning is attended with no danger from poiſonous ingredients, as no lead is uſed in it; the tinning, too, is exceedingly durable, adds ſtrength to the copper veſſel, and ſecures it from the action of acids much longer than the common tinning:—When the veſſel has been prepared and cleaned in the uſual manner, it muſt be roughened on the inſide by being beat on a rough anvil, in order that the tinning may hold better, and be more intimately connected with the copper. The proceſs of tinning muſt then be begun with perfectly pure grained tin, having an addition of ſal-ammoniac inſtead of the common colophonium. Over this tinning, which muſt cover the copper in an even and uniform manner throughout, a ſecond harder coat muſt be applied, as the firſt forms only a kind of medium for connecting the ſecond with the copper. For this ſecond tinning you employ pure grained tin mixed with zinc in the proportion of two to three, which muſt be applied alſo with ſal-ammoniac ſmooth and even, ſo that the lower ſtratum may be entirely covered with it.

This coating, which, by the addition of the zinc, becomes pretty hard and ſolid, is then to be hammered with a ſmoothing-hammer, after it has been properly rubbed and ſcowered with chalk and water, by which means it becomes more ſolid, and acquires a ſmooth compact ſurface.

Veſſels and utenſils may be tinned in this manner on both ſides. In this caſe, after being expoſed to a ſufficient heat, they muſt be dipped in the fluid tin, by which means both ſides will be tinned at the ſame time.

As this tinning is exceedingly durable, and has a beautiful colour, which it always retains, it may be employed for various kinds of metal inſtruments and veſſels which it may be neceſſary to ſecure from ruſt.

Another durable, though ſomewhat expenſive, Method of Tinning.

This tinning, which conſiſts of more articles and is dearer than the former, can be applied to metals and metallic mixtures, and, when well prepared, is exceedingly durable; which makes up, in ſome meaſure, for the coſt. It is as follows:—

Take pure grained tin one pound, good malleable iron one ounce and a half, platina one dram, silver twenty-four grains, and gold three grains. These five metals must be well fused together in a crucible with one ounce of pounded borax and two ounces of pounded glass, and the liquid matter must be formed into small ingots. These ingots are to be again heated and reduced to powder in a warm mortar with a hot pestle. This powder is then to be put in an iron pan over the fire, where it must be again fused, stirring it well round: it is then to be poured into small flat moulds, where it is suffered to cool, and it is then fit for use.

This tinning is to be applied in the following manner:— First tin the vessel with grained tin and sal-ammoniac in the common manner; clean and scower this coating; then apply the composition with sal-ammoniac according to the usual process, and when it is well diffused suffer the vessel and the tinning to cool. Then expose it every where to a gentle heat to render the adhesion stronger, and immerse it while hot into cold water to give it that hardness and solidity which it had lost by being heated. The surface is somewhat rough and gritty: but you then rub it hard with a scratch-brush; and, in order to make it even, you afterwards smooth it completely with fine sand or any polishing ingredient.

If one coating does not appear sufficient, a second, and even a third, may be applied in the manner above described. Two coatings, however, are fully sufficient for kitchen utensils which have been a good deal used; and, if you wish to have the surface perfectly smooth and even, you may apply a thin coating of tin, which will fill up all the cavities and render it quite even. A coal fire is the best for this tinning; for turf coals attack the metal, and interrupt the union and fusion of the coating.

V. *Remarks on Dr.* GIRTANNER's *Memoir respecting the Question, whether Azot be a simple or a compound Body. By* C. BERTHOLLET *.

DR. PRIESTLEY long ago made experiments which at first induced him to believe, that water distilled from lime, and, above all, from argil, or even without addition in an earthen retort, or one of rough glass, would be converted into air; but he afterwards proved that this phenomenon was fallacious, and that a transmission only of the surrounding air took place on this occasion. Guyton added some further observations to those of Priestley †, and the circumstance seemed to be sufficiently cleared up.

Wiegleb, however, published a memoir in which he pretended to prove, by his own experiments and those of some others, that the gas obtained by making water pass through ignited earthen tubes, ought to be ascribed only to the combination of the aqueous vapour with the matter of heat, and that this combination gave birth to azotic gas.

This memoir gave rise to an answer published by Deiman, Van Trooftwyk, and Lauwrenburg. These learned chemists examined, with the greatest care, all the circumstances respecting the pretended production of azotic gas by water; and I should have believed that no further doubt could exist respecting the consequence they deduced from their experiments, *viz.* "that the azotic gas, obtained in some cases by making the steam of water pass through ignited tubes, arose merely from the exterior air deprived of its oxygen gas by the fire in which the tubes are placed, and that thus the pretended conversion of water into azotic gas, by its combination with the matter of heat, vanishes."

After all these discussions, Girtanner supports the new ideas which he brings forwards in his memoir ‡ on several facts, the greater part of them borrowed, and already discussed with

* From *Annales de Chimie*, No. 103. † Encyclop. Vol. I. p. 674.
‡ See this Memoir, p. 335.

care.

care. Among those which he affirms, we ought to consider the following as his fundamental one: "When water is boiled in a retort of glass, or any other material, you obtain azotic gas." The author prescribes these precautions:—"To obtain azotic gas in the greatest quantity, the water must be evaporated only very slowly and by a very gentle heat, which you must take care not to augment too much."

"It may be observed in all these experiments, without exception, that, when the last drop of water is evaporated, the azotic gas ceases to be produced, though the fire be continued."

An assertion so positive induced me to repeat the experiment with the precautions prescribed by Girtanner. It was made with water recently distilled, and the precipitate from sulphat of alum by potash well washed; it was made also with very white argil furnished by Guyton: but though the quantity of water was considerable, and these two experiments lasted, therefore, a long time, no gas was disengaged, and the result was the same as that of the Dutch chemists.

It was on this fact, however, so easy to be ascertained, that Girtanner chiefly established the composition of azot; and hence that of the atmosphere, "which (says he) is not, as hitherto believed, a mixture of oxygen gas and azotic gas, but rather a mixture of oxygen gas and hydrogen; a water in the form of gas, if I may be allowed the expression. When, by chemical experiments called improperly *eudiometric*, the oxygen is separated from the hydrogen, this separation can never be entirely and completely effected; a part of the oxygen remains united to the hydrogen, and forms that chemical combination which we call azot, and which we obtain in these experiments."

Thus, when we make a mixture of oxygen gas and hydrogen gas, we form atmospheric air; and the differences of specific gravity, of properties found by all the tests, of products in combustion, are only trifling circumstances, to which Girtanner pays little attention.

C. Bouillon-Lagrange, in consequence of Girtanner's memoir, has made experiments more numerous than mine, but which

which conduct to the ſame reſults. I ſhall here give a ſhort view of them, which he was ſo kind as to communicate to me.

He did not obtain azotic gas by boiling diſtilled water alone, or with argil or alumine, in a glaſs retort, having adapted to it a glaſs or porcelain tube.

He had no diſengagement of azotic gas when he made the ſame experiment with ſilex obtained from the fluoric acid.

Having placed lime obtained from white marble in a ſimilar tube, he cauſed the ſteam of water to paſs through the tube, the other end of which was immerſed in lime-water. There was ſtill diſengaged a little carbonic acid, which formed carbonat of lime, but there was no azotic gas.

If, inſtead of lime, argil be uſed with the ſame apparatus, there is formed a little carbonat of lime, but there is no diſengagement of azotic gas.

If water be made to paſs through a porcelain tube into which lead has been put, this metal paſſes partly to the ſtate of a yellow oxyd and becomes vitrified, but no azotic gas is diſengaged.

If, inſtead of lead, you put tin into the tube, that metal becomes oxydated, and you obtain hydrogen gas. Zinc gives the ſame reſult.

To aſcertain whether this hydrogen gas contained azotic gas, C. Bouillon-Lagrange made it paſs over ſulphur in a ſtate of fuſion, and obtained ſulphurated hydrogen gas, but no azot. He ſtill ſubjected it to another trial.

He mixed the hydrogen gas he had obtained by the means of zinc with oxygen gas; and, having inflamed the mixture by an electric ſpark, water was formed, but there was no azotic gas.

Girtanner's opinion is connected with that of Humboldt on the abſorption of oxygen by the ſimple earths, and particularly by alumin. There is this difference, however: Humboldt conſiders the ſuppoſed phenomenon as a ſimple ſeparation of the oxygen, which becomes fixed, from the azot which remains in a gaſeous ſtate: but, according to Girtanner, " the azot obtained by the experiments being always a product of the operation, and not having existed before the experiment under the form of azot in the air examined; Humboldt, who is fond of drawing general conclusions from single facts, seems to have been deceived when he advances that earths might be employed to determine the quantity of azot contained in atmospheric air: earths do not indicate the azot contained in the atmospheric air, they change that air into azot."

Young Saussure * has formally contradicted the result announced by Humboldt. He agrees that the *humus*, which is the result of a mixture of decomposed vegetables with others not yet decomposed, absorbs oxygen gas, and is a fact well known; but he attests that "this effect does not take place when the earths are pure, and deprived of all vegetable substance." He describes several experiments which he made with alumin, calcareous earth, and silex.

In the *Journal de Physique* for Pluviose the same year, there is an answer of Humboldt, written in a magisterial tone, which contains nearly the assertions contained in the first memoir, with a kind of attestation, because he made his observations in the laboratories of Vauquelin and Fourcroy. This authority would be imposing, if these two learned chemists had co-operated in the observations; but so far from this, we see by the statement that the only experiments tried in their presence would not succeed.

I have been assured that the celebrated Fabbroni, of Florence, repeated the experiments of Humboldt at the same period without success.

Champy the son, a very correct observer, repeated them at Cairo on alumin, on lime, and on the mud of the Nile, at a temperature which varied from nearly 30 to 36 degrees of the centigrade thermometer, but he obtained no absorption: it is to be remarked that the mud of the Nile contains some remains of vegetable substances, so that when distilled there is extracted from it a certain quantity of carbonic acid and carbonated hydrogen gas.

Chaptal repeated them at Montpellier without obtaining an absorption.

* *Journal de Physique*, Frimaire, an. 7.

I kept

I kept moistened alumine a long time in contact with atmospheric air and with oxygen gas, without observing the least absorption. A chemist having related that the absorption took place by the means of agitation, agitation was employed with much patience, but the result was the same. The operation was repeated with white clay which had been sent to me by Guyton, but still without absorption. This cargil, however, exposed to a strong heat, gave a little carbonic acid and a small quantity of carbonated hydrogen gas.

VI. *Description of an improved Apparatus for Distilling, by which a considerable Saving may be made in the Article of Fuel; and of an Apparatus for the Rectification of Spirit of Wine.* By J. W. C. Fischer *.

THE following apparatus, if not entirely new, is attended with such considerable advantage in regard to the saving of fuel, as must be acknowledged by every impartial and competent judge. The principal improvements may be found in Göttling's Pocket-book † for Chemists and Apothecaries, 1798; but, as far as I know, little or no use has hitherto been made of them. The reason of this may be, that the above work comes very seldom into the hands of those who have most occasion to make use of it. Distillers by profession continue, for the most part, those modes of operation which they have been taught in learning their business, partly because they have no real opportunity of intro-

* From *Journal für Fabrik, Manufactur*, &c. December 1799.

† According to the above pocket-book, this distilling apparatus was first made in Denmark, where it has been used above fifty times on a large scale in one of the distilleries. The person who employed it got the first idea of it from a Danish journal, the author of which had copied it from a German work entitled *Auserlesene Sammlung Oekonomischer Schriften*, Dresden 1790. The whole apparatus, the cooler excepted, cost in Denmark, where every thing is dearer than in Germany, no more than 17 or 18 rix-dollars; whereas a large still would cost from four to five hundred rix-dollars. An apparatus which can be purchased at so cheap a rate, and by which one-half is saved in the article of fuel, must no doubt be of importance to those who may have occasion to employ it.

 ducing

ducing improvements into their procefs, and partly becaufe many of them, from a ftrong propenfity to oppofe all innovations, adhere to their old practices without taking the trouble to examine the advantage or difadvantage of the improvements propofed *.

If I am not miftaken, this apparatus originated in Denmark, where it has been employed with great fuccefs: I have, however, ventured to introduce an improvement, the advantage of which, I flatter myfelf, cannot be contefted. That this apparatus is not without defects, and confiderable defects, I will readily admit; and thefe I fhall explain, as well as the advantages: but many of thefe defects attend the common apparatus now employed, and, in all probability, may be obviated by experience and attention.

At the fame time I have made an application of this method to the apparatus employed for the firft and fecond rectification of fpirit of wine. Demachy long ago propofed the water-bath for the rectification of fpirit of wine; but, however convenient the apparatus propofed by him for this purpofe, it is attended with confiderable expenfe, and the faving of fuel, which is of fo much importance, has not been taken into confideration.

Fig. 1. (Plate VI.) reprefents the common apparatus for diftilling. A is a large wooden veffel bound round with iron, into which the liquid to be diftilled is introduced, and which may be of any fize at pleafure.

A hole is made in the bottom of the veffel which occupies about a fourth part of its furface. In this opening, which ferves as an air-hole, the furnace *b* is placed. This furnace may be made with moft advantage of ftrong copper, as iron would be foon rufted by the furrounding fluid, and therefore would not laft half fo long as copper. This furnace is in nothing different from a common wind-furnace, only that the upper part of it, which in the former is open for the

* The account we gave in a former Number of the improvements introduced by the Scotch diftillers, proves that this remark is not applicable to the whole trade: it is to be obferved, however, that the Scotch aimed at faving time and excife duties, while the intention of the apparatus defcribed in this paper is to fave fuel.—EDIT.

the convenience of the workmen, is here ſhut, and the aperture for introducing the fuel is lengthened.

c is the tube deſtined for introducing the fuel: it paſſes through the ſide of the veſſel, and can be ſhut by the door *d*, in order to afford an uniform paſſage to the heat through the draught tubes.

It is neceſſary that the tube deſtined for introducing the fuel ſhould have the ſame height and breadth as the fire-place *e*, and it will be better that the whole furnace ſhould be rather ſquare than round, as the fire by theſe means can be better managed; and when the tube is ſhut by the door *d*, no unneceſſary eſcape of heat, notwithſtanding its width, is to be apprehended.

f is the aſh-hole, which is deſtined not only to receive the aſhes that fall from the grate, but to favour the acceſs of the atmoſpheric air neceſſary for maintaining the fire. It is therefore requiſite that the veſſel ſhould be placed on two blocks, that the air may find a paſſage under the bottom. It is of great importance that the lower part of the furnace be well joined to the bottom of the veſſel, to guard againſt the fluid running out.

From the upper part of the furnace a draught tube, *g*, proceeds alſo through the ſide of the veſſel. This tube, according to the ſize of the apparatus, muſt project from it from two to ſix inches. Above this tube there is a ſecond draught tube, *h*, which, by the tube *i*, having a right-angled knee at each end, communicates with the tube *g*, while the other end of it is deſtined to convey off the ſmoke and foul air. According to the ſize of the veſſel, this tube is at the diſtance of from ſix inches to a foot above the tube *g*. If it be neceſſary that the furnace ſhould be well faſtened to the bottom of the veſſel, it is no leſs neceſſary that the parts where the tubes paſs through the ſides of the veſſel ſhould be well joined and completely water-tight, in order to prevent the fluid from oozing through.

Around the upper edge of the veſſel there is a copper ring from three to four inches in breadth, in order to receive the copper ſhoulder *l*, which, as well as the cover, may be rendered air-tight by means of paper or linen rags daubed over

 with

with paste. In a common still, this part is not separated from the body, but makes with it a whole: but here this advantage is obtained, that the vessel, by removing the shoulder, may be much easier cleaned than the common still.

It will be of considerable advantage if the upper aperture of the shoulder be equal in breadth to two-thirds of the breadth of the vessel; and the circumference of the head, *m*, must be little inferior to that of the vessel. The vapours thereby escape more easily; by which means the distillation is much promoted. The necessity of the gutter which goes round the top is evident, and in most apparatuses it is employed *.

It is necessary that the tube *n* should at the upper end be as wide as possible. In most distilleries it is a common fault that this tube is too narrow. By this fault the free escape of the vapour is impeded, and the distillation not a little retarded; whereas, when the tube is sufficiently large, half the time will be sufficient for the operation.

As the condensation of the steam, particularly in large distillations, is not completely effected by a simple tube proceeding through the cooler, particularly in distilling on a large scale, where the necessary degree of heat cannot be always accurately maintained, spiral tubes have hitherto been employed; which, indeed, answer the purpose, but, on account of their expense and the difficulty of cleaning them, cannot always be employed with convenience. For this reason, I conceived the idea of an apparatus by which a considerable surface is presented to the steam for cooling it, without being attended with the above inconveniences.

This apparatus consists of a copper box, open at both ends, from one foot to a foot and a half in breadth, and from two to three inches in height, lying on its flat side. It is tinned on the inside, and to both ends of it are applied close covers,

* There is no need for a head upon any still: it only tends to promote a condensation of the vapour, and its falling back into the body of the still. A flat cover is all that is necessary. The vapour then, by its continued elastic force, finds its way into the worm, and is condensed in its descent. The space between the surface of the liquor and the cover should, however, be sufficient to prevent the contents from boiling up into the worm.—EDIT.

from

from the middle of each of which a tube projects; the one, *o*, in the upper cover, is destined for receiving the beak of the still. From the under one the condensed vapour flows into the lower vessel. The copper box *p* passes through the oblique-lying cask or vessel *q*, and projects some inches beyond both its ends, in order to receive the covers. The box may be cleaned without any difficulty: the expense of it will not be one-half that of a worm, and yet it will answer the purpose of the latter completely.

When you proceed to distillation, the vessel may be filled half a foot above the upper tube *h*, and at the same time that tube may be connected with the tube *i*, by slipping their ends into each other. The fire made in the furnace will then exercise an equal action on all sides: the heat proceeds through the tube *g* into the upper draught tube *h*; both tubes, as well as the furnace, are completely surrounded by the fluid; and the advantage of this apparatus in regard to the saving of fuel may be readily conceived, as that in the furnace can have no action but on the fluid, and as the heat which passes through the tubes is applied to the same purpose. If we calculate, on the other hand, how much fuel is necessary before the thick brickwork of common stills can be sufficiently heated, and how much heat is lost without being able to exercise any action on the still itself, the advantage of this apparatus will be placed beyond all doubt.

If so much of the distilled liquor has passed over as to convince one that the tube *h* can no longer be completely surrounded with the liquid, but that it must lie above it; in that case it will be necessary, in order that the ascending vapour may not be dilated too much by superfluous heat, to remove the connecting tube *i*, by which means the communication between the upper tube and the furnace is intercepted.

One defect, which this apparatus has in common with the usual still, is, that the grains and slimy parts burn very easily in the distillation of spirits from corn; yet one might think that this fault would not take place in this apparatus to the same extent as in the common way of applying the fire. The grains, on account of their gravity, sink to the bottom, on which, in the common still, the fire can exercise the strongest

strongest action; whereas in this still the bottom of the vessel is exposed to the least heat, and, consequently, is better secured from burning. It however cannot be denied, that by the heat of the furnace the burning sometimes may take place, especially when sufficient caution is not observed in managing the fire. Might not this fault be remedied by coating the furnace with a slight covering of slightly burnt clay or of plaster or gypsum, which, as they can hold a great deal of water, would guard against burning without interrupting much the action of the heat on the fluid?

Rectification of Spirit of Wine by the Water-Bath.

The apparatus destined for this purpose is almost the same in the principal parts as the above, only that during distillation the fire cannot act immediately on the fluid, but the fluid receives the heat necessary for its conversion into steam through the medium of water brought to a state of ebullition by the fire. As, in distilling spirit of wine made from corn spirit, the degree of heat cannot be so accurately graduated as to prevent the rising of aqueous vapour, which again renders the distilled liquor impure, the advantageous employment of the water-bath was long ago thought of, by which the highest possible degree of heat short of that of boiling water can be obtained. Demachy proposed for this purpose a copper kettle filled with water, in which the still was suspended. But it may be readily seen, that a much larger furnace than common would be necessary for this purpose, and consequently a larger proportion of fuel; I shall therefore describe an apparatus destined chiefly to promote a saving of fuel, and which, I am persuaded, is in nothing inferior to that proposed by Demachy.

Fig. 2. represents the apparatus without the cooling part, which is entirely the same as that already described for simple distillation. A is a wooden vessel bound round with iron hoops, to the bottom of which is fastened the before-described furnace *e*, with the ash-hole *f*, and the tube which serves for introducing the fuel, only that it is not applied to the middle but to the side of the vessel. The tube *g g*, destined to afford a passage to the air, and to convey off the smoke, does not consist,

consist, as in the former apparatus, of three pieces, but of one whole, which does not pass directly through the side opposite to that where the fuel is supplied, but extends at most only to two-thirds of the width of the vessel, where, by means of a knee, it turns up, and, by a second knee, is brought back to the other side. The whole furnace occupies, in general, only one-half of the bottom of the vessel, and is equal in height to only one-fourth of that of the vessel.

Over the furnace there is a copper box *b*, which occupies nearly two-thirds of the diameter of the vessel, as if in a semicircle, and completely covers the furnace as well as the tubes. Its height is about two-sevenths of that of the vessel. Strictly speaking, it has only one side; for the rest are formed by the sides of the vessel. From the upper part of it there proceeds a smaller longish box, *c*, which passes through the side of the vessel, and the upper part of which above the vessel is open. In the last place, on that part of the bottom of the vessel covered by the box there is a cock, *b*, which serves for conveying off the water. The remaining parts of the apparatus are not different from those of that above described.

When you proceed to distillation, the box is completely filled with water through the part that protrudes beyond the vessel; and when the water has been brought to a state of ebullition by the furnace, it can communicate to the spirit of wine the heat necessary for converting it into vapour. If you are desirous, however, to carry on the operation with as much accuracy as possible, it will be necessary to introduce a thermometer into the box *c*, which is open without; for, as the spirit of wine boils at 165 degrees of Fahrenheit, and water requires 212, the heat of the boiling water will be sufficient to raise the watery particles, though the fluid in the vessel will not acquire that temperature. It will be most advantageous to maintain the heat till the thermometer indicates from 170 to 175 degrees. If this degree of heat be continued without suffering it, by carelessness, to be raised higher, the spirit of wine, by the first rectification, will be pretty pure, and free from water; but it is necessary, towards the end of the distillation, to strengthen a little the degree of heat, as, without

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this precaution, the laft fpirit of wine that paffes over would be rendered impure by more or lefs water.

In common, corn fpirit has an unpleafant fmell and tafte, the primary caufe of which is carelefs management of the fire during the firft diftillation as well as during the rectification; as by thefe means, in the firft diftillation in particular, the grains readily become burnt, and confequently communicate that tafte and fmell to the diftilled liquor. The decompofition of the gelatinous matter of the grain feems alfo to contribute to this defect. The addition of lixivious falts, chalk, or charcoal, during the rectification, is the beft means to improve fpirits which have been hurt in this manner.

I am however of opinion, that it is poffible to preferve the corn fpirit in a perfect ftate during the firft diftillation. The fault generally lies in the fermentation of the wafh being too long continued. In general, the diftillation is not begun till the fermenting liquor throws up no more bubbles, and fhows itfelf perfectly tranfparent under the fcum: but it has been confirmed by feveral experiments, that the corn fpirit is obtained ftrongeft, and in the greateft abundance, when the mixture is fubjected to diftillation in its higheft degree of fermentation. If the fermentation be fuffered to obtain its utmoft term completely, a quantity of vinegar will be formed, which not only contributes a great deal to give the liquor an unpleafant tafte, but actually occafions a confiderable lofs of the faccharine matter neceffary for the production of fpirit of wine.

VII. *On the Preparation of Amber Varnifh, and the Application of it to different Kinds of ftained Wood.* By Nils Nyström, *Apothecary of Norrköping* *.

As furniture of foreign wood is in general expenfive, the ufe of the indigenous kinds of wood ought not to be neglected, efpecially when they are of a compact texture, have a fine grain, and are fufficiently hard. Furniture made of thefe

* From the *Tranfactions of the Academy of Sciences at Stockholm for the Year* 1797.

kinds

kinds of wood, after it has been well polished, may be stained of different colours, and then done over with linseed oil and amber varnish.

As my principal view, however, was to try in what manner different kinds of wood could be stained so as to retain their colour longest, and in what manner mahogany could be best imitated, I applied such a composition as I thought would best answer the purpose on the twelve following kinds of wood; *viz.* elm, oak, red and white beech, maple, pear-tree, wild hawthorn, white beam-tree, ash, alder, birch, and pine. Of these, the maple, birch, alder, and white beech, when stained with a solution of iron, had the greatest resemblance to mahogany. The appearance of the other kinds was various, according to the diversity of their colour and veins, and according as they were more or less porous, and imbibed a greater or less quantity of the stain.

The amber varnish prepared in different places is not always of equal goodness. This is owing to two causes: 1st, The careless manner in which the amber is melted; 2d, Because the linseed oil, being too much or too little boiled, acquires, in a greater or less degree, a drying property. A proof of amber varnish being good, is, if, when applied on any article in an apartment of the common temperature, it becomes dry within from twelve to twenty-four hours.

By the experiments which I made, I have found that the following process for preparing it is the best:—Put pounded and sifted amber into a pan of cast iron with a flat bottom, and let the amber be spread out at most to the thickness of an inch. Place the pan in an upright position over an uniform coal fire, and let it remain till the amber fuses and becomes liquid; then pour it out on a cold plate of copper or iron. When the amber has cooled, break it into pieces; and if it has a bright blackish-brown appearance on the fracture, and weighs about one-half of what it did before being melted, you may be certain that it is proper for the intended purpose. Particular attention must be paid to this circumstance; for, if the amber is fused too little, so that part of it remains unmelted, it will not be diſſolved by the varniſh; and if the operation be performed with a continued and ſtrong heat, it will be burnt and unfit for uſe.

When the amber is kept over a weak fire it will not melt, but becomes a ſort of blackiſh brown incruſtation, which alſo may be employed for varniſh provided it has not been too much or too little evaporated: it will be in the beſt condition when you obtain half a pound from a pound. If the amber is too little evaporated, it muſt be again put into the pan till it be reduced to the proper weight. The ſame thing is to be obſerved when you melt it; but the parts which are not fuſed muſt be picked out, in order to be afterwards alſo melted.

I have found that a pan with a flat bottom is better than one with a round bottom, becauſe the melting or evaporation is effected ſooner in the former than in the latter; for in the latter the amber lies thick in the middle, and is burnt at the bottom and ſides before it can be brought to melt or evaporate.

It is not neceſſary, for making varniſh, to pick out pure and tranſparent amber, but only the common yellow ſmall fragments, which may be procured for half the price of that in lumps. The earthy part, which is found in amber not of the cleareſt kind, ſeparates itſelf from the warm varniſh when it is ſuffered to ſtand ſome time before it is decanted.

Method of preparing Linſeed-Oil Varniſh.

One pound of well pulveriſed and ſifted litharge, four ounces of finely pounded white vitriol, and one quart of linſeed oil. Put theſe ingredients into an iron pan of ſuch a ſize that it may be only half full; mix them well together, and boil them till all the moiſture is evaporated, which may be known by a pellicle being formed on the ſurface, or by the barrel of a quill burſting when thruſt to the bottom of the boiling varniſh. Then take it from the fire and pour off the clear liquid, taking care to keep back the thick part which has depoſited itſelf at the bottom. While boiling, it muſt be ſtirred ſeveral times round, that the litharge may not fall to the bottom; but you muſt not ſtir it conſtantly, elſe ſuperfluous litharge

litharge would be diſſolved, and the varniſh become too thick.

The compoſition of amber varniſh conſiſts of half a pound of melted or roaſted amber, one pound and a half of linſeed-oil varniſh, and two pounds of turpentine oil. The amber and linſeed-oil varniſh are to be mixed together in a deep caſt-iron pan, of ſuch a ſize as to be only one-third full, and to be kept over a ſlow fire till the amber is diſſolved, which may be known by its ſwelling up: the operator, therefore, muſt have at hand a large copper or iron veſſel, that the varniſh may be held over it in caſe it ſhould riſe above the ſides of the pan, and to prevent the loſs that would thereby be occaſioned. When the varniſh is diſſolved, the pan muſt be taken from the fire; and when the mixture has cooled, the turpentine oil is to be poured into it, continually ſtirring it. Then let it ſtand ſome time, that the coarſe undiſſolved particles may depoſit themſelves at the bottom; after which pour off the clear varniſh, and, having ſtrained it through a piece of linen, put it into bottles for uſe.

In boiling the varniſh, care muſt be taken that it may not boil over or catch fire. Should this happen to be the caſe, it muſt not be extinguiſhed by water; for this mode would occaſion ſuch a ſputtering, that the operator would be in danger of having his face beſpattered with the boiling varniſh. The beſt method, therefore, is to cover the veſſel in ſuch a manner as to exclude the air, and with any thing that may be at hand, ſuch as a piece of wood, plate of iron, or any thing elſe that may cover the veſſel and extinguiſh the flame.

1. *Iron Stain.*

Eight ounces of iron-filings and thirty-two ounces of common aquafortis. The aquafortis muſt firſt be mixed with ſixteen ounces of water in a ſtone jar, and then a few of the filings are to be added, and well ſtirred round with an iron or wooden ſpatula. This preparation muſt be made in a chimney, becauſe the ſolution is attended with heat, efferveſcence, and the diſengagement of noxious vapours: it is of importance, alſo, that the jar ſhould be of ſuch a ſize as to prevent the matter from running over. After the greater part of the iron has been diſſolved in this manner, the ſolution will be of a yellowiſh brown colour. As ſoon as the mixture has cooled, pour it into a bottle placed in a pan, and let it ſtand a day or two, without being corked, over a warm ſtove; during which time ſhake the bottle frequently. The bottle being then ſuffered to cool, pour into it one pound three quarters of river water, ſtirring it well round, and leave it at reſt for a few minutes till the undiſſolved part ſinks to the bottom: then pour the ſolution into another bottle, and cork it up for uſe.

When this preparation has been carefully made according to the above directions, you obtain a yellowiſh brown ſolution, which may be employed for ſtaining. In caſe too much water, however, has been at firſt employed, or too weak aquafortis, the colour becomes dark brown; and therefore the addition of the aquafortis muſt be increaſed in that proportion which the ſolution requires, obſerving the above rules.

This ſtain may be applied to all kinds of wood except oak, which contains too much aſtringent matter, and therefore on the firſt application becomes almoſt black. It communicates to the various kinds of wood different colours, according as it is applied in greater or leſs quantity; ſuch as yellow, yellowiſh brown, and dark brown, with reddiſh brown ſtripes or ſpots.

As far as I have been able to aſcertain by experience, this ſtain is one of the moſt durable; it withſtands the air and rays of the ſun without changing its colour. I have ſpecimens of it which have been expoſed four years continually to the ſun and air, without their colour being altered.

2. *Brazil Wood Stain.*

Eight ounces of real Brazil wood, four ounces of alum, and four ounces of finely pounded reddle or red ochre. Pour over this mixture a proper quantity of water; and, having ſuffered it to remain in that ſtate twenty-four hours, boil it to a fourth part; then ſtrain it through a piece of linen cloth, and preſerve it in a glaſs bottle for uſe.

If one pound of the ſtain No. 1. be diluted with three pounds of water, immerſe ſome pieces of wood in it, and

depoſit

depoſit the whole in a warm place: the wood will imbibe the liquid to the depth of a quarter of an inch, and in the ſoft parts of the wood it will penetrate ſtill further.

It muſt however be remarked, that the ſtaining liquor muſt be diluted with a greater or leſs quantity of water, according as the wood is darker or whiter, more or leſs aſtringent. The wood muſt often be turned, and care muſt be taken that it may acquire an uniform and proper colour; after which it may be taken out and dried.

The like proceſs muſt be followed when the ſtain No. 2. is employed, only that the ſame attention is not neceſſary in regard to obtaining an uniformity of colour. This mode, however, can be uſed only on a ſmall ſcale.

Method of ſtaining Articles of White Wood with the above Compounds.

Rub the ſtain No. 1. over the wood with a piece of ſponge five or ſix times till it acquire a proper mahogany colour. While the liquor is applied, ſhake it or ſtir it carefully round, that the iron ochre, which has depoſited itſelf at the bottom, may be well mixed with the ferruginous ſolution that ſtands over it; and between each application of the liquid the wood muſt be ſuffered to become thoroughly dry. After this, it muſt be rubbed over once or twice with linſeed oil, letting it dry before the oil is applied the ſecond time. The more the wood is ſoaked with linſeed oil, ſo much the better; as in that caſe it does not imbibe ſo much of the amber varniſh, which only depoſits itſelf on the ſurface, and gives it a bright appearance.

When the wood has been thus rubbed over, and well dried, the amber varniſh muſt be applied in an uniform manner with a ſponge once or twice, or until it acquires a ſmooth ſhining ſurface. If the wood has been well done over with linſeed oil, one application of the varniſh only will be neceſſary; but on bedſteads, cheſts of drawers, chairs and other furniture which are expoſed to more uſe, it muſt be applied ſeveral times, and each time they muſt be well dried.

After the application of the varniſh, if any inequalities or lumps appear, they may be removed, after the article is dry, by means of a carpenter's rush, and a fresh coating of varnish applied to the place. For applying the varnish, I have found it most convenient to use a sponge; as by these means it can be laid on in a much more uniform manner than by a painter's brush, which, for the most part, leaves stripes or loose hairs behind it. The sponge with which the varnish is laid on, must, between each application, be well shut up in a wide-mouthed bottle, that the varnish it has imbibed may not be dried by the air, which would render it hard, and unfit for use.

The same process is to be followed with the stain No. 2. When applied to knotty birch wood or alder, the wood becomes undulated, because the liquor extends itself more crosswise. The case is the same with fat and knotty pine wood or fir; for the resinous spots do not so strongly attract the stain as the other parts. This, however, makes the appearance of the wood not disagreeable.

The amber varnish may be applied also over almost all oil colours except blue, which it would change to green. It is attended with this advantage, that it never cracks; as is the case in general with lack varnishes, and those prepared with spirit of wine.

I have found, also, that this varnish is proper for being applied on real gilding, because it makes no perceptible alteration in the colour; especially when laid on thin, and in an uniform manner. This object also is obtained, that the gilding may be cleaned by means of spirit of wine, or of soap and water, without sustaining any injury. For false gilding, however, it is improper; as it attacks it, and makes it rusty and green.

I have applied this varnish, in the above manner, to bed-posts, drawers, tables, and chairs, which at the end of two years were little or nothing changed in their appearance. When this varnish has become perfectly dry and hard, it withstands boiling heat and friction; and does not lose its splendour by the course of time, unless rubbed or scoured with sand. It prevents ink, or any other coloured liquid that may be spilt on furniture, from penetrating into the wood, and causes stains to be easily removed by washing with water.

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On this account, it is very proper for being applied on articles of mahogany. It renders the soft kinds of wood much harder at the surface.

This varnish used in this manner would not be expensive, as eight ounces of it is sufficient for a common card-table. Should the table be scratched, the injured part may be easily repaired by a new coating of varnish; and if it be new varnished every four or five years, it will always retain a beautiful and bright appearance.

Tables and other flat articles which require a great deal of polishing, when the stain and linseed oil have been applied, may be rubbed with a piece of pumice stone before they are done over with the varnish. This pumice stone must be made flat on one side, and must be free from sand, so as not to scratch the wood. As the softer kinds of wood swell up, for the most part, when the stain is applied, and lose their smoothness, rubbing them in the above manner with pumice stone is the more necessary.

VIII. *Comparative View of some dangerous Diseases, supposed to be occasioned by Insects, which prevail in Sweden, Russia, Siberia, and the adjacent Countries.*

[Concluded from Page 144.]

THE late Dr. J. J. Lerche, of Petersburgh, communicated to me the following account of some pestilential diseases which manifest themselves in the marshy districts around that city and in other places during dry summers, and which appear to me to be of the same nature as the Siberian disease before mentioned. But that I may not be accused of endeavouring to pervert facts for the purpose of supporting my own opinions, I shall here give the author's own words:

" In the year 1756, a very hot dry summer, unhealthful for men as well as cattle, took place after a very unhealthful winter. The disease which then prevailed continued till the harvest, and extended on one side to Moscow, and on the other to Livonia and through Finland. As soon as the senate were informed of it, Condoide, physician to the court, was ordered

ordered to dispatch physicians and surgeons to those places where their presence was most necessary. The chief complaint was respecting the mortality among the cattle; but in some places men and women were seized with the disorder, and died suddenly. I was obliged, therefore, to set out in all haste on the 26th of July, accompanied by Chemnitz the surgeon, for the village Tschudowo, at the distance of 116 wersts from Petersburgh, where we arrived next day. On inquiry, we learned from ensign Solopof, who commanded the post-people in that district, that, since the 17th of June, seven persons had died, in the course of from two to four days, with boils and tumours, and that two were still exceedingly ill: that, on account of the dryness of the land, several cows, but particularly horses, had died also. On receiving this intelligence, we caused the road on each side of the village to be barricadoed with planks, established guards at the barriers, and obliged the post and other travellers to pass round over the fields to the next station. We then entered the village to visit the sick, and found that one person had died in the night-time with carbuncles on the neck and legs. A woman who had a large carbuncle on her face was still alive. In the course of about a month, four cows and twenty-eight horses had died, all of which had tumours on the neck, breast, belly, or scrotum. I desired an officer and twenty soldiers to keep strict watch, in order to prevent all communication between the village and the neighbourhood; and we then took up our quarters on the rivulet called Keretz, in a house belonging to field-marshal Albrecht, which was then unoccupied. In a little time several more were attacked by the like affections, and died in the course of one or two days. They all had tumours on their neck, breast, and head, with black blisters like real carbuncles; but we observed in none of them either cold or heat: their pulse was scarcely perceptible; they lay some time senseless, and then expired.

" As the disease now began to extend itself to the neighbouring villages, I sent for more soldiers, and the senate dispatched lieutenant De Cominges with twenty men. The post-stage at the distance of a werst and a half from us, where there were twenty people and thirty horses, appeared

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to be in the greateſt danger. As the people and horſes began to be attacked with the like ſymptoms, and to drop off, no time was to be loſt; we therefore ordered the people and horſes to be removed to the woods, and to be cloſely guarded. Travellers were obliged to proceed, without ſtopping, to the next ſtage; and in conſequence of a repreſentation made by us on the ſubject, the whole road from Novogorod to Peterſburgh was afterwards ſtopped, ſo that travellers were forced to purſue the old road to Novogorod by Kraſnoe Selo. The preventing of all intercourſe with the ſurrounding country ſeemed the more neceſſary, as in other places, ſuch as Toſna, Luban, and from Soſninſkoi to Novogorod, ſeveral horſes and ſome men had already died. The road alſo by Iſchora and Novogorod was ſtopped; and this circumſtance occaſioned great alarm at Peterſburgh. Lieutenant Freyman, at Iſchora, ſuffered no perſon from us to approach that place; and would not receive our report, till we ſent a ſubaltern to repreſent to him the neceſſity of ſending off our information; which he at laſt did, but not till he had employed the precautions uſed in regard to letters from places where peſtilential diſeaſes prevail, ſuch as fumigating them and drawing them through vinegar.

" As all the means propoſed for checking this diſeaſe were of no avail, I at laſt conceived the idea of laying aſide all other medicines, and confining myſelf to cinchona alone. This remedy, indeed, produced an evident change; for the mortality ſoon after ceaſed. The patients acquired a ſtronger pulſe, perſpiration and evacuation by ſtool were promoted, and they ſoon recovered their appetite. The tumours in the face diſappeared, and the carbuncles dried up of themſelves. I applied to them neither ſalves nor plaſters; for I clearly ſaw that theſe tumours would not be affected by external means: veſicatories and ſcarification made them only more difficult to be healed. At length the evil ceaſed; yet care was taken to keep the diſeaſed ſeparate from the ſound, and to employ all the other precautions uſual in the time of the plague.

" On cloſer examination, I found that all thoſe attacked by the diſeaſe, without exception, had handled or touched diseased cattle, or had proceeded incautiously in interring them: but all those who had touched the cattle were not infected; for a great many horses in all the villages, and even in Petersburgh and on the road to Peterhof, died with the like symptoms. Yet in some of these places none of the people were attacked by the disease. In Tschudow, however, twenty-five persons were infected, of whom ten recovered. The diseased never communicated the infection to any of those who attended them, or lived with them; so that the disease could be no plague; nor were any buboes observed under the arm-pits, or in any of the other glandular parts. Of our people, who attended the sick and bound up their sores, not one was infected. The disease at length began to disappear among the people; and about the 20th of July, when a considerable quantity of rain fell, and the air became cooler, very few of the cattle were any longer sick: for this reason, the senate again opened the roads on the 1st of August, and caused the guards to be removed. Tschudow, where the disease had ceased among the people about the 18th of July, continued still shut four weeks longer; but on the 16th of August we obtained leave to return.

"In the year 1761, the heat during summer was exceedingly intense, and in the districts of Narva, Novogorod, and Kexholm, had occasioned a mortality among the cattle like that which usually takes place during hot summers in the marshy parts of Finland. Some of the people also were infected with exanthemata, or were attacked with carbuncles, or rather gangrenous tumours, and some of them died suddenly. As I at that time presided over the medical department, I dispatched, by command of the senate, doctors Ens and Dahl, together with some surgeons, to the place where the distemper was said to prevail; but about the beginning of September it entirely ceased.

"In the month of July 1764, information was brought to the senate from the district of Jamburg, that several cattle had died on the Schepelow estate; and that one man had been attacked by the disease, and died suddenly. When this intelligence was communicated to the empress, she immediately gave orders to baron Tscherkassof, president of the college of medicine, to ſend me thither, as had been the caſe before on ſimilar occaſions. On the 29th of July the preſident made me acquainted with this order, and ſent for Chemnitz the ſurgeon. We ſet out the ſame day, with one of the medical pupils and a few ſoldiers, and in the evening of the following day arrived at Ugoric, the place of our deſtination. I ſent notice of our arrival to Kudrawzof, the commiſſary at Jamburg, who had firſt communicated intelligence of the diſeaſe having broken out, and who joined us the day following. In conſequence of falſe information given to him by a peaſant, Kudrawzof had firſt ſtated, that after twenty-four head of cattle had died, a farmer named Iwan Petrof had loſt his horſe, which had dropped down and expired as he was riding home from the fields; that the farmer had been infected with the diſeaſe, which ſhowed itſelf in livid tumours on his face and breaſt, and that he had died the third day after. We learned, however, that the caſe was ſomewhat different. Petrof had indeed loſt a horſe, which he had ſent with his ſon in the beginning of June to Narva, where the animal died: Petrof himſelf had walked out into the fields; but when he returned his eyes were ſwelled, and his whole face, breaſt, and ſhoulders, being ſoon after affected in the like manner, he died on the fifth day. From theſe ſymptoms it would appear that his diſorder was a malignant eryſipelatous ſwelling in the head, which became ſtill worſe becauſe no remedy was applied. All theſe circumſtances were told us by his wife and ſon, and confirmed by the prieſt of the place. The diſeaſe, therefore, of which he died, could not be conſidered as infectious: three children whom he left, together with their mother, were in perfect health; but we cauſed them to obſerve the neceſſary precautions for eight days.

" In regard to the diſtemper among the cattle, it began on the 15th of June in three places. Twenty-two horſes and two cows had died at Ugorie, three horſes at Letoſchez, and five ſwine at Lätzi. The ſame ſymptoms were obſerved on all theſe animals, *viz.* tumours on the neck, breaſt, and ſhoulders, or under the belly. They had all died ſuddenly, and moſt of them within twenty-four hours after the diſeaſe had made its appearance. Only two of the horſes attacked by the diſeaſe eſcaped. The peaſants had ſcarified all the tumours with a red-hot iron, but without any benefit, though that proceſs had been ſerviceable on former occaſions. The diſeaſed cattle were able to walk about till the laſt moment.

"On receiving this information I ordered the ſtaroſt to bring us the diſeaſed cattle; and we reſolved, inſtead of ſcarifying the tumours, that ſetons ſhould be put into their breaſts, that they ſhould be driven about till they perſpired, and that they ſhould then be curried and carefully rubbed down. This, however, was not attended with the deſired effect. On the 7th of Auguſt one of the horſes died in the fields with a tumour on his breaſt. But this was the laſt: the cool wet weather which now took place contributed in a great meaſure to put an end to the malady; and we returned on the 11th of Auguſt.

"The great heat of the ſummer in the year 1766 occaſioned a mortality among the cattle in ſeveral places, but chiefly in the neighbourhood of Peterſburgh. One-half of the oxen died; all of them were attacked with a kind of glanders and ſore eyes. They generally died in the courſe of three days. On account of this diſeaſe I was obliged to proceed to the eſtate of prince Orlow, at Schunderowſka Muiſa, fourteen werſts from Kraſnoi Zelo, accompanied by a ſurgeon and ſeveral ſoldiers. This place is ſituated at the diſtance of eight werſts from Goreloi Kabak, on a conſiderable eminence, from which the capital may be ſeen. I was informed by the bailiff on the eſtate, that twelve horſes had died, all with tumours on the breaſt and ſcrotum, in the courſe of a few hours. Two horſes and three cows died during my ſtay in the place. The preceding winter, the itch had attacked a great many of the horſes; all of whom the bailiff, with the count's conſent, had cauſed to be ſhot.

"On the 13th I was informed by the ſtaroſt, that at the diſtance of ſix werſts, two cows out of twelve, belonging to a farmer, had died ſuddenly without any external ſymptoms of diſeaſe, and that the farmer's wife had at the ſame time tumours on her head. Though I received this information in the evening, I immediately proceeded thither; but did not find

find the woman at home. Being brought to me next morning, I found that she had actually a black tumour not unlike a carbuncle. We gave her the necessary medicines; and, as she found herself considerably better, we made preparations next morning to return.

"When we got back to Goreloi Kabak, we were informed by the postman, who three days before had driven us through Schunderow, that the same day he had been attacked with a tumour above the right eye. In the middle of it there was now a black ulcer not unlike a carbuncle, but unattended with heat. I administered to him the proper medicines; but I received no further account of him, though I gave directions to the bailiff for that purpose."—Thus far Dr. Lerche.

I shall leave it to intelligent physicians to determine whether this disease, so dangerous in particular to horses, which prevails only during dry and hot summers in Ingermannland and Finland, be not of the same kind with that observed in Siberia; and whether it be not free from all dangerous consequences in regard to infection, and may be cured by the common topical means. The use of bark, which Dr. Lerche found beneficial, ought undoubtedly to be employed for curing this disease, and preventing the gangrene, which so suddenly takes place, from spreading. It is to be ascribed, in all probability, to a like power of checking gangrene, that the Calmucs have found it of benefit to give to horses, when attacked by these tumours, as much as they can eat of that statice which I have called *trigona* *. They, however, assert too much when they say, that a horse which has been once cured by this astringent plant will never be again attacked by the disease.

* Pallas *Reise*, Part III. p. 519, note. Gmelin's *des Jüngern Reise*, Part II. plate 34.

IX. Ob-

IX. *Observations on the Transition of animal or absorbing Earth to the State of calcareous Earth**. *By* B. G. SAGE, *Director of the first School of Mines.*

THE ashes produced by burnt bones are white, and composed of more than two-thirds of animal earth, and of a part of that same earth combined with phosphoric acid. They produce by lixiviation a pretty large quantity of natron, from which the fire, according to all appearance, has separated the phosphoric acid.

Bone-ashes ought to be considered as a phosphoric salt with an excess of animal earth. These ashes, deprived of their natron by lixiviation, are insipid, and insoluble in water; but the phosphoric salt becomes soluble if it be disengaged from the excess of animal earth with which it is combined, and which may be effected by means of the vitriolic acid. Fourteen parts of this acid concentrated are requisite for twenty-four of bone-ashes: the more they are calcined to whiteness, the larger will be the quantity of vitrifiable phosphoric acid extracted from them. It is contained in them in the proportion of a third. This salt, composed of animal earth and phosphoric acid, may be decomposed by fixed alkali, which precipitates from it an insoluble calcareous phosphorous salt. This character of insolubility serves to show that there is a difference between the animal earth and the calcareous earth, since the phosphoric acid salt, with a base of animal earth, is soluble in water. When vitrified, it produces a pellucid mass of a light blue tint; while the phosphoric salt with a calcareous base produces by fusion a semi-transparent white glass, crystallised at its surface into a kind of dendrites.

Six ounces of vitrifiable phosphoric acid salt, dried into a soft paste, required four ounces of fixed alkali of tartar to be decomposed.

* Calcareous earth differs from that of bones in being composed of *acidum pingue* and an excess of animal earth. Calcination reduces it to lime: but this is not the case with the earth of bones, which is a phosphoric salt with excess of animal earth. The name of *phosphat of lime*, given to the earth of bones, is consequently improper.

decomposed. The white precipitate, washed and dried, weighed one ounce six drams.

The leys, when evaporated, produced three ounces five drams of phosphoric tartar. This salt, when exposed to heat in a crucible, liquefies, swells up, and becomes fused. If it be poured on a plate of iron, it has the transparency of glass as long as it is warm, but becomes white and opake on cooling. This salt when fused is sapid, and soluble in water: it loses by fusion three-eighths of its water of crystallisation.

In the decomposition of the vitrifiable phosphoric acid salt by fixed alkali, more than half of the alkali is decomposed; since there are obtained no more than three ounces five drams of phosphoric tartar, which contain three eighths of water.

The portion of *acidum pingue*, the principle of the fixed alkali *, which modifies itself into mephitic acid gas, is very small. This *acidum pingue* combines with the animal earth, and constitutes calcareous earth, which saturates itself with phosphoric acid, and forms the insoluble salt above mentioned †.

X. *Account of a new, easy, and more convenient Process for resolving Minerals by Alkalies.* By M. LOWITZ ‡.

THE decomposition of mineral bodies is, without doubt, one of the most difficult and most laborious operations of chemistry, and which must still become more difficult and complex in proportion as we are acquainted with a greater number of new and component parts in fossil bodies; for every new discovered earth or species of metal requires after-

* When fixed alkali is employed for the precipitation of any substance, there is always a part of the alkali decomposed, and the acid and earth contained in it form part of the precipitate. Of this, mercury disengaged from the nitrous acid by fixed alkali affords an instance. If this precipitate be distilled, it is sublimated into a mercurial salt *sui generis*. The earth of the alkali remains at the bottom of the retort.

† In the above curious paper the author's own nomenclature has been followed.—EDIT.

‡ From Crell's *Chemical Annals*, 1799, Vol. II. part 10.

wards,

wards, in the examination of other minerals, that our operations should be multiplied. As this occasions a great consumption of time, it may at length happen that the chemist, in the whole course of his life, especially as human life is so short in general, would not be able to examine, with the requisite accuracy and patience, but a very small number of minerals. The worst consequence of this would undoubtedly be, that our progress in the study of nature would be much retarded.

This inconvenievce, however, might be remedied, if chemists in their researches would endeavour to shorten and simplify their operations as much as possible; and, when they discover easier and shorter processes, if they would make them known in a chemical journal, which might be entitled, Collections towards facilitating Chemical Decompositions. After this short digression, I shall now proceed to the main object of this paper, which, I hope, will be considered a contribution towards a work of the above kind, not unimportant, and which may be thought worthy of some attention.

It is well known that most fossils, particularly those of the earthy or siliceous kind, to be rendered soluble by acids, and consequently fit for being analysed, require a preparation with fixed alkalies; an operation which is called *resolution.*

It has hitherto been believed that this resolution could be effected only by reducing the fossil to an exceedingly fine powder, mixing it with alkaline salts, and then keeping it for several hours in a state of ignition. Formerly, the alkaline carbonats were made choice of for this purpose, till professor Klaproth, some years ago, showed that the pure or caustic alkalies were preferable. This meritorious chemist, who has contributed so much to the improvement of our chemical knowledge, recommends that the fossil, finely pounded, should be first boiled to dryness with caustic alkali, and that the dry mass should then be kept at a red heat for several hours in a silver crucible, taking care, however, not to melt the crucible. This method has hitherto been considered, both by myself and by other chemists, as the best, and on that account has been followed.

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For some time past, however, this method of keeping the mass in a state of ignition has appeared to me objectionable; and for the following reasons:

1st, It happens not unfrequently that the mass itself is really fused, by which means the softening it afterwards by water is rendered much more difficult.

2d, By the least accident in regard to the proper management of the fire, there will be great danger of the crucible itself being fused,

3d, If the chemist, by pursuing the above process, discovers in any fossil a new earth or metal, it is often objected to him by other chemists, that the object of his supposed discovery may be nothing else than a product of some peculiar modifications of the component parts of the fossil subjected to examination, effected by the action of caloric.

4th, As it is an acknowledged truth, established from a comparative view, that the wet way deserves, in many respects, to be preferred to the dry way, it must be highly unpleasant on all occasions, especially in the commencement of the analysis of a mineral body, to be obliged to proceed in the dry way.

On this account, therefore, I resolved to make some experiments on the resolution of fossils in the wet way; and the result exceeded my expectation; as I was able to perform this operation (which hitherto, on account of the strong fire requisite for the purpose, was obliged to be performed in a laboratory, or, at least, under a chimney,) not only by the help of the weak flame of a very simple spirit-of-wine lamp, but also in a more convenient, more perfect, and much more certain manner than ever was effected in the dry way.

My whole apparatus for this purpose, and which may be carried in the pocket, is as follows:

1st, A small cylindric furnace for placing the lamp in, four inches high and three in diameter, made of tin-plate, furnished with a door, and with draught-holes at the upper edge. The cover, which fits closely to it, has a round hole in the middle capable of admitting the crucible. This hole, to prevent the crucible from falling through, must have a small rim bent inwards.

2d, A fmall lamp capable of containing one ounce and a half of fpirit of wine, furnifhed with a cover, a pair of fmall tongs, and cotton to ferve as wicks.

3d, A crucible capable of containing from two and a half to three ounces of fluid, together with a fmall fpatula for ftirring round the matter, both of the fineft filver.

The procefs of refolution is performed in the following manner:—Put the foffil, reduced to a very fine powder, into the crucible, and mix it with the ufual quantity of cauftic alkali diffolved in diftilled water; then light the lamp and boil the whole, taking care to ftir it often round. As foon as the mafs is dry, pour hot [diftilled] water over it; then boil it to drynefs again; and repeat this procefs, fhould the refractory nature of the foffil require it, for two or three times more, ftill adding a like quantity of warm water.

If large tough bubbles arife during the procefs of boiling, it is in general a fign that the operation will be attended with a good refult.

I found the experiment fucceed in this manner with feveral of the hardeft kind of foffils; fuch, for example, as the Siberian beryl, quartz, feld-fpar, the topaz, and even the fapphire: the laft, however, was the moft refractory; but it was equally fo in the dry way, of which I have had an opportunity of being fully convinced.

Argillaceous earth, and alfo the metallic oxyds, for example thofe of titanium, tin, iron, &c. (which, as is well known, lofe their property of being diffolved in acids, either by too ftrong drying, or by too high a degree of oxydation), by this eafy treatment, in the wet way, with cauftic alkali, may be rendered again foluble, in the courfe of five minutes, merely by once boiling them to drynefs; an effect which, it was formerly believed, could not be produced but by bringing them to a ftate of ignition with alkalies.

The chief advantages of this new method of refolving foffils in the wet way, are briefly as follow:

1ft, The whole operation may be performed with the greateft eafe and convenience on a table in any apartment, even while reading a book.

2d, All the concomitating phenomena, and thofe in particular which relate to the frequent change of colour, may be observed with the utmost accuracy.

3d, The operator has it perfectly in his power, when the fossil operated upon is highly refractory, to promote the process by the immediate addition of more caustic ley or water, without the least interruption, but which cannot be done in the dry way without great loss of time.

4th, By this method the operator, if he finds a new earth or metal, obviates entirely the suspicion of its arising merely from some modification, or unmasking of component parts already known, effected by the strong heat.

5th, In the dry way, the operation often requires three, and even four hours; but by the proposed wet way it may be effected, with much more certainty than before, in one hour and a half at most.

6th, The disagreeable circumstance, attended with many difficulties, of the solution of the fossil subjected to the operation forming a kind of jelly with the acid, is entirely obviated by my method; at least, as I have hitherto experienced in all my trials.

7th, The silver crucible besides, being free from the danger of melting, as was formerly to be apprehended, sustains very little damage by being used.

XI. *Cursory View of some of the late Discoveries in Science.*

[Continued from Page 152.]

VAUQUELIN, who has given so many excellent analyses of minerals, has described the processes he employed. The stone intended to be analysed is first reduced to powder; it is then mixed with three times the quantity of potash, and, the whole being put into a crucible, is exposed to a heat strong enough to convert it into a frit. When the vessel is taken from the fire, the frit is thrown into distilled water; and, when the matter is dissolved, different chemical means are employed to discover the substances contained in the fossil. The following are the characters by which each of the eight earths already known, and which may be contained in mineral ſubſtances, can be diſcovered:

1ſt, *Silex.* It diſſolves in the cauſtic alkalies, eſpecially by the help of heat, from which it is precipitated by acids; an exceſs of which rediſſolves it. The ſolution of this earth in acids forms itſelf into a jelly by evaporation, and when it has been dried it becomes inſoluble in theſe menſtrua: this furniſhes an excellent method for ſeparating it from other earths. In this ſtate it is white, granulated, dry to the touch, and perfectly inſoluble.

2d, *Alumine.* It diſſolves alſo in fixed alkalies and acids, from which it does not ſeparate itſelf, as ſilex does, by evaporation. It retains the water with force, and its parts become agglutinated and are brought into cloſer union by heat. In this ſtate it is white, ſemi-tranſparent, and adherent to the tongue. The combination of alumine with the ſulphuric acid gives, by the addition of a few drops of the ſulphat of potaſh, octaedral cryſtals of alum.

3d, *Zirconia.* It is not attacked by the cauſtic alkalies; acids, however, diſſolve it when it is very much divided, but not when it has been ſtrongly calcined; with the ſulphuric acid it forms an inſoluble ſalt; it adheres weakly to all the other acids, which it abandons at a very moderate degree of heat: in a word, when very much divided, it diſſolves in the alkaline carbonats, when completely ſaturated with carbonic acid. When it is pure, and ſtill contains water, it has a light ſtraw colour, is ſemi-tranſparent, and has a vitreous fracture like gum arabic; but when it has been calcined in a crucible, it is white, opake, rough to the touch, and diſſolves only with difficulty in acids.

4th, *Glucine.* This earth diſſolves, as alumine does, in acids and alkalies; but it diſſolves alſo in carbonat of ammonia, and furniſhes no alum with the ſulphuric acid or with potaſh. The ſalts which it forms with the acids are highly ſaccharine: when dry, it has a beautiful white colour, is exceedingly light, and ſoft to the touch, and has no ſavour. Its parts are not agglutinated by heat as thoſe of alumine are.

5th,

5th, *Magnesia.* It unites with all the acids, and forms with them salts exceedingly soluble and bitter: it is not precipitated from its solutions by carbonat of potash completely saturated with carbonic acid, and ammonia precipitates it only in part: it is not at all soluble in caustic alkalies, and it has a great affinity for alumine. When pure it is of a white colour, is exceedingly light, has no taste, and is insoluble in water.

6th, *Lime.* It combines with acids, and forms with them salts sometimes soluble and sometimes insoluble: it does not dissolve in alkalies: it dissolves in water: its solution is rendered turbid by the carbonic acid, but not at all so by the sulphuric acid. It is not precipitated from its solutions by ammonia, and it precipitates all the preceding earths. In its state of purity it has an acrid caustic taste, becomes hot with water, and a solution of it in that fluid does not crystallise.

7th, *Strontian.* It readily combines with acids, and with the sulphuric acid forms a salt very little soluble: it dissolves very abundantly in warm water, and its solution crystallises on cooling in very beautiful crystals, which form groupes almost like those of sal-ammoniac. The sulphat of lime produces a precipitate in a solution of it: its combination with the muriatic acid dissolved in alcohol burns with a purplish flame. This earth has an acrid taste, and with water becomes strongly heated.

8th, *Barytes.* This earth has many properties common with strontian, from which it cannot easily be distinguished but by its being more soluble in cold water and by its combination with the muriatic acid, being only a little soluble in alcohol, to the flame of which it does not communicate a purplish colour: it crystallises from its solution on cooling. It has an acrid taste, becomes hot with water, forms an insoluble salt with the sulphuric acid, and decomposes the alkaline sulphats and carbonats as strontian does, but with phenomena which to those not much habituated to chemical labours differ by imperceptible shades,

The

The ſame chemiſt has analyſed various ſpecies of the emerald. He obtained from the gray:

Silex - - -	50·0
Alumine - -	07·0
Magneſia - -	08·0
Lime - -	17·0
Oxyd of iron - -	14·5

The green emerald gave:

Silex - - -	51·0
Alumine - -	13·5
Magneſia - -	05·0
Lime - - -	14·5
Oxyd of iron - -	08·0
Oxyd of copper -	00·5
Oxyd of chromium	04·0

The green and whole emerald of Corſica gave:

Silex - - -	50·0
Alumine - -	11·0
Magneſia - -	06·0
Lime - - -	13·0
Oxyd of iron - -	05·5
Oxyd of copper -	01·1
Oxyd of chrome -	07·5

He concludes that this ſubſtance is coloured green by chrome.

He has analyſed the pyroxene of Ætna, and obtained from it:

Silex - - -	52·00
Lime - - -	13·20
Alumine - -	03·83
Magneſia - -	10·00
Oxyd of iron -	14·66
Oxyd of manganeſe	02·00
Loſs - - -	04·49

The granatite of Saint-Gothard, or the ſtaurolite, gave:

Alumine - -	47·06
Silex - - -	30·59
Oxyd of iron -	15·30

Lime

Lime	- - -	03·00
Loss	- - -	04·50

The farinaceous chlorite gave:

Oxyd of iron	- -	43·3
Silex	- - -	26·0
Alumine	- -	15·5
Magnesia	- -	08·2
Muriat of potash	-	02·0
Water	- - -	04·0

The tourmaline of Ceylon gave:

Silex	- - -	40·0
Alumine	- - -	39·0
Lime	- - -	04·0
Oxyd of manganese	-	02·5
Oxyd of iron	- -	12·0
Loss	- - -	02·5

The zeolithe of Feroe gave:

Silex	- - -	50·24
Alumine	- -	29·30
Lime	- - -	09·46
Water	- - -	10·00
Loss	- - -	01·00

Lelievre has given an excellent description of the lepidolite. It appears that it was first found by the abbé Poda, of Neuhaus, and that Born gave the first description of it in the *Annales de Chimie* for 1791. That described by Lelievre is of a lilac colour, and seems to be composed of small brilliant laminæ which might be taken for mica. Its specific gravity, according to Haüy, is 2·8549. By the blow-pipe it is easily fused, and without swelling up. It gives a white semi-transparent glass filled with blisters. An analysis of it gave:

Silex	- - -	54
Alumine	- - -	20
Fluat of lime	- -	04
Oxyd of manganese	-	03
Oxyd of iron	- -	01
Potash	- - -	18

Vauquelin

Vauquelin has analyfed the green feld-fpar, called by fome the amazon ftone. It gave:

Silex	- - -	62·83
Alumine	- - -	17·00
Lime	- - -	03·00
Oxyd of iron	- -	01·00
Potafh	- - -	16·00
Lofs	- - -	03·13

Here then we have potafh extracted from the lepidolite and feld-fpar, fubftances of the primary ftrata.

Dr. Kennedy has extracted potafh from pumice ftone. The analyfis he gives of it is as follows:

Silex,
Alumine,
Oxyd of iron,
Potafh.

The lava of Ætna and bafaltes gave him foda or natron.

We have therefore feven mineral fubftances from which fixed alkalies have been extracted, *viz.*

1ft, The aluminite of Tolfa, according to Monnet and Bergmann, contains potafh.

2d, The filiceous depofits of Geyer contain foda, according to Black.

3d, The leucite, according to Klaproth, contains potafh.

4th, The lepidolite, according to Vauquelin and Klaproth, contains potafh.

5th, Green feld-fpar, according to Vauquelin, contains potafh.

6th, Pumice ftone contains potafh, according to Kennedy.

7th, Lava and bafaltes contain foda, according to Kennedy *.

It has been propofed, therefore, to make of the ftones which contain alkalies, a genus under the name of the *alkaliferous.*

* When we confider how abundant muriat of foda is in nature, a fubftance which, with the aid of heat, forms a good flux for many fubftances, we need not be furprifed at finding foda in any volcanic production.—Edit.

Napione

Napione has described a calcareous stone found in a vein of the primitive mountains of the valley of Sessia. It is white, has a pearly splendour, and produces no sensible effervescence with acids. When calcined it makes good lime, when rubbed it becomes phosphorescent. It gave when analysed:

Lime - - -	31·79
Magnesia - -	10·41
Oxyd of iron - -	01·00
Carbonic acid - -	42·00
Water - - -	12·00

Poiret has made known a kind of fossil wood, which he found near Laon. The inside of this wood was covered with a stony substance exceedingly hard, siliceous, and deposited in laminæ; but the external strata, those which appear to have belonged to the *liber*, and even to the *aubier*, appeared in long, capillary, brittle filaments, which, when struck by the contact of the air, assumed the form of woolly flakes. They were interwoven in the same reticulated form as that which they affect in the living timber. In the last place, the part which constituted the *epidermis* had become a very fine black carbonaceous powder, which stained the fingers, and had all the characters of charcoal.

Pontier has sent to the school of mines at Paris, a new mineral substance found *à la Bastide de la Carrade*, near Cassin, in the department of the Var. It is of a dark brown colour almost like that of brown blend, and has metallic splendour: its specific gravity is 4·0326. It is difficult to be fused by the blowpipe. The glass it produces is of a dirty greenish colour. Tassaert has analysed this substance, and found that it contains:

Chromic acid - -	63·6
Oxyd of iron - -	36·0
Loss - - - -	00·4

But a new analysis of it by Vauquelin and Tassaert has given:

Chromic acid - -	43·0
Oxyd of iron - -	34·7
Alumine - - -	20·3
Silex - - - -	02·0

 Lelievre

Lelievre has defcribed a mine of uranite difcovered in the department of Saone and Loire.

The *perlftein* of Werner (pearl-ftone) is found in alternate ftrata with argillaceous porphyry, refting on trapp, on the road between Tokai and Kereftour. This is what Fichtel calls *volcanic zeolite.* This fubftance is of a tarnifhed blue colour, has a refinous fracture and a pearly appearance. The interior of it is formed by the union of feveral fmall grains of black obfidian ftone inclining to blue, each furrounded by a pearly pellicle of that fubftance. It is tranflucid, fragile, and pretty light. Its gravity, according to Haüy, is 2·540.

Launoi has brought from Carboneira, in Spain, a fubftance analogous to the above, which, in imitation of the Germans, he calls *lucbs-faphir.*

Werner is of opinion that this ftone is not volcanic; he places it between the quartz and the hornftein: the hornftein is a kind of fubftance which the French, in imitation of the Swedes, call *petro-filex.*

Humboldt is of opinion that the pumice ftone found on the peak of Teneriffe is only obfidian ftone decompofed by fire. According to this naturalift, its origin cannot be afcribed to feld-fpar. He collected a great many fragments, which were half obfidian and half fibrous pumice ftone: they were of a black olive colour. He faw fpecimens of the like kind in the cabinets of Madrid.

Fortis fays that the Venetian fhips bring from the ifland of Candia, as ballaft, a kind of ftones among which are obferved large blocks of black glafs, or obfidian, exceedingly pure, without any mixture of foreign bodies, which are vifibly changed into white pumice ftone by a gradual tranfition, fo that there are fome fragments part of which is obfidian and part pumice ftone.

The fame thing is obferved in the pure obfidian of Lipari, and the ifland of Procida near Naples.

Klaproth has analyfed a pumice ftone, which gave:

Silex - - -	77·50
Alumine - - -	17·50
Oxyd of iron - -	01·75

Oxyd

Oxyd of manganese	-	0·1
Loss	- - -	3·25

We have, however, seen that Dr. Kennedy extracted from it potash.

Vauquelin has obtained potash also from feld-spar.

I have described in my Theory of the Earth * a stone of a rose colour crystallised, almost hexaedral, with a triedral pyramid, which was sold to me as coming from Dauria, and for that reason I gave it the name of *Daurite*. It has been examined also by Hermina, who calls it *Siberite*. He observed that it is pyro-electric, like the tourmalin. Garin and Pecheur analysed it, and found that it contains:

Alumine	- - -	48
Silex	- - -	36
Lime	- - -	03·5
Oxyd of manganese	-	09

Abilgaard's *fluat of alumine*. This is a new stony substance found in Greenland. It is formed of whitish semi-transparent laminæ which subdivide themselves into right prisms, which, according to Haüy, appear rectangular. Its specific gravity, according to the same author, is 2,949. It fuses by the flame of a taper, and flows like glass by the blowpipe: for this reason it has been called at Copenhagen *cryolite*. It is composed of alumine and fluoric acid.

Melarite. Klaproth has given this name to a stone hitherto called the black granite of Frescati. The form of its crystals is a dodecaedron, with rhomboidal planes truncated on the twenty-four edges, which gives it thirty-six facets. Vauquelin has analysed it.

Sage has published several memoirs.

1st, This chemist has confirmed, that what has been considered as a white volcanic glass is only a kind of chalcedony.

2d, He has obtained beautiful crystals of gold by reducing it by ether.

3d, He has analysed the ashes of vareeh, in which, he says, he found only a little soda; but an anonymous author

* This article is taken from the *Journal de Physique*, of which Delametherie, who here speaks, is the editor.—EDIT.

 informs

informs us that he extracted from it a great deal; he thence concludes that those sent to Sage were not pure.

4th, He has shown that antimony strongly heated by the blowpipe inflames spontaneously.

5th, He has analysed *pulvis stercoreus*, and extracted from it:

Mould - - - - -	16·
Animal matter - - - - -	16·
Vitriolic salt and calcareous marine salt -	2·
Calcareous earth - - - -	36·
Divided quartz - - - -	12·
Iron - - - - - - -	1·
Loss - - - - - - -	17·

6th, The Dutch red precipitate or red oxyd of mercury gave him nearly a third of minium or red oxyd of lead.

7th, He considers calcareous earth as a combination of lime with *acidum pingue*, as Meyer had asserted before. The alkali, rendered caustic by lime, contains, according to him, a sixth part of *acidum pingue*. A hundred grains of this acid, saturated with the phlogiston of charcoal, produce 120 cubic inches of inflammable air. Scheele also extracted inflammable air from caustic alkali.

8th, He has examined a kind of argil near Mans, which he considers as a decomposition of feld-spar.

9th, In examining what took place when the Odeon was burnt, he has shown, that in all conflagrations there is disengaged a great deal of inflammable air which fills the building on fire. This air does not inflame as long as it is kept from coming into contact with the external air, but as soon as the latter penetrates to the interior of the building the inflammation becomes general; which makes people erroneously conclude that the building has caught fire in different places. He found on this occasion a great deal of sulphur, which he considers as a product of the decomposition of the plaster.

10th, He has examined the calcareous *ludi* of Dauphiny. In their fissures they contain beautiful crystals of quartz.

[To be continued.]

XII. De-

XII. *Description of a Machine by which the Strength of Horses may be employed to knead Paste for baking Bread.*

THIS machine might be useful in armies and in the hospitals of large cities, as it would produce a great saving in regard to the number of hands employed. Two horses, going round in a circle, give motion to a kind of pestles, which, by their action, continually knead the paste.

Fig. 1. (Plate VII.) represents a plan of the tub which contains the paste. This tub, which is six feet in diameter in the inside, consists of thirty-eight staves, and rests on a wooden frame in the form of a cross, the extremities of which are seen at A, B, C, D. (Fig. 1.) The bottom of the tub is formed of strong planks, the structure of which will be seen in the next figure.

Fig. 2. represents a transverse section of the tub passing through the centre; E, the edge of the staves thicker at the bottom than the top, and sunk into the lower cross, F. G, G, G, shows three iron hoops for binding together the staves. The bottom, H, supports the wooden cylinder, I, which is in the centre of the tub, fixed in its place by three iron bolts similar to that marked K, and separated from each other as expressed by the points L, Fig. 3.

M, an iron arbor of the form of an inverted cone, the lower point of which rests on the brass bed, N, and is kept perpendicular by a brass collar sunk into the upper end of the wooden cylinder, having the form indicated in Fig. 3, with its three ears, made fast by the screws P, P, P.

This brass collar is covered with a thin plate of iron marked Q, which receives the heads of the large bolts, K, which also pass through a similar plate, R, in the bottom of the tub, H; the bottom plate receives the nuts of the bolts.

S, Fig. 2, is the upper cross, which turns, fixed to the iron arbor, M, and to which are suspended the four wooden pestles, two of which are visible in this section at T and V, and of which the relative positions are indicated by X, X, X, X, Fig.

Fig. 4. These pestles or kneaders are let into the upper cross, and kept fast by means of horizontal bolts, Y, Y, fig. 2.

Z, two arms, each twelve feet in length, to the extremity of which are suspended the apparatus to which the harness for the horses is attached.

Fig. 3. This figure of the cylinder in the centre of the tub, from which the parts were detached in the explanation of Fig. 2, is drawn on a scale double to that of the other figures to render it more distinct.

Fig. 4. Plan of the upper cross above explained; where is seen, by the punctured circular segments, the path described by the kneaders in their revolution, the external angles of which are covered by each other about eight lines, in order that, succeeding each other in their circular progress, they may each take up a small portion of the paste which the preceding has kneaded.

NEW PUBLICATIONS.

The Geographical System of Herodotus examined and explained, &c. By James Rennell, *F. R. S. &c.* Nicol, Pall-Mall, 1800: 4to. 767 Pages.

By the imperfection of astronomy, the comparative infrequency of the mutual intercourse of nations, the want of the compass, the narrowness of the sphere of knowledge and civility, and by various other causes, the antients were long hindered from making any considerable proficiency in geographical science.

The writings of Herodotus, Strabo, Pausanias, Xenophon, Cæsar, Arrian, Pliny, the Roman itineraries, and the great work of Ptolomy, contain the principal part of those facts in antient geography which have been preserved for our information.

They are, though imperfect, yet, for many reasons, highly interesting. The particulars of place, as of time, render the other facts of history more authentic and more capable of impressing the imagination. An acquaintance with the scenes

ſcenes of the moſt remarkable events of antient hiſtory, brings us to a knowledge of many circumſtances of thoſe events which we ſhould not have been otherwiſe able to diſcover. Facts the moſt important in the phyſical hiſtory of the globe are to be learned by a compariſon, which without ſome ſkill in antient geography would be impoſſible, of the antient with the modern appearance of all that is moſt ſtriking on its ſurface. Dates of time are not to be underſtood unleſs they be connected with thoſe of place, which it is the object of geography to aſcertain. Aſtronomy, natural hiſtory, natural philoſophy, geometry, navigation, and all the principal arts of civil life, are, in truth, intimately related to the ſcience of geography, either as ſupplying its facts and principles, or as deriving their beſt lights from it. A certain diſcovery of the ſcenes of ancient hiſtory may even ſeem, to a keen and perſpicacious mind, to annihilate the ages that have intervened, and to give us all the privileges of contemporaries of thoſe earlier races of men whoſe progreſs in civility we delight to contemplate. But for a knowledge of their geography, more than half the information which we find in the writings of the antients would be loſt to us.

Yet there is nothing more difficult in ſcience or erudition than to acquire a thorough knowledge of the facts of antient, in clear ſatisfactory compariſon with thoſe of modern, geography. The notions of the antients reſpecting the figure of the earth, and its relations to the heavenly bodies, were not, even after the time of Hipparchus, ſuch as to afford that accuracy in the diviſions of latitude and longitude without which local diſtances can never be very diſtinctly recorded. Their meaſures of diſtance were various and unfixed; and they truſted often, without actual meaſurement, to uncertain computation. The names which they apply to places vary or agree in different writings, with a very troubleſome irregularity. And the changes which have taken place on the face of the globe ſince our favourite antients wrote, have been ſo very conſiderable, that theſe, above every thing elſe, render all the parts of antient geography in the utmoſt degree difficult and obſcure.

We have accuſtomed ourſelves, therefore, to reſpect the labours

labours of an Ortelius, a Cluverius, a Cellarius, a D'Anville, as highly beneficial alike to learning and to ſcience. We have been pleaſed with the reſearches of Saint Croix and of Vincent. We are grateful for the inſtruction which Mr. Rennell has already given concerning the antient geography of India. And we open, with eager curioſity and reſpect, this new volume, in which he attempts to illuſtrate the geography of the father of profane hiſtory.

This, it ſeems, is but a part of a great work on antient geography, upon which Mr. Rennell's leiſure has been for ſeveral years employed. We doubt not but the favourable reception of the preſent volume will encourage him to give the remainder of his labours on this plan, with as little delay as poſſible, to the public.

He begins with ſome prefatory matter on the general character of Herodotus as a geographer and hiſtorian: that Herodotus meant to produce in his work an univerſal hiſtory: that he viſited in perſon as wide a range as poſſible of the inhabited world, and, in regard to what he could not perſonally viſit, uſed the utmoſt diligence of inquiry: that his work faithfully repreſents the beſt truth of hiſtory which could in that age be known: that it was the ignorance alone of the Romans which led them to impeach his veracity: that modern inveſtigations have tended continually to vindicate, even in minute particulars, the truth of his narrative; are the chief facts which Mr. Rennell here ſtates.

The length of the Grecian ſtadium, and eſpecially of that of Herodotus, next engages the reſearches of Mr. Rennell. He aſcertains the length of the ſtadium employed by Herodotus in his ſtatements of diſtance, to have been the $\frac{1}{732}$ part of a geographical degree; that of Xenophon, $\frac{1}{750}$; that of Eratoſthenes, $\frac{1}{700}$; that of Strabo, the ſame; that of Pliny, $\frac{1}{727}$; that of Nearchus, $\frac{1}{720}$ and $\frac{1}{3}$; the mean ſtadium of the antients, $\frac{1}{718}$, or $505\frac{1}{2}$ Engliſh feet. The pace, in the meaſurements of the antients, he juſtly ſtates at five feet.

Herodotus's geography of Europe is next examined in the progreſs of this work. The Mediterranean Sea, and its European coaſts, were evidently well known to this hiſtorian. He knew a little, and but a little, of the weſtern ſhores of

Europe.

Europe. With the extent and ſubdiviſions of the ſouthern countries on the Danube, he appears to have been well acquainted. His Scythia is the Weſtern Sarmatia of ſome later writers among the antients, the Ukraine of modern geography. But his notion of the form of this country was incorrect, as he ſuppoſed it to be one-half leſs than it really was. Mr. Rennell purſues his examination further, through the countries which Herodotus has deſcribed as bordering on Weſtern Scythia; remarks, after Gibbon, that the Genoeſe conſtructed thoſe fortifications of the Crimea, whoſe origin has been by ſome referred to earlier times; traces the courſe of the famous expedition of Darius into Weſtern Scythia; attends Herodotus into the countries beyond the Euxine ſea; deſcribes the Hyperboreans of the Romans as being the ſame with the Gog and Magog of the Scriptures, the Juje Majuje of the preſent Orientals; and takes occaſion to mention Herodotus, Pythagoras, Anacharſis, Peter Alexiowitz, and Sir Joſeph Banks, as the only men who have ſplendidly diſtinguiſhed themſelves by dangerous, ardent, and ſucceſsful travels in purſuit of knowledge.

The Aſia of Herodotus comes next under Mr. Rennell's examination. Its weſt and ſouth-weſt boundaries were the ſhores of the Arabian gulph, and of the Mediterranean and Euxine ſeas: its eaſtern limits were the country of the Oigurs, the deſert of Kobi, and India taken incluſively: on the ſouth, its only boundary known to Herodotus was the Erythrean ſea. The north of Aſia was to him unknown. He had little knowledge of India. And the length of Aſia eaſtward was leſs known to Herodotus than to ſome later geographers among the antients. With the bounds and extent of the Caſpian ſea he was better acquainted than the followers of Alexander. He knew it to be a lake; they ſuppoſed it to communicate with the great northern ocean. China, Chineſe and Weſtern Tartary, Thibet, the peninſula beyond the Ganges, and the greater part of Siberia, were entirely unknown to Herodotus. To him and the other Greeks of his time Eaſtern Scythia was little known. The Maſſagetæ, the Sacæ, and the Dahæ, were the great Scythian tribes. He has deſcribed the geography of the

 Perſian

Persian empire as divided into twenty governments. Mr. Rennell examines, in minute detail, his account of each of these. The maritime strength of the Phœnicians under the Persian power is well marked by the fact which Mr. Rennell notices, that they furnished to the fleet of Xerxes 450 triremes, of which 150 were derived from their establishment in the island of Cyprus. Of the Ganges it appears that Herodotus had never even heard. The public revenue of the Persian empire was, according to his account, about 3½ millions sterling. The site of antient Babylon, as Mr. Rennell informs us upon good authority, is still sufficiently accessible, and is covered with ruins which it might be highly interesting to examine.

Africa was best known to Herodotus where it is adjacent to the middle and the eastern limits of the Mediterranean sea. Of its great rivers, he knew only the Niger and the Nile. He knew those *oases*, or insulated fertile spots in the sandy desert, which have been lately re-discovered by Brown. He knew that Africa had been circumnavigated. The voyage was performed at the rate of thirty-two geographical miles a day, and finished in the space of two years. The whole tract of the earth known to Herodotus might be measured by a radius of 1000 British miles, moving from the centre of Halicarnassus. The Seewa of Brown the traveller is the Oasis of Ammon of Herodotus. Of the *lotos* there were two species; the one an aquatic plant, the other a dry shrub. The aquatic lotos was the same as the *colocassia*, and is still abundant in Egypt. After finishing his discussion of the geography of Herodotus, Mr. Rennell enters into an interesting illustration of the memoir of the voyage of Hanno. His work is accompanied with excellent maps, constructed upon those principles and views which he explains in it. Under every leading head of his work, Mr. Rennell descends into much minuteness and accuracy of detail. It is interspersed with various pleasing digressions; and many curious articles of collateral information are incidentally thrown out in it.

Such are in general the plan, the execution, and the leading facts of this work. It were easy to dispute some of its

 positions;

positions; to wish, in one place, for more of science; in another, for more of erudition; in a third, for greater accuracy of local knowledge. But it is written with such patience and accuracy of investigation, and in such a spirit of candour; it elucidates so remarkably the pages of a favourite author; it connects Grecian learning so advantageously with the researches of modern travellers; and it makes so many interesting additions to the facts of geographical science, that we cannot doubt but the public must receive it as an important addition to this branch of literature: and we offer our best thanks to the author for having thus far executed so laborious and useful a task.

INTELLIGENCE, AND MISCELLANEOUS ARTICLES.

LEARNED SOCIETIES.

ACADEMY OF BERLIN.

THE Royal Academy of Sciences at Berlin has proposed the following prize questions for 1800, 1801, and 1802.

The Mathematical Class has continued for the year 1802, with a double prize, the following question:

"Since, notwithstanding the labours of the ablest astronomers, there still remain several points respecting the variation of the obliquity of the ecliptic to be cleared up, the Academy invites the learned to occupy themselves again on this important object, and will adjudge a prize to that memoir which shall contain the most interesting researches and the best illustrations respecting this important point."

The Class of Speculative Philosophy proposed the following question for the year 1799:

"The important question of the origin of our knowledge, agitated

agitated at all times, has been difcuffed at prefent with more intereft than ever. It is certainly of great importance; and it is much to be wifhed that the proofs on both fides were carried to fuch a degree of perfection, and rendered fo fatisfactory, as to enable philofophers to form a decifive opinion on this point without falling into fyncretifm, which, by fubftituting indifference for intereft, is highly detrimental to the progrefs of philofophy. The Academy does not enter into the ideas of thofe who confider it as demonftrated by mathematical evidence, that a part of our knowledge has its origin merely in the nature of our underftanding: on the contrary, it is perfuaded that to this opinion effential objections may be made; objections which hitherto have not been anfwered in a fatisfactory manner; efpecially as it is convinced that there are very ftrong proofs in favour of the opinion which deduces all our knowledge from experience, though thefe proofs, perhaps, have not yet been difplayed in their full light. The Academy, being defirous to contribute as much as poffible to the folution of this problem, propofes as the fubject for which the Clafs of Philofophy ought to adjudge a prize:

" To demonftrate, in an inconteftable manner, the origin of all our knowledge; either by prefenting arguments never before employed, or arguments already employed, but exhibited with more clearnefs, and with fuch force as to obviate every objection."

But as none of the memoirs tranfmitted to the Academy have been thought fatisfactory, the Academy finds itfelf obliged to defer the adjudication of the prize till the public fitting of the month of Auguft 1801. It repeats at the fame time its invitation to philofophers to contribute as much as in their power towards fixing the important point of the origin of human knowledge, by going back to the evidence, in order that the refult may be, either that the fcience of metaphyfics is a chimera, or that its principles are fo determined that we cannot refufe to them univerfality, and fuch a comparative force of evidence, that there can no longer be room for difputing refpecting principles; and that all deviations muft be confidered merely as logical errors, of too little confequence to promote a rage for fyftems and the fpirit of party.

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The Phyfical Clafs has propofed for the year 1801 the following queftion:

"Has electricity any action on matters which ferment? And in that cafe, What is its action? Is it favourable to fermentation, or does it prevent it? What advantages may be derived from an illuftration of this point, in regard to improving the art of making wines, of brewing, and of diftilling fpirits?"

The learned of all countries, the ordinary members of the Academy excepted, are invited to exert their talents on thefe queftions. The prizes are, a gold medal, equal in weight to fifty ducats, for the philofophical queftion; two medals for the mathematical, and a hundred crowns for the queftion in phyfics. The papers, written in a legible hand, muft be tranfmitted, with the ufual formalities, to the perpetual fecretary of the Academy, before the firft of May of the years above mentioned.

BATAVIAN SOCIETY AT HAARLEM.

On the 24th of May laft this Society held their annual meeting, which the director opened by giving an account of the anfwers tranfmitted to the prize queftions to be anfwered before the firft of November 1799; the refult of which was as follows:

On the queftion refpecting the utility and manner of employing ventilators in fhips, two papers were tranfmitted, written in Dutch, one of which obtained a golden medal. The fuccefsful candidate was L. Bicker, M. D. director of the Society of Experimental Philofophy at Rotterdam.

On the other queftions, refpecting the improvement of downs, or fandy and dry land, two papers were received without any devices. It was refolved, therefore, that no further notice fhould be taken of them; and that the queftions fhould not be repeated.

The three remaining queftions to which no anfwers had been fent, have been again propofed in the following manner:

"I. What is the prefent ftate of our knowledge in regard to the motion of the fap in trees and plants? In what manner can a more complete knowledge be obtained of what is

ftill

ſtill obſcure or dubious in this reſpect? and, Can conſequences uſeful for the cultivation of trees and plants be deduced from what has been already confirmed on this ſubject by deciſive experiments?

"II. As it is probable, if more attention were paid to thoſe phyſical cauſes which force back ſmoke into chimneys, that means might be deviſed to prevent apartments from being expoſed to it, except in the caſe when the chimneys are expoſed to violent winds, the Society requires:

"1ſt, A theory or brief phyſical explanation of the cauſes which force back ſmoke in chimneys, or prevent it from aſcending.

"2d, Rules, deduced from this theory, for building chimneys, and in which precepts muſt be given, taking into conſideration every poſſible circumſtance to prevent apartments from being expoſed to ſmoke.

"III. What indigenous plants, hitherto not uſed, are capable, according to well-atteſted experiments, of producing good dye ſtuffs, and might be employed with advantage for that purpoſe? and, What foreign plants might be reared in the leaſt fruitful and little cultivated parts of the republic, in order to procure dyes from them?"

The anſwers to theſe three queſtions muſt be tranſmitted to the Society before the firſt of November 1801.

For the preſent year, the Society has propoſed the following queſtions:

"I. What can we conſider as proved, or as highly probable, according to the lateſt aſtronomical obſervations, particularly thoſe of Herſchel and Schröter, in regard to the extent of the univerſe, and the order in which the heavenly bodies ſtand?"

In the anſwers to this queſtion, the Society requires that the authors will give a ſhort and comprehenſive view of the preſent ſtate of aſtronomy in regard to this point; and ſhow how improbable or ill founded ſome of the hypotheſes formed on this ſubject are.

"II. How far do we know, according to the lateſt improvements in the phyſiology of plants, in what manner the different kinds of earth promote the vegetation of plants? and,

and, What consequences can be deduced from our knowledge on this subject in regard to the choice of manure, and the improvement of waste and barren lands?

"III. Is the study of natural history of so much importance to youth that it deserves to be considered as an essential part of a well regulated education? And if this be the case, what part of this science deserves the preference? and what is the properest method of exciting young persons to study it, and of making it as useful to them as possible?

"IV. As it appears, from the experiments of Dr. Chladni, that, when a plate of metal or glass has been strewed over with light sand or dust, if a violin bow be rubbed against it so as to produce a tone, the sand assumes various figures; the Society requires a theory of this phenomenon, which shall be the result only of observation; and in particular:

"1st, As complete an account as possible of all the figures which each tone produces, and a classification of these figures according to their different kinds.

"2d, A physical explanation of the reason why the sand assumes these figures, and of their relation to the respective tones."

The answers to the three first questions must be transmitted to the Society before the first of November 1801; that to the last, before the first of November 1802.

The following questions proposed last year, to be answered before the first of November 1800, remain to be answered:

"I. What light has been thrown by the new chemistry on the physiology of the human body?

"II. How far has the light thrown by the new chemistry on the physiology of the human body contributed to make us better acquainted with the nature and causes of certain diseases? and, What useful consequences, more or less confirmed by experience, can be thence deduced for the practice of medicine?

"III. How far has the new chemistry served to give us more accurate ideas of the efficacy of external or internal medicines long used or recently recommended? and of what advantage may a more accurate knowledge in this subject be in the treatment of certain diseases?"

As most philosophers, in applying the principles of the new chemistry to the physiology of the human body, to pathology, and therapeutics, have introduced hypotheses built on too weak a foundation; and as this is, no doubt, prejudicial to the progress of these sciences, to which the new chemistry promises so much improvement; if, according to the rule of Lavoisier, we admit in chemistry, or in the application of its principle, those things only which are supported by decisive experiments; the Society wishes that those inclined to answer these questions will accurately distinguish what is actually known, from what is merely hypothetical; and that, in regard to hypotheses, they do confine themselves merely to pointing them out, and to showing, in a brief manner, the weakness of the grounds on which they rest; as the principal object of these questions is to procure to the physicians and surgeons of the Batavian republic, who have not sufficiently attended to the application of the principles of this science, already well confirmed, to physiology, pathology, and therapeutics, appropriate treatises, from which they may easily be enabled to know what light the modern chemistry has really diffused on this branch of knowledge; and what is too ill founded, too hastily adopted, or too doubtful to be depended on. The papers on these questions will be examined separately. Those inclined to answer more than one of them are requested to write distinct papers on each question.

The following questions, first proposed in 1794, and afterwards repeated in 1796, are also still to be answered:

" IV. A proper plan, founded on facts, and capable of being carried into execution, for improving and cultivating the waste lands, whether sands, morasses, or heaths, within the republic, and particularly in the districts of Guelderland, Overyssel, Drenthe, and Dutch Brabant.

" V. What are the different breeds of sheep in the United Provinces, and the different methods of breeding them? The Society wishes also to be informed of their size, their usual weight, the measure and weight of their different limbs, the length and fineness of the wool, the number of lambs which they annually produce: whether the several joints, which Daubenton observed in some breeds, are to be con-

sidered

ſidered as peculiar to one breed, or merely as a *luſus naturæ*; and whether, among all theſe breeds, there is one which originally belongs to Holland?"

The following queſtion, firſt propoſed in 1796, and repeated in 1799, is extended to the firſt of November 1802:

"A natural hiſtory and phyſical deſcription of the whale, which may ſerve to explain and point out the beſt means for diſcovering the places where theſe animals are to be found, as well as the eaſieſt and ſureſt means either known and uſed, or that can be employed, for immediately killing them, and of getting poſſeſſion of them in the ſpeedieſt and ſafeſt manner?"

The following queſtions, the firſt propoſed in 1790, the ſecond in 1794, the third and fourth in 1795, are ſtill continued for an unlimited period:

"I. What have the lateſt chemical diſcoveries taught concerning the nature of fermentation? and what advantage can be derived from this knowledge in regard to certain manufactures in which fermenting ſubſtances are employed?

"II. What have we been taught by experience in regard to the utility of certain animals, particularly in the Netherlands, which, according to appearance, are deſtructive? and what means muſt be employed to extirpate them?

"III. What indigenous plants, the virtues of which are as yet very little known, might be uſed with advantage in our *materia medica*, and be ſubſtituted for foreign plants?"

The papers which the Society expects on this queſtion, muſt contain proofs of the virtue and advantages of theſe indigenous plants, not merely from the teſtimony of foreigners, but from experiments and obſervations made originally in theſe provinces.

"IV. What indigenous plants, not yet uſed, might be employed as good and wholeſome nouriſhment? and what nutritive foreign plants might be cultivated in the United Provinces?"

The Society requeſts that the competitors will compreſs their papers as much as poſſible, and omit every thing that does not eſſentially relate to the queſtion. All the members

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of the Society are at liberty to be candidates, on condition that their papers are diſtinguiſhed by ſome device, and marked with the letter L.

The papers may be written either in Dutch, French, Latin, or German, and muſt be accompanied with a ſealed note containing the name and addreſs of the author. They muſt all be tranſmitted to Van Marum, ſecretary to the Society.

The prize for the beſt anſwer to any of the above queſtions is a gold medal, with the uſual impreſſion of the Society, containing on the edge the name of the author, and the year in which he obtained the prize; or, in lieu of it, at the option of the author, thirty ducats. Thoſe, however, who obtain the prize, or an aceſſit, ſhall not, without the expreſs permiſſion of the Society, publiſh their papers, either whole or in part, in the Tranſactions of the Society, nor in any other work, or ſeparately by themſelves.

The Society takes this opportunity of repeating, that they reſolved, in their ſitting of 1798, to examine, in every annual ſitting, whether, among the papers communicated in the courſe of the year, and not anſwers to queſtions propoſed, there are any reſpecting natural hiſtory or natural philoſophy which deſerve a particular reward; and, when this is the caſe, to adjudge to that paper, or, when there are ſeveral, to the moſt intereſting, a ſilver medal with the Society's impreſſion, and a preſent of ten ducats beſides.

ACADEMY OF LYONS.

C. Verniac, the prefect of the department of the Rhone, and C. Noel, commiſſioner-general of the police at Lyons, in conjunction with ſome members of the antient Academy of Lyons, have revived that valuable inſtitution under the title of the *Atheneum*. The number of the members of which it is to conſiſt has been fixed at forty-five; among whom are comprehended thoſe who made a part of the old academy, and who now are reduced to ſixteen. The Atheneum will have alſo fifteen aſſociates, and an unlimited number of correſpondents. It is divided into two claſſes; one

one for the arts and ſciences, and the other for the *belles lettres* and fine arts. The firſt public ſitting was held on the 8th of Auguſt.

The Atheneum has already propoſed the following queſtions as the ſubjects of a prize:

"I. To point out thoſe indigenous mineral and vegetable ſubſtances which may be capable of producing a colouring principle applicable to ſilk, cotton, linen, hemp, woollen, and paper.

"II. To explain the proceſſes for extracting, fixing, and brightening the colours which may be furniſhed by ſimple indigenous ſubſtances not yet known in the art of dyeing."

The prize will be adjudged in the public ſitting of Meſſidor 24th, year 9. (July 14th, 1800.) It conſiſts of two medals of the value of 300 francs each. The ordinary members of the Atheneum are excluded from the competition.

MISCELLANEOUS ARTICLES.

HUMBOLDT'S TRAVELS THROUGH SPANISH AMERICA.

The following letter from this enterpriſing philoſopher to Fourcroy has lately been publiſhed in one of the French journals:

"Guyara, January 25, 1800.

"As the yellow fever, which deſolates this part of South America, obliges us to make our ſtay here as ſhort as poſſible, I ſnatch this opportunity of ſending you a few lines, and repeating, from the middle of the torrid zone, how much my thoughts are occupied with you and your illuſtrious colleagues, among whom I met with ſuch a flattering reception during my laſt ſtay at Paris. Since our departure from Saint Croix in Teneriffe, where I deſcended into the crater of the volcano, the atmoſpheric air being there at 0·8 of R. and at 0·19 of oxygen, I have written to you twice. I have ſent to Delambre and Lalande an extract of my aſtronomical labours; the longitude of ſome important places; an obſervation of the eclipſe on the 28th of October laſt; immerſions

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of the satellites, and researches respecting the intensity of light of the austral stars, measured by means of diaphragms. I have addressed to the Institute a chemical memoir on the phosphorescence of the sea; observations on a particular gas furnished by the fruit of the *coffea ar..bica* when exposed to the sun; on a snow white feld-spar, which, when moistened, absorbs all the moisture of the atmosphere; experiments on the milk of the *cecropia peltata* and the *euphorbia curassavica,* which will form a supplement to your excellent memoir on the *cabout-chouc,* and to that of our friend Chaptal; and on the air which circulates in vegetables.

" The cruisers which cover the seas here make me apprehend that a part of my letters may not have reached you; though I sent them sometimes by way of Guadaloupe, and sometimes by that of Spain. These few lines I have sent by an American vessel, which will sail in a few days for Boston; and, though they cannot reach you but through Hamburgh, they will, perhaps, be less liable to miscarry. People here are accustomed to make four or five duplicates of their letters. But how can I find time, when I have so many things to observe, arrange, and calculate?

" I shall confine myself, therefore, to letting you once more know that I still enjoy the best health possible, and that I am treated with the utmost kindness by the inhabitants of these countries: that the passports and letters of recommendation from the Spanish government procure me every facility for making researches useful to the sciences; that none of my instruments, even the most delicate, such as barometers, thermometers, hygrometers, Bordas' dipping needle, &c. are deranged: and that, at the extremity of the missionary establishments among the Chayma Indians, in the mountains of Toumiriquiri, I have my laboratory mounted as if I were in the Hotel Boston, in the Rue du Colombier. My fellow-traveller, Bonpland, educated in the *Jardin des Plantes,* becomes every day more valuable to me. To an extensive knowledge of botany and comparative anatomy he unites indefatigable zeal. I hope one day to restore him to his country, worthy of attracting public attention. Never did any foreigner enjoy such permission as that granted

granted to me by the king of Spain. This idea alone is capable of exciting us to redouble our activity. During the ſeven months we have been in this beautiful continent, we have dried (with duplicates) nearly 4000 plants, written more than 800 deſcriptions of new ſpecies or ſpecies little known, particularly new genera of palms, cryptogamia, bifaria, melaſtoma; collected inſects and ſhells, and made many drawings reſpecting the anatomy of marine worms; with obſervations on magnetiſm, electricity, humidity, the temperature of the atmoſphere, and the quantity of oxygen it contains. We have meaſured that immenſe and high chain of mountains extending to the coaſt of Paria, and examined their volcanoes, which vomit forth kindled inflammable air, ſulphur, and hydro-ſulphurous water. We have alſo collected many ſeeds, which we ſhall ſend off in three or four weeks for Europe, addreſſed to the *Jardin des Plantes*. We have ſpent five months in the interior of New Andaluſia and on the coaſts of Paria, where we experienced very violent earthquakes in the end of laſt year. One part of theſe countries is ſtill inhabited by the ſavage Indians, and others have begun to be cultivated only within five or ſix years. In what words ſhall I deſcribe to you the majeſty of the vegetation here; the foreſts of Ceiba, Hura, Hymenea, which the rays of the ſun never penetrate; the variety of the animals; the ſuperb plumage of the birds; the apes, the tigers; the hideous aſpect of the crocodiles (*caimans*) which ſwarm in the rivers, and of which ſome are thirty feet in length? From Cumana we proceeded to Caraccas, where we remained during the month of November and part of December: A charming capital ſituated in a valley which has 426 toiſes of elevation; and, though in latitude 10° 31′, enjoys the coolneſs, and I might ſay the cold, of Paris! From this place we aſcended to the ſummit of the famous Silla de Caraccas, or Sierra de Avila, where, at the height of 1316 toiſes, we diſcovered beautiful cryſtals of titanium. I found alſo dendrites ſimilar to thoſe of manganeſe, which are oxyd of titanium.

We ſhall proceed hence for Varina and the ſnow-covered mountains of Merida, the cataracts of Rio Nigro, and the unknown

unknown world of Oronoco, in order to return by Guyana to Cumana, from which we shall set out for the Havannah and Mexico.

"We shall take care to transmit the seeds we have collected for the *Jardin des Plantes* at Paris, the Museum, and Sir Joseph Banks, as we agreed with Jussieu - - - -.

"How much I lament the fate of Dolomieu, detained a prisoner in Sicily! If he should return, communicate to him the following fact:—It is more than three years since I announced to Lametherie, that, in the primitive mountains of Italy, France, Swisserland, Poland, and I can now add Spain, there exists a *parallelism of direction* between the strata of foliaceous granites, slate, micaceous schists, corneous schists: that these strata incline (sink down) towards the north-west, and that their direction makes with the axis of the globe an angle of 45° 57′: that this inclination and direction in no manner depend on the form and direction of the mountains: that it is not any way affected by the valleys, but that it announces a cause much greater, and more general: that it depends on a phenomenon of attraction which has acted at the time of the consolidation of the globe. Having travelled over the greater part of Europe on foot, and with sextants and compasses, I have a very extensive collection of observations on that subject. My manuscript on the identity of strata in the construction of the globe, is in the hands of my brother. I was employed on it since 1791, but it will not appear till I have seen more of the globe. To my great astonishment, I have observed in the Cordillera of Paria, New Andalusia, New Barcelona, and Venzuela, that in the new world, near the equator, the strata follow the same laws and the same parallelism.

"You remember the last ingenious observations of Coulomb on the air which issues with explosion from the trunks of trees when they are pierced. I have made experiments on the *clusea rosea*, in which (in the interior of the *pneumatochimifer* vessels of Hedwig, the *vasa cochleata* of Malpighi,) there circulates an immense quantity of air. This air contains as far as $\frac{35}{100}$ of oxygen. The leaves of the same tree, when exposed to the sun under water, do not give a cubic mellimetre

mellimetre of air. This air, which circulates, certainly ferves, as in the animal body, to coagulate the fibrous part by abforption of oxygen. The *clufea* is a milky plant, and elaftic gluten is formed in it.

"Though the purity of the atmofpheric air amounts here, particularly in the night, to 0·305 of oxygen, I have found that the air contained in the pods and capfules of equinoctial plants, for inftance the *paullinia*, is more azotous than our atmofpheric air. It does not exceed 0·24 or 0·25 of oxygen. The air in the *culmi geniculati* here, has only 0·15 of oxygen. All this proves that the air which circulates is purer, and that the air which is in a ftate of reft, depofited in the capfules or *utriculi*, is lefs pure than atmofpheric air. The former is recently produced by the organs which decompofe the water; it proceeds to thofe parts where it ought to ferve, by its abundance of oxygen, to precipitate the fibrous principle to form the fibrous tiffue. The other is the refiduum of a gas which has already difcharged thefe functions.

ALEX. HUMBOLDT."

VACCINE INOCULATION.

Tranflation of a Letter to George Pearfon, M. D. Phyfician to the Vaccine Inftitution at London.

"SIR, "Paris, Germinal 14, (May 5,) 1800.

"We have been informed by the public journals of the trials made in England with regard to the vaccine inoculation, the fuccefs of which has, in a great meafure, been owing to your *enlightened zeal*. Some friends of humanity, and of that fcience which you cultivate in fo diftinguifhed a manner, have formed a plan for introducing into France a practice which feems to promife advantages of fo much magnitude. For this purpofe they have propofed a fubfcription, which is now nearly filled up. You will find annexed a profpectus of their plan; but, to carry into execution the views announced in it, we have need of your affiftance; and we now folicit it with confidence.

"Though fome trials have been made here in regard to the vaccine inoculation, with which the School of Medicine

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has been more particularly occupied, we have not yet been able to obtain any poſitive reſult; the principal cauſe of which, no doubt, has been, the difficulty of procuring matter ſufficiently recent. Our veterinary doctors do not ſeem to be acquainted with this diſeaſe among cows. One inſtance only has occurred where they thought they obſerved this diſeaſe among theſe animals; and we embraced that opportunity of collecting the virus, which was employed, together with matter received from Geneva and England, but hitherto without any certain ſucceſs. We find ourſelves, therefore, deprived of the chief means neceſſary for commencing our trials; and we addreſs ourſelves to you to aſſiſt us in procuring them.

"We requeſt, but only in caſe you think theſe precautions neceſſary, to receive ſcabs or puſtules taken from cows or from perſons who have been inoculated, and threads or ſmall bits of ſponge, impregnated with matter taken from either; alſo, ſome of theſe ſcabs or puſtules quite freſh, and ſome of the matter produced in the place formed by the local affection of the part inoculated. The different kinds of matter or ſubſtances ought to be particularly marked. If your method of preſerving threads in azotic or hydrogen gas ſeems to be attended with any advantage, we expect from your zeal that you will employ it. In conjunction with our miniſter for foreign relations, we have take every neceſſary meaſure that the ſafeſt, the moſt convenient, and, in particular, the ſpeedieſt mode of conveyanc ſhall be at your command. The Society, with equal eagerneſs, will make known the ſervices that you may render to it, and communicate to you the reſult of its reſearches the merit of which will in a great meaſure belong to you if, along with the matter which we requeſt, you will have the goodneſs to add ſome particular inſtructions, which we ſhall be very happy to follow.

"Health, eſteem, and devotion,

"*The Members of the Medical Committee of the Society formed at Paris for the Vaccine Inoculation;*

"Pinel, Profeſſor in the School of Medicine;

"Thouret, Director of the School of Medicine;

"Parfait,

" PARFAIT, Inoculating Surgeon;
" ROUSELLE CHAMSERU, Phyſician to the Army;
" DE LA PORTE, Phyſician to the Military Hoſpital;
" HUZARD, Member of the Inſtitute;
" TESSIER, Member of the Inſtitute;
" CABANNEZ, Member of the Inſtitute, Profeſſor in the School of Medicine."

The above letter, as appears, was addreſſed to Dr. Pearſon, who, however, in place of returning an anſwer in his own name, preſented it to the Vaccine Inſtitution, where it was determined that the commiſſion ſhould be executed by the medical eſtabliſhment. Accordingly, a packet containing vaccine matter on glaſs, on lancets, and on thread, incloſed in a bottle filled with hydrogen gas, and a letter of inſtructions ſigned by all the members of the medical eſtabliſhment, was ſent to Paris on the 12th of May laſt, with the permiſſion of Lord Grenville, through the hands of the French commiſſioner, Mr. Otto. How well the vaccine matter thus forwarded anſwered the ſanguine expectations of the Pariſian phyſicians, notwithſtanding the journey and the delay, is ſufficiently proved by the following extract from the *Gazette Nationale*; and other French papers furniſh ſimilar reports.

" *Gazette Nationale, Tridi,* 23 *Prairial, an.* 8 *de la République Françoiſe, une et indiviſible.*

" On the 13th of Prairial the vaccine inoculation was performed on thirty children with matter ſent from England, and according to the method recommended. Signs of infection manifeſted themſelves on nine of them at the periods and with the characters announced by Dr. Pearſon and other members of the committee inſtituted at London for the vaccine inoculation. It is to be remarked, that the Engliſh phyſicians expreſſed in their letter, ' that it ought to be eſteemed fortunate, on account of the time that muſt elapſe between collecting the matter and applying it, if, out of twenty individuals inoculated, one only ſhould take the diſeaſe.'

" On the 19th, 20th, and 21ſt, eighteen children, on whom the first inoculation produced no effect, were again inoculated with matter collected from those on whom the first inoculation took effect. The committee continue their trials, and will inform the public of their further results.

"*In the name of the Medical Committee.* THOURET."

"The Vaccine Institution, which has already been so useful in England, and which has communicated on the subject with the learned in various parts of Europe to introduce the cow-pock inoculation, has sent us the following notice:

"Vaccine Institution, Warwick-street,
Golden-square, July 28, 1800.

"From the increase of patients, and the numerous applications for cow-pock matter, the subscribers are hereby informed, that it has been resolved to inoculate every Friday as well as Tuesday, under the direction of the medical establishment, which at present is as follows:

"George Pearson, M.D. F.R.S. L. Nihell, M.D. and Thomas Nelson, M.D. physicians.

"Thomas Keate, Esq. and John Rush, Esq. consulting surgeons.

"R. Keate, Esq. John Gunning, Esq. and J. Carpue, Esq. surgeons.

"A. Brande, Esq. F. Rivers, Esq. and Mr. E. Brande, visiting apothecaries.

"Mr. J. Lewis, resident apothecary, to whom letters (post paid) may be addressed.

"The public are desired to notice, that the medical establishment do not authenticate vaccine matter unless it be delivered under the appropriate seal of the institution.

"By order, WM. SANCHO, Secretary."

FRUCTIFICATION OF THE FUCUS NATANS.

A small treatise has lately been published at Madrid by Hippolitus Ruiz, member of the Royal Medical Society of that city and botanist on the expedition to Peru, under the title of *De vera Fuci natantis fructificatione commentarius.* As this work is scarce, the following extract from it may not

not be unacceptable to our botanical readers:—The author, who is well known by his voyage to Peru for the improvement of botany, having found in the Atlantic, on his voyage back to Europe in the year 1788, a great quantity of floating ſea-graſs, *Fucus natans* Linn., he carefully examined the bladders of this marine production, which Linnæus erroneouſly conſidered as its fruit, but obſerved nothing in them that could induce him to accede to the opinion of that eminent naturaliſt. In the night of the firſt of Auguſt, when in the 24th degree of north latitude, the captain ſent him ſome of theſe fuci, which had been found adhering to one of the ropes, and which, as ſoon as agitated in the water, exhibited blueiſh luminous phoſphoric points. One of theſe he put into a veſſel filled with ſea-water, and obſerved that, when the plant was moved, a phoſphoric light ran along it, from the one end to the other, in a ſerpentine direction. In the day-time he found white threads adhering to it along its whole length, having at their extremities ſmall cups and ears, which he immediately conjectured to be the parts of fructification long ſought for without ſucceſs, but which were exceedingly different from thoſe which Linnæus and other botaniſts thought they obſerved in this ſpecies of plants. The fucus was kept four days in water; and always, when ſhaken in a dark place, it emitted a phoſphoric light which had a perfect reſemblance to that produced by mercury when agitated in vacuo. After the above period this phenomenon entirely ceaſed; but the author was able to exhibit it at pleaſure by obtaining freſh plants. The parts which produced the luminous appearance were the white threads, before mentioned, adhering to the *fucus*, which terminated either in ſmall cups or ears, or ears with ſmall knobs. He, however, did not find all theſe three forms of the ſuppoſed flowers on the ſame plant, but in different individuals. He found alſo that theſe bodies could not be eaſily ſeparated from the plant, even though the incruſtations with which the fucus was abundantly covered could be ſeparated with little difficulty. From this he concluded that the ſingle cups were the male flowers, the ears the hermaphrodite, and the ears with knobs the female.

 A writer

A writer in one of the foreign journals, who mentions this work, makes the following obſervations on the ſubject:—"By the kindneſs of one of my friends I have obtained from Spain a ſpecimen of the ſuppoſed flowers of the *fucus natans*, and I muſt confeſs that the above deſcription of the parts does not appear to me ſatisfactory. Mr. Ruiz is certainly right when he ſays that the bladders in all the kinds of the *fuci* contain nothing that can ſerve for fructification; and the experiment he made by carefully ſtripping a *fucus natans* of all its bladders, in conſequence of which it ſunk in the water, clearly proves, what the German botaniſts have long ago obſerved, that the bladders of all the *fuci* ſerve to raiſe them like balloons, and to keep them floating in the water, as is the caſe with the *utricularia* found in our ditches. The real parts of fructification in this plant have been clearly pointed out in various kinds of it, by Roth and Stackhouſe. What the author conſiders as flowers are ſmall polypes, of that kind which Blumenbach calls *Brachionus*. Their white colour, the threads with which they were connected, and their phoſphoric light, (for he found nothing of the ſort in plants deſtitute of theſe appendages,) ought to have led him to the idea, that theſe bodies do not naturally belong to the plant, and are merely adventitious. Polypes have already often deceived botaniſts, who have conſidered them as the flowers of zoophytes without ſuſpecting that they might be the inhabitants of them.

"The account given by the author of the manner in which this plant grows is curious. The *fucus natans* has no roots, but excreſcences by which it adheres to the bottom of the ſea. As ſoon as it is full grown it becomes covered with bladders, which render it lighter than the water, and it then riſes to the ſurface. In this ſtate it floats on the waves from the month of July to September; but after this period it is no longer to be ſeen between the 24th and the 36th degree of north latitude. It ſometimes covers very extenſive parts of the ocean, which, by theſe means, have the appearance of meadows."

COMBINA-

COMBINATIONS OF LIGHT.

The celebrated Brugnatelli has lately publiſhed at Pavia, in the *Annali di Chimica,* the following obſervations on light:—Light is either chemically united with bodies, or merely accumulated and mixed in them in a mechanical manner but inviſible, or it is accumulated in bodies in a viſible ſtate.

Light chemically united with bodies, ſeparates itſelf from them in conſequence of its affinity for caloric. A red heat, however, is not neceſſary to diſengage light when in this ſtate. The black calx of manganeſe ſhines with great vivacity when thrown upon a very hot, but not ignited, plate of iron. The caſe is the ſame with the muriat of mercury (corroſive ſublimat), gray calx of mercury, gray calx of antimony, all calcareous ſalts, &c. Fine ſugar and ſugar of milk, when very dry and pounded, ſhine with great vivacity on a plate of iron which has been merely heated, but they do not exhibit the ſame phenomenon on iron brought to a red heat. Feathers, cotton, and wool, ſhine when drawn lightly over a piece of hot iron; and the ſpots of a playing card, under the mecircumſtances, emit a faint light. Camphor and choco thrown upon a piece of iron of this kind form luminous vapours. Several fluids, under the ſame circumſtances, produce the like effects. Oil of turpentine poured on hot iron gives a very perceptible light; and the caſe is the ſame with fat, oils, ſweat, wax, tallow, &c. Atmoſpheric air has not the leaſt influence in regard to the luminous appearance of theſe ſubſtances; as they ſhine alſo in vacuo, in carbonic acid gas, in hydrogen gas, &c. Nay, ſeveral bodies ſuffer their light to be diſengaged when they are immerſed in ſulphurous acid or in boiling oil. Different bodies, under ſuch circumſtances, give different quantities of light.

In regard to bodies in which light is merely accumulated mechanically, nothing is neceſſary to render it free but an approximation of their parts. In this manner the light is, as it were, ſqueezed out; as is the caſe with the quickſilver in the barometer when it becomes luminous, vitrioliſed tartar,

tartar, and other salts, which shine at the moment when they crystallise. This phenomenon is exhibited also by the water of the sea when it becomes phosphoric, the eye when suddenly struck, sugar when pounded*, cream of tartar, alum and borax when struck. Among mineral bodies, quartz in particular contains a great deal of accumulated light: to this head belongs the luminous appearance of certain plants.

Light is accumulated in a visible state in the so-called light magnets, which, when they have been exposed for a considerable time to the light, imbibe a certain quantity of it, and then throw it out in the dark. Of these bodies, those which deserve the first place are the diamond, blende, and the carbuncle. *Lapis lazuli* throws out in the dark a great quantity of light which it imbibes in the day-time. The Bologna phosphorus, which is nothing else than sulphat of barytes, does not become luminous in the dark till it has been exposed for some minutes to the light of the sun; the case is the same with nitrat of lime. Brugnatelli saw a diamond become luminous in the dark after it had been exposed to the light of a candle. Animal and vegetable matters, putrid fish, rotten wood, &c. shine in this manner; also individual parts of animals, the eyes of the hyæna and cats, the body of the glow-worm, &c.

MONSTROUS SERPENT.

The following article is copied from the Bombay Courier, of August 31st, 1799:

"A letter from Amboyna, received within these few days, gives the following account of an enormous snake, which made its appearance at a place called Golontala, in the island of Celebes. A Malay prow making for that port, and finding she could not enter it before dark, came to anchor close in shore for the night. One of the crew went on shore in quest of betel-nut in the woods, and on his return lay down, as is supposed, to sleep on the beach; a common custom with people of that description. In the course of the night his

* When a person champs fine sugar between the teeth in the dark, the mouth appears to be full of fire

comrades

comrades in the boat heard his cries, and went immediately to his affiftance, but too late, an immenfe fnake having crufhed him to death. Thefe people knowing that this kind of fnake never diverts its attention from the prey which it has once feized until it is devoured, went boldly up to the monfter and cut its head off, carrying it and the body of the deceafed on board their boat. The gentleman to whom we are indebted for this account, faw both the next morning; and found, on examining the latter, that the fnake had feized the unhappy man by the right wrift, where the marks of the animal's fangs were very diftinct; and the mangled corpfe bore evident figns of having been crufhed, by the fnake twifting itfelf round the head, neck, breaft, and thigh. Our correfpondent extended the jaws of the fnake, ftiff as they were, wide enough to admit a body the fize of a man's head; and the whole length of the animal is defcribed to have been from about 28 to 30 feet, and equal in circumference to a moderate fized man. By the account of the furvivors, this kind of fnake fwallows men and bullocks, after having crufhed them, as in this inftance; which our friend, judging from the capacity of the jaws in the ftate in which he faw them, found no difficulty in believing: and this furnifhes a proof that fimilar facts, ftated by certain naturalifts, to which many have refufed their belief, are entitled to more deference than they generally meet with."

METEOROLOGY.

The heat of the prefent fummer has been very great, and much more uniform and uninterrupted than for many preceding years. During the afternoon of feveral days this month, the thermometer in the neighbourhood of London, expofed in a northerly afpect, was as high as 86° of Fahr. In London it was higher, owing to the heat reflected from the furrounding buildings.

MEDICAL NOTICE.

Profeffor Callifen, of Copenhagen, read lately before the Royal Medical Society a paper in which he fhowed that the external ufe of boiling water, in cafes of internal inflammation, produces a much ſpeedier and more efficacious effect than veſicatories. We have reaſon to hope that this matter will be further examined and illuſtrated.

DEATHS.

On the 17th of June, at Berlin, J. Abrahamſon, the celebrated dye-ſinker. He was born in 1722, in the duchy of Mecklenburgh-Strelitz, and learned, from a very indifferent artiſt of Liſſa, the art of engraving coats of arms, and of engraving on gems; but, being poſſeſſed of an excellent genius, he was able, by his own talents, combined with reflection, diligence, and cloſe application, to raiſe himſelf to that degree of celebrity to which he was ſo juſtly entitled. Without being able to draw or to model, he diſplayed in all his works great ability and readineſs. Among his principal productions may be reckoned his medals on the victories of Frederick II. during the ſeven years war, and particularly that on the battle of Torgau, after Ramler's idea and a drawing by Meils. He entered into the ſervice of his Pruſſian majeſty in 1750.

On the 20th of June, at Gottingen, that reſpectable veteran among the German mathematicians, Abraham Gotthelf Käſtner, in the eighty-firſt year of his age. He was a counſellor of ſtate to his Britannic majeſty, profeſſor of mathematics and natural philoſophy in the univerſity of Gottingen, member of the Royal Academy of Sciences of that city, of the Agricultural Society of Brunſwick Lunenburgh, of the Academies of Stockholm and Berlin, of the Electoral Academy of the Uſeful Sciences at Erfurt, of the Academy of Sciences at Bologna, &c. &c. Beſides poſſeſſing great mathematical knowledge, he was well acquainted with literature in general, and had a rich vein of wit, as appears by his proſe writings and his epigrams.

THE

PHILOSOPHICAL MAGAZINE.

SEPTEMBER 1800.

I. *On the Electricity excited by the mere Contact of conducting Substances of different Kinds. In a Letter from* Mr. ALEXANDER VOLTA, *F.R.S. Professor of Natural Philosophy in the University of Pavia, to the Right Hon.* Sir JOSEPH BANKS, *Bart. K.B. P.R.S.**

Como in the Milanese, March 20, 1800.

AFTER a long silence, for which I shall offer no apology, I have the pleasure of communicating to you, and through you to the Royal Society, some striking results I have obtained in pursuing my experiments on electricity excited by the mere mutual contact of different kinds of metal, and even by that of other conductors, also different from each other, either liquid or containing some liquid, to which they are properly indebted for their conducting power. The principal of these results, which comprehends nearly all the rest, is the construction of an apparatus having a resemblance in its effects (that is to say, in the shock it is capable of making the arms, &c. experience) to the Leyden flask, or, rather, to an electric battery weakly charged acting incessantly, which should charge itself after each explosion; and, in a word, which should have an inexhaustible charge, a perpetual action or impulse on the electric fluid; but which differs from it essentially both by this continual action, which is peculiar

* Translated from the author's paper published in French in the Philosophical Transactions for 1800, part 2.

to it, and becaufe, inftead of confifting, like the common electric jars and batteries, of one or more infulating plates or thin ftrata of thofe bodies which are alone thought to be *electric*, armed with conductors, or bodies called *non-electric*, this new apparatus is formed merely of feveral of the latter bodies, chofen from among thofe which are the beft conductors, and therefore the moft remote, as has hitherto been believed, from the electric nature. The apparatus to which I allude, and which will, no doubt, aftonifh you, is only the affemblage of a number of good conductors of different kinds arranged in a certain manner. Thirty, forty, fixty, or more pieces of copper, or rather filver, applied each to a piece of tin, or zinc, which is much better, and as many ftrata of water, or any other liquid which may be a better conductor, fuch as falt water, ley, &c. or pieces of pafteboard, fkin, &c. well foaked in thefe liquids; fuch ftrata interpofed between every pair or combination of two different metals in an alternate feries, and always in the fame order of thefe three kinds of conductors, are all that is neceffary for conftituting my new inftrument, which, as I have faid, imitates the effects of the Leyden flafk, or of electric batteries, by communicating the fame fhock as thefe do; but which, indeed, is far inferior to the activity of thefe batteries when highly charged, either in regard to the force and noife of the explofions, the fpark, the diftance at which the difcharge may be effected, &c. as it equals only the effects of a battery very weakly charged, though of immenfe capacity: in other refpects, however, it far furpaffes the virtue and power of thefe batteries, as it has no need, like thefe, of being previoufly charged by means of foreign electricity, and as it is capable of giving a fhock every time it is properly touched, however often it may be.

To this apparatus, much more fimilar at bottom, as I fhall fhow, and even fuch as I have conftructed it, in its form to the *natural electric organ* of the torpedo or electric eel, &c. than to the Leyden flafk and electric batteries, I would wifh to give the name of the *artificial electric organ*: and, indeed, is it not, like it, compofed entirely of conducting bodies? Is it not alfo active of itfelf without any previous charge, without the aid of any electricity excited by any of the means

hitherto

hitherto known? Does it not act inceſſantly, and without intermiſſion? And, in the laſt place, is it not capable of giving every moment ſhocks of greater or leſs ſtrength, according to circumſtances—ſhocks which are renewed by each new touch, and which, when thus repeated or continued for a certain time, produce the ſame torpor in the limbs as is occaſioned by the torpedo, &c.?

I ſhall now give a more particular deſcription of this apparatus and of others analogous to it, as well as of the moſt remarkable experiments made with them.

I provide a few dozens of ſmall round plates or diſks of copper, braſs, or rather ſilver, an inch in diameter more or leſs (pieces of coin for example), and an equal number of plates of tin, or, what is better, of zinc, nearly of the ſame ſize and figure. I make uſe of the term *nearly*, becauſe great preciſion is not neceſſary, and the ſize in general, as well as the figure of the metallic pieces, is merely arbitrary: care only muſt be taken that they may be capable of being conveniently arranged one above the other, in the form of a column. I prepare alſo a pretty large number of circular pieces of paſteboard, or any other ſpongy matter capable of imbibing and retaining a great deal of water or moiſture, with which they muſt be well impregnated in order to enſure ſucceſs to the experiments. Theſe circular pieces of paſteboard, which I ſhall call moiſtened diſks, I make a little ſmaller than the plates of metal, in order that, when interpoſed between them, as I ſhall hereafter deſcribe, they may not project beyond them.

Having all theſe pieces ready in a good ſtate, that is to ſay, the metallic diſks very clean and dry, and the non-metallic ones well moiſtened with common water, or, what is much better, ſalt water, and ſlightly wiped that the moiſture may not drop off, I have nothing to do but to arrange them, a matter exceedingly ſimple and eaſy.

I place then horizontally, on a table or any other ſtand, one of the metallic pieces, for example one of ſilver, and over the firſt I adapt one of zinc; on the ſecond I place one of the moiſtened diſks, then another plate of ſilver followed immediately by another of zinc, over which I place another of the moiſtened diſks. In this manner I continue coupling a plate of ſilver with one of zinc, and always in the ſame order, that is to ſay, the ſilver below and the zinc above it, or *vice verſa*, according as I have begun, and interpoſe between each of theſe couples a moiſtened diſk. I continue to form, of ſeveral of theſe ſtories, a column as high as poſſible without any danger of its falling.

But, if it contain about twenty of theſe ſtories or couples of metal, it will be capable not only of emitting ſigns of electricity by Cavallo's electrometer, aſſiſted by a condenſer, beyond ten or fifteen degrees, and of charging this condenſer by mere contact ſo as to make it emit a ſpark, &c. but of giving to the fingers with which its extremities (the bottom and top of the column) have been touched ſeveral ſmall ſhocks, more or leſs frequent, according as the touching has been repeated. Each of theſe ſhocks has a perfect reſemblance to that ſlight ſhock experienced from a Leyden flaſk weakly charged, or a battery ſtill more weakly charged, or a torpedo in an exceedingly languiſhing ſtate, which imitates ſtill better the effects of my apparatus by the ſeries of repeated ſhocks which it can continually communicate.

To obtain ſuch ſlight ſhocks from this apparatus which I have deſcribed, and which is ſtill too ſmall for great effects, it is neceſſary that the fingers, with which the two extremities are to be touched at the ſame time, ſhould be dipped in water, ſo that the ſkin, which otherwiſe is not a good conductor, may be well moiſtened. To ſucceed with more certainty, and receive ſtronger ſhocks, a communication muſt be made, by means of a metallic plate ſufficiently large, or a large metallic wire, between the bottom of the column (that is to ſay, the lower piece of metal,) and water contained in a baſon or large cup, in which one, two, or three fingers, or the whole hand is to be immerſed, while you touch the top or upper extremity (the uppermoſt or one of the uppermoſt plates of the column) with the clean extremity of another metallic plate held in the other hand, which muſt be very moiſt, and embrace a large ſurface of the plate held very faſt. By proceeding in this manner, I can obtain a ſmall pricking or ſlight ſhock in one or two articulations of a finger a finger immersed in the water of the bason, by touching, with the plate grasped in the other hand, the fourth or even third pair of metallic pieces. By touching then the fifth, the sixth, and the rest in succession till I come to the last, which forms the head of the column, it is curious to observe how the shocks gradually increase in force. But this force is such, that I receive from a column formed of twenty pairs of pieces (not more) shocks which affect the whole finger with considerable pain if it be immersed alone in the water of the bason; which extend (without pain) as far as the wrist, and even to the elbow, if the whole hand, or the greater part of it, be immersed; and are felt also in the wrist of the other hand.

I still suppose that all the necessary attention has been employed in the construction of the column, and that each pair or couple of metallic pieces, resulting from a plate of silver applied over one of zinc, is in communication with the following couple by a sufficient stratum of moisture, consisting of salt water rather than common water, or by a piece of pasteboard, skin, or any thing of the same kind well impregnated with this salt water. The disk must not be too small, and its surface must adhere closely to those of the metallic plates between which it is placed. This exact and extensive application of moistened disks is very important, whereas the metallic plates of each pair may only touch each other in a few points, provided that their contact is immediate.

All this shows that, if the contact of the metals with each other in some points only be sufficient (as they are excellent conductors) to give a free passage to a moderately strong current of electricity, the case is not the same with liquids, or bodies impregnated with moisture, which are conductors much less perfect; and which, consequently, have need of more ample contact with metallic conductors, and still more with each other, in order that the electric fluid may easily pass, and that it may not be too much retarded in its course; especially when it is moved with very little force, as in the present case.

In a word, the effects of my apparatus, that is to say, the shocks felt, are considerably more sensible in proportion as the

the temperature of the ambient air, or that of the water or moiſtened diſks which enter into the compoſition of the column, and that of the water even in the baſon, is warmer, as heat renders the water a better conductor. But almoſt all the ſalts, and particularly common ſalt, will render it a ſtill better. This is one of the reaſons, if not the only one, why it is ſo advantageous that the water of the baſon, and, above all, that interpoſed between each pair of metallic plates, as well as the water with which the circular pieces of paſteboard are impregnated, &c. ſhould be ſalt water, as already obſerved.

But all theſe means and all theſe attentions have only a limited advantage, and will never occaſion your receiving very ſtrong ſhocks as long as the apparatus conſiſts but of one column, formed only of twenty pair of plates, even though they may conſiſt of the two metals propereſt for theſe experiments, *viz.* ſilver and zinc; for if they were ſilver and lead, or tin, or copper and tin, the half of the effect would not be produced, unleſs the weaker effect of each pair were ſupplied by a much greater number. What really increaſes the electric power of this apparatus, and to ſuch a degree as to make it equal or ſurpaſs that of the torpedo or electric eel, is the number of plates arranged in ſuch a manner, and with the attention before mentioned. If to the twenty pairs above deſcribed twenty or thirty others be added diſpoſed in the ſame order, the ſhocks which may be communicated by a column lengthened in this manner will be much ſtronger, and extend to both arms as far as the ſhoulder; and eſpecially of that, the hand of which has been immerſed in the water: this hand, with the whole arm, will remain more or leſs benumbed, if by frequently renewing the touches theſe ſhocks be made to ſucceed each other rapidly, and without intermiſſion. This will be the caſe if the whole hand, or the greater part of it, be immerſed in the water of the baſon; but if only one finger be immerſed, either wholly or in part, the ſhocks being almoſt entirely concentrated in it alone, will become ſo much the more painful, and ſo acute as to be ſcarcely ſupportable.

It may readily be conceived that this column, formed of forty

forty or fifty couples of metals, which gives ſhocks more than moderate to both the arms of one perſon, is capable of giving ſenſible ſhocks alſo to ſeveral perſons, holding each other by the hands (ſufficiently moiſt) ſo as to form an uninterrupted chain.

I ſhall now return to the mechanical conſtruction of my apparatus, which is ſuſceptible of ſeveral variations, and deſcribe not all thoſe which I have invented or made, either on a ſmall or a large ſcale, but only a few, which are either curious or uſeful, which exhibit ſome real advantage, as being eaſier or ſooner conſtructed, and which are certain in their effects, or can be longer preſerved in good order.

I ſhall begin by one which, uniting nearly all theſe advantages, differs moſt in its figure from the columnar apparatus above deſcribed, but which is attended with the inconvenience of being much more voluminous. This new apparatus, which I ſhall call a *couronne de taſſes* (a chain of cups), is repreſented Plate VIII. fig. 1.

I diſpoſe, therefore, a row of ſeveral baſons or cups of any matter whatever, except metal, ſuch as wood, ſhell, earth, or rather glaſs (ſmall tumblers or drinking glaſſes are the moſt convenient), half filled with pure water, or rather ſalt water or ley: they are made all to communicate by forming them into a ſort of chain, by means of ſo many metallic arcs, one arm of which, S*a*, or only the extremity S, immerſed in one of the tumblers, is of copper or braſs, or rather of copper plated with ſilver; and the other, Z*a*, immerſed into the next tumbler, is of tin, or rather of zinc. I ſhall here obſerve, that ley and other alkaline liquors are preferable when one of the metals to be immerſed is tin: ſalt water is preferable when it is zinc. The two metals of which each arc is compoſed, are ſoldered together in any part above that which is immerſed in the liquor, and which muſt touch it with a ſurface ſufficiently large: it is neceſſary therefore that this part ſhould be a plate of an inch ſquare, or very little leſs; the reſt of the arc may be as much narrower as you chooſe, and even a ſimple metallic wire. It may alſo conſiſt of a third metal different from the two immerſed into the tumblers, ſince the action on the electric fluid which reſults
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from all the contacts of several metals that immediately succeed each other, or the force with which this fluid is at last impelled, is absolutely the same, or nearly so, as that which it would have received by the immediate contact of the first metal with the last without any intermediate metals, as I have ascertained by direct experiments, of which I shall have occasion to speak hereafter.

A series of 30, 40, or 60 of these tumblers connected with each other in this manner, and ranged either in a straight or curved line, or bent in every manner possible, forms the whole of this new apparatus, which at bottom and in substance is the same as the other columnar one above described; as the essential part, which consists in the immediate communication of the different metals which form each couple, and the mediate communication of one couple with the other, *viz.* by the intervention of a humid conductor, exist in the one as well as the other.

In regard to the manner of trying these tumblers, and the different experiments for which they may be employed, there is no need of saying a great deal after the ample explanation I have already given respecting the columnar apparatus. It may be readily comprehended, that to obtain a shock it will be sufficient to immerse one hand into one of the tumblers, and a finger of the other hand into another of the tumblers at a considerable distance from the former: that this shock will be stronger the further these glasses are from each other; that is to say, in proportion to the number of the intermediate glasses, and consequently, that the strongest shock will be received when you touch the first and last end of the chain. It will be readily comprehended also, how and why the experiments will succeed much better by grasping and holding fast in one hand, well moistened, a pretty large plate of metal (in order that the communication may be more perfect, and formed in a great number of points), and touching with this plate the water in the tumbler, or rather the metallic arc, while the other is immersed in the other distant tumbler, or touches with a plate, grasped in the like manner, the arc of the latter. In a word, one may comprehend and even foresee the success of a great variety of experiments which may be made with this apparatus or chain of cups much more easily, and in a manner more evident, and which, if I may be allowed the expression, speak more to the eyes than those with the columnar apparatus. I shall therefore forbear from describing a great number of these experiments, which may be easily guessed; and shall relate only a few which are no less instructive than amusing.

Let three twenties of these tumblers be ranged, and connected with each other by metallic arcs, but in such a manner, that, for the first twenty, these arcs shall be turned in the same direction; for example, the arm of silver turned to the left, and the arm of zinc to the right; and for the second twenty in a contrary direction, that is to say, the zinc to the left, and the silver to the right: in the last place, for the third twenty, the silver to the left, as is the case in regard to the first. When every thing is thus arranged, immerse one finger in the water of the first tumbler, and, with the plate grasped in the other hand, as above directed, touch the first metallic arc (that which joins the first tumbler to the second), then the other arc which joins the second and third tumbler, and so on, in succession, till you have touched them all. If the water be very salt and luke-warm, and the skin of the hands well moistened and softened, you will already begin to feel a slight shock in the finger when you have touched the fourth or fifth arc (I have experienced it sometimes very distinctly by touching the third), and by successively proceeding to the sixth and the seventh, &c. the shocks will gradually increase in force to the twentieth arc, that is to say, to the last of those turned in the same direction; but by proceeding onwards to the 21st, 22d, 23d, or 1st, 2d, 3d, of the second twenty, in which they are all turned in a contrary direction, the shocks will each time become weaker, so that at the 36th or 37th, they will be imperceptible, and be entirely null at the 40th, beyond which (and beginning the third twenty, opposed to the second and analogous to the first,) the shocks will be imperceptible to the 44th or 45th arc; but they will begin to become sensible, and to increase gradually, in proportion as you advance to the 60th, where they will have attained the ſame force as that of the 20th arc.

If the twenty arcs in the middle were all turned in the ſame direction as the preceding twenty and the following twenty, that is to ſay, if the whole 60 conſpired to impel the electric fluid in the ſame direction, it may readily be comprehended how much greater the effect will be at the end, and how much ſtronger the ſhock; and it may be comprehended, in general, to what point it muſt be weakened in all caſes where a greater or ſmaller number of theſe forces act contrary to each other by an inverted poſition of metals.

If the chain be in any part interrupted, either by one of the tumblers being empty of water, or one of the metallic arcs being removed or divided into two pieces, you will receive no ſhock when you immerſe your finger into the water of the firſt and another into that of the laſt veſſel; but you will have it ſtrong or weak, according to circumſtances (leaving theſe fingers immerſed), at the moment when the interrupted communication is reſtored; at the moment when another perſon ſhall immerſe into the two tumblers, where the arc is wanting, two of his fingers (which will alſo receive a ſlight ſhock), or rather, when he ſhall immerſe the ſame arc which has been taken away, or any other; and in the caſe of the arc ſeparated into two pieces, at the moment when theſe pieces are again brought into mutual contact (in which caſe the ſhock will be ſtronger than in any other); and, laſtly, in the caſe of the empty tumbler, at the moment when water poured into it ſhall riſe to the two metallic arms immerſed in this cup which before were dry.

When the chain of cups is of ſufficient length, and capable of giving a ſtrong ſhock, you will experience one, though much weaker, even though you keep immerſed two fingers, or the two hands, in one baſon of water of pretty large ſize, in which the firſt and laſt metallic arcs are made to terminate: provided that either of theſe hands thus immerſed, or rather both of them, be kept reſpectively in contact, or nearly in contact, with theſe arcs, you will, I ſay, experience a ſhock at the moment when (the chain being interrupted

interrupted in any part) the communication is reftored, and the circle completed in any of the ways before mentioned. One might be furprifed that in this circle the electric current having a free paffage through an uninterrupted mafs of water, that which fills the bafon, fhould quit this good conductor to throw itfelf and purfue its courfe through the body of the perfon who holds his hands immerfed in the fame water, and thus to take a longer paffage. But the furprife will ceafe if we reflect, that living and warm animal fubftances, and above all, their humours, are, in general, better conductors than water. As the body, then, of the perfon who immerfes his hands in the water, affords an eafier paffage than this water does to the electric current, the latter muft prefer it though a little longer. In a word, the electric fluid, when it muft traverfe imperfect conductors in a large quantity, and particularly moift conductors, has a propenfity to extend itfelf in a larger ftream, or to divide itfelf into feveral, and even to purfue a winding courfe, as it thereby finds lefs refiftance than by following one fingle channel, though fhorter; in the prefent cafe it is only a part of the electric current, which, leaving the water, purfues this new route through the body of the perfon, and traverfes it from the one arm to the other: a greater or lefs part paffes through the water in the veffel. This is the reafon why the fhock experienced is much weaker than when the electric current is not divided when the perfon alone forms the communication between one arc and another, &c.

From thefe experiments one might believe, that when the torpedo wifhes to communicate a fhock to the arms of a man or to animals which touch it, or which approach its body under the water (which fhock is much weaker than what the fifh can give out of the water), it has nothing to do but to bring together fome of the parts of its electric organ in that place, where, by fome interval, the communication is interrupted, to remove the interruptions from between the columns of which the faid organ is formed, or from between its membranes in the form of thin difks, which lie one above the other from the bottom to the fummit of each column: it has, I fay, nothing to do but to remove thefe interrup-

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tions in one or more places, and to produce there the requifite contact, either by compreffing thefe columns, or by making fome moifture to flow in between the pellicles or diaphragms which have been feparated, &c. This is what may be, and what I really conclude to be, the tafk of the torpedo when it gives a fhock; for all the reft, the impulfe and movement communicated to the electric fluid, is only a neceffary effect of its fingular organ, formed, as is feen, of a very numerous feries of conductors, which I have every reafon to believe fufficiently different from each other to be *exciters* of the electric fluid by their mutual contacts; and to fuppofe them ranged in a manner proper for impelling that fluid with a fufficient force from top to bottom, or from the bottom to the top, and for determining a current capable of producing the fhock, &c. as foon and as often as all the neceffary contacts and communications take place.

But let us now leave the torpedo, and its *natural electric organ*, and return to the *artificial electric organ* of my invention, and particularly to my firft *columnar apparatus*, that which imitates the firft even in its form (for that compofed of tumblers is different in that refpect). I might fay fomething alfo in regard to the conftruction of the faid apparatus with tumblers or *a chain of glaffes*; for example, that the firft and laft tumbler fhould be of fuch a fize that, when neceffary, the whole hand might be immerfed in it, &c.; but, to enter into all thefe details, would require too much time.

In regard to the columnar apparatus, I endeavoured to difcover the means of lengthening it a great deal by multiplying the metallic plates in fuch a manner as not to tumble down; and I difcovered, befides others, the following, which are reprefented in the annexed figures. (Plate VIII. fig. 2, 3, 4.)

In Fig. 2, *mmmm* are rods, three, four, or more in number, which rife from the bottom of the column, and confine, as in a cage, the plates or difks, placed each above the other in fuch number and to fuch a height as you choofe, and which thus prevent them from falling. The rods may be of glafs, wood, or metal, only that, in the laft cafe, you muft prevent them from coming into immediate contact with the plates; which may be done either by covering each of them with a glaſs tube, or interpoſing between them and the column a few ſtripes of wax cloth, oiled paper, or even plain paper, and, in a word, any other body that may either be a *cohibent* or a bad conductor: wood or paper will be ſufficiently ſo for our purpoſe, provided only that they are not very damp or moiſt.

But the beſt expedient, when you wiſh to form an apparatus to conſiſt of a great number of plates, above 60, 80, or 100 for example, is, to divide the column into two or more, as ſeen Fig. 3 and 4, (Plate VIII.) where the pieces all have their reſpective poſitions and communication as if there were only one column. Fig. 4, as well as Fig. 3, may indeed be conſidered as a bent column.

In all theſe figures the different metallic plates are denoted by the letters S and Z (which are the initials of ſilver and zinc); and the *moiſtened diſks* (of paſteboard, ſkin, &c. interpoſed between each pair of metals), are repreſented by a black ſtratum. The plates of metal may either be laid ſimply upon each other and ſo brought into union in an indefinite number of points, or they may be ſoldered together. It is altogether indifferent whichever of theſe methods be followed. *cc*, *cc*, *cc*, are the metallic plates which form a communication between each column, or ſection of a column, and another; and *bb*, *bb*, *bb*, are the baſons of water in communication with the lower part or extremities of theſe columns.

An apparatus thus prepared is exceedingly convenient without being bulky; and it might be rendered portable, with ſtill more eaſe and ſafety, by means of circular caſes or tubes, in which each column might be incloſed and preſerved. It is only to be regretted that it does not long continue in a good ſtate: the moiſtened diſks become dry in one or two days to ſuch a degree that they muſt be again moiſtened; which, however, may be done without taking to pieces the whole apparatus, by immerſing the columns, completely formed, in water, and wiping them, when taken out ſome time after, with a cloth, or in any other manner.

The beſt method of making an inſtrument as durable as can be wiſhed, would be, to incloſe and confine the water

interpoſed

interpofed between each pair of metals, and to fix thefe metallic plates in their places by enveloping the whole column with wax or pitch: but this would be fomewhat difficult in the execution, and would require a great deal of patience. I have, however, fucceeded; and have formed in this manner two cylinders confifting of twenty pair of metals, which can ftill be employed though made feveral weeks, and which, I hope, will be ferviceable for months.

Thefe cylinders are attended with this advantage, that they may be employed for experiments either in an erect, inclined, or lying pofition, according as you choofe, or even immerfed in water, provided the top of it be above the furface of the fluid: they might alfo give a fhock when entirely immerfed if they contained a greater number of plates, or if feveral of thefe cylinders were joined together, and if there were any interruption that could be removed at pleafure, &c. by which means thefe cylinders would have a pretty good refemblance to the electric eel; and, to have a better refemblance to it even externally, they might be joined together by pliable metallic wires or fcrew-fprings, and then covered with a fkin terminated by a head and tail properly formed, &c.

The effects fenfible to our organs produced by an apparatus formed of 40 or 50 pair of plates (and even by a fmaller, if one of the metals be filver or copper and the other zinc,) are reduced merely to fhocks: the current of the electric fluid, impelled and excited by fuch a number and variety of different conductors, filver, zinc, and water, difpofed alternately in the manner above defcribed, excites not only contractions and fpafms in the mufcles, convulfions more or lefs violent in the limbs through which it paffes in its courfe; but it irritates alfo the organs of tafte, fight, hearing, and feeling, properly fo called, and produces in them fenfations peculiar to each.

And firft, in regard to the fenfe of feeling: If, by means of an ample contact of the hand (well moiftened) with a plate of metal, or rather, by immerfing the hand to a confiderable depth in the water of the bafon, I eftablifh on one fide a good communication with one of the extremities of my *electro-motive* apparatus, (we muft give new names to inftru-

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ments that are new not only in their form, but in their effects or the principle on which they depend); and on the other I apply the forehead, eye-lid, tip of the nose, also well moistened, or any other part of the body where the skin is very delicate: if I apply, I say, with a little pressure, any one of these delicate parts, well moistened, to the point of a metallic wire, communicating properly with the other extremity of the said apparatus, I experience, at the moment that the conducting circle is completed, at the place of the skin touched, and a little beyond it, a blow and a prick, which suddenly passes, and is repeated as many times as the circle is interrupted and restored; so that, if these alternations be frequent, they occasion a very disagreeable quivering and pricking. But if all these communications continue without these alternations, without the least interruption of the circle, I feel nothing for some moments; afterwards, however, there begins at the part applied to the end of the wire, another sensation, which is a sharp pain (without shock), limited precisely by the points of contact, a quivering, not only continued, but which always goes on increasing to such a degree, that in a little time it becomes insupportable, and does not cease till the circle is interrupted.

What proof more evident of the continuation of the electric current as long as the communication of the conductors forming the circle is continued?—and that such a current is only suspended by interrupting that communication? This endless circulation of the electric fluid (this *perpetual motion*) may appear paradoxical and even inexplicable, but it is no less true and real; and you feel it, as I may say, with your hands. Another evident proof may be drawn from this circumstance, that in such experiments you often experience, at the moment when the circle is suddenly interrupted, a shock, a pricking, an agitation, according to circumstances, in the same manner as at the moment when it is completed; with this only difference, that these sensations, occasioned by a kind of reflux of the electric fluid, or by the shock which arises from the sudden suspension of its current, are of less strength. But I have no need, and this is not the place to bring forward proofs of such an endless circulation of the electric

electric fluid in a circle of conductors, where there are some, which, by being of a different kind, perform, by their mutual contact, the office of exciters or *movers:* this proposition, which I advanced in my first researches and discoveries on the subject of galvanism, and always maintained by supporting them with new facts and experiments, will, I hope, meet with no opposers.

Recurring to the sensation of pain which is felt in the experiments above described, I must add, that if this pain be very strong and pricking in the parts covered by the skin, it is much more so in those where the skin has been taken off—in recent wounds for example. If by chance there should be a small incision or bit of the skin rubbed off in the finger which I immerse in the water that communicates with one of the extremities of the *electro-motive* apparatus, I experience there a pain so acute, when, by establishing the proper communication with the other extremity, I complete the circle, that I must soon desist from the experiment; that is to say, must withdraw my finger, or interrupt the circle in some other manner. I will say more; that I cannot even endure it above a few seconds when the part of the apparatus which I put in play, or the whole apparatus, contains only twenty pair of plates, or about that number.

One thing, which I must still remark, is, that all these sensations of pricking and pain are stronger and sharper, every thing else being equal, when the part of the body which is to feel them is towards the negative electricity; that is to say, placed in such a manner in the conducting circle, that the electric fluid traversing that circle is not directed towards that sensible part, does not advance towards it, and enter from the outside inwards, but takes its direction from the inside outwards; in a word, that it issues from it: in regard to which it is necessary to know, of the two metals that enter by pairs into the construction of the machine, which is the one that gives off to the other. But I had already determined this respecting all the metals by other experiments, published a long time ago at the end of my first memoirs on galvanism. I shall therefore say nothing further here, than that the whole is completely confirmed by the

the experiments, equally and still more demonstrative and striking, with which I am at present employed.

In regard to the sense of taste, I had before discovered, and published in these first memoirs, where I found myself obliged to combat the pretended animal electricity of Galvani, and to declare it an external electricity moved by the mutual contact of metals of different kinds,—I had discovered, I say, in consequence of this power which I ascribed to metals, that two pieces of these different metals, and particularly one of silver and one of zinc, applied in a proper manner, excited at the tip of the tongue very sensible sensations of taste; that the taste was decidedly acid, if, the tip of the tongue being turned towards the zinc, the electric current proceeded against it, and entered it; and that another taste, less strong but more disagreeable, acrid, and inclining to alkaline, was felt, if (the position of the metals being reversed) the electric current issued from the tip of the tongue; that these sensations continued and received even an increase for several seconds, if the mutual contact of the two metals was maintained, and if the conducting circle was nowhere interrupted. But when I have said here, that exactly the same phenomena take place when you try, instead of one pair of these metallic pieces, an assemblage of several of them ranged in the proper manner; and that the said sensations of taste, whether acid or alkaline, increase but a little with the number of these pairs, I have said the whole. It only remains for me to add that, if the apparatus put in play for these experiments on the tongue be formed of a sufficiently large number of metallic pairs of this kind, for example, if it contain 30, 40, or more, the tongue experiences not only the sensation of taste already mentioned, but, besides that, a blow which it receives at the moment when the circle is completed, and which occasions in it a pricking more or less painful, but fleeting, followed some moments after by a durable sensation of taste. This blow produces even a convulsion or agitation of a part or of the whole of the tongue; when the apparatus, formed of a still greater number of pairs of the said metals, is more active, and if, by means of good communicating conductors, the electric current which it excites be able to pass every where with perfect freedom.

I must often recur to, and insist on, this last condition, because it is essential in all experiments when you wish to obtain sensible effects on the body, or commotions in the limbs, or sensations in the organs of the senses. It is necessary, therefore, that the non-metallic conductors which enter into the circle should be as good conductors as possible, well moistened (if they are not themselves liquid) with water, or with any other liquid that may be a better conductor than pure water; and it is necessary, besides, that the well moistened surfaces, by which they communicate with the metallic conductor, should be sufficiently large. The communication ought to be confined or reduced to a small number of points of contact only in that place where you wish to concentrate the electric action on one of the most sensible parts of the body, on any of the sensitive nerves, &c. as I have already remarked in speaking of the experiments on feeling, *viz.* those by which acute pains are excited in different parts. The best method which I have found for producing on the tongue all the sensations above described, is, to apply the tip of it to the pointed extremity (which, however, must not be too much so) of a metallic rod, which I make to communicate properly, as in the other experiments, with one of the extremities of my apparatus, and to establish a good communication between the hand, or, what is better, both the hands together, and the other extremity. This application of the tip of the tongue to the end of the metallic rod, may either exist already, when you are going to make the other communication to complete the circle (when you are going to immerse your hand into the water of the bason), or be made after the establishment of this communication, while the hand is immersed; and in the latter case I think I feel the pricking and shock in the tongue, a very short time before actual contact. Yes; it always appears to me, particularly if I advance the tip of my tongue gradually, that, when it has arrived within a very small distance of the metal, the electric fluid (I would almost say spark), overcoming this interval, darts forwards to strike it.

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In regard to the sense of sight, which I also found might be affected by the weak current of the electric fluid, arising from the mutual contact of two different metals in general, and in particular, of a piece of silver and one of zinc, it was natural to expect that the sensation of light, excited by my new apparatus, would be stronger in proportion as it contained a greater number of pieces of these metals; each pair of which, arranged in the proper manner, adds a degree of force to the said electric current, as all the other experiments show, and particularly those with the electrometer assisted by the condenser, which I have only mentioned, and which I shall describe on another occasion. But I was surprised to find that, with 10, 20, 30 pairs, and more, the flash produced neither appeared longer and more extended, nor much brighter than with one pair. It is true, however, that this sensation of weak and transient light, is excited by such an apparatus much easier and in different ways. To succeed, indeed, with one pair, the following are almost the only methods; *viz.* that one of the metallic pieces should be applied to the ball of the eye, or the eye-lid well moistened, and that it should be made to touch the other metal applied to the other eye, or held in the mouth, which produces a a flash much more beautiful; or, that this second metallic piece should be held in the moistened hand and then brought into contact with the former; or, in the last place, that these two plates should be applied to certain parts of the inside of the mouth, making them communicate with each other. But with an apparatus of 20 or 30 pairs, &c. the same flash will be produced by applying the end of a metallic plate or rod, placed in communication with one of the extremities of the apparatus, to the eye, while with one hand you form a proper communication with the other extremity; by bringing, I say, this plate into contact not only with the eye or any part of the mouth, but even the forehead, the nose, the cheeks, lips, chin, and even the throat; in a word, every part and point of the visage, which must only be well moistened before they are applied to the metallic plate. The form as well as the force of this transient light which is perceived varies a little, if the places of the face to which the action of the electric current is applied, be varied: if it be on the forehead, for example, this light is moderately bright, and appears like a luminous circle, under which figure it presents itself also in several other experiments.

But the most curious of all these experiments is, to hold the metallic plate between the lips, and in contact with the tip of the tongue; since, when you afterwards complete the circle in the proper manner, you excite at once, if the apparatus be sufficiently large and in good order, and the electric current sufficiently strong and in good order, a sensation of light in the eyes, a convulsion in the lips, and even in the tongue, and a painful prick at the tip of it, followed by a sensation of taste.

I have now only to say a few words on hearing. This sense, which I had in vain tried to excite with only two metallic plates, though the most active of all the *exciters* of electricity, *viz.* one of silver or gold, and the other of zinc, I was at length able to affect it with my new apparatus, composed of 30 or 40 pairs of these metals. I introduced, a considerable way into both ears, two probes or metallic rods with their ends rounded, and I made them to communicate immediately with both extremities of the apparatus. At the moment when the circle was thus completed I received a shock in the head, and some moments after (the communication continuing without any interruption) I began to hear a sound, or rather noise, in the ears, which I cannot well define: it was a kind of crackling with shocks, as if some paste or tenacious matter had been boiling. This noise continued incessantly, and without increasing, all the time that the circle was complete, &c. The disagreeable sensation, and which I apprehended might be dangerous, of the shock in the brain, prevented me from repeating this experiment.

There still remains the sense of smelling, which I have hitherto tried in vain with my apparatus. The electric fluid, which, when made to flow in a current in a complete circle of conductors, produces in the limbs and parts of the living body effects correspondent to their excitability, which stimulating in particular the organs or nerves of touch, taste, sight, and hearing, excite in them some sensations peculiar to each of

these

theſe ſenſes, as I have found, produces in the interior of the noſe only a pricking more or leſs painful, and commotions more or leſs extenſive, according as the ſaid current is weaker or ſtronger. And whence comes it, then, that it does not excite any ſenſation of ſmell, though, as appears, it ſtimulates the nerves of that ſenſe? It cannot be ſaid that the electric fluid of itſelf is not proper for producing odorous ſenſations, ſince, when it diffuſes itſelf through the air in the form of aigrettes, &c. in the common experiments made with electric machines, it conveys to the noſe a very ſenſible ſmell reſembling that of phoſphorus. Taking ſimilitude into conſideration, and reaſoning from its analogy with other odoriferous matters, I will ſay, that it muſt completely diffuſe itſelf throughout the air to excite ſmell; that it has need, like other effluvia, of the vehicle of the air to affect that ſenſe in ſuch a manner as to excite the ſenſations of ſmell. But in the experiments of which I ſpeak, that is to ſay, of an electric current in a circle of conductors, all contiguous, and without the leaſt interruption, this abſolutely cannot take place.

All the facts which I have related in this long paper in regard to the action which the electric fluid excited, and when moved by my apparatus, exerciſes on the different parts of our body which the current attacks and paſſes through;—an action which is not momentaneous, but which laſts, and is maintained during the whole time that this current can follow the chain not interrupted in its communications; in a word, an action the effects of which vary according to the different degrees of excitability in the parts, as has been ſeen;—all theſe facts, ſufficiently numerous, and others which may be ſtill diſcovered by multiplying and varying the experiments of this kind, will open a very wide field for reflection, and of views, not only curious, but particularly intereſting to medicine. There will be a great deal to occupy the anatomiſt, the phyſiologiſt, and the practitioner.

It is well known, by the anatomy which has been made of it, that the electric organ of the torpedo or electric eel, conſiſts of ſeveral membranaceous columns, filled from one end to the other with a great number of plates or pellicles,

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in the form of very thin difks, placed one upon the other, or fupported at very fmall diftances by intervals, into which, as appears, fome liquor flows. But we cannot fuppofe that any of thefe laminæ are of an infulating nature, like glafs, refin, filk, &c. and ftill lefs that they can either become electric by friction, or be difpofed and charged in the fame manner as the fmall Franklinian plates or fmall electrophores; nor even that they are fufficiently bad conductors to perform the office of a good and durable condenfer, as Mr. Nicholfon has fuppofed. The hypothefis of this learned and laborious philofopher, by which he makes of each pair of thefe pellicles, which he compares to leaves of talc, as many fmall *electrophores* or *condenfers*, is indeed very ingenious, and is, perhaps, the beft theory that has been devifed to explain the phenomena of the torpedo, adhering to the hitherto known principles and laws of electricity. For, befides that the mechanifm, by which, every time that the fifh intended to give a fhock, the refpective feparation of the plates of the whole or a great number of thefe electrophores or condenfers ought to be effected all at once, and ought to eftablifh on the one hand a communication between themfelves of all the plates electrified *pofitively*, and on the other a communication between all thofe electrified *negatively*, as Mr. Nicholfon fuppofes—befides, that this very complex mechanifm appears too difficult, and little agreeable to nature;—and befides, that the fuppofition of an electric charge originally impreffed, and fo durable in thefe pellicles performing the office of electrophores, is altogether gratuitous,—fuch a hypothefis falls entirely, fince thefe pellicles of the organ of the torpedo are not, and cannot be, in any manner infulating or fufceptible of a real electric charge, and much lefs capable of retaining it. Every animal fubftance, as long as it is frefh, furrounded with juices, and more or lefs fucculent of itfelf, is a very good conductor. I fay more, inftead of being as *cohibent* as refins or talc, to leaves of which Mr. Nicholfon has compared the pellicles in queftion, there is not, as I have affured myfelf, any living or frefh animal fubftance which is not a better *deferent* than water, except only greafe and fome oily humours. But neither thefe humours nor greafe, efpe-

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cially semi-fluid or entirely fluid, as it is found in living animals, can receive an electric charge in the manner of insulating plates, and retain it: besides, we do not find that the pellicles and humours of the organ of the torpedo are greasy or oily. This organ therefore, composed entirely of conducting substances, cannot be compared either to the electrophore or condenser, or to the Leyden flask, or any machine excitable by friction or by any other means capable of electrifying insulating bodies, which before my discoveries were always believed to be the only ones originally electric.

To what electricity then, or to what instrument ought the organ of the torpedo or electric eel, &c. to be compared? To that which I have constructed according to the new principle of electricity, discovered by me some years ago, and which my successive experiments, particularly those with which I am at present engaged, have so well confirmed, *viz.* that conductors are also, in certain cases, exciters of electricity in the case of the mutual contact of those of different kinds, &c. in that apparatus which I have named the *artificial electric organ*, and which being at bottom the same as the natural organ of the torpedo, resembles it also in its form, as I have advanced.

II. *Investigation of the Powers of the Prismatic Colours to Heat and Illuminate Objects; with Remarks that prove the different Refrangibility of Radiant Heat: to which is added, an Inquiry into the Method of viewing the Sun advantageously, with Telescopes of large Apertures and high Magnifying Powers.* By WILLIAM HERSCHEL, *LL.D. F.R.S.**

IT is sometimes of great use in natural philosophy to doubt of things that are commonly taken for granted; especially as the means of resolving any doubt, when once it is entertained, are often within our reach. We may therefore say that any experiment which leads us to investigate the truth of

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of what was before admitted upon trust may become of great utility to natural knowledge. Thus, for instance, when we see the effect of the condensation of the sun's rays in the focus of a burning lens, it seems to be natural to suppose that every one of the united rays contributes its proportional share to the intensity of the heat which is produced; and we should probably think it highly absurd, if it were asserted that many of them had but little concern in the combustion or vitrification which follows, when an object is put into that focus. It will therefore not be amiss to mention what gave rise to a surmise, that the power of heating and illuminating objects might not be equally distributed among the variously coloured rays.

In a variety of experiments which I have occasionally made, relating to the method of viewing the sun with large telescopes to the best advantage, I used various combinations of differently-coloured darkening glasses. What appeared remarkable was, that when I used some of them I felt a sensation of heat, though I had but little light; while others gave me much light, with scarce any sensation of heat. Now, as in these different combinations the sun's image was also differently coloured, it occurred to me, that the prismatic rays might have the power of heating bodies very unequally distributed among them; and, as I judged it right in this respect to entertain a doubt, it appeared equally proper to admit the same with regard to light. If certain colours should be more apt to occasion heat, others might, on the contrary, be more fit for vision, by possessing a superior illuminating power. At all events, it would be proper to recur to experiments for a decision.

Experiments on the heating Power of coloured Rays.

I fixed a piece of pasteboard, AB, (Plate IX.) in a frame, mounted upon a stand, CD, and moveable upon two centres. In the pasteboard I cut an opening, *mn*, a little larger than the ball of a thermometer, and of a sufficient length to let the whole extent of one of the prismatic colours pass through. I then placed three thermometers upon small inclined planes, EF:

EF; their balls were blacked with japan ink. That of No. 1 was rather too large for great ſenſibility. No. 2 and 3 were two excellent thermometers, which my highly eſteemed friend Dr. Wilſon, late profeſſor of aſtronomy at Glaſgow, had lent me for the purpoſe: their balls being very ſmall, made them of exquiſite ſenſibility. The ſcales of all were properly diſengaged from the balls.

I now placed the ſtand, with the framed paſteboard and the thermometers, upon a ſmall plain board, GH, that I might be at liberty to move the whole apparatus together, without deranging the relative ſituation of the different parts.

This being done, I ſet a priſm, moveable on its axis, into the upper part of an open window, at right angles to the ſolar ray, and turned it about till its refracted coloured ſpectrum became ſtationary upon a table placed at a proper diſtance from the window.

The board containing the apparatus was now put on the table, and ſet in ſuch a manner as to let the rays of one colour paſs through the opening in the paſteboard. The moveable frame was then adjuſted to be perpendicular to the rays coming from the priſm; and the inclined planes carrying the three thermometers, with their balls arranged in a line, were ſet ſo near the opening, that any one of them might eaſily be advanced far enough to receive the irradiation of the colour which paſſed through the opening, while the reſt remained cloſe by, under the ſhade of the paſteboard.

By repeated trials, I found that Dr. Wilſon's No. 2, and mine, always agreed in ſhowing the temperature of the place where I examined them, when the change was not very ſudden; but that mine would require ten minutes to take a change, which the other would ſhow in five. No. 3 never differed much from No. 2.

1ſt Experiment. Having arranged the three thermometers in the place prepared for the experiment, I waited till they were ſtationary. Then, advancing No. 1. to the red rays, and leaving the other two cloſe by, in the ſhade, I marked down what they ſhowed at different times.

No. 1.	No. 2.	No. 3.	
43½	43½	43¼	This, in about 8 or 10 minutes, gave 6½ degrees, for the rising produced in my thermometer by the red rays, compared to the two standard thermometers.
48	43½	43½	
49½	43¼	43¼	
49¾	43¼	43¼	
50	43¼	43¼	

2d Experiment. As soon as my thermometer was restored to the temperature of the room, which I hastened, by applying it to a large piece of metal that had been kept in the same place, I exposed it again to the red rays, and registered its march, along with No. 2 as a standard, which was as follows:

No. 1.	No. 2.	
45	45	Hence, in 10 minutes, the red rays made the thermometer rise 7 degrees.
48	45	
51	45	
51	44½	
51	44	

3d Experiment. Proceeding in the same manner as before, in the green rays I had,

No. 1.	No. 2.	
43	43	Therefore, in 10 minutes, the green rays occasioned a rise of 3¼ degrees.
45½	43	
46	43	
46	42¾	
46	42¾	

4th Experiment. I now exposed my thermometer to the violet rays, and compared it with No. 2.

No. 1.	No. 2.	
44	44	Here we have a rising of 2 degrees in 10 minutes, for the violet rays.
44	44	
44¾	43½	
45	43	

5th Experiment. I now exposed Dr. Wilson's thermometer No. 2 to the red rays, and compared its progress with No. 3.

No. 2.	No. 3.	
44	44	Here the thermometer, exposed to red, rose in five minutes 2¾ degrees.
46	44	
46½	43¾	
46¾	43¾	

6th Ex-

6th Experiment. In red rays again.

No. 2.	No. 3.	
44	44	And here the thermometer, expofed to red, rofe in five minutes 4 degrees.
46	44	
$46\frac{1}{2}$	$43\frac{1}{2}$	
47	$43\frac{1}{2}$	
47	43	

7th Experiment. In green rays.

No. 2.	No. 3.	
$43\frac{1}{2}$	$43\frac{1}{2}$	This made the thermometer rife, in the green rays, $1\frac{1}{2}$ degree.
$44\frac{1}{2}$	$43\frac{1}{2}$	
$44\frac{1}{2}$	43	

8th Experiment. Again in green rays.

No. 2.	No. 3.	
43	43	Here the rifing by the green rays, was 2 degrees.
$44\frac{1}{2}$	$42\frac{1}{4}$	
$44\frac{3}{4}$	$42\frac{3}{4}$	

From thefe experiments we are authorifed to draw the following refults: in the red rays my thermometer gave $6\frac{3}{4}$ degrees in the 1ft, and 7 degrees in the 2d, for the rifing of the quickfilver: a mean of both is $6\frac{7}{8}$. In the 3d experiment we had $3\frac{1}{4}$ degrees, for the rifing occafioned by the green rays; from which we obtain the proportion of 55 to 26, for the power of heating in red to that in green. The 4th experiment gave 2 degrees for the violet rays; and therefore we have the rifing of the quickfilver in red to that in violet, as 55 to 16.

A fufficient proof of the accuracy of this determination we have in the refult of the four laft experiments. The rifing for red rays in the 5th, is $2\frac{3}{4}$; and in the 6th, 4 degrees: a mean of both is $3\frac{3}{8}$. In the 7th experiment we have $1\frac{1}{2}$, and in the 8th 2 degrees, for the rifing in the green: a mean of thefe is $1\frac{3}{4}$. Therefore, we have the proportion of the rifing in red to that in green, as 27 to 11, or as 55 to 22,4.

We may take a mean of the refult of both thermometers, which will be 55 to 24,2, or more than $2\frac{1}{4}$ to 1, in red to green; and about $3\frac{1}{2}$ to 1, in red to violet.

It appears remarkable, that the moft fenfible thermometer fhould give the leaft alteration, from the expofure to the coloured rays. But fince, in thefe circumftances, there are two caufes conftantly acting different ways: the one to raife the

 thermometer,

thermometer, the other to bring it down to the temperature of the room, I suppose that, on account of the smallness of the ball in Dr. Wilson's No. 1, which is but little more than $\frac{1}{8}$th of an inch, the cooling causes must have a stronger effect on the mercury which it contains than they can have on mine, the ball of which is half an inch.

More accuracy may hereafter be obtained by attending to the circumstances of blacking the balls of the thermometers, and their exposure to a more steady and powerful light of the sun, at greater altitudes than it can be had at present; but the experiments which have been related are quite sufficient for my present purpose; which only goes to prove that the heating power of the prismatic colours is very far from being equally divided, and that the red rays are chiefly eminent in that respect.

Experiments on the illuminating Power of coloured Rays.

In the following examination of the illuminating power of differently coloured rays, I had two ends in view. The first was with regard to the illumination itself, and the next with respect to the aptness of the rays for giving distinct vision; and, though there did not seem to be any particular reason why these two should not go together, I judged it right to attend to both.

The microscope offered itself as the most convenient instrument for this investigation; and I thought it expedient to view only opaque objects, as these would give me an opportunity to use a direct prismatic ray, without running the risk of any bias that might be given to it, in its transmission through the colouring particles of transparent objects.

1st Experiment. I placed an object that had very minute parts under a double microscope; and, having set a prism in the window, so as to make the coloured image of the sun stationary upon the table where the microscope was placed, I caused the differently coloured rays to fall successively on the object by advancing the microscope into their light. The magnifying power was 27 times.

In changing the illumination, by admitting a different colour, it always becomes necessary to re-adjust the instrument. It is well known, that the different refrangibility of the rays will

will fenfibly affect the focal length of object-glaffes; but in compound vifion, fuch as in a microfcope, where a very fmall lens is made to caft a lengthened fecondary focus, this difference becomes ftill more confiderable.

By an attentive and repeated infpection, I found that my object was very well feen in red, better in orange, and ftill better in yellow; full as well in green; but to lefs advantage in blue; indifferently well in indigo, and with more imperfection in violet.

The trial was made upon one of the microfcopic objects which are generally prepared for tranfparent vifion; but as I ufed it in the opaque way, I thought that others might be chofen which would anfwer the purpofe better; and, in order to give fome variety to my experiments, and to fee the effect which differently coloured fubftances might have on the rays of light, I provided the following materials to be viewed:—red paper, green paper, a piece of brafs, a nail, a guinea, black paper. Having alfo found that a higher power might be ufed, with fufficient convenience for the rays of light to come from the prifm to the object, I made the microfcope magnify 42 times.

The appearance of the nail in the microfcope is fo beautiful, that it deferves to be noticed; and the more fo, as it is accompanied with circumftances that are very favourable for an inveftigation, fuch as that which is under our prefent confideration. I had chofen it on account of its folidity and blacknefs, as being moft likely to give an impartial refult of the modifications arifing from an illumination by differently coloured rays; but, on viewing it, I was ftruck with the fight of a bright conftellation of thoufands of luminous points fcattered over its whole extent, as far as the field of the microfcope could take it in. Their light was that of the illuminating colour, but differed confiderably in brightnefs; fome of the points being dim and faint, while others were luminous and brilliant. The brighteft of them alfo admitted of a little variation in their colour, or rather in the intenfity of the fame colour; for in the centre of fome of the moft brilliant of thefe lucid appearances, their light had more vivacity, and feemed to deviate from the illuminating tint towards white-

nefs;

ness, while on and near the circumference, it appeared to take a deeper hue.

An object so well divided by nature into very minute and differently arranged points, on which the attention might be fixed, in order to ascertain whether they would be equally distinct in all colours, and whether their number would be increased or diminished by different degrees of illumination, was exactly what I wanted; nor could I think it less remarkable, that all the other objects I had fixed upon, besides many more which have been examined, such as copper, tin, silver, &c. presented themselves nearly with the same appearance. In the brass, which had been turned in a lathe, the luminous points were arranged in furrows; and in tin they were remarkably beautiful. The result of the examination of my objects was as follows:

2d Experiment. Red paper.—In the red rays I view a bright point near an accidental black spot in the paper, which serves me as a mark; and I notice the space between the point and the spot; it contains several faint points: In the orange rays I see better; the bright point I now perceive is double: In the yellow rays, I see the object still better: In the green rays, full as well as before: In the blue rays, very well: In the indigo rays, not quite so well as in the blue: In the violet rays very imperfectly.

3d Experiment. Green paper.—*Red.* I fix my attention on many faint spots in a space between two bright double points.—*Orange.* I see those faint points better.—*Yellow.* Still better.—*Green.* As well as before; I see remarkably well.—*Blue.* Less bright, but very distinct.—*Indigo.* Not well.—*Violet.* Bad.

4th Experiment. A piece of very clean turned brass.—*R.* I remark several faint luminous points between two bright ones; the colour of the brass makes the red rays appear like orange.—*O.* I see better, but the orange colour is likewise different from what it ought to be; however, this is not at present the object of my investigation.—*Y.* I see still better.—*G.* I see full as well as before.—*B.* I do not see so well now.—*I.* I cannot see well.—*V.* Bad.

5th Experiment. A nail.—*R.* I remark two bright points and

and ſome faint ones.—*O.* Brighter than before, and more points viſible; very diſtinct.—*Y.* Much brighter than before, and more points and lines viſible; very diſtinct.—*G.* Full as bright, and as many points viſible; very diſtinct.—*B.* Much leſs bright; very diſtinct.—*I.* Still leſs bright; very diſtinct.—*V.* Much leſs bright again; very diſtinct.

6th Experiment. I viewed a guinea at 9 feet 6 inches from the priſm, and adjuſted the place of the object in the ſeveral rays by the ſhadow of the guinea; if this be not done, deceptions will take place.—*R.* Four remarkable points; very diſtinct.—*O.* Better illuminated; very diſtinct.—*Y.* Still better illuminated; very diſtinct. The points all over the field of view are coloured; ſome green, ſome red, ſome yellow, and ſome white encircled with black about them; between yellow and green is the maximum of illumination. Extremely diſtinct.—*G.* As well illuminated as the yellow; very diſtinct.—*B.* Much inferior in illumination; very diſtinct.—*I.* Badly illuminated; diſtinct.—*V.* Very badly illuminated: I can hardly ſee the object at all.

7th Experiment. The nail again at 8 feet from the priſm.—*R.* I attended to two bright points, with faint ones between them; almoſt all the points in the field of view are red; very diſtinct.—*O.* I ſee all the points better; they are red, green, yellow, and whitiſh, with black about them; very diſtinct.—*Y.* I ſee better; more bright points and more faint ones; the points are of various colours; very diſtinct.—*G.* I ſee as well; the points are moſtly green and brightiſh-green, inclining to white; very diſtinct.—*B.* Much worſe illuminated; very diſtinct.—*I.* Badly illuminated; very diſtinct.—*V.* There is hardly any illumination.

8th Experiment. The nail again at 9 feet 6 inches from the priſm, by way of having the rays better ſeparated.—*R.* Badly illuminated; the bright points are very diſtinct.—*O.* Much better illuminated; the bright points very diſtinct. *Y.* Still better illuminated; all points extremely diſtinct.—*G.* As well illuminated, and equally diſtinct.—*B.* Badly illuminated; the bright points are diſtinct, but the others are not ſo.—*I.* Very badly illuminated; I do not ſee diſtinctly; but I believe it to be for want of light.—*V.* So badly illuminated

illuminated that I cannot see the object; or at least but barely perceive that it exists.

9th Experiment. Black paper at 8 feet from the prism.—*R.* The object is hardly visible; I can only see a few faint points.—*O.* I see several bright points and many faint ones.—*Y.* Numberless bright and small faint points; between yellow and green is the maximum of illumination.—*G.* The same as the yellow.—*B.* Very indifferently illuminated; but not so bad as in the red rays.—*I.* I cannot see the object.—*V.* Totally invisible.

From these observations, which agree uncommonly well with respect to the illuminating power assigned to each colour, we may conclude that the red-making rays are very far from having it in any eminent degree. The orange possess more of it than the red; and the yellow rays illuminate objects still more perfectly. The maximum of illumination lies in the brightest yellow, or palest green. The green itself is nearly equally bright with the yellow; but, from the full deep green, the illuminating power decreases very sensibly. That of the blue is nearly upon a par with that of the red; the indigo has much less than the blue; and the violet is very deficient.

With regard to the principle of distinctness, there appears to be no deficiency in any one of the colours. In the violet rays, for instance, some of the experiments mention that I saw badly; but this is to be understood only with respect to the number of small objects that could be perceived; for, although I saw fewer of the points, those which remained visible were always as distinct as, in so feeble an illumination, could be expected. It must indeed be evident that, by removing the great obstacle to distinct vision, which is the different refrangibility of the rays of light, a microscope will be capable of a much higher degree of distinctness than it can be under the usual circumstances. A celebrated optical writer has formerly remarked that a fly, illuminated by red rays, appeared uncommonly distinct, and that all its minute parts might be seen in great perfection; and, from the experiments which have been related, it appears that every other colour is possessed of the same advantage.

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I am well aware that the refults I have drawn from the foregoing experiments, both with regard to the heating and illuminating powers of differently coloured rays, muft be affected by fome little inaccuracies. The prifm, under the circumftances in which I have ufed it, could not effect a complete feparation of the colours; on account of the apparent diameter of the fun, and the confiderable breadth of the prifm itfelf, through which the rays were tranfmitted.

Perhaps an arrangement like that in Fig. 16. of the Newtonian experiments might be employed, if inftruments of fufficient fenfibility, fuch as air thermometers, can be procured, that may be affected by the enfeebled illumination of rays that have undergone four tranfmiffions and eight refractions; and efpecially when their incipient quantity has been fo greatly reduced in their limited paffage through a fmall hole at the firft incidence.

But it appeared moft expedient for me, at prefent, to neglect all further refinements, which may be attempted hereafter at leifure. It may even be prefumed that, had there not been fome fmall admixture of the red rays in the other colours, the refult would have been ftill more decifive with regard to the power of heating vefted in the red rays. And it is likewife evident, that at leaft the red light of the prifmatic fpectrum was much lefs adulterated than any of the other colours; their refractions tending all to throw them from the red. That the fame rays which occafion the greateft heat, have not the power of illumination in any ftrong degree, ftands on as good a foundation. For, fince here alfo they have undergone the faireft trial, as being moft free from other colours, it is equally proved that they illuminate objects but imperfectly. There is fome probability that a ray, purified in the Newtonian manner above quoted, efpecially in a well darkened room, may remain bright enough to ferve the purpofe of microfcopic illumination, in which cafe more precifion can eafily be obtained.

The greateft caufe for a mixture of colours, however, which is the breadth of the prifm, I faw might eafily be removed; therefore, on account of the coloured points, which have been mentioned in the 6th and 7th experiments, I was willing to try whether they proceeded from this mixture; and therefore covered the prifm in front with a piece of pafteboard, having a flit in it of about one-tenth of an inch broad.

10th Experiment. The nail at 9 feet 2 inches from the prifm.—*R.* I fix my attention on two fhining red points; they are pretty bright.—*O.* I fee many more points; the object is better illuminated than in the red; the points are furrounded by black, but are orange-coloured.—*Y.* The points now are yellow, and white furrounded by black; the object is better illuminated than in orange. The maximum of illumination is in the brighteft yellow or paleft green.—*G.* The points are green and white, as before furrounded by black; better illuminated than in orange.—*B.* The illumination is nearly equal to red.—*I.* Very indifferently illuminated.—*V.* Very badly illuminated.

The phænomena of the differently coloured points being now completely refolved, fince they were plainly owing to the former admixture of colours, and the illuminating power remaining afcertained as before, I attempted alfo to repeat the experiments upon the thermometer, with the prifm covered in the fame manner; but I found the effect of the coloured rays too much enfeebled to give a decifive refult.

I might now proceed to my next fubject; but it may be pardonable if I digrefs for a moment, and remark that the foregoing refearches ought to lead us on to others. May not the chemical properties of the prifmatic colours be as different as thofe which relate to light and heat? Adequate methods for an inveftigation of them may eafily be found; and we cannot too minutely enter into an analyfis of light, which is the moft fubtile of all the active principles that are concerned in the mechanifm of the operations of nature. A better acquaintance with it may enable us to account for various facts that fall under our daily obfervation, but which have hitherto remained unexplained. If the power of heating, as we now fee, be chiefly lodged in the red-making rays, it accounts for the comfortable warmth that is thrown out from a fire, when it is in the ftate of a red glow; and for the heat which is given by charcoal, coke, and balls of fmall-

 coal

coal mixed up with clay, used in hot-houses; all which, it is well known, throw out red light. It also explains the reason why the yellow, green, blue, and purple flames of burning spirits mixed with salt, occasion so little heat, that a hand is not materially injured when passed through their corruscations. If the chemical properties of colours also, when ascertained, should be such that an acid principle, for instance, which has been ascribed to light in general, on account of its changing the complexion of various substances exposed to it, may reside only in one of the colours, while others may prove to be differently invested, it will follow that bodies may be variously affected by light, according as they imbibe and retain, or transmit and reflect, the different colours of which it is composed.

Radiant Heat is of different Refrangibility.

I must now remark that my foregoing experiments ascertain, beyond a doubt, that radiant heat, as well as light, whether they be the same or different agents, is not only refrangible, but is also subject to the laws of the dispersion arising from its different refrangibility; and, as this subject is new, I may be permitted to dwell a few moments upon it. The prism refracts radiant heat, so as to separate that which is less efficacious from that which is more so. The whole quantity of radiant heat contained in a sun-beam, if this different refrangibility did not exist, must inevitably fall uniformly on a space equal to the area of the prism; and, if radiant heat were not refrangible at all, it would fall upon an equal space in the place where the shadow of the prism, when covered, may be seen. But neither of these events taking place, it is evident that radiant heat is subject to the laws of refraction, and also to those of the different refrangibility of light. May not this lead us to surmise that radiant heat consists of particles of light of a certain range of momenta, and which range may extend a little further, on each side of refrangibility, than that of light? We have shown that, in a gradual exposure of the thermometer to the rays of the prismatic spectrum, beginning from the violet, we come to the maximum of light long before we come to that of heat, which lies at the other extreme. By several experiments, which time will not allow me now to report, it appears that the maximum of illumination has little more than half the heat of the full red rays; and, from other experiments, I likewise conclude that the full red falls still short of the maximum of heat; which perhaps lies even a little beyond visible refraction. In this case, radiant heat will at least partly, if not chiefly, consist, if I may be permitted the expression, of invisible light; that is to say, of rays coming from the sun, that have such a momentum as to be unfit for vision. And admitting, as is highly probable, that the organs of sight are only adapted to receive impressions from particles of a certain momentum, it explains why the maximum of illumination should be in the middle of the refrangible rays; as those which have greater or less momenta are likely to become equally unfit for impressions of sight. Whereas in radiant heat, there may be no such limitation to the momentum of its particles. From the powerful effects of a burning lens, however, we gather the information, that the momentum of terrestrial radiant heat is not likely to exceed that of the sun; and that, consequently, the refrangibility of *calorific* rays cannot extend much beyond that of *colourific* light. Hence we may also infer that the invisible heat of red-hot iron, gradually cooled till it ceases to shine, has the momentum of the invisible rays which, in the solar spectrum viewed by day-light, go to the confines of red; and this will afford an easy solution of the reflection of invisible heat by concave mirrors.

Application of the Result of the foregoing Observations to the Method of viewing the Sun advantageously, with Telescopes of large Apertures and high Magnifying Powers.

Some time before the late transit of Mercury over the disk of the sun, I prepared my 7-feet telescope, in order to see it to the best advantage. As I wished to keep the whole aperture of the miror open, I soon cracked every one of the darkened slips of wedged glasses, which are generally used with achromatic telescopes: none of them could withstand the accumulated heat in the focus of pencils, where these glasses

glaſſes are generally placed. Being thus left without reſource, I made uſe of red glaſſes; but was by no means ſatisfied with their performance. My not being better prepared, as it happened, was of no conſequence; the weather proving totally unfavourable for viewing the ſun at the time of the tranſit. However, as I was fully aware of the neceſſity of providing an apparatus for this purpoſe, ſince no method that was in uſe could be applied to my teleſcopes, I took the firſt opportunity of beginning my trials.

The inſtrument I wiſhed to adapt for ſolar inſpection, was a Newtonian reflector, with 9 inches aperture; and my aim was to uſe the whole of it open.

I began with a red glaſs; and, not finding it to ſtop light enough, took two of them together. Theſe intercepted full as much light as was neceſſary; but I ſoon found that the eye could not bear the irritation from a ſenſation of heat, which it appeared theſe glaſſes did not ſtop.

I now took two green glaſſes: but found that they did not intercept light enough. I therefore ſmoked one of them; and it appeared that, notwithſtanding they now ſtill tranſmitted conſiderably more light than the red glaſſes, they remedied the former inconvenience of an irritation ariſing from heat. Repeating theſe trials ſeveral times, I conſtantly found the ſame reſult; and, the ſun in the firſt caſe being of a deep red colour, I ſurmiſed that the red-making rays, tranſmitted through red glaſſes, were more efficacious in raiſing a ſenſation of heat than thoſe which paſſed through green, and which cauſed the ſun to look greeniſh. In conſequence of this ſurmiſe, I undertook the inveſtigations which have been delivered under the two firſt heads.

As ſoon as I was convinced that the red light of the ſun ought to be intercepted on account of the heat it occaſions, and that it might alſo be ſafely ſet aſide, ſince it was now proved that pale green light excels in illumination, the method which ought to be purſued in the conſtruction of a darkening apparatus was ſufficiently pointed out; and nothing remained but to find ſuch materials as would give us the colour of the ſun, viewed in a teleſcope, of a pale green light, ſufficiently tempered for the eye to bear its luſtre.

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To determine what glaffes would moft effectually ftop the red rays, I procured fome of all colours, and tried them in the following manner:—I placed a prifm in the upper part of a window, and received its coloured fpectrum upon a fheet of white paper. Then I intercepted the colours, juft before they came to the paper, fucceffively, by the glaffes, and found the refult as follows: a deep red glafs intercepted all the rays; a paler red did the fame.

From this we ought not to conclude, that red glaffes will ftop the red rays; but rather, that none of the fun's light, after its difperfion by the prifm, remains intenfe enough to pafs through red glaffes in fufficient quantity to be perceptible when it comes to the paper. By looking through them directly at the fun, or even at day objects, it is fufficiently evident that they tranfmit chiefly red rays.

An orange glafs tranfmitted nearly all the red, the orange, and the yellow. It intercepted fome of the green, much of the blue, and very little of the indigo and violet.—A yellow glafs intercepted hardly any light of any one of the colours.—A dark green glafs intercepted nearly all the red, and partly alfo the orange and yellow. It tranfmitted the green, intercepted much of the blue, but none of the indigo and violet.—A darker green glafs intercepted nearly all the red, much of the orange, and a little of the yellow. It tranfmitted the green, ftopped fome of the blue, but tranfmitted the indigo and violet.—A blue glafs intercepted much of the red and orange, fome of the yellow, hardly any of the green, none of the blue, indigo, or violet.—A purple glafs tranfmitted fome of the red; a very little of the orange and yellow. It alfo tranfmitted a little of the green and blue, but more of the indigo and violet.

From thefe experiments we fee that dark green glaffes are moft efficacious for intercepting red light, and will therefore anfwer one of the intended purpofes; but fince in viewing the fun we have alfo its fplendour to contend with, I proceeded to the following additional trials:

White glafs, lightly fmoked, apparently intercepted an equal fhare of all the colours; and when the fmoke was laid on thicker, it permitted none of them to pafs. Hard pitch, melted

melted between two white glaſſes, intercepted much light; and when put on ſufficiently thick, tranſmitted none. Many differently coloured fluids that were alſo tried, I found were not ſufficiently pure to be uſed, when denſe enough to ſtop light. Now red glaſſes, and the two laſt-mentioned reſources of ſmoke and pitch, any one of which, it has been ſeen, will ſtop as much light as may be required, had ſtill a remaining trial to undergo, relating to diſtinctneſs; but this I was convinced could only be decided by actual obſervations of the ſun. As an eaſy way of ſmoking glaſſes uniformly is of ſome conſequence to diſtinct viſion, it may be of ſervice here to give the proper directions how to proceed in the operation:—With a pair of warm pliers, take hold of the glaſs, and place it over a candle at a ſufficient diſtance not to contract ſmoke. When it is heated, but no more than ſtill to permit a finger to touch the edges of it, bring down the glaſs at the ſide of the flame, as low as the wick will permit, which muſt not be touched; then, with a quick vibratory motion, agitate it in the flame from ſide to ſide; at the ſame time advancing and retiring it gently all the while. By this method you may proceed to lay on ſmoke to any required darkneſs. It ought to be viewed from time to time, not only to ſee whether it be ſufficiently dark, but whether any inequality may be perceived; for if that ſhould happen, it will not be proper to go on.

The ſmoke of ſealing-wax is bad; that of pitch is worſe. A wax candle gives a good ſmoke; but that of a tallow candle is better. As good as any I have hitherto met with is the ſmoke of ſpermaceti oil. In uſing a lamp, you may alſo have the advantage of an even flame extended to any length.

Teleſcopic Experiments.

No. 1. By way of putting my theory to the trial, I uſed two red glaſſes, and found that the heat which paſſed through them could not be ſuffered a moment; but I was now alſo convinced that diſtinctneſs of viſion is capitally injured by the colouring matter of theſe glaſſes.

No. 2. I ſmoked a white glaſs till it ſtopped light enough to permit the eye to bear the ſun. This deſtroyed all diſtinctneſs;

nefs; and alfo permitted fome heat to come to the eye by tranfmitting chiefly red rays.

No. 3. I applied two white glaffes, with pitch between them, to the telefcope; and found that it made the fun appear of a fcarlet colour. They tranfmitted fome heat; and diftinctnefs was greatly injured.

No. 4. I ufed a very dark green glafs to ftop heat; and behind it, or towards the eye, I placed a red glafs to ftop light. The firft glimpfe I had of the fun was accompanied with a fenfation of heat; diftinctnefs alfo was materially injured.

No. 5. I ufed a dark green and a pale red; but the fun not being fufficiently darkened, I fmoked the red glafs, and putting a fmall partition between the two, placed the fmoke towards the green glafs. This took off the exuberance of light; but did not remedy the inconvenience arifing from heat.

No. 6. I ufed two pale green glaffes, fmoking that next to the eye, and placing it as in No. 5, fo that the fmoke might be inclofed between the two. This acted incomparably well; but in a very fhort time, the heat which paffed the firft glafs (though not the fecond, for I felt no fenfation of it in the eye,) difordered the fmoke, by drawing it up into little blifters or ftars, which let through light; and this compofition therefore foon became ufelefs.

No. 7. I ufed two dark green glaffes, one of them fmoked, as in No. 5. Thefe alfo acted well; but became ufelefs for the reafon affigned in No. 6, though fomewhat lefs fmoke had been required than in the former compofition. I felt no heat.

No. 8. I ufed one pale green, with a dark green fmoked glafs upon it, as in No. 5. It bore an aperture of four inches very well, and the fmoke was not difordered; but when all the tube was open, the pale green glafs cracked in a few minutes.

No. 9. Placing now a dark green before a fmoked green, I faw the fun remarkably well. In this experiment I had made a difference in the arrangement of the apparatus. The cracking of the glaffes, I fuppofed, might be owing to their receiving

receiving heat in the middle, while the outside remained cold; which would occasion a partial dilatation. I therefore cut them into pieces about a quarter of an inch square, and set three of them in a slider, so that I could move them behind the smoked glass without disturbing it. After looking about three or four minutes through one of them, I moved the slider to the second, and then to the third. This kept the glasses sufficiently cool: but the disturbance of the alterations proved hurtful to vision, which requires repose; and, if perchance I stopped a little longer than the proper time, the glass cracked with a very disagreeable explosion that endangered the eye.

No. 10. Two dark green glasses, both smoked, that a thinner coat might be on each; but the smoke still contracted blisters, though less dense than before.

No. 11. To get rid of smoke entirely, I used two dark green glasses, two very dark green, two pale blue, and one pale green glass, together. Distinctness was wanting, nor was light sufficiently intercepted.

No. 12. A dark green and a pale blue glass smoked. The green glass cracked.

No. 13. A pale blue and a dark green glass smoked. The blue glass cracked. The eye felt no sensation of heat.

No. 14. Two pale blue glasses, one smoked. The first glass cracked.

It was now sufficiently evident that no glass which stops heat, and therefore receives it, could be preserved from cracking, when exposed to the focus of pencils. This induced me to try an application of the darkening apparatus to another part of the telescope.

The place where the rays are least condensed, without interfering with the reflections of the mirrors, is immediately close to the small one. I therefore screwed an apparatus to the speculum arm, into which any glass might be placed.

No. 15. A dark green glass close to the small speculum, and smoked pale green in the focus of pencils as before. I saw remarkably well.

No. 16. The dark green as before; but, that more light might be admitted, a white smoked glass near the eye. Better than No. 15; but the green glass cracked.

No. 17. A very dark green and white smoked glass, as before. Very distinct, but the green glass cracked in about six or seven minutes.

No. 18. A dark blue glass, as in No. 15, and white smoked. This was distinct, and no heat came to the eye. The sun appeared ruddy.

No. 19. A dark blue and a yellow glass, close together, as in No. 15, and a white smoked one as before. This was not distinct.

No. 20. A purple glass, as in No. 15, with a white smoked one. This gave the sun of a deep orange colour, approaching to scarlet. It was not distinct.

No. 21. An orange glass, as in No. 15, with a white smoked one. The colour of the sun was too red.

No. 22. A white smoked glass, as in No. 15, without any other at the eye. This gave the sun of a beautiful orange colour; but distinctness was totally destroyed.

No. 23. The heat near the small speculum being still too powerful for the glasses, I had a bluish dark green glass made of a proper diameter to be inclosed between the two eye-glasses of a double eye-piece. All glass I knew would stop some heat; and was therefore in hopes that the interposition of this eye-glass would temper the rays, so as in some measure to protect the coloured glass. In the usual place near the eye, I put two white glasses, with a thin coat of pitch between them. These glasses, when looked through by the natural eye, give the sun of a red colour; I therefore entertained no great hopes of their application to the telescope. They darkened the sun not sufficiently; and when the pitch was thickened distinctness was wanting.

No. 24. The same glass between the eye-glasses, and a dark green smoked glass at the eye; very distinct. This arrangement is preferable to that of No. 15; after some considerable time, however, this glass also cracked.

No. 25. I placed a very dark green glass behind the second eye-glass, that it might be sheltered by both glasses, which in

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my double eye-piece are clofe together, and of an equal focal length. Here, as the rays are not much concentrated, the coloured glafs receives them on a large furface, and ftops light and heat in the proportion of the fquares of its diameter now ufed, to that on which the rays would have fallen had it been placed in the focus of pencils; and for the fame reafon I now alfo placed a dark green fmoked glafs clofe upon the former, with the fmoked fide towards the eye, that the fmoke might likewife be protected againft heat by a paffage of the rays through two furfaces of coloured glafs.

This pofition had moreover the advantage of leaving the telefcope, with its mirrors and glaffes, completely to perform its operation before the application of the darkening apparatus; and thus to prevent the injury which muft be occafioned by the interpofition of the heterogeneous colouring matter of the glafles and of the fmoke.

No. 26. I placed a deep blue glafs with a blueifh green fmoked one upon it, as in No. 25, and found the fun of a whiter colour than with the former compofition. There was no difagreeable fenfation of heat, though a little warmth might be felt.

No. 27. I ufed two black glaffes, placed as in No. 25. Here there was no occafion for fmoke; but the fun appeared of a bright fcarlet colour, and an intolerable fenfation of heat took place immediately. I rather fufpect that thefe are very deep red glaffes, though their outward appearance is black.

In order to have a more fure criterion of heat, I applied Dr. Wilfon's thermometer, No. 2, to the end of the eye-piece, where the eye is generally placed. With No. 25, it rofe from 34 to 37 degrees; with No. 26, it rofe from 35 to 46; and with No. 27, it rofe very quickly from 36 to 95 degrees. I am pretty fure it would have mounted up ftill higher; but, the fcale extending only to 100, I was not willing to run the rifk of breaking the thermometer by a longer expofure.

It remains now only to be added, that with No. 25 and 26 I have feen uncommonly well; and that, in a long feries of very interefting obfervations upon the fun, which will

will soon be communicated, the glasses have met with no accident. However, when the sun is at a considerable altitude, it will be advisable to lessen the aperture a little, in telescopes that have so much light as my 10-feet reflector; or, which will give us more distinctness, to view the sun earlier in the morning and later in the afternoon; for the light, intercepted by the atmosphere in lower altitudes, will reduce its brilliancy much more uniformly than we can soften it by laying on more smoke upon our darkening glasses. Now, as few instruments in common use are so large as that to which this method of darkening has been adapted, we may hope that it will be of general utility in solar observations.

Slough, near Windsor,
March 3, 1800.

III. *Account of a Series of Experiments, undertaken with the View of decomposing the Muriatic Acid. By Mr.* WILLIAM HENRY.

[Concluded from Page 218.]

A GREAT variety of similar experiments convinced me, that, by electrifying together the carbonated hydrogenous and muriatic gases, not the smallest progress was made towards the decomposition of the latter. All that was thus effected consisted in the decomposition of the water of the two gases by the carbon of the combustible gas; and, when this was completely accomplished, no further effect ensued from continuing the electrisation. The generation of carbonic acid was proved by the following experiment:

Exper. 14. To a mixture of carbonated hydrogen and muriatic gases, after having received above 100 shocks, a drop of water was admitted, which absorbed the muriatic acid. The liquid was then taken up by blotting-paper; and the residuary gas, being transferred into another tube, was brought into contact with a solution of pure barytic earth. The precipitation of this solution evinced the presence of carbonic acid.

It was desirable, however, that the effects should be ascertained;

tained, of electrifying together pure muriatic acid and pure carbonated hydrogenous gas, both perfectly free from water. Now, from the experiments related in the first section, it appears highly probable that a complete purification from moisture is produced, in both gases, by the action of the electric fluid; all the water they before contained being thus decomposed. In the following experiments, therefore, the two gases were separately electrified before they were submitted to this process conjointly.

Exper. 15. To a portion of muriatic acid, diminished by the action of electricity from 144 to 121 measures, 27 measures of carbonated hydrogenous gas, expanded as far as possible, were added, and 200 shocks passed through the mixture. The addition of permanent gas amounted to 14 measures; 10 of which may be traced to the muriatic acid, and were evolved by its separate electrisation. The remaining four measures, which remain to be accounted for, are too small a quantity to be ascribed to the decomposition of the acid.

Exper. 16. To a quantity of carbonated hydrogenous gas, which had received 400 shocks, and occupied the space of 212 measures, I added 232 of muriatic acid, through which 200 shocks had previously been passed. The electrisation of the mixture was next continued till 800 discharges had taken place. On examining the mixture of gases during this operation, no change whatever took place; and, after its close, no more muriatic acid had disappeared than would have been deficient after the first electrisation; nor was there any further production of permanent gas.

Exper. 17. The same result was obtained by electrifying together 280 measures of carbonated hydrogenous gas, previously expanded by 600 shocks, and 114 of muriatic acid, after 400 shocks. The additional discharge, through this mixture, of 1000 shocks, did not evince the smallest progress towards the decomposition of the muriatic acid.

Exper. 18. In the naturally moist state of these gases, it follows, from the 14th experiment, that carbonic acid is produced by electrifying them in conjunction. It appeared to me of some importance to ascertain whether, after a pre-

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vious decompofition of their moifture, carbonic acid would continue to be generated. But the electrified carbonated hydrogenous gas itfelf contains carbonic acid, which, unlefs removed, would render the refult of the experiment undecifive. This was accomplifhed by paffing up, to a portion of electrified gas, a bubble or two of dry ammoniacal gas; which, uniting with the carbonic acid, would condenfe any portion of it that might be prefent. The remainder was transferred into another tube; and to this carbonated hydrogenous gas, perfectly deprived both of moifture and carbonic acid, muriatic acid gas, previoufly electrified, was added, and electrical fhocks were paffed through the mixture. A drop of water was then admitted; and the refiduary gas, after having been dried, was transferred into another tube. On paffing up barytic water, not the fmalleft trace of carbonic acid could be difcovered.

From the preceding experiments, the following conclufions may be deduced:

1. The muriatic acid gas, in the drieft ftate in which it can be procured, ftill contains a portion of water. From a calculation founded on the experiments defcribed in the firft fection, the grounds of which are too obvious to require being ftated, it follows that 100 cubical inches of muriatic gas, after expofure to muriat of lime, ftill hold in combination 1.4 grain of water.

2. When electrical fhocks are paffed through this gas, the watery portion is decompofed. The hydrogen of the water, uniting with the electric matter, conftitutes hydrogenous gas, and the oxygen unites with the muriatic acid; which laft, acting on the mercury, compofes muriat of mercury.

3. The electric fluid ferves as an intermedium in combining oxygen with muriatic acid.

4. The really acid portion of muriatic gas does not fuftain any decompofition by the action of electricity.

5. When electric fhocks are paffed through a mixture of carbonated hydrogen and muriatic acid gafes, the water held in folution by thefe gafes is decompofed by the carbon of the compound inflammable gas; and carbonic acid and hydrogenous gafes are the refult.

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6. When all the water of the two gafes has been decompofed, no effect enfues from continuing the electrifation; or, if the water of each gas has been previoufly deftroyed by electrifying them feparately, no further effect enfues from electrifying them conjointly.

7. Since, therefore, carbon, though placed under the moft favourable circumftances for abftracting from the muriatic acid, and combining with its oxygen, evinces no fuch tendency, it may be inferred, that, if the muriatic acid be an oxygenated fubftance, its radical has a ftronger affinity for oxygen than charcoal poffeffes.

Though the firft impreffions excited in my mind by the total failure of the above experiments, in accomplifhing one of the greateft objects of modern chemiftry, have induced me for fome time to withhold them from the fociety, I am fatisfied, by reflection, that this communication is not without expediency. The means employed in attempting the analyfis of the muriatic acid, were fuch as, after mature deliberation, appeared to me moft to promife fuccefs; and the experiments were attended with a degree of labour, which can only be eftimated by thofe who have been engaged in fimilar purfuits; not one third of thofe which were really made having been defcribed in the foregoing account of them. It may fpare, therefore, to others, a fruitlefs application of time and trouble, to be made acquainted with what I have done; and the collateral facts, which have prefented themfelves in the inquiry, are perhaps not without curiofity or value.

From the refult of thefe experiments, I apprehend, all hope muft be relinquifhed of effecting the decompofition of the muriatic acid in the way of fingle elective affinity. They furnifh alfo a ftrong probability that the bafis of the muriatic acid is fome unknown body; for no combuftible fubftance with which we are acquainted can retain oxygen, when fubmitted in contact with charcoal, to the action of electricity, or of a high temperature. The analyfis of this acid muft, in future, be attempted with the aid of complicated affinities. Thus, in the mafterly experiment of Mr. Tennant, phofphorus, which attracts oxygen lefs ftrongly than charcoal, by the intermediation of lime, decompofes the carbonic acid.

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Yet, led by the analogy of this fact, its discoverer found that a similar artifice did not succeed in decomposing the muriatic acid. "As vital air," he observes, "is attracted by a compound of phosphorus and calcareous earth more powerfully than by charcoal, I was desirous of trying their efficacy upon those acids which may from analogy be supposed to contain vital air, but which are not affected by the application of charcoal. With this intention, I made phosphorus pass through a compound of marine acid and calcareous earth, and also of fluor acid and calcareous earth, but without producing in either of them any alteration. Since the strong attraction which these acids have for calcareous earth tends to prevent their decomposition, it might be thought, that in this manner they were not more disposed to part with vital air than by the attraction of charcoal: but this, however, does not appear to be the fact. I have found that phosphorus cannot be obtained by passing marine acid through a compound of bones and charcoal when red-hot. The attraction, therefore, of phosphorus and lime for vital air exceeds the attraction of charcoal by a greater force than that arising from the attraction of marine acid for lime *."

By means similar to those employed in attempting the analysis of the muriatic acid, I tried to effect that of the fluoric acid. When electrified alone, in a glass tube coated internally with wax, it sustained a diminution of bulk, and there remained a portion of hydrogenous gas. But neither in this mode, nor by submitting it, mixed with carbonated hydrogenous gas, to the action of electricity, was any progress made towards its analysis. These experiments, however, render it probable, that the fluoric acid, like the muriatic, is susceptible of still further oxygenation, in which state it becomes capable of acting on mercury. The carbonic acid, on the contrary, appears not to admit of two different degrees of oxygenation. When the electric shock has been repeatedly passed through a portion of this acid gas, its bulk is enlarged, and a permanent gas is produced, which is evidently a mixture of oxygenous and hydrogenous gases; for, when

* Philosophical Transactions, Vol. LXXI. p. 184.

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an electric ſpark is paſſed through the gas that remains after the abſorption of the carbonic acid by cauſtic alkali, it immediately explodes: Theſe reſults even take place on electrifying carbonic acid from marble, previouſly calcined in a low red heat, to expel its water, and then diſtilled in an earthen retort*.

IV. *Experiments in Galvanic Electricity, by Meſſrs.* NICHOLSON, CARLISLE, CRUICKSHANK, &c.

IN the fourth volume of the Philoſophical Magazine, p. 59, 163, and 306, we laid before our reader's M. Volta's account of the progreſs he had then made in Galvaniſm; and in our number for May laſt we ſtated that Mr. Carliſle had been making ſome experiments which were likely to lead to ſome important diſcovery in Galvanic electricity. We ſhould have then mentioned, but we knew not the fact, that he made them in conjunction with Mr. Nicholſon, editor of a well conducted and well known philoſophical journal. When we learnt the circumſtance, motives of delicacy, which our readers muſt approve, forbade our attempting to give any further particulars till Mr. Nicholſon himſelf, who had the beſt right, ſhould firſt lay them before the public.

Since that time Mr. Nicholſon has publiſhed ſeveral curious and important papers on this ſubject, from which we ſhall now ſelect a few particulars for the information of our readers; referring thoſe who may wiſh for a fuller account, to Mr. Nicholſon's journal.

The general apparatus we need not particularly to deſcribe, having in the preſent number given a tranſlation of M. Volta's paper which appeared in the ſecond part of the Philoſophical Tranſactions for the preſent year, juſt publiſhed.

* Meſſieurs Landriani and Van Marum (*An. de Chemie*, Tome II. p. 270.) obtained only hydrogenous gas by electrifying the carbonic acid gas. But the conductor of their apparatus was an iron one; which metal would combine with the oxygen of the water, and prevent it from appearing in a gaſeous ſtate. In my experiments the conductors were of platina.

Mr. Anthony Carlifle, having been favoured with a perufal of this paper by the very refpectable Prefident of the Royal Society, the Right Hon. Sir Jofeph Banks, Bart. foon after, in conjunction with Mr. Nicholfon, began to repeat the experiments of M. Volta, and obtained fimilar refults. Very early in this courfe, the contacts being made fure by placing a drop of water upon the upper plate of the pile, Mr. Carlifle obferved a difengagement of gas round the conducting wire. This gas, though minute in quantity, feemed to Mr. Nicholfon to have the fmell of hydrogen when the wire of communication was fteel. This, with fome other facts, led him to propofe to break the circuit by the fubftitution of a tube of water between two wires. Accordingly, a brafs wire through each of two corks was inferted at the oppofite ends of a glafs tube about half an inch in diameter, filled between the corks with water: the diftance between the points of the wires in the water, was about an inch and three quarters.

"This compound difcharger was applied fo that the external ends of its wire were in contact with the two extreme plates of a pile of thirty-fix half crowns with the correfpondent pieces of zinc and pafteboard. A fine ftream of minute bubbles immediately began to flow from the point of the lower wire in the tube, which communicated with the filver, and the oppofite point of the upper wire became tarnifhed, firft deep orange, and then black. On reverfing the tube, the gas came from the other point, which was now loweft, while the upper, in its turn, became tarnifhed and black. Reverfing the tube again, the phenomena again changed their order. In this ftate the whole was left for two hours and a half. The upper wire gradually emitted whitifh filmy clouds, which, towards the end of the procefs, became of a pea-green colour, and hung in perpendicular threads from the extreme half inch of the wire, the water being rendered femi-opaque by what fell off, and in a great part lay, of a pale green, on the lower furface of the tube, which, in this difpofition of the apparatus, was inclined about forty degrees to the horizon. The lower wire, three quarters of an inch long, conftantly emitted gas, except when another circuit, or complete wire, was applied to the apparatus; during which time

time the emiffion of gas was fufpended. When this laft mentioned wire was removed, the gas reappeared as before, not inftantly, but after the lapfe of four beats of a half fecond clock ftanding in the room. The product of gas, during the whole two hours and a half, was two-thirtieths of a cubic inch. It was then mixed with an equal quantity of common air, and exploded by the application of a lighted waxed thread."

To have reverfed the tube would have anfwered the fame purpofe, but they chofe to do this, "and found that, when the zinc was at the bottom, its effects were reverfed; that is to fay, the gas ftill came from the wire communicating with the filver," &c.

Meffrs. Carlifle and Nicholfon were "led, by reafoning on the firft appearance of hydrogen, to expect a decompofition of water; but it was with no little furprife that they found the hydrogen extricated at the contact with one wire, while the oxygen fixed itfelf in combination with the other wire at the diftance of almoft two inches. As the diftance between the wires formed a ftriking feature in this refult, it became defirable to afcertain whether it would take place to greater diftances. When a tube three quarters of an inch in diameter, and thirty-fix inches long, was made ufe of, the effect failed, though the very fame wires, inferted into a fhorter tube, operated very brifkly."

The experiment being tried with tincture of litmus in place of water, and the oxydating wire, namely, from the zinc fide, being loweft in the tube, it changed the tincture red in about ten minutes as high as the upper extremity of the wire. The other portion remained blue. Hence it feems either an acid was formed, or that a portion of the oxygen combined with the litmus, fo as to produce the effect of an acid.

"It may be here offered as a general remark, that the electric pile with card, or with woollen cloth, continues in order for about two days, or fcarcely three; that, from a feries of glaffes fet up by Mr. Carlifle, as well as from the pile itfelf, it appears that the fame procefs of decompofition of water is carried on between each pair of plates, the zinc being oxyded on the wet face, and hydrogen given out; that

 the

the common falt is decompofed, and exhibits an efflorefcence of foda round the edges of the pile, extruded, moft probably, by the hydrogen: and that, on account of the corrofion of the faces of the zinc, it is neceffary to renew them previoufly to each conftruction of the pile. This may be done by fcraping or grinding."

By feveral accurate and well conducted experiments, Mr. Nicholfon afcertained that the electricity of the zinc was plus, and that of the filver minus, which ever of them were at the top of the pile. The electric fpark was even rendered vifible; fo that there can be no doubt of the identity of the electric and Galvanic fluid.

The decompofition of water, and oxydation of metallic wire, fuggefted other experiments. Two fmall wires of platina were inferted, as before, in a fhort tube. When the connection with the pile was formed, the wire from the filver gave a plentiful ftream of gas, and that from the zinc a fmaller one. In four hours, neither turbidnefs, oxydation, nor tarnifh appeared. The larger ftream was naturally fuppofed to be hydrogen, the fmaller oxygen.

With thick gold leaf inftead of platina, the refult was the fame, only the extremity of the flip connected with the zinc acquired a coppery or purplifh tinge.

A brafs wire was fubftituted for one of the flips of gold. When the former was joined to the filver end, the two ftreams were extricated as before; but when joined to the zinc, it became oxyded, as when both the wires were of brafs.

The fimple decompofition of water by platina wires, without oxydation, offered a means of obtaining the gafes feparate from each other. This was tried with a pile of fixty-eight fets. A wire from each end of the pile paffed under feparate phials full of water inverted in a faucer of water. "A cloud of gas arofe from each wire, but moft from the filver or minus fide. Bubbles were extricated from all parts of the water, and adhered to the whole internal furface of the veffels. The procefs was continued for thirteen hours, after which the wires were difengaged, and the gafes decanted into feparate bottles. On meafuring the quantities, which

was

was done by weighing the bottles, it was found that the quantities of water difplaced by the gafes were, refpectively, 72 grains by the gas from the zinc fide, and 142 grains by the gas from the filver fide; fo that the whole volume of gas was 1.17 cubic inches, or near an inch and a quarter. Thefe are nearly the proportions in bulk of what are ftated to be the component parts of water. The gas from the zinc fide being tried with one meafure of nitrous gas, contracted to 1.25, and did not contract more by the addition of another meafure; the gas from the filver fide, by the fame treatment, contracted to 1.6. The air of the room, on trial, contracted to 1.28. From the fmallnefs of the quantity, no attempt was made to detonate the air from the zinc fide; but a portion of that from the filver fide, being mixed with one-third of atmofpheric air, gave a loud detonation.

" Upon the above it may be remarked, that it does not feem probable that oxygen was afforded by both wires, but that they were mixed by the circumftances of the experiment."

Mr. Cruickfhank, of Woolwich, alfo made fome interefting experiments on this fubject. He employed plates of zinc and filver about 1.6 inches fquare; and the number of each varied from 40 to 100, according to the power required. He ufed filver wire both from the zinc and filver plate in his firft experiments; but to diftinguifh the ends of the pile, that wire only is called the filver wire, which was connected with the filver plate; the other wire he calls (to fave circumlocution) the zinc wire. Thefe wires were paffed through corks fitted into a glafs tube filled with water, and one of the corks made perfectly tight by means of cement. " The tube was then placed upright in a cup containing water, with the uncemented end downwards. As foon as the communication was made between the extremities of the pile by the wires, a quantity of fmall air bubbles began to afcend from the end of the wire connected with the filver, as obferved by Meffrs. Nicholfon and Carlifle; but a white cloud at the fame time made its appearance at the one proceeding from the zinc, or the zinc wire. This cloud gradually increafed, and affumed a darker colour, and at laft it became purple, or even black. A very few air bubbles were likewife collected upon and

afcended

ascended from this wire; but when the machine was in full force, a considerable stream could be observed.

"The gas was collected, and found to be a mixture of hydrogen and oxygen, in the proportion of three parts of the former to one of the latter. No great dependence, however, was placed upon this in point of accuracy. The zinc wire was found to be much corroded, and looked as if a considerable portion of it had been dissolved. As the cloud which was formed around this wire became purple on exposure to the light, Mr. Cruickshank suspected it might be luna cornea, or muriat of silver proceeding from the silver, which had been somehow dissolved, and afterwards precipitated in this state, by the muriatic salts in the common water."

Distilled water, to which a little tincture of litmus was added, was next employed in the tube. Gas arose from both wires, but in greatest quantity from the silver wire. In a short time, the whole fluid below the point of the zinc wire became red, and the fluid above the silver wire looked of a deeper blue than before, the slight tinge of purple being destroyed.

Distilled water, tinged with Brazil wood, soon became of as deep a purple as could be produced by ammonia, while the portion of the fluid round the zinc wire became very pale. From these experiments it appears to Mr. Cruickshank, that an acid, probably the nitrous, is produced at the wire connected with the zinc, and an alkali, probably ammonia, at that connected with the silver, end of the pile.

When lime-water was employed, the wire was likewise acted upon, but in a less degree. The cloud at first had an olive colour, exactly resembling the precipitate of silver by lime water.

In these experiments the quantity of silver dissolved was considerable, and, where water was employed, a portion of it remained in solution, which was proved by adding muriatic acid. More would probably have been suspended, but that an evident precipitation near the upper extremity of the zinc wire, was occasioned by the alkali generated by the process.

As hydrogen gas, when heated, or in its nascent state,

reduces

reduces metallic oxyds, Mr. Cruickſhank filled the glaſs tube with a ſolution of acetite of lead to ſeparate the hydrogen from the oxygen, and thus obtain the latter pure. An exceſs of acid was added to the acetite to take up the alkali: in a minute or two after the communication was made, ſome fine metallic needles, which afterwards aſſumed the form of a feather, or rather that of the cryſtals of ammonia, were perceivable at the end of the ſilver wire. The lead was in its metallic ſtate.

Solutions of ſulphat of copper and nitrat of ſilver, were tried in the ſame way, and with ſimilar reſults. The metals were revived.

When pure water, mixed with diſtilled vinegar or with a very little ſulphuric acid, were employed in the tube, metallic ſilver was precipitated by the ſilver wire, the acid employed preventing the alkali from precipitating the ſilver diſſolved by the generated acid; in conſequence of which, when a ſufficient quantity of the metal was taken up, it was again thrown down by the ſilver wire in its metallic form.

Muriat of ammonia in ſolution being tried, a little gas was diſengaged from the ſilver wire: an incruſtation of luna cornea was formed round the zinc wire. The liquor remaining after the experiment, ſmelled ſtrongly of ammonia. Common ſalt was decompoſed alſo. Indeed, when a ſolution of muriat of ſoda or of ammonia is employed to moiſten the papers in the pile, the ſalt is always decompoſed.

Nitrat of magneſia was decompoſed by the ſame means.

In ſome after experiments gold wires were tried, and the quantity of oxygen gas obtained was much greater than when ſilver wires were employed.

Two gold wires were paſſed through a cork looſely introduced into the mouth of a three-ounce phial filled with lime water: the phial being inverted over pure water, the exterior ends of the wires were connected with the pile in the uſual way. In four hours the phial was filled with gas extricated from the wires, eſpecially the one connected with the ſilver. One meaſure of the gas mixed with two of nitrous gas, a diminution of one meaſure took place: the reſiduum contained nitrous gas mixed with hydrogen. Four meaſures exploded

exploded by the electric ſpark over mercury, diſappeared, except about $\frac{1}{15}$ of a meaſure, which appeared to be azot.

Two gold wires were paſſed through corks, ſecured by cement in the ends of a glaſs tube, about 10 inches long, bent into the form of the letter V; they reached to within an inch of each other, at the angle, in which there was a hole about one-tenth of an inch in diameter. The tube was then filled with diſtilled water, and the opening at the angle being ſhut with the finger to keep in the water, it was thus placed in a cup of water with the angle downwards. The extremities of the wires being then joined to thoſe of the pile, gas was diſengaged from both, but moſt from that connected with the ſilver; the gaſes were thus kept diſtinct. One meaſure of the gas from the ſilver end, mixed with one of nitrous gas, gave red fumes, a diminution of one-third of a meaſure, and a reſiduum conſiſting of nitrous and hydrogen gas. Two meaſures with one of oxygen being exploded over mercury diſappeared, except about one-fifth of a meaſure, which by the nitrous teſt appeared to be chiefly oxygen. A denſe white vapour was perceived over the mercury for ſome time after the exploſion. One meaſure of the gas from the zinc end, being mixed with two of nitrous gas, the whole nearly diſappeared: another meaſure of the latter being added, the total diminution was nearly three meaſures.

With platina wires Mr. Cruickſhank obtained almoſt ſimilar reſults. The one connected with the zinc end became tarniſhed; the ſame thing happened when gold wires were uſed.

A ſolution of cryſtallized muriat of lime, incloſed in a tube in the common manner, and gold wires being employed, the one from the ſilver gave little gas, but that from the zinc a conſiderable quantity, and the fluid ſurrounding it aſſumed a fine yellow colour, a ſolution of the gold having been effected. After a time ſome gas came from the firſt wire, but there was no precipitation of lime. When the tube was opened, the fluid ſmelled of aqua regia, or the oxy-muriatic acid. When platina wires were employed in place of gold, the ſmell of nitro-muriatic acid was ſoon obſervable, but no ſolution of the platina. When the tube was filled with a ſolution of muriat of ſoda, a nitro-muriatic acid was likewiſe produced.

Mr. Cruickſhank from theſe experiments draws the following concluſions:

"1. That hydrogen gas, mixed with a very ſmall proportion of oxygen and ammonia, is ſomehow diſengaged at the wire connected with the ſilver extremity of the machine; and that this effect is equally produced, whatever the nature of the metallic wire may be, provided the fluid operated upon be pure water.

"2. That where metallic ſolutions are employed inſtead of water, the ſame wire which ſeparates the hydrogen revives the metallic calx, and depoſits it at the extremity of the wire in its pure metallic ſtate; in this caſe no hydrogen gas is diſengaged. The wire employed for this purpoſe may be of any metal.

"3. That of the earthy ſolutions, thoſe of magneſia and argill only are decompoſed by the ſilver wire; a circumſtance which ſtrongly favours the production of ammonia.

"4. That when the wire connected with the zinc extremity of the pile conſiſts either of gold or platina, a quantity of oxygen gas, mixed with a little azot and nitrous acid, is diſengaged; and the quantity of gas thus obtained is a little better than one-third of the hydrogen gas ſeparated by the ſilver wire at the ſame time.

"5. That when the wire connected with the zinc is ſilver, or any of the imperfect metals, a ſmall portion of the oxygenous gas is likewiſe given out, but the wire itſelf is either oxydated or diſſolved, or partly oxydated and partly diſſolved: indeed, the effect in this caſe, produced upon the metal, is very ſimilar to that of the concentrated nitrous acid, where a great deal of the metal is oxydated, and but a ſmall quantity held in ſolution*.

* The great difference in the effect produced by this influence on gold and ſilver, which have always been conſidered as equally difficult to oxydate, can only be explained on the ſuppoſition, that nitrous acid is generated; for this acid, it is well known, acts powerfully on ſilver, but has no action whatever on gold. The ſame obſervation applies to platina.

 "6. That

"6. That when the gases, obtained by gold or platina wires, are collected together and exploded over mercury, the whole nearly disappears and forms water, with probably a little nitrous acid, for there was always a thick white vapour perceived for some time after the explosion. The residuary gas in this case appeared to be azot."

Acid solutions of metals having been decomposed, Mr. Cruickshank tried their solution in alkalies. Pure ammonia was added to a dilute solution of nitrat of silver, till the mixture smelled strongly of the former. Being put into a tube in the usual way, with silver wires, and the communication made, a rapid production of gas took place from the silver end, but hardly any from the zinc. Grayish flashes of metallic silver were separated from the silver wire, and on the zinc wire a dark gray substance was deposited, which, on afterwards endeavouring to scrape it off with the finger, exploded, though still moist. The wire was corroded and full of holes. The fulminating silver of Berthollet had in fact been produced in this experiment.

When pure ammonia was introduced into the tube in place of the solution of the nitrat of silver, the result was the same—the silver wire from the zinc being corroded, &c. was taken up by the alkali, and afterwards deposited in its metallic form by the other wire. To the first adhered some of the fulminating silver, and a portion was also deposited from the fluid, after standing some time.

From pure ammonia, with copper wires, a quantity of very pure metal was precipitated—from an ammoniacal solution of copper the same pure metallic precipitate. From these experiments Mr. Cruickshank remarks, that it appears that the Galvanic influence might be employed with success in the analysis of minerals.

Pure ammonia being introduced into a bottle, and inverted over the same fluid, with a cork and two wires, as before described, (the wire connected with the zinc being platina and the other silver), a rapid decomposition of the alkali was effected. Two ounce measures of gas being collected and examined, was found to consist of 15 parts of hydrogen gas, 13 of azotic, and two nearly of oxygen gas.

The

The nitrous acid ſeemed to be little or not at all acted upon by the Galvanic influence, from which Mr. Cruickſhank infers that it is ſo perfect a conductor of the fluid as to tranſmit it like metals, without experiencing any change; and this he thinks may poſſibly be owing to the great proportion of oxygen, which enters into its compoſition, having before remarked that all fluids, containing little or no oxygen, are non-conductors, or nearly ſo.

The corroded matter generated at the wire connected with the zinc are not, Mr. Cruickſhank obſerves, pure oxydes; and it follows that an acid, as before mentioned, and probably the nitrous, is produced; for all the green oxyds of copper contain an acid of ſome kind or other, the pure oxyds of that metal being either dark red or deep brown.

Mr. Nicholſon's Journal alſo contains ſome intereſting experiments on this ſubject made by Mr. Davy, of Briſtol, which gave reſults that almoſt all admit of the ſame inferences that have been made by Meſſrs. Nicholſon, Carliſle, and Cruickſhank. He made an ingenious diverſity in the circuit. Tubes filled with diſtilled water, and furniſhed with gold wires connected with the pile, were inverted in different glaſſes, and the communication between them formed by muſcular fibre; the gaſes were given off in great quantity, and from ſeveral experiments pure oxygen and pure hydrogen were ſeparately obtained, nearly in the proportions required to form water.

Some ingenious experiments have alſo been made by Mr. Henry, of Mancheſter, and by Lieut. Col. Henry Haldane; for an account of which we muſt refer our readers to the Philoſophical Journal.

V. *Letter from* HENRY MOYES, *M. D. to* MAXWELL GARTHSHORE, *M. D. containing an Account of ſome intereſting Experiments in Galvanic Electricity. Communicated by Dr.* GARTHSHORE.

DEAR SIR, Pittenweem, Fifeſhire, Aug. 15, 1800.

HAVING once more returned to my ſummer reſidence, where ſolitude increaſes the attractions of ſcience, I ſhall

 now

now fulfil my promise of writing to you; and having nothing better at present to communicate, request your acceptance of the following facts, results of some experiments, which, with the aid of Mr. Nicol, I have lately made in my leisure hours.

1st, When 72 square pieces of copper, 72 of zinc, and 71 of moistened pasteboard, each containing four square inches, and not exceeding in thickness 1-16th of an inch, were formed into a column by being applied to each other in the Galvanic series, *viz.* zinc, copper, pasteboard—zinc, copper, pasteboard, &c. &c., the column, though entirely composed of oxydable materials, discovered a strong Galvanic power. When the bottom of the column was touched with one hand, and the top at the same time with the other, both hands being wet with brine, a shock was felt fully as strong as that which was given by an electrical jar having 80 square inches of coated surface. Hence it appears that silver, or a non-oxydable metal, is by no means essential to the excitation of the Galvanic power; and indeed I apprehend that a Galvanic column, consisting of copper, zinc, and pasteboard, will be found as powerful as an equal column consisting of silver, zinc, and pasteboard. The above column, consisting of copper, zinc, and pasteboard, retained its power with little diminution during a period of 36 hours.

2nd, When pieces of copper, zinc, and moistened alumine were formed into a column by being applied to each other in the Galvanic series, *viz.* zinc, copper, alumine, &c. the column gave shocks remarkably stronger than those it would have given had pasteboard been used. Hence a series of curious experiments spontaneously present themselves, demanding to be tried. Hence a probability that the Galvanic power may sometimes occur among the strata of the earth; and hence a new theory of earthquakes will in all probability ere long appear.

3rd, When the alumine in the Galvanic column was moistened with brine, instead of water, the surface of the column became, in the course of three or four days, covered with a copious white efflorescence, which when collected and examined proved to be soda nearly saturated with carbonic acid. A like efflorescence was found in a column, in the

construction

construction of which no alumine was used. The column consisted of copper and zinc, stratified with paper moistened with brine, and furnished, you perceive, a pleasing probability that the Galvanic action of the metallic substances may one day conduce to the arts of utility; it being capable of furnishing, when duly employed, genuine soda from the salt of the sea.

4th, When the Galvanic influence was transmitted, by means of two brass wires, through an aqueous solution of the carbonat of potash, both were corroded where they touched the solution, and both produced or extricated gas. The wire, however, which came from the copper, was manifestly less corroded than the other, though it yielded the largest proportion of gas; and the phænomena were not perceptibly changed by substituting lime in place of the alkali.

Both the wires also gave gas when they were properly immersed in some other fluids; but when inserted in the legs of an inverted glass syphon, which had been previously filled with distilled water, the mode of their action was seemingly changed: one was corroded without giving gas, whilst the other gave gas without being corroded; and these phænomena were not interrupted by filling the bend of the syphon with mercury. One of the wires was constantly corroded without giving gas, and the other gave gas without being corroded, whether the interval between their extremities was filled with pure water alone, or partly with water and partly with mercury. The mercury however must have wholly precluded the transmission of oxygen from wire to wire. It seems impossible for oxygen, in any condition, first to descend and then to ascend through a column of a fluid full ten thousand times heavier than the air at the level of the ocean. Nothing, except the Galvanic influence, appears to have passed from wire to wire either without or within the syphon; and, if this be admitted, it will scarcely be affirmed that the corrosion of the wire was a simple oxydation.

The Galvanic action of various fluids, upon the whole or most of the oxydable metals, has lately opened a field of research, which seems well entitled to persisting attention. I have already projected a set of experiments, which I trust

will

will afford it some degree of light. You shall have the results as soon as they are obtained, provided they shall seem deserving your notice; and I shall now conclude with briefly subjoining, that when a metallic communication was formed in the dark between the ends of a powerful Galvanic column, an appearance of light resembling a small electric spark was distinctly perceived at the moment of contact: that the copper and zinc, when applied to each other, discover a strong Galvanic power; yet plates of brass had no such power, even when piled with moistened alumine: that alumine appeared to be more Galvanic when moistened with brine, than when moistened with water; that pasteboard was decidedly more Galvanic when moistened with weak sulphuric acid, than when moistened either with water or brine; and that by inclosing a powerful Galvanic column in a box, armed in the manner of a magnet, I have constructed a kind of Galvanic machine, which is easily carried from place to place, which is extremely convenient in many experiments, and which has enabled me to explain the powers of the torpedo more clearly, I believe, than has hitherto been done.

I have the honour to be, with great esteem,

Dear Sir,

Your much obliged servant,

To Dr. Garthshore. HENRY MOYES.

VI. *A Project for extending the Breed of Fine-Woolled Spanish Sheep, now in the Possession of His Majesty, into all Parts of Great Britain, where the Growth of fine Clothing Wools is found to be profitable.*

AFTER experiments had been tried for several years, by the King's command, with Spanish sheep of the true Merino breed, imported from various parts of Spain, all of which concurred in proving, that the valuable wool of those animals did not degenerate in any degree in this climate, and that the cross of a Merino ram uniformly increased the quantity and

and meliorated the quality of the wool of every kind of short-woolled sheep on which it was tried, and more particularly so in the case of the South Down, Hereford, and Devonshire breeds; His Majesty was pleased to command that some Merino sheep should be procured from a flock, the character of which for a fine pile of wool was well established.

Application was accordingly made to Lord Auckland, who had lately returned from an embassy to Spain; and in consequence of his Lordship's letters, the Marchioness del Campo di Alange was induced to present to his Majesty five rams and thirty-five ewes from her own flock, known by the name of Negretti, the reputation of which, for purity of blood and fineness of wool, is as high as any in Spain. For this present his Majesty was pleased to send to the Marchioness in return eight fine English coach horses.

These sheep, which were imported in the year 1792, have formed the basis of a flock, now kept in the park of His Royal Highness the Duke of York at Oatlands, the breed of which has been preserved with the utmost care and attention.

The wool of this flock, as well as that of the sheep procured before from Spain, was acknowledged by the manufacturers who saw it, to be to all appearance of the very first quality; yet none of them chose to offer a price for it at all equal to what they themselves gave for good Spanish wool, lest, as they said, it should not prove in manufacture so valuable as its appearance promised: it became necessary, therefore, that it should be manufactured at the King's expense, in order that absolute proof might be given of its actual fitness for the fabric of superfine broad cloth; and this was done year after year in various manners, the cloth always proving excellent: yet the persons to whom the wool was offered for sale still continued to undervalue it, being prepossessed with an opinion, that though it might not at first degenerate, it certainly sooner or later would alter its quality much for the worse.

In 1796 it was resolved to sell the wool at the price that should be offered for it, in order that the manufacturers themselves might make trial of its quality, although a price equal

equal to its real value should not be obtained: accordingly, the clip of that year was sold for 2*s*. a pound, and the clip of the year 1797, for 2*s*. 2*d*.

The value of the wool being now in some degree known, the clip of 1798 was washed in the Spanish manner, and it sold as follows:

The number of fleeces of ewes and wethers was 89;	
Which produced in wool washed on the sheep's backs	295 *lb.*
Loss in scowering - - - - - -	92
Amount of scowered wool - - - - -	203

		lb.		*per lb.*	
Which produced,	Raffinos	167	at	5*s*.	47*l*. 8*s*.
	Finos	23	at	3*s*. 6*d*.	
	Terceros	13	at	2*s*. 6*d*.	

The clip of 1799 was managed in the same manner, and produced as follows:

The number of fleeces of ewes and wethers was 101;	
Which produced in wool washed on the sheep's backs	346 *lb.*
Loss in scowering - - - - - -	92
Amount of scowered wool - - - - -	254

		lb.		*per lb.*	
Which produced,	Raffinos	207	at	5*s*. 6*d*.	63*l*. 14*s*. 6*d*.
	Finos	28	at	3*s*. 6*d*.	
	Terceros	19	at	2*s*.	

The rams' wool of the two years sorted, together produced as follows:

Quantity of wool washed on the sheep's back	314 *lb.*
Loss in scowering - - - - - -	99
Amount of scowered wool - - - - -	215

		lb.		*per lb.*	
Which produced,	Raffinos	181	at	4*s*. 6*d*.	45*l*. 15*s*. 6*d*.
	Finos	22	at	3*s*. 6*d*.	
	Terceros	12	at	2*s*.	

It is necessary to account for these extraordinary prices by stating, that in the year 1799, when both sales were effected, Spanish wool was dearer than it ever before was known to be; but it is also proper to add, that 5*s*. 6*d*. was then the price of the best Spanish piles; and that none were sold higher, except, as it is said, a very small quantity for 5*s*. 9*d*.

The King has been pleased to give away to different persons, who undertook to try experiments by crossing other breeds

breeds of ſheep with the Spaniſh, more than one hundred rams and ſome ewes. In order, however, to make the benefit of this valuable improvement in the ſtaple commodity of Great Britain, acceſſible to all perſons who may chooſe to take the advantage of it, his Majeſty is this year pleaſed to permit ſome rams and ewes to be ſold, and alſo to command that reaſonable prices ſhall be put upon them, according to the comparative value of each individual; in obedience to which it has been ſuggeſted that five guineas may be conſidered as the medium price of a ram, and two guineas that of a ewe; a ſum which it is believed the purchaſer will in all caſes be able to receive back with large profit, by the improvement his flock will derive from the valuable addition it will obtain.

Though the mutton of the Spaniſh ſheep was always excellent, their carcaſes were extremely different in ſhape from that mould which the faſhion of the preſent day teaches us to prefer; great improvement has however been already made in this article, by a careful and attentive ſelection of ſuch rams and ewes as appeared moſt likely to produce a comely progeny; and no doubt can be entertained, that, in due time, with judicious management, carcaſes covered with ſuperfine Spaniſh wool may be brought into any ſhape, whatever it may be, to which the intereſt of the butcher, or the caprice of the breeder, may chuſe to affix a particular value.

Sir Joſeph Banks, who has the honour of being intruſted with the management of this buſineſs, will anſwer all letters on the ſubject of it, addreſſed to him in Soho-ſquare. The rams will be delivered at Windſor; the ewes at Weybridge, in Surrey, near Oatlands.

As thoſe who have the care of his Majeſty's Spaniſh flock may naturally be ſuppoſed partial to the project of introducing ſuperfine wool in theſe kingdoms, it has been thought proper to annex the following notice, in order to ſhow the opinion held of a ſimilar undertaking in a neighbouring country, where individuals, however they have miſtaken their political intereſt, are rather remarkable for purſuing and thoroughly weighing their own perſonal advantage in all their private undertakings, and for ſagacity in ſeizing all opportunities of improving, by public eſtabliſhments, the reſources of their nation.

FRENCH ADVERTISEMENT.

On the 24th of May laſt, an advertiſement appeared in the *Moniteur*, giving notice of a ſale of two hundred and twenty ewes and rams of the fineſt woolled Spaniſh breed, part of the flock kept on the national farm of Rambouillet; alſo two thouſand pounds of ſuperfine wool, being the preſent year's clip of this national flock; and one thouſand three hundred pounds of wool, the produce of the mixed breeds of ſheep kept at the Menagerie at Verſailles.

This advertiſement, which is official, is accompanied by a notice from Lucien Bonaparte, Miniſter of the Interior, as follows:

"The Spaniſh breed of ſheep that produce the fineſt wool, introduced into France thirty years ago, has not manifeſted the ſmalleſt ſymptom of degeneration: ſamples of the wool of this valuable flock, which was brought from Spain in the year 1786, are ſtill preſerved, and bear teſtimony that it has not in the leaſt declined from its original excellence, although the diſtrict where theſe ſheep have been kept is not of the beſt quality for ſheep-farming; the draughts from this flock, that have been annually ſold by auction, have always exceeded in value the expectation of the purchaſers in every country to which they have been carried, that is not too damp for ſheep.

"The weight of their fleeces is from ſix * to twelve pounds each, and thoſe of the rams are ſometimes heavier.

"Sheep of the ordinary coarſe-woolled breeds, when croſſed by a Spaniſh ram, produce fleeces double in weight, and far more valuable, than thoſe of their dams; and if this croſs is carefully continued, by ſupplying rams of the pure Spaniſh blood, the wool of the third or fourth generation is ſcarce diſtinguiſhable from the original Spaniſh wool.

* This muſt mean fleeces unwaſhed, or in the yoke, as it is technically termed.

"Theſe

"Theſe mixed breeds are more eaſily maintained, and can be fattened at as ſmall an expenſe as the ordinary breeds of the country.

"No ſpeculation whatever offers advantages ſo certain, and ſo conſiderable, to thoſe who embark in it, as that of the improvement of wool, by the introduction of rams and ewes of the true Spaniſh race, among the flocks of France, whether the ſheep are purchaſed at Rambouillet, or elſewhere; in this buſineſs, however, it is of the greateſt importance to ſecure the Spaniſh breed unmixed, and the utmoſt precaution on that head ſhould be uſed, as the avarice of proprietors may tempt them to ſubſtitute the croſſed breeds inſtead of the pure one, to the great diſappointment of the purchaſer.

"The amelioration of wool at Rambouillet has made ſo great a progreſs, that in a circle from twenty-four to thirty-ſix miles in diameter, the manufacturers purchaſe thirty-five thouſand pounds of wool, improved by two, three, or four croſſes. Thoſe who wiſh to accelerate the amelioration of their flocks by introducing into them ewes of this improved ſort, may find abundance to be purchaſed in that neighbourhood at reaſonable rates."

VII. *On a periodical Variation of the Barometer, apparently due to the Influence of the Sun and Moon on the Atmoſphere.* By LUKE HOWARD, *Eſq. Read before the Aſkeſian Society, London* *.

THAT the moon and planets exerciſe a certain influence (excluſive of the more evident effects of the ſun) on the ſtate of the globe we inhabit, appears to have been the opinion of

* This ſociety conſiſts of a ſelect number of gentlemen, aſſociated for their mutual improvement in the different branches of natural philoſophy. It was inſtituted in March 1796, and the regular meetings are held every other week during the winter.

The preſent eſſay is extracted, by permiſſion, from the collection of thoſe furniſhed at different times by the members; and from the ſame ſource we have been promiſed to be favoured with ſome other papers which may prove gratifying to our philoſophical readers.

 mankind

mankind from the moſt remote antiquity. The true nature and extent of their influence ſeems, however, not to have been well underſtood until the epoch of the ſublime diſcoveries of Newton.

Whether the firſt profeſſors of aſtrology, a ſcience which we find to have been ſo antiently cultivated in the Eaſt, entertained more juſt ideas of the planetary influence than their ſucceſſors in later times, we may not now be able to determine. With reſpect to the pretenſions of the latter, it is, however, certain, that the maſs of abſurd notions, the product of credulity and imagination, under which is buried every thing true or valuable in their writings, has given occaſion to their falling into general contempt, more eſpecially in the preſent advanced ſtate of aſtronomy. The more myſterious and impoſing ſtudy of aſtrology has ſtill nevertheleſs its advocates; and the pretenders to it, availing themſelves of the meteorological facts recorded by their predeceſſors, and countenanced by ſomething not well underſtood in the experience of the multitude, continue to put forth their annual predictions of the weather; and, in this reſpect, in ſpite of ſufficiently frequent proofs of this want of *ſpecial* foreſight, they ſucceed often enough to keep up a ſort of credit with their wondering readers. In the mean time, is it any credit to thoſe who have long had in their hands correct regiſters of the weather, and other means of forming an accurate judgment of ſuch matters, that they either ſuffer theſe deceptions to paſs unnoticed, or content themſelves with ſubſtituting ridicule for inquiry?

Obſerving that the ſubject of lunar influence on the atmoſphere promiſes at length to obtain due conſideration (by ſome paſſages in foreign journals, as the Theory given by Lamarck in the *Journal de Phyſique, Floréal, An.* 8, *&c.**), I am inclined to bring forward ſome obſervations which firſt occurred in keeping the regiſter of the barometer for 1798 annexed,

* In particular a paper by Toaldo, in the *Journal des Sciences utiles*, tranſlated in the Philoſophical Magazine, Vol. III. p. 121, with which the reader is requeſted to compare theſe obſervations (which were written previous to the author's knowledge of Toaldo's Theory), as they mutually ſupport each other.

annexed, and which have been amply confirmed in extending the inquiry to other regiſters.

The dotted line, which, in the barometric chart* (Pl. IX.) repreſents the courſe of the barometer, was traced by daily obſervations (with an excellent barometer made by Haas) on a ſet of blank charts, each of which ſerves for a month; being ruled horizontally with red lines, dividing the ſpace into inches and tenths, and ſerving for a ſcale, and perpendicularly with others, two-tenths diſtant, one of which is allotted to each day †.

My regiſter was kept at Plaiſtow in Eſſex, about five miles eaſt of London, except three ſpaces diſtinguiſhed by full dots at each extremity, which were noted in London at an elevation not much more than twenty feet greater, and for moſt part of which I am indebted to the obſervations of a friend. As the barometer rarely changes its *direction* during the night, it is evident that, by an enlarged ſcale and frequent obſervations, a very accurate account of its variations might be kept in this way; and none can give a more pleaſing ſynoptical idea of theſe, with the correſponding changes of weather, for the paſt month or year.

As the moon's phaſes had been inſerted in the explanatory plate of Dr. Buxton, by a character affixed to the day, I continued this practice, and after ſome time began to ſuſpect a coincidence between theſe and the courſe of the barometer, which at length became very evident, and gave occaſion to further inquiry. This coincidence conſiſts in the depreſſion of the barometrical line on the approach of the new and full moon, and its elevation on that of the quarters. In above thirty out of the fifty lunar weeks in this year, the barometer will be found to have changed its *general direction* once in each week, in ſuch manner as to be either riſing or

* The accompanying plate is engraved on a ſcale of half an inch to an inch.

† The blank charts are ſold by Edward Nairne, Cornhill, and were publiſhed by Dr. Buxton in 1794, who has obliged me with the inſpection of his regiſter from that time to the preſent. There are columns added for wind, rain, &c. which it was not neceſſary to introduce on this occaſion.

at its *maximum* for the week preceding and following, about the time of either quarter; and to be either falling or at its *minimum* for the two weeks, about the new and full. It is remarkable, that the point of greateſt depreſſion during the year, *viz.* to 28,67, is found about twelve hours after the new moon on the 8th of the eleventh month; and that of its greateſt and extraordinary elevation to 30,89 on the 7th of the ſecond month at the time of the laſt quarter. Moreover, this coincidence appeared to obtain the moſt regularly in fair and moderate weather; and, in general, when the barometer fell during the interval between the new or full moon and the quarters, an evident perturbation in the atmoſphere accompanied; of which may be inſtanced II. 15 to 23, when the barometer, after an uncommon riſe, continued to fall rapidly after the new moon, with ſevere cold, which ended ſuddenly in ſtormy and wet weather; again, VI. 14 to 20, when two weeks of fair weather ended in a thunderſtorm. In the greater part of XII. the uſual coincidence diſappears, and the *converſe* takes place, the barometer being low at the quarter and high at the full, amidſt continued alternations of rain, froſt, and ſnow; and, for part of the time, high winds. The remarkable depreſſion between the 24th and 28th was attended with intenſe cold, the thermometer being noted on four ſucceſſive days at 15°. 14°. 9°. 13°. reſpectively. On the two days preceding the laſt quarter, the barometer roſe rapidly, and rain followed!

In the ſeventh month, which was wet and windy throughout nearly, there is the leaſt of this coincidence to be ſeen. Inſtances may be obſerved in which the tendency to riſe or fall at the times pointed out, after being interrupted by an occaſional cauſe, is reſumed. On the whole I thought there appeared ſufficient ground, on the evidence of the year 1798, to ſuppoſe that the gravity of our atmoſphere, as indicated by the barometer, may be ſubject to certain periodical changes, effected by a cauſe more ſteady and regular than either change of temperature, currents, or ſolution and precipitation of water, to which I believe the whole variation has been heretofore attributed. To diſcover theſe periodical changes, if poſſible, and to aſcertain their amount, inde-

pendently

pendently of the operation of the last-mentioned causes, I took the following method: Passing regularly through the register of the barometer which I had chosen for examination, I extracted one observation for each lunar week, as near as possible to the time of the change, which I ranged under its proper title, either new moon or full, &c. Having gone through the year, I took the mean of the whole of these notations, and afterwards the mean of those under the title of new moon, full moon, first and last quarter, respectively; by comparison of which with the general mean I attained my object.

The annexed register being examined thus, by taking the numbers marked on the plate under each change, the results were as follows:

Mean of the Whole.	Full Moon.	Last Quarter.	New Moon.	First Quarter.
29,9638	29,906	30,153	29,719	29,980
Difference - -	—,0578	+,1892	—,2448	+,0262

It will appear, by inspecting the plate, that if the numbers had been taken with a latitude of only 36 hours on either side the moon's changes, the results would have been still more favourable to the supposed coincidence.

The register of the Royal Society, given in the Philosophical Transactions, affording an opportunity of trying it on a much larger scale, I extracted weekly observations for ten years, *viz.* from 1787 to 96, adhering strictly to the rule I had laid down, and compared the mean of each class with the mean of the whole for ten years, as also with the mean of the register at large for that time, which gave the following results.

Mean of the Whole.	New Moon.	First Quarter.	Full Moon.	Last Quarter.
29,818	29,7946	29,8910	29,7812	29,8823
Difference from the Total Mean.	—,0234	+,0730	—,0368	+,0643

The mean of the register at large appeared (on computation from the mean of each year as given in the register) to be 29,89, whence it appears that the depressions at the new and full moon either amounted to more, on the whole, than the elevations at the quarters, or that they fell out nearer to the time.

time. I was quite satisfied, in passing through this register, that if I had allowed myself to choose the higher notations about the quarters, and the lower about the new and full, with a latitude of 24 or 36 hours, it would have made the results as much more favourable to my conclusions as in her former case.

Now, to omit the consideration of other proofs for the present, it appears to me evident, that the atmosphere is subject to a periodical change of gravity, whereby the barometer, on a mean of ten years, is depressed at least one-tenth of an inch while the moon is passing from the quarters to the full and new; and elevated, in the same proportion, during the return to the quarter. To what causes shall we attribute this periodical change, other than the attraction of the sun and moon for the matter composing the atmosphere?

The atmosphere is a gravitating fluid, differing, in a physical sense, from the water, chiefly in possessing less gravity; and it is demonstrable *à priori* on the principles of the Newtonian philosophy, that it ought to have its tides as well the ocean, although in a degree as much less perceptible as is its gravity.

I suppose, therefore, that the joint attractions of the sun and moon at the new moon, and the attraction of the moon predominating over the sun's weaker attraction at the full, tend to depress the barometer, by taking off from the gravity of the atmosphere, as they produce a high tide in the waters, by taking off from their gravity: and again, that the attraction of the moon being diminished by that of the sun at her quarters, this diminution tends to make a high barometer, together with a low tide, by permitting each fluid to press with additional gravity upon the earth.

I am aware that several circumstances attending the facts I have brought forward prevent them from making a case fairly parallel with the tides; on which I shall make such remarks as occur in this early stage of the inquiry.

In the first place, the waters have daily tides corresponding with the motions of the earth and moon; and there is additional elevation and depression about the phases of the moon, amounting, suppose, to a fourth part of the whole.

In

In the atmoſphere we have at preſent no proof of diurnal tides, which ought to be the moſt apparent *.

The elevations and depreſſions of the barometer, *which appear to be periodical*, are ſometimes more, ſometimes leſs, conſiderable; but in moſt caſes, when they are regular, greatly exceed the proportion which they ought to bear to the extremes of the ſcale, on the ſuppoſition of their being due to the relative poſitions of the ſun and moon only. They arrive at their extent ſometimes before, ſometimes after, the time of the moon's phaſes, and their direction is even ſometimes contrary to the theory propoſed, for many days together; in all which particulars they diſagree with the tides of the ocean.

Now in order to a proper conſideration of theſe differences, it is neceſſary to keep in view the different conſtitution of the two fluids, which are the ſubjects of the compariſon.

The ocean is a denſe fluid, incumbent on the ſolid earth, pretty much alike in temperature and compoſition throughout, ſubject indeed to certain currents which are found to be pretty conſtant and appreciable. It does not appear to vary in quantity. It has a well-defined ſurface, by which we can meaſure the alterations of level it is ſubject to:

The atmoſphere is a much rarer and an elaſtic fluid, incumbent partly on tracts of land, of various qualities, partly on the moveable ſurface of ſeas, partly on ſnow and ice. It differs, at different times, in quantity, and varies much in denſity, temperature, and compoſition; is moveable through its whole extent by different and oppoſite currents, for the moſt part uncertain and changeable; and we know nothing about its ſurface. Theſe circumſtances being conſidered, we ſhall not expect, even in theory, the ſame regularity in the tides of the atmoſphere as in thoſe of the ocean.

The want of facts to prove the exiſtence of diurnal tides appears indeed at firſt view an inſuperable difficulty; ſince, if theſe did take place, the barometer ought to indicate them,

* The means of aſcertaining theſe were not ſo obvious in the time of Newton; and it is accordingly ſaid, in B. II. chap. 6, of Pemberton's Newton, that "the gravitation of theſe fluids (water and air) in the earth towards the moon produces no ſenſible effect, except in the ſea, where it cauſes the tides."

by rising and falling twice in each day, in a degree proportioned to the supposed weekly tide. But perhaps even this difference may be found to result from the different constitution of the two fluids. The water, being of more uniform density and possessing so much greater gravity, is elevated and subsides with proportionate steadiness. The air, falling greatly short of the water in gravity, and its density continually decreasing upwards, must needs be very differently affected. If we take notice also of the greater ease with which the latter is thrown into currents, and lastly of its remarkable property of dissolving mere water as it increases in density and temperature, and letting it fall as it decreases in either, it will not seem impossible, that the daily flux and reflux, which is so manifest in the waters, should be lost and counteracted in the atmosphere; while, on the other hand, the progressive increase of either, through the lunar week, should, by the very same causes, be sometimes promoted and rendered more conspicuous.

It will, however, be soon enough to enter upon the theory of the atmospherical tides, when the preceding facts shall have been examined, and the influence of the sun and moon on the gravity of the atmosphere established, by more extensive observation. It is for this purpose that the subject is now brought forward, and the cooperation of meteorological observers, in this or other countries, into whose hands this may come, is requested. The coincidence, so far as hitherto observed, is an important fact, and should it be found to obtain generally, it will necessarily lead to several important consequences; and, in the first place, to a new and more satisfactory theory of the barometer; in consequence of which that instrument may be more successfully applied to foretell the changes of the weather. The true reason likewise of the weather so frequently coinciding, in the time of its various alterations, with the changes of the moon (a coincidence which has long served to direct the predictions of the almanac-makers), will be apparent, and the diligent meteorologist will avail himself of it in forming *probable conjectures* on the changes likely to ensue for a certain time, not exceeding that which limits the operation of the known cause or causes.

P.S. That

P. S. That the exiftence of a daily flux and reflux in the atmofphere has not been hitherto generally admitted, may be in part owing to the want of frequent obfervations upon inftruments with a fufficient range of fcale. It is, however, more probable, that the lunar weekly flux or reflux, as either prevails, may have the greateft fhare in preventing the detection of the contrary femi-diurnal movement; for, at Calcutta, where the range of the barometer is at all times inconfiderable (if we may judge by one year's obfervations), and where the weekly lunar influence can fcarcely be traced, a manifeft regular daily tide has been detected by the diligent obfervations of Francis Balfour, Efq. in 1794, which took place, as follows, during the month called April:—Beginning from fix in the morning, the barometer rofe for four hours, then fell during eight hours; then rofe four hours, and fell eight again; which took place daily, and, with very little exception, uninterruptedly.—(See the Regifter in the Afiatic Refearches.)

VIII. *Letter from* C. H. TATHAM, *Efq. Architect, containing a brief Account of the grand antique Bacchanalian Vafe, late in the Poffeffion of the Right Hon. Lord Cawdor, now at Woburn Abbey.*

SIR, September 15, 1800.

THE attention of the amateurs of the fine arts having been lately excited by the fale of Lord Cawdor's noble collection of antiques, and particularly of the celebrated Bacchanalian vafe, formerly an object univerfally admired in the Lanti Palace at Rome, the following brief account of it may not be unacceptable to your readers.

The forms of all antique vafes, whether bell-fhaped or the tazza, are fuppofed to have been firft taken from the calyx of the lotus; the latter reprefenting the plant in a flatter form, as it appears when fully blown, and the former in the more early ftage of inflorefcence.

The lotus is a celebrated water-plant, well known in Upper Afia, which, from its ftructure, and its reputed quality of being generated by heat and moisture, was selected by the antients as the symbol of the generation and procreation of the human species; it was therefore deified and held sacred, and became the model from which, as it is thought, they copied the form of their principal vases.

The grand Bacchanalian vase in question (see a geometrical elevation of it, Plate X.) is of the lotus form, bell-shaped, and was most probably consecrated to the god Bacchus, as may be concluded from the finely-sculptured Bacchanalian masks, and other features that accompany it: it must therefore have been used either as a laver or symbol only of this part of the heathen mythology, and for no other use; for it is certain that no wine was ever poured into it.

This superb monument of antique decoration was dug up, some centuries ago, among the ruins of Adrian's villa, together with the fragments of three other vases of nearly similar dimensions; all of which appeared, by the situation in which they were found, to have occupied the same particular spot of that once extensive and magnificent emporium of art. It was then removed to the villa Lanti, near Rome, where for many years it attracted the notice and excited the admiration of both the traveller and the artist. This, and one at Warwick castle, which is somewhat more decorated, are the only complete vases, of the same dimensions, extant; and are unquestionably the most magnificent and nobly-sculptured specimens of antique decoration of this kind ever discovered.

The Lanti vase was brought from Rome about twelve years ago, at a considerable risk and expense, by the Right Hon. Lord Cawdor, on whose classical taste and judgment it must ever confer the highest credit. The removal of this grand work of art from that city, caused great jealousy among the superintendants of the Vatican Musæum, then forming under the auspices of the reigning pontiff, the late Pius the Sixth; who, it is well known, in his resentment on this occasion, threatened several persons concerned in the removal of the vase, with the gallies.

The dimensions of the vase are: diameter of the mole, six feet three inches; height, with its present plinth, six feet

feet nine inches. It was originally in one piece of Parian marble.

It was purchafed at the public fale of Lord Cawdor's mufæum in Oxford-ftreet, the 6th of June 1800, for feven hundred guineas, and is now in the poffeffion of the Duke of Bedford, who has caufed it to be removed to Woburn Abbey, where it is to be placed in an appropriate fituation, in the centre of an extenfive and beautiful greenhoufe, defigned and executed for his Grace by Mr. Holland.

I am, Sir,

Yours, &c.

CHARLES HEATHCOTE TATHAM.

To the Editor of Philofophical Magazine.

NEW PUBLICATIONS.

New Obfervations concerning the Inflexions of Light, accompanying thofe of Newton, but differing from his, and appearing to lead to a change of his Theory of Light and Colours. Cadell and Davies; 1799. 134 Pages, with Eight Copperplates.

THE author of this effay has carefully repeated thofe experiments by which Sir Ifaac Newton effected his analyfis of light. The experiments have produced to his obfervation, phenomena materially different from thofe which appeared to Newton. He feems to have obferved the phenomena with very accurate attention. He concludes from the whole, that Newton has erred in believing every ray of common light to be compofed of feven differently coloured, primigenial, elementary rays. On the contrary, this author infers from his obfervations, that *all light is originally of one uniform white colour*; that *its diverfity of colours, in inflexion, is occafioned by the bendings, feparations, and other changes of its parts in paffing through a tranfparent medium, or under attraction by the edge, angle, or fide of an approaching body*; and that, by confequence, the Newtonian Theory of light and colours is not fundamentally true.

The apparent accuracy of thefe obfervations; the logical fairnefs

fairnefs of the induction; the literary compofition of the effay, deferve every praife. Without having ourfelves repeated the experiments, and without knowing them to have been repeated, with fimilar refults, by others, we would not prefume to decide concerning the truth of the doctrine.

The fame doctrine has been more haftily and obfcurely fuggefted by Mr. Heron, in his Elements of Chemiftry.

The Philofophical Tranfactions of the Royal Society of London for 1800. *Parts I. and II.* Elmfly, London.

THE Firft Part contains: 1. The Croonian Lecture. On the Structure and Ufes of the Membrana Tympani of the Ear. By Everard Home, Efq. F.R.S.—2. On the Methods of determining, from the real Probabilities of Life, the Nature of contingent Reverfions, in which three Lines are involved in the Survivorfhip. By William Morgan, Efq. F.R.S.—3. Abftract of a Regifter of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, for the Year 1798. By Thomas Barker, Efq.—4. On the Power of penetrating into Space by Telefcopes, with a comparative Determination of the Extent of that Power in natural Vifion and in Telefcopes of various Sizes and Conftructions: illuftrated by felect Obfervations. By William Herfchel, LL.D. F.R.S.—5. A fecond Appendix to the improved Solution of a Problem in Phyfical Aftronomy, inferted in the Philofophical Tranfactions for the Year 1798, containing fome further Remarks, and improved Formulæ, for computing the Co-efficients A and B, by which the Arithmetical Work is confiderably fhortened and facilitated. By the Rev. John Hellins, B.D. F.R.S. and Vicar of Potter's Pury, in Northamptonfhire.—6. Account of a Peculiarity in the Diftribution of the Arteries fent to the Limbs of flowly-moving Animals; together with fome other fimilar Facts. By Anthony Carlifle, Efq.—7. Outlines of Experiments and Inquiries refpecting Sound. By Thomas Young, M.D. F.R.S.—8. Obfervations on the Effects which take place from the Deftruction of the Membrana Tympani of the Ear. By Mr. Aftley Cooper.—9. Experiments and Obfervations on the Light which is fpontaneoufly emitted, with fome degree of permanency, from

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INTEL-

INTELLIGENCE,

AND

MISCELLANEOUS ARTICLES.

LEARNED SOCIETIES.

FRENCH NATIONAL INSTITUTE.

ON the 4th of July laſt, the following account of the labours of the Mathematical and Phyſical Claſs during the preceding three months, was read by C. Cuvier, ſecretary:

The antients admitted only four elements, by the combination of which they ſuppoſed all other terreſtrial ſubſtances to be formed. Modern chemiſtry has rejected this vulgar opinion, and has given the name of *elementary* to all ſubſtances which it is not able to decompoſe, but it at the ſame time endeavours to diminiſh the number of them; and whenever it is able to prove that a ſubſtance, which appears to be ſimple, reſults from the combination of two others, it obtains the double advantage of ſimplifying the theory of the ſcience, and of being able to reproduce, at pleaſure, that ſubſtance, with the analyſis of which it is acquainted.

It appears at preſent that it is on the eve of a diſcovery of this kind in regard to two ſubſtances, very intereſting on account of the part which they act throughout nature, and of the uſe to which they are applied in the arts. I here allude to the acid of ſea-ſalt, called by the modern chemiſts the *muriatic acid*; and the two fixed alkalies, *ſoda* and *potaſh*.

It has lately been diſcovered, that almoſt all the *acids* are obtained from the combuſtion of certain ſubſtances; the ſubſtance which muſt be burned to obtain an acid is called the *radical* of that acid, that is to ſay, according to the pneumatic theory, which muſt be combined with oxygen to produce that acid. The *marine acid* was one of thoſe the radical

dical of which was not known; and it is this radical which Berthollet has been endeavouring to difcover.

This chemift thinks himfelf authorifed to believe that the muriatic acid is a triple compound of azot and a fmall quantity of hydrogen and oxygen. It would be tedious to give an account of the various and accurate experiments which conducted him to this refult. It will be fufficient to fay in general, that he always faw muriatic acid formed, when to the prefence of the nitric acid there was joined any circumftance which forced the water to be decompofed. But the radical of the nitric acid is known to be azot, and water can add only hydrogen to the principles of that acid. It was to an induction nearly of the fame kind, that Berthollet was indebted fome years ago, for his difcovery of the compofition of *volatile alkali* or *ammonia*, which he found to be formed of *hydrogen* and *azot*.

Chemifts then employed themfelves in endeavouring to difcover the compofition of the two fixed alkalies. It was natural to think that one at leaft of the two principles of the volatile alkali would be common to them and deferve the name of *alcaligen*, as *dephlogifticated* air has been called *oxygen* becaufe it is the common principle of all the acids. But on one hand they were ignorant whether the azot or the hydrogen was the common principle of the alkalies, and on the other, they did not know with what fubftances it ought to be combined to produce them.

Guyton has prefented fome experiments made by Desformes, which he has in part repeated, and which tend to prove that *potafh*, or fixed *vegetable alkali*, is a compound of *hydrogen* and *lime*. Thefe chemifts have feen lime produced under circumftances when, in every inftance of all the bodies brought into contact, none but the *potafh* could contain it. They have feen alfo that this production of lime was preceded by the combuftion and difappearance of the hydrogen.

Soda or fixed mineral alkali having given magnefia under fimilar circumftances, they think that this alkali is a combination of *hydrogen* with that earth. Desformes goes much further, for he is of opinion that alumine, magnefia, and lime, are only the fame earth combined with more or lefs

 azot.

azot. Were his ideas confirmed, we ſhould not only approach near to the doctrine of the antients reſpecting the elements, but have an eaſy explanation of a multitude of phenomena difficult to be accounted for in the hiſtory of nature; as, the ſaltneſs of the water of the ſea; the formation of nitre and marine ſalt in inhabited places; that of potaſh in vegetables, of ſoda in animals; the converſion of ſome of the earths into each other, &c. All theſe facts, ſo myſterious in appearance, and which have exerciſed the ingenuity of the philoſophers for ſo many ages, would be conſequences eaſy to be deduced from theſe principles. Unfortunately, however, theſe chemiſts have not confirmed their analyſes by ſyntheſis; that is to ſay, they have not yet remade potaſh by combining directly lime and hydrogen.

The labour of Berthollet on the muriatic acid had been preceded and occaſioned by another on an object no leſs important—*eudiometry*. This is the name given to the art of analyſing atmoſpheric air, and in particular of aſcertaining how many parts of pure vital air or oxygen the atmoſphere of any place contains. The health of mankind as well as philoſophy is intereſted in the perfection of this art. The combuſtion of phoſphorus has been long employed for this analyſis, becauſe this combuſtion abſorbs the oxygen in a concrete acid, and it is then eaſy to meaſure what remains under a gaſeous form, which is azot.

Mr. Humboldt, a learned German philoſopher, thought he found this method defective, becauſe, according to his account, there always remained oxygen not fixed, and becauſe a portion of azot mixed itſelf with the acid. He entertained the like opinion of liver of ſulphur, or alkaline hydrogenated ſulphuret, which ſome employ alſo for the ſame uſe, and prefers the means pointed out by Fontana, or nitrous gas, which is an aëriform combination of azot, with leſs oxygen than is neceſſary to form nitric acid. When this gas is mixed with new oxygen, it abſorbs it more exactly, according to Mr. Humbolt, than the above ſubſtances; but as it may contain a variable quantity of azot, this mean is not unattended with inconveniences. Mr. Humboldt thinks that this might be remedied by examining each time the gas to be used, by washing it with a solution of copperas, or sulphat of iron; which, according to his opinion, will absorb all the nitrous gas, but leave the azot, which is mixed with it.

It is this action of the sulphat of iron, or nitrous gas, with which Berthollet has been occupied; and it was on it indeed that the solution of the problem depended. He has found that under this circumstance, as well as under a multitude of others, the nitrous gas is not only absorbed, but decomposed, and that it abandons a part of its azot to convert itself into nitrous acid. He is of opinion that the nitrous gas may differ in the proportions of its two principles, oxygen and azot: but that it does not contain azot in simple mixture.

These interesting results are the consequences of the author's ideas on chemical affinities, communicated to the public above six months ago. He has made a new application of them by comparing the action of the metallic oxyds on substances, such as acids, the composition of which is invariable; and he has proved that the action of these oxyds is modified by their state of oxydation; that the more they are oxydated, for example, the less affinity they have for the nitric and sulphuric acids, while a contrary effect takes place with regard to the muriatic acid. Besides, the combinations of these oxyds with other substances depend also, whatever may be the state of their oxydation, on the proportion in which they enter into these combinations. All the phenomena therefore depend on the proportions, and not on the constant affinity peculiar to each metal.

Three memoirs have been presented on natural history; one of these, by Lacepede, contains some observations on the ant-eater, an American animal, which lives only on ants. The author has rectified the description given by Buffon of the second species of this animal, called *tamandua*, and described a blackish variety hitherto undescribed.

The second memoir is on the ibis of the antients, by Cuvier. The author has discovered from mummies, monuments, and the descriptions of Homer and Plutarch, that this bird was not the same as that which modern naturalists have confidered as the ibis, but another fpecies which he defcribes.

The third memoir is by Haüy on a variety of martial pyrites, or fulphat of iron, to which the author gives the name of *triacontaedra*, becaufe its cryftals have thirty faces, fix of which are rhombs and twenty-four trapezoids. Haüy has explained the laws of drecrement from which thefe cryftals refult, and demonftrate fome curious properties of this folid, to which geometricians hitherto have paid little attention.

The ftructure of the earth, and the refpective pofition of the materials of which it is compofed, are among thofe curious points of natural hiftory ftill involved in confiderable obfcurity. We can ftudy only the outer cruft of the globe, and even this cruft is attended with many difficulties. Happily for our curiofity, the nucleus in the high chains of mountains feems, by fwelling up, to have burft the matter by which it is enveloped, and to have fhown itfelf uncovered. The labours of a Sauffure, a Deluc, a Pallas, and a Dolomieu, have confirmed that the greater part of the large chains are compofed of fmall parallel chains, of which that in the middle, commonly the higheft, confifts of granite, the two collateral ones confift of fchift and ftones of an analogous kind, and the outer ones of calcareous fubftances. This general rule, which throws the greateft light on the theory of the earth, did not feem to be applicable to the Pyrennees; there every thing feemed to be without order. The higheft fummits of Mount Perdu and le Marboré are certainly calcareous, and, as fome affert, contain petrified fhells. C. Ramond, by laborious journeys and ingenious deductions, has difcovered the fource of this embarraffment. It arifes from the fmall chains being in an oblique direction, and from the fchiftous and calcareous bands fituated towards Spain, being not only higher than thofe of the fame nature towards France, but furpaffing even the granitic band, which forms the axis of the chain; fo that the ridge which determines the fall of the ftreams is different from the geological ridge.

The clafs in its laft fittings has been occupied with two objects

objects of great importance for the public prosperity: the introduction into France of a new kind of domestic animal; and the means of making sugar from indigenous plants.

The buffalo, originally a native of the warm and marshy countries of Asia and Africa, was introduced into Italy about the fourth century. Though smaller than the ox, it is more vigorous; the milk of the female is more abundant than that of the cow, and yields more butter and cheese. Every part of its body may be employed in the arts or for the purposes of life; and it possesses this advantage, that it can live in bogs, and feed on aquatic plants, which oxen and horses refuse. Its introduction, therefore, into France, will enable the farmers to turn to advantage a great number of marshes, which at present are entirely useless for want of animals capable of living in them. The possession of this animal is one of the benefits for which France is indebted to its first conquest of Italy. A part of the buffaloes brought to France has been killed by the peasants, and another was neglected by those to whom they were intrusted; but the remainder are in good condition, and in full increase, at the rural establishment of Rambouillet, where they have been examined by Tessier, Huzzard, and Buniva.

The art of extracting sugar from European plants is a foreign discovery. M. Achard, of the Academy of Berlin, seems to have carried it very near to perfection. The National Institute is anxious to ascertain the reality of it, and Deyeux is about to communicate the results which have been obtained by the French chemists.

FULMINATING SILVER.

Mr. Howard has just discovered a new fulminating silver. We understand the process for preparing it is very similar to that followed in preparing his fulminating mercury; but the silver, it is thought, detonates with greater force.

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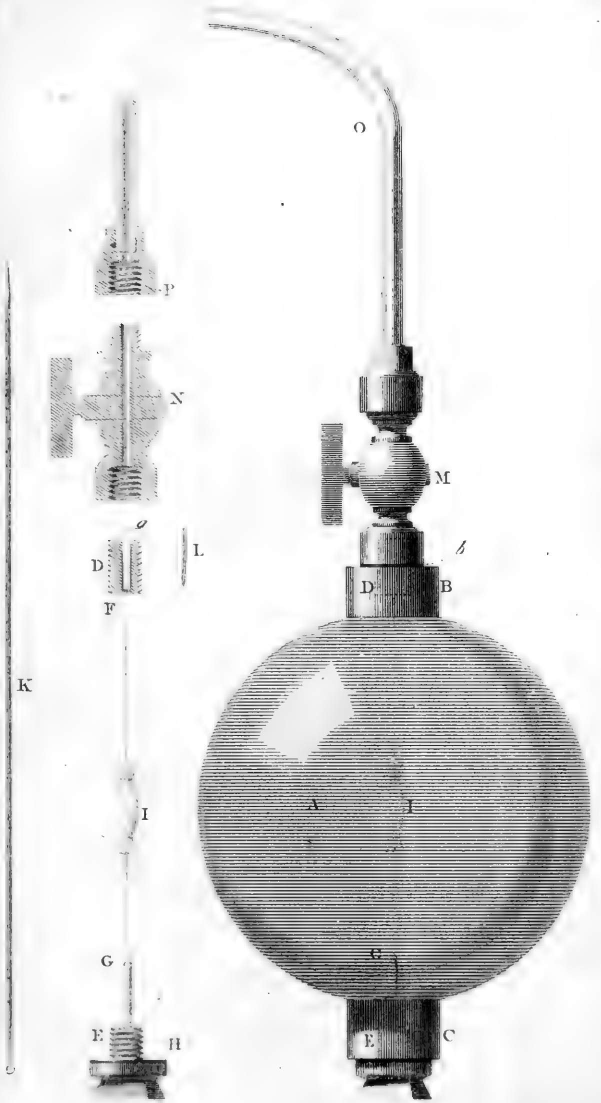

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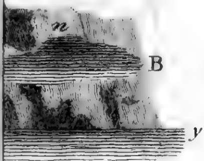

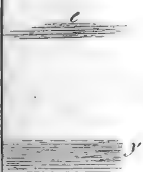

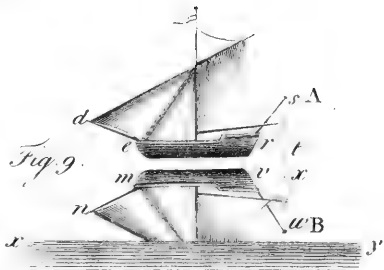

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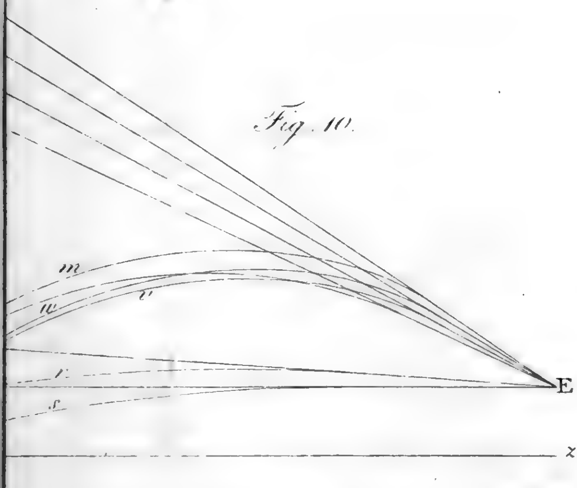

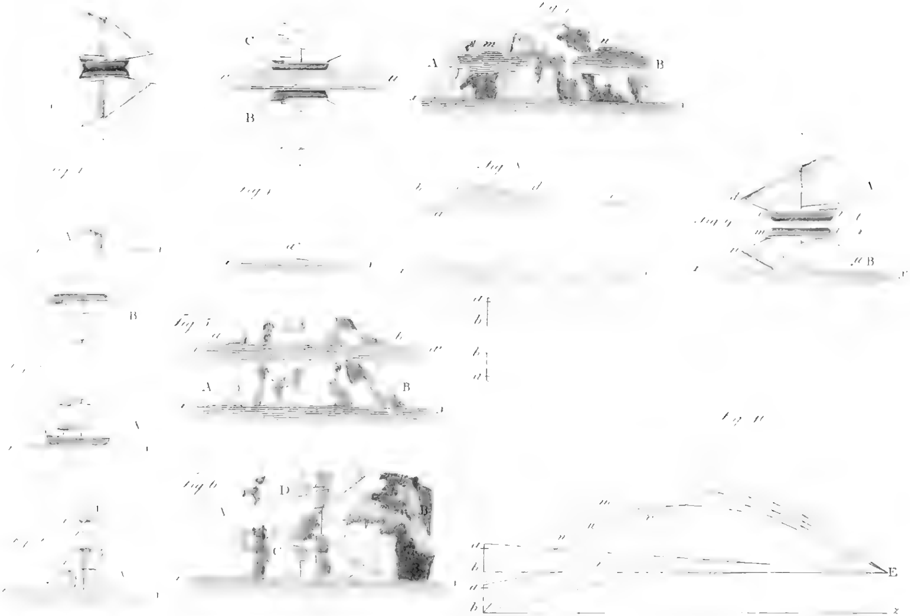

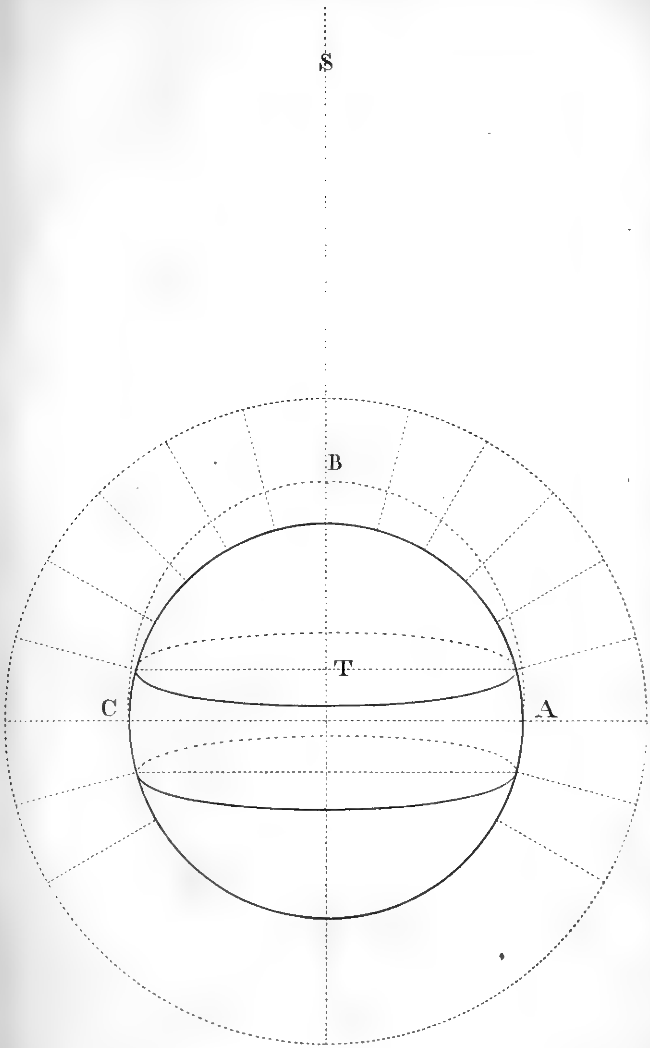
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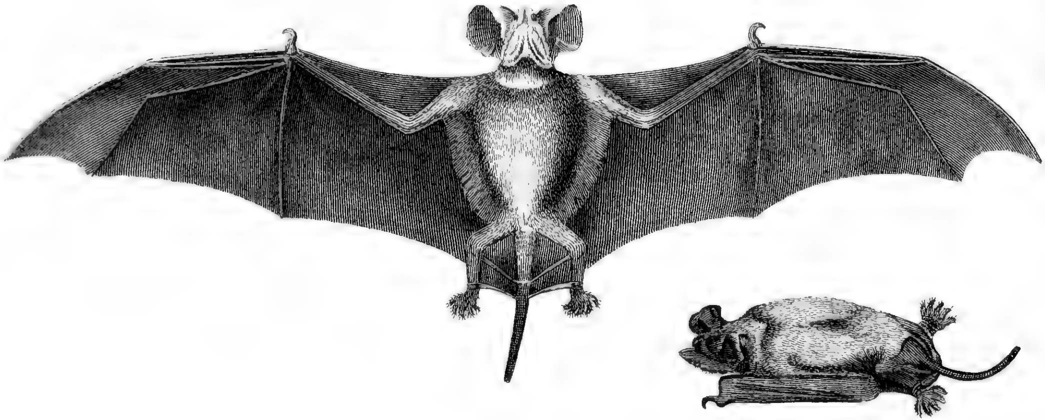

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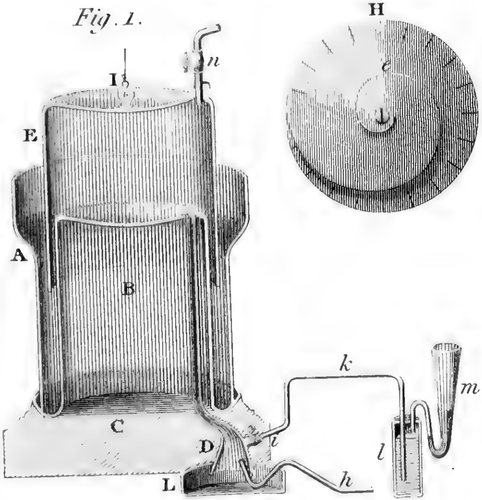

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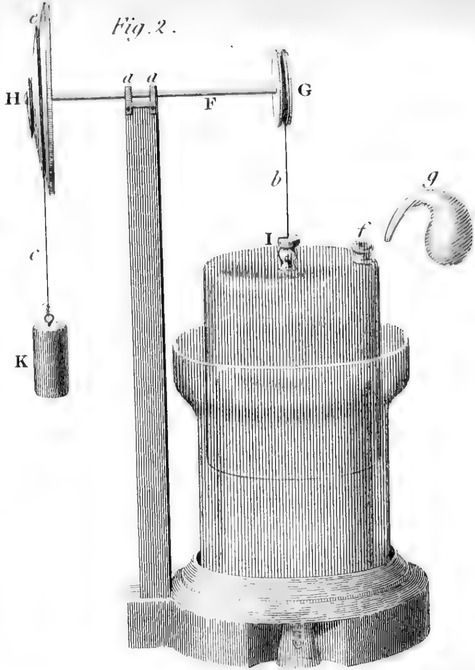

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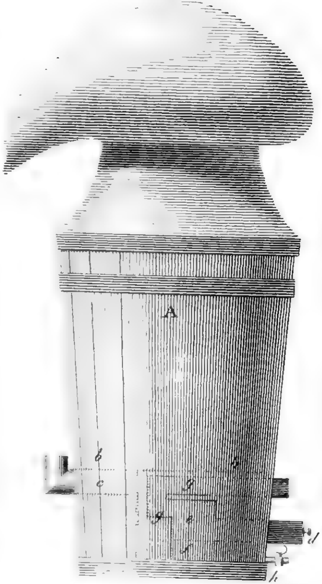

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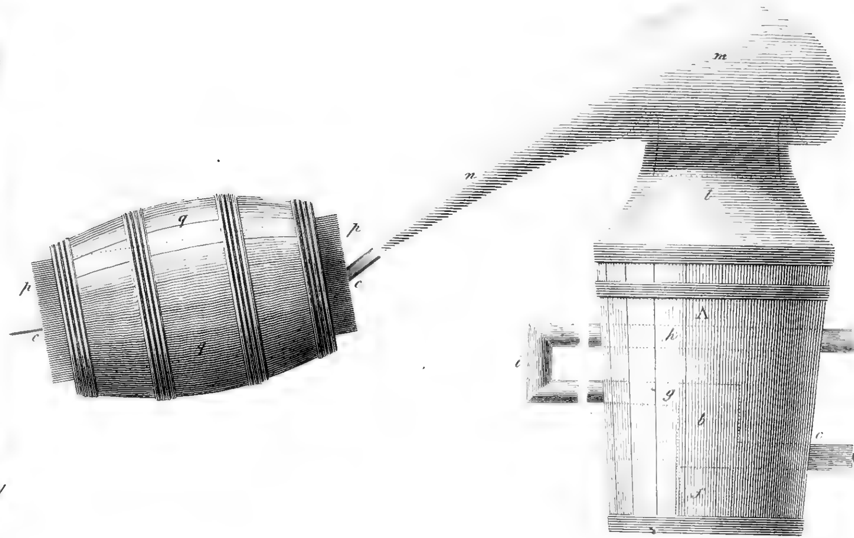

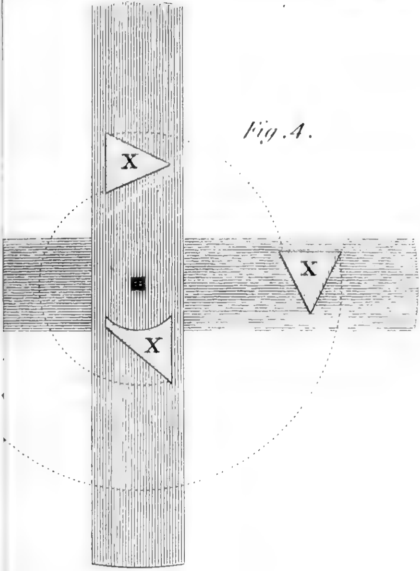

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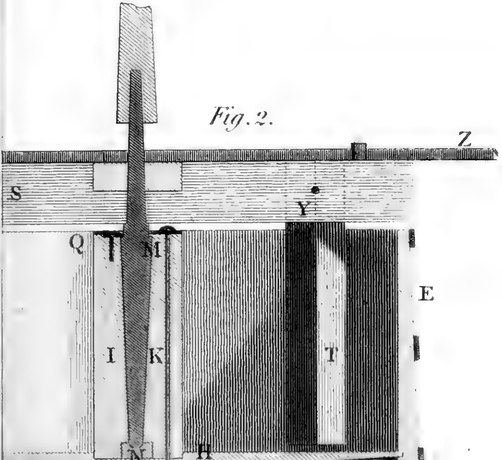

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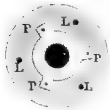

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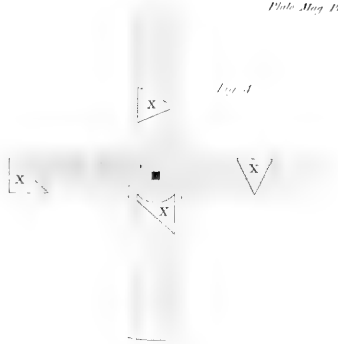

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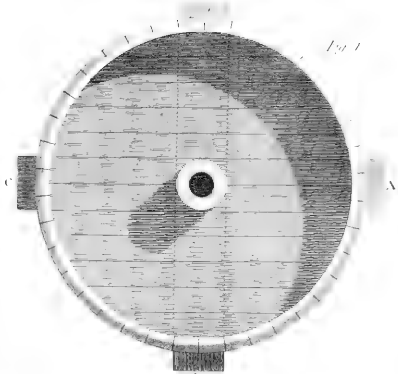

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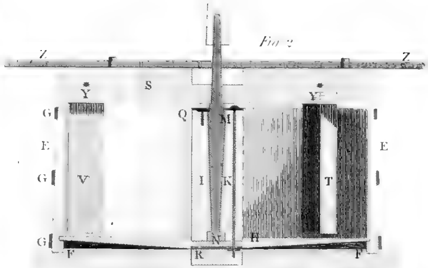

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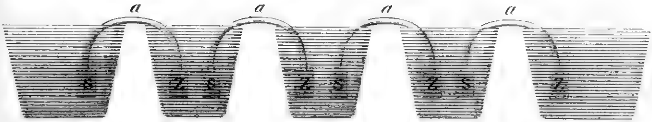

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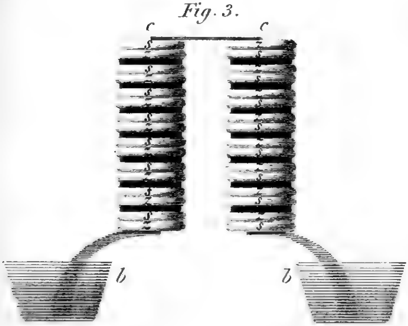

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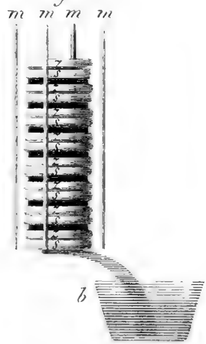

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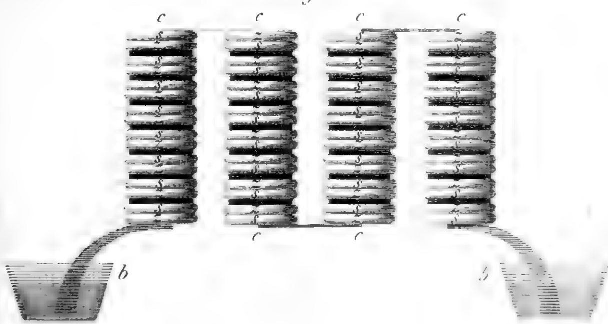

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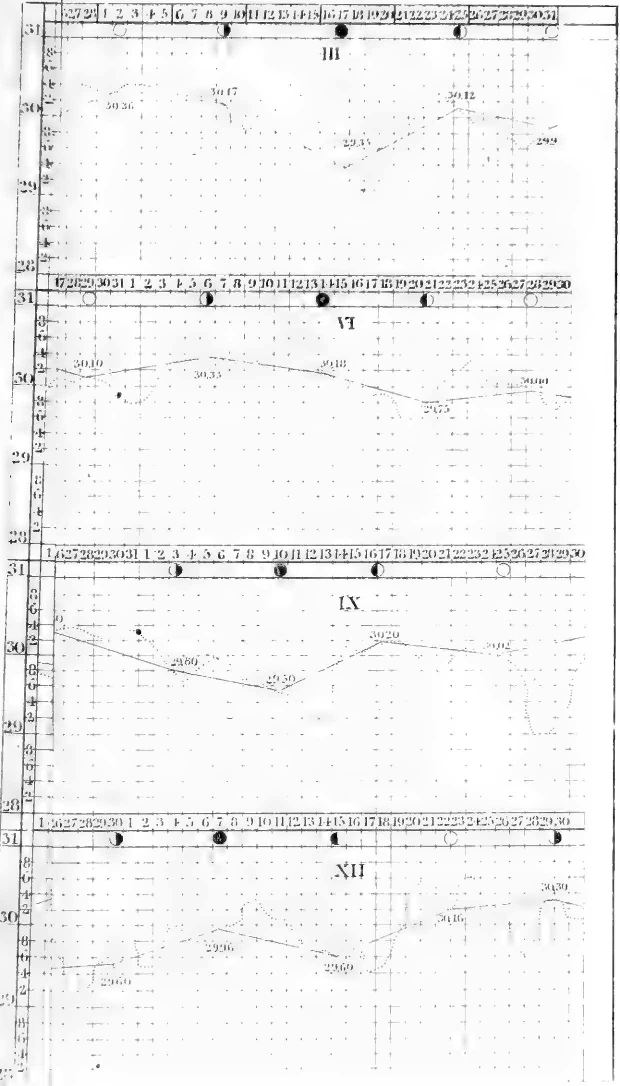

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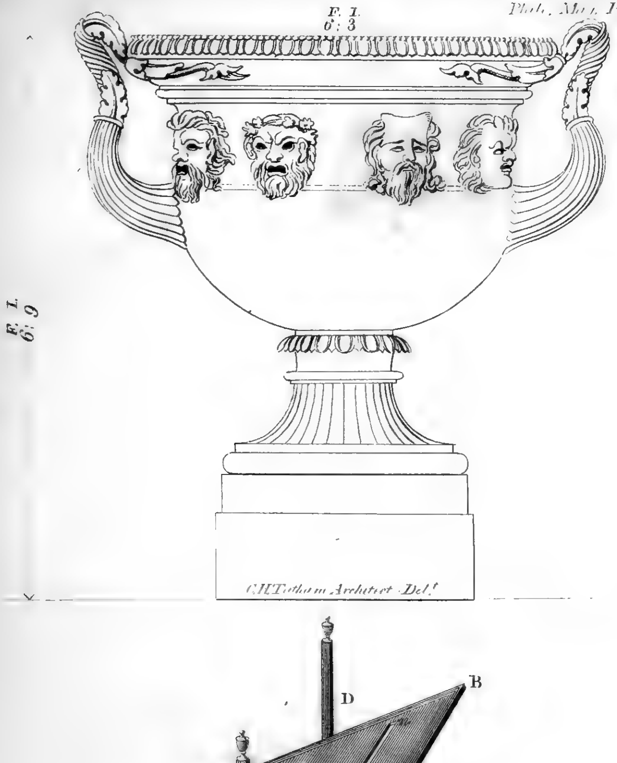
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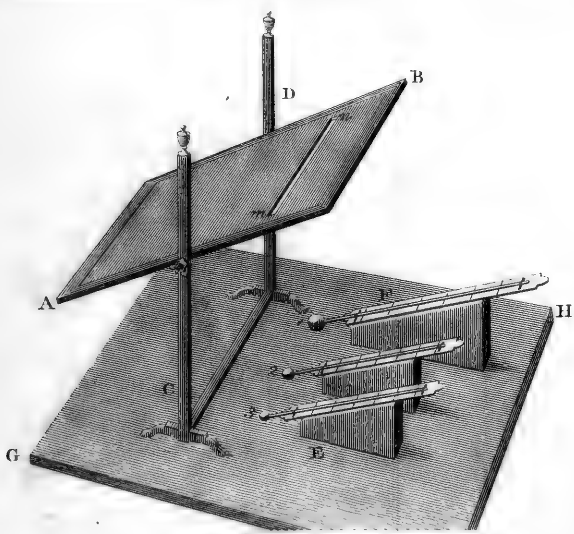
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*** Communications for this Work, addressed to Messrs. Richardsons, Cornhill; or to the Editor, at No. 1, Carey-street, Lincoln's Inn, will meet with every attention.

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