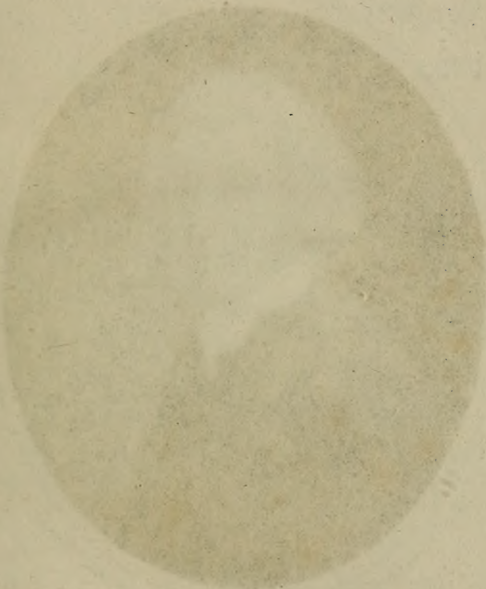

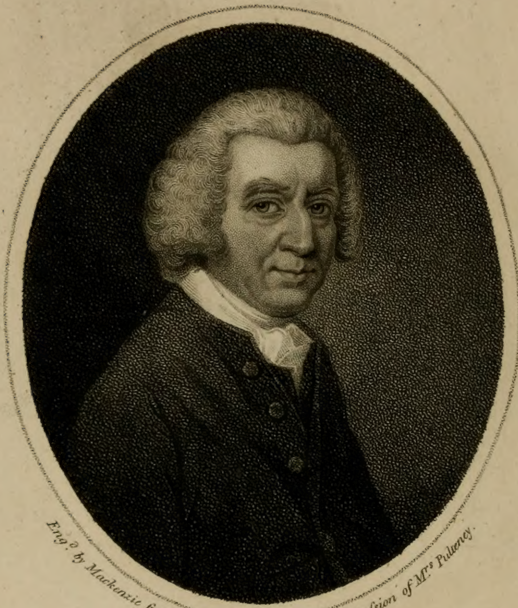

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Richard Pulteney M.D.

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THE

PHILOSOPHICAL MAGAZINE:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

AGRICULTURE, MANUFACTURES,

AND

COMMERCE.

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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[*Wilks and Taylor, Printers, Chancery-Lane.*]

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THE

THE

PHILOSOPHICAL MAGAZINE.

I. *Description of the Arseniates of Copper and of Iron. By the Count de* BOURNON*.

SECTION I.

Arseniates of Copper.

THE natural combination of the arsenic acid with copper, and the different aspects under which this combination appears, according to the proportions in which these two substances are united, were among those objects of mineralogy respecting which our imperfect knowledge required the aid of study and observation. A new copper mine, lately worked, called Huel Gorland, in the parish of Gwennap, in the county of Cornwall, having, within the last two years, enriched the cabinets of London with some very fine specimens of these arseniates, I have been induced to pay particular attention to them; and I offer the result of my observations to the Royal Society as an acknowledgment of that gratitude which I and all Frenchmen, faithful to their king, ought to feel and profess to a country which has distinguished itself as the protector of honour and loyalty.

Although there appears, according to some German authors, reason to think that arseniate of copper has been found in Silesia, its much greater abundance, as well as the various aspects under which it exists, in the county of Cornwall, may entitle it to be considered as one of the many mineral substances which are peculiar, or nearly so, to England.

Of the various works on mineralogy lately published, there are few which have not mentioned arseniate of copper among the ores of this metal. It seems, however, that some of their authors had no knowledge of this ore, except from the very imperfect account communicated by the celebrated Klaproth, in 1787, in the *Schriften der Gesellschaft Naturforschender Freunde*, vol. vii. in which he has given an interesting sketch

* From *Transactions of the Royal Society of London for* 1801.

 of

of the mineralogy of the county of Cornwall, as far as it was then known. Others feem to have poffeffed only imperfect fpecimens of arfeniate of copper, as none of the forms which they attribute to its cryftals can belong to it. Befides, they all confound with this ore thofe cubic cryftals, of a very beautiful green colour, which are found in Muttrell mine, contiguous to Huel Gorland mine; and which, according to the analyfis, made with the greateft care and ability, by Mr. Chenevix, are of a nature totally different, and cannot properly be claffed among copper ores, as they contain but a very inconfiderable quantity of that metal.

The exiftence of arfeniate of copper feems, however, even at this day, to be an object of doubt among the French mineralogifts; for the abbé Haüy does not mention it in the 28th and following numbers of the *Journal des Mines*, although they contain an interefting extract of a fyftem of mineralogy, which he was then preparing for the prefs; nor has M. Fourcroy even hinted at it, in his *Syftème des Connoiffances Chimiques*, lately publifhed.

It is now above twenty years fince arfeniate of copper was difcovered in the county of Cornwall; it was firft found either in Carrach mine, in the parifh of Gwennap, or in Tincroft mine, in the parifh of Allogan. Its matrix, like that of almoft all the copper ores of this country, was filiceous, and confifted of a decompofed granite, of which the greateft part of the feld-fpar had paffed into the ftate known by the name of *kaolin*. It was accompanied with gray vitreous copper ore, frequently in confiderable maffes; alfo with much black oxide of copper; and with various oxides of iron.

The arfeniate here fpoken of, which had never been found in large quantity, had ceafed to exift in the above-mentioned mines, when Huel Gorland mine, lately wrought, began to enrich mineralogy with this uncommon fubftance. The matrix of this is likewife filiceous; fometimes cryftalline; and fometimes in an amorphous mafs. Here and there we find mixed with it, in greater or lefs profufion, all the known oxides of copper; many of the argillaceous oxides of iron; alfo gray vitreous copper ore; arfenical pyrites; and the rich deep-coloured yellow copper ore. This laft is often found differing from its ufual appearance, in a manner which, I believe, has not hitherto been taken notice of. I think it fhould make a diftinct variety among the deep yellow copper ores, under the name of *yellow hematitic copper ore*.

When the combination of copper with iron and fulphur is rich in metal, (for, when it is poor, it is only a martial pyrites mixed with a little copper,) its appropriate colour,

when

when a piece is fresh broken, is a deep yellow; and this yellow colour is more deep in proportion as the quantity of copper is more abundant. In its richest state, it assumes a more or less greenish tint. The surface of a fresh fracture is very brilliant, and appears rather uneven, as if composed of small laminæ crossing one another in an irregular manner. When it begins to decompose, its surface is covered with the most beautiful colours; among which the most predominant are, violet, blue, and green: this has caused it to be compared to a pigeon's breast. When these colours are very deep, and occupy the whole surface of a piece, we commonly perceive, here and there, some little points in the state of red oxide of iron; and others of a green colour, in the state of green carbonate of copper.

This kind of copper ore is sometimes of a deep yellow colour, which inclines the more to green, as it is destitute of brilliancy. It is very compact, and, when broken, the fracture appears smooth, sometimes a little conchoidal; its surface, however, has a very fine grain, which, when viewed with a powerful lens, resembles the aggregation of a very close compact mass of the finest sand. Its most usual texture is that of thin layers or plates, lying one upon another, and being very closely united, so as to be scarcely perceptible to the naked eye; but they are very readily distinguished with the lens. These layers, however, do not adhere very strongly, as they may always be separated by the stroke of a hammer.

This ore frequently assumes a mamillated form; the mamillæ being of different dimensions, from the size of a man's head, and even larger, to that of a small pea. In the latter case, the mamillæ are very often united, as in that kind of iron ore which is called cluster, botryoid, or kidney hematites. Sometimes the surface of the mamillæ is covered with small points; but more frequently it is smooth, so as very much to resemble a piece of polished metal; and, as the surface of the mamillæ inclines rather to a brown colour, they have the appearance of antique bronze. The green oxide of copper, which sometimes is observed on it, completes the illusion, by assuming the aspect of that fine patina which often covers pieces of antique bronze.

This ore is likewise found in the form of small cylinders, often placed one against the other, and sometimes ramified, in the same way as is observed in some kinds of hematites. When the broken surface of it is exposed for some time to the air, it assumes the colour of tarnished gold. It acquires also, by the decomposition of its surface, the same violet, blue, and green colours, as the kind already deſcribed; but, although theſe colours are frequently very deep, they never are ſo brilliant as in that kind.

It is very uncommon to find pieces of this ore that are not mixed, and frequently even penetrated, with gray vitreous copper ore. That which accompanies the arſeniate of Huel Gorland mine, offers, in this reſpect, an uncommon and very particular appearance. The yellow ore is mechanically mixed with the vitreous ore, ſo as to form a compound, in which, by the aſſiſtance of a lens, the ſmall particles belonging to each of thoſe two ores may be very clearly diſtinguiſhed. The ſpecific gravity, as well as the quantity of copper, in this ore, varies conſiderably, according to the proportions in which the yellow copper ore and gray vitreous copper ore are mixed together; ſometimes they ſeem to be mixed in equal proportions, or nearly ſo.

Nature has eſtabliſhed very remarkable differences between the arſeniates of copper; and theſe take place not only in their forms, but likewiſe in their hardneſs and ſpecific gravity. Theſe differences ariſe, either from the manner in which the arſenic acid is combined with the copper, or from the different proportions in which theſe two ſubſtances are combined. I have been naturally led to follow the ſame order, and to divide the arſeniates of copper into four different ſpecies: and the very intereſting analyſis of this ſubſtance, made by Mr. Chenevix, has afforded me the moſt ſatisfactory ſanction to this diviſion. It is thus that the chemiſt and the naturaliſt, by freely uniting their labours, without jealouſy or prejudice, ought in all caſes to proceed, in order to attain that certainty which is the deſirable recompenſe of their endeavours.

First Species.—*Arſeniate of Copper in the Form of an obtuſe Octaedron.*

The moſt ſimple form under which this ſpecies appears, is a very obtuſe octaedron, formed by the united baſes of two tetraedral pyramids, with iſoſceles triangular planes; and this appears to be its original form. This octaedron has, in each of its pyramids, two oppoſite planes more inclined than the other two; which gives a parallelogrammic form to their common baſe. (Fig. 1. Plate I.) The two planes more inclined than the others, meet at the apex of each of the pyramids, in an angle of 130°; and at the common baſe, in one of 50°. The two planes which are leſs inclined, meet at the apex, in an angle of 115°; and at the baſe, in one of 65°.

Thefe planes are commonly fmooth and bright; fometimes, however, they are ftriated in a direction parallel to their edges.

The four planes feldom terminate in one and the fame point: more commonly the apex is formed into a ridge, the octaedron being lengthened, parallel to its lefs inclined planes; the bafe is then a fquare, or at leaft approaches very nearly to that form. (Fig. 2.)

Thefe two varieties are the only ones I have obferved in the form of the cryftals of this fpecies, although I have had the opportunity of examining a great number of fpecimens.

This arfeniate is very light; its average fpecific gravity, taken on fix pieces perfectly pure, was 2,881. Its hardnefs is likewife very inconfiderable; it eafily fcratches calcareous fpar, but makes no impreffion on fluor fpar.

It is feldom perfectly tranfparent, but has generally a cloudy afpect.

The ufual colour of this fpecies (for this character is as effential in metallic fubftances as it is immaterial in ftones) is a beautiful deep fky-blue; fometimes, but very rarely, it inclines more or lefs to Pruffian blue. It is frequently of a very fine grafs green; the cryftals have then a much more beautiful tranfparency. I have feen fome which were of a fine apple green; others white, having a flight blue caft. In one piece, the cryftals of which were of a green colour, and lefs tranfparent than they commonly are, I difcovered, on breaking them, that the colour of their central part, for about half their thicknefs, was blue. From the obfervations made by Mr. Chenevix in his analyfis of thefe arfeniates, it appears that the variation in their colour principally depends on the quantity of water which enters as a conftituent part into their formation.

This fpecies is found mixed with all the other kinds of arfenical copper ore; but that which moft commonly accompanies it, is the prifmatic triedral fpecies.

I have never difcovered in this fpecies any thing which could induce me to fuppofe it fufceptible of decompofition, or even of change.

Second Species.—*Arfeniate of Copper in hexaedral Laminæ, with inclined Sides.*

This fpecies is commonly found in very fine hexaedral laminæ. The fix narrow fides of thefe laminæ have an inclined pofition, alternately in a contrary direction, on the two broad planes, in fuch a manner that each of the planes is encompaffed by three fides, which are inclined upon it.

it. (Fig. 3.) As far as the ſmall ſize, and more particularly the thinneſs of theſe cryſtals, has enabled me to judge, two of theſe three inclined ſides form an angle of about 135° with the broad planes on which they incline; and the third, one of 115°.

The two broad planes are ſmooth, and have a very brilliant luſtre. The ſix narrow ſides are rendered very dull by the great number of ſtriæ with which they are covered; moſt of which are very prominent, and all are parallel to the edges of the broad planes. In conſequence of this, theſe cryſtals may be divided, parallel to the planes, almoſt as eaſily as cryſtals of mica.

This ſtructure prevents the cryſtal from being conſidered as a modification of the octaedron: that which would be produced by an increaſe of the inclined ſides, would only be a ſecondary cryſtal; and none of the ſpecimens I have ſeen give me reaſon to ſuppoſe the exiſtence of ſuch a variety.

The colour of this ſpecies is a fine deep emerald green; ſometimes, though rarely, it is found of a lighter colour. The luſtre of its broad planes, which are the only parts of the cryſtal that forcibly ſtrike the eye, give it pretty much the appearance of thoſe coloured metal plates which are known by the name of *foil*.

This ſpecies is ſtill leſs heavy than the preceding, its ſpecific gravity being only 2,548.

It is alſo leſs hard; it eaſily ſcratches gypſum, but not calcareous ſpar.

When its cryſtals are very thin, they are very tranſparent; but their tranſparency is diminiſhed when they have any degree of thickneſs.

When expoſed to fire, this ſpecies decrepitates very ſtrongly.

This arſeniate, the matrix of which is generally quartzous, is occaſionally found mixed with ſome other arſeniates of copper, and particularly with the acute octaedron in the capillary or fibrous ſtate. (Spec. 3. Var. 1 and 2.) But the ore moſt commonly found with it is the red copper ore, which is frequently very abundant.

I have never obſerved in this ſpecies any appearance of decompoſition.

Third Species.—*Arſeniate of Copper in the Form of an acute Octaedron.*

The moſt ſimple form in which this third ſpecies is found, is likewiſe an octaedron; but this octaedron, inſtead of being obtuſe, like that of the firſt ſpecies, is ſlightly acute. It has, like that, in each of its pyramids, two oppoſite planes more inclined

inclined than the other two. The more inclined planes meet at the apex in an angle of 84°; and at the bafe, in one of 96°: the other two meet at the apex in an angle of 66°; and at the bafe, in one of 112°. (Fig. 4.)

In this octaedron it fometimes happens, that the planes which compofe its pyramids tend to one and the fame point, in order to form the apex; but it is much more common to find it extended in a line parallel to the lefs inclined planes of the pyramid. (Fig. 5.) The cryftal is ftill more frequently found in the form of a long tetraedral rhomboidal prifm of 84° and 96°, terminated by a diedral apex, with ifofceles triangular planes, which are placed on the angles of 84°, and meet in an angle of of 112°. (Fig. 6.)

Moft commonly, both in the perfect and the lengthened octaedron, the angles of 96° are replaced by a plane, which is equally inclined on the adjacent fides, (fig. 7.) and is frequently very broad: (fig. 8.) then the tetraedral prifm of 84° and 96°, is changed into a flat hexaedral prifm, having two angles of 84°, and the other four of 138°. I never faw the angles of 84° replaced.

The average fpecific gravity of this arfeniate of copper, taken on five pure pieces, was 4,280.

It is fufficiently hard to fcratch fluor fpar, but is not hard enough to fcratch glafs.

Its ufual colour is a brown, or bottle green, fo dark that the cryftals appear of a blackifh colour, when they are not oppofed to the light: fometimes, but very feldom, in the regular cryftals, which happen to be rather thicker, this colour is a clearer green; in other fpecimens the cryftals have a yellowifh caft, and the furface then often reflects the light of a golden tint.

The tranfparency of this fpecies is generally pretty great.

It is not always cryftallized in a determinate form, but is an abfolute Proteus, both with refpect to the different forms in which it appears, and the various colours it exhibits. I have obferved the five following varieties of it:

Variety 1. *Capillary, of a determinate Form.*

In this variety, the cryftals are extremely flender, yet preferve their form, which is that of a very lengthened octaedron. The fmall flender cryftals often form themfelves into a confufed group; fometimes, however, they form fmall mamillæ, by the divergence of a number of them from a common centre. Their colour is either a fine grafs green, or a yellowifh green, or a golden yellow; and they have generally a beautiful tranfparency.

Variety 2. *Capillary, of an indeterminate Form.*

In this variety, the very thin needle-like cryftals are not terminated by the diedral apex of 112°, reprefenting two planes of the octaedron, but gradually become fmaller, and terminate in a very fharp pyramid. This variety has the fame colours as the preceding; and its very flender cryftals are grouped in the fame manner as in that.

Subftances in a cryftallized ftate, in paffing from a determinate form to an indeterminate or fibrous one, frequently affume an intermediate form, in which the cryftal infenfibly terminates in a very acute pyramid.

Variety 3. *In Cryftals perfectly regular for a Part of their Length, and fibrous at their Extremity.*

In this variety, the cryftals are perfect during a part of their length; but their fubftance infenfibly divides as it approaches the extremity, which very often is in fact nothing but a clufter of extremely delicate fibres, the colour of which always appears lighter than that of the folid part of the cryftal.

Variety 4. *Amianthiform.*

This variety is compofed of fibres as delicate as thofe of amianthus, of the flexibility of which they frequently poffefs a certain degree. Thefe fibres are either parallel or divergent from one common centre, in which cafe they nearly refemble a hair pencil. Their colour varies confiderably: I have feen them of different fhades of green, from a grafs green to a dark brown green, of a golden brown, of a ftraw colour, of a golden yellow, of a greenifh blue colour; and even perfectly white, having frequently the luftre of fatin.

The fibres are fometimes fo delicate, fo fhort, and fo confufedly grouped together, that the whole appears like a dufty cottony mafs, the true nature of which is difcoverable only by the lens. At other times, this variety appears in fmall thin laminæ, rather flexible; fometimes fcarcely perceptible to the naked eye, fometimes tolerably large, and perfectly like *amianthus papyraceus*. I have feen the laft-mentioned form of this variety, of a light green colour, and alfo of a very delicate white.

Variety 5. *Hematitiform.*

This variety is in layers, either flat or mamillated; and is of a fibrous texture; but is rendered compact by the clofe manner in which the fibres are united to each other, in the fame way as is obferved in many martial hematites, and more particularly in that kind of tin ore which is known by the

the name of *wood tin*, to which ſome pieces of this arſeniate of copper have a very great reſemblance. Yet it ſometimes happens, as in many aggregate pyrites of a globular form, that the ſurface of the ſmall mamillæ is covered with little rough points: theſe are the diedral apices, which terminate the little cryſtals ſuppoſed to contribute to their formation.

This hematitic variety is found with the ſame diverſity of colours as the preceding, or amianthiform variety.

Fourth Species.—*Arſeniate of Copper in the Form of a triedral Priſm.*

The primitive form of this ſpecies is a triedral priſm, the baſes of which are equilateral triangles, (fig. 9.): this priſm is often conſiderably lengthened in a direction parallel to one of its baſes. (Fig. 10.) This form is one of the moſt rare in cryſtallography. The cryſtals have all their ſides ſmooth and brilliant; yet there are obſervable in ſome of them, when examined with a magnifying glaſs, tranſverſe ſtriæ on the ſides of the priſm, all of which are parallel to the edges of the baſes. It is therefore chiefly on the planes of the baſes that the cryſtalline laminæ appear laid upon one another, to produce either the increaſe or the modification of the primitive cryſtal.

As the cryſtals of this ſpecies are ſeldom ſufficiently detached to be eaſily perceived, and indeed are very frequently ſo ſmall as to eſcape the obſervation of the naked eye, I think it neceſſary to deſcribe here all the various forms in which I have ſeen them, with the progreſs obſerved in their paſſage from one form to the other, however ſmall the difference between theſe forms may appear. Such a deſcription will lead to a better knowledge, not only of the primitive cryſtal itſelf, but alſo of thoſe forms of it which ſeem to be the moſt diſtant from its original one.

Very frequently the triedral priſm paſſes to a tetraedral modification by the ſimple replacing of one of its edges by a plane equally inclined on the adjacent ones. This plane is either very narrow, (fig. 11.) or of a more conſiderable width. (Fig. 12.) Sometimes the width of the plane is ſuch, that it reduces the primitive adjacent planes to extreme narrowneſs. (Fig. 13.) In this laſt caſe, the cryſtal appears under the form of a rectangular plate or lamina, having two of its narrow oppoſite ſides or planes inclined, in one and the ſame direction, on one of the two broad planes. It ſometimes happens, in this variety, that the two broad oppoſite planes approach more or leſs to a ſquare form. (Fig. 14.) I have alſo ſeen ſome cryſtals in which the two other edges of the priſm

prism seemed to have likewise very slight secondary planes; but, when that happens, they are always very narrow, especially when compared with the secondary plane of the third edge. This modification, in its various forms, is the most common one of this species.

Sometimes one of the solid angles of the triedral prism is replaced, on one side only, by a plane that is much inclined to the edge of the prism on which it is situated; but the crystals are always too small to admit of being measured with precision. (Fig. 15.) This plane, assuming a more considerable extent, replaces the same edge of the prism by another plane, much broader at one extremity than the other, as is shown by the lines of large dots in the same figure. Sometimes it has a very considerable extent, as is represented in fig. 16. It then reduces one of the bases of the triedral prism to the form of a very narrow trapezium, while that of the opposite base remains very broad. By a still more considerable increase, the plane of this base totally disappears, and the crystal is terminated at that extremity by a ridge. (Fig. 17.) In this variety, the crystal is often seen placed upon one of its scalene triangular sides, and then presents the other at its upper extremity; an appearance which is apt to puzzle the observer, particularly when he perceives, among the triangular sides which most generally terminate the crystals, nothing but equilateral triangles. This modification, in all its forms, is much less common than the preceding one; it is, however, occasionally met with.

[To be continued.]

II. *On preserving Fresh Water sweet during long Voyages.* By SAMUEL BENTHAM, *Esq.**

THE Society for the Encouragement of Arts, &c. having thought proper to offer a premium in order to ascertain, for the use of the public, the best mode of preserving fresh water sweet at sea, I request you to lay before the Society an account of the method which I have employed for this purpose on board two ships, and which has been attended with all the success that can be reasonably expected.

The mode in which I conceived fresh water might be preserved sweet, was merely by keeping it in vessels of which the interior lining at least should be of such a substance as

* From the *Transactions of the Society of Arts, &c. Adelphi, London, for* 1801.—The Society awarded their gold medal to Mr. Bentham for this communication.

should

should not be acted upon by the water, so as to become a cause of contamination. Accordingly, on board the two ships here alluded to, the greater part of the water was kept, not in casks, but in cases or tanks, which, though they were made of wood, on account of strength, were lined with metallic plates, of the kind manufactured by Mr. Charles Wyatt, of Bridge-street, under the denomination of tinned copper-sheets; and the junctures of the plates or sheets were soldered together, so that the tightness of the cases depended entirely on the lining, the water having no where access to the wood. The shape of these cases was adapted to that of the hold of the ship, some of them being made to fit close under the platform, by which means the quantity of water stowed was considerably greater than could have been stowed, in the same space, by means of casks; and thereby the stowage room on board ship was very much increased.

The quantity of water kept in this manner on board each ship was about forty tons, divided into sixteen tanks; and there was likewise on board each of the ships about thirty tons stowed in casks as usual.

As the stowing the water in tanks was considered as an experiment, the water in the casks was used in preference; that in the tanks being reserved for occasions of necessity, excepting that a small quantity of it was used occasionally for the purpose of ascertaining its purity, or when the water in the casks was deemed, when compared with that in the tanks, too bad for use.

The water in thirteen of the tanks on board one ship, and in all the tanks on board the other, was always as sweet as when first taken from the source; but in the other three of the tanks, on board one ship, the water was found to be more or less tainted as in the casks. This difference, however, is easily accounted for, by supposing that the water of these tanks was contaminated before it was put into them; for in fact the whole of the water was brought on board in casks for the purpose of filling the tanks, and no particular care was taken to taste the water at the time of taking it on board.

After the water, kept in this manner, had remained on board a length of time which was deemed sufficient for experiment, it was used out, and the tanks were replenished as occasion required: but in some of the tanks, on board one ship at least, the original water had remained three years and a half, as appears by the certificates herewith inclosed. About twenty-five gallons of the water, which had remained this length of time in the ship, are sent to the Society, in two vessels

ſels made of the ſame ſort of tinned copper with which the tanks were lined. I am, Sir,

Mr. Taylor.

Your obedient ſervant,
SAMUEL BENTHAM.

A certificate from captain William Bolton, commander of the ſaid veſſel, dated Sheerneſs, 28th of June 1800, accompanied this letter, ſtating, that the water delivered to the Society was taken from a tank holding about ſeven hundred gallons, and which his predeceſſor, captain Portlock, had informed him had been poured into this tank in December 1796, except about thirty gallons added in 1798, and had remained good during the whole time.

The ſignatures to the above accounts were certified on the 28th of June 1800, by the Rev. C. Thee, miniſter of Sheerneſs.

In a letter dated January 27, general Bentham alſo ſtates, that the water which had been preſerved ſweet on board his majeſty's ſloops Arrow and Dart, and of which he had ſent ſpecimens to the Society, was taken from the well at the king's brewhouſe at Weevil, from whence ſhips of war lying at or near Portſmouth are uſually ſupplied with water for their ſea ſtore, as well as for preſent uſe.

III. *Deſcription of a Machine for raiſing Water. By Mr.* H. SARJEANT, *of Whitehaven, in Cumberland* *.

SIR,

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

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

To this end a dam was made at a ſhort diſtance above, ſo as to cauſe a fall of about four feet; and the water was brought by a wooden trough, into which was inſerted a piece of two-inch leaden pipe, a part of which is ſeen at A. (fig. 1. Plate II.)

* From the *Tranſactions of the Society of Arts, &c. Adelphi, London, for* 1801.—The Society awarded their ſilver medal to Mr. Sarjeant.

of

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

D, is an iron cylinder attached to the pump rod, which paſſes through it. It is filled with lead, and weighs about 240 lbs. This is the power which works the pump, and forces the water through 420 feet of inch pipe from the pump up to the houſe.

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

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

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

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

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

I am, Sir, your humble ſervant,

Warwick Court, Holborn. H. SARJEANT.

Mr. Charles Taylor.

In another letter, dated Whitehaven, April 28, 1801, Mr. Sarjeant further obſerves, that the pump requires about eighteen gallons of water in the bucket to raiſe the counterweight, and make a freſh ſtroke in the pump; that it makes three

three ſtrokes in a minute, and gives about a half gallon into the ciſtern at each ſtroke. He adds, "I ſpeak of what it did in the dryeſt part of laſt ſummer; when it ſupplied a large family, together with work-people, &c. with water for all purpoſes, in a ſituation where none was to be had before, except ſome bad water from a common pump, which has been ſince removed. But the above ſupply being more than ſufficient, the machine is occaſionally ſtopped to prevent wear, which is done by merely caſting off the ſtring of the bucket valve."

P. S. I have juſt been informed that a drawing of the engine, which I had communicated to a perſon in this neighbourhood, was ſent to a colliery near Swanſea ſome months ago, and that it has already been applied to uſe there; it is not, however, ſufficiently powerful for the coal-works in this neighbourhood.

The following Certificate accompanied theſe Letters.

I do hereby certify, that the water engine, conſtructed near my houſe under the direction of Mr. Sarjeant, has been eight months in uſe, and fully anſwers the purpoſe intended.

EDMUND L. IRTON.

Irton Hall, March 18, 1801.

I do alſo certify the above to be true.

ROBERT WILKINSON,
One of his majeſty's juſtices of the peace
for the county of Cumberland.

March 13, 1801.

IV. *Obſervations on the* Ipomæa Hiſpida, *and ſome other Plants of the Family of the* Convolvulus. *By* FELIX FONTANA*.

I. THE *ipomæa hiſpida*, and other plants of the *convolvulus* kind, have the property, as they grow up, of twiſting themſelves around bodies in the neighbourhood with which they come in contact. They exerciſe this ſingular property on every kind of body, whether vegetable or mineral, great or ſmall, even when they are as ſlender as a thread.

II. The ſpirals formed by theſe plants are looſe, and at a diſtance from each other, when the diameter of the body which they embrace is of a conſiderable ſize. If it decreaſes they become cloſer, and approach each other more, in ſuch

* From *Mémoires de la Société Médicale d'Emulation. Quatrième année.* Paris 1801.

a manner

a manner that one is ſometimes tempted to believe that they are in contact. I have preſented to theſe plants metals, ſtones, and the branches of trees, ſometimes green and ſometimes dry, the figures of which were infinitely varied, and always obſerved that the ſpirals they form adhere, by immediate contact, to the whole length of theſe bodies. I tried to change the direction of theſe ſpirals, to make them aſcend or deſcend from left to right, if they inclined before from right to left; and they always yielded to theſe changes with facility in a very little time.

III. When they are abandoned to themſelves, and find no bodies near them to which they can attach themſelves ſo as to climb around them, they creep along the ground, fall back on themſelves, become interwoven in a thouſand different ways, and force themſelves into the ground: they extend to a great diſtance on every ſide, and form a net of ſpirals cloſely united.

IV. After having long ſtudied the character of theſe plants, I muſt confeſs that I am acquainted with no mechanical principle by which the movements and ſingular properties which they exhibit can be explained. This kind of inſtinct, which makes them ſearch for neighbouring bodies to aſſiſt them to riſe; thoſe ſpirals, which they conſtantly form as they grow up; thoſe angles and thoſe folds, which are found always proportioned to the figure of the bodies they meet with; the facility with which their ſtems, even when exceedingly hard and robuſt, can fall back upon themſelves, and form angles ſo much the ſmaller as the bodies to which they attach themſelves are ſlenderer: all theſe phænomena, abſolutely contrary to the rectilineal progreſs affected by the fibres of plants, as well as by their veſſels and the liquors they contain, make me ſuſpect, in thoſe which we examine, a new principle of ſenſation and life.

V. It muſt not be believed that the *ipomæa hiſpida* thus twiſts round upon itſelf merely becauſe it is formed of ſpiral fibres, and that all its movements are only a neceſſary effect of its interior organization. Such an opinion is entirely void of foundation. It would indeed be fruitleſs labour to ſearch for theſe pretended ſpiral fibres by the help of the microſcope, for that inſtrument exhibits only rectilineal fibres and canals. Beſides, I have always found by experience that the direction of theſe plants may be changed at pleaſure; that, if the ſpirals they deſcribe around any body are formed on the right, they may be immediately turned to the left, and *vice verſa*. Their movements, therefore, can be aſcribed neither to any peculiar mechaniſm, nor to local circumſtances; and they ſeem equally capable of moving on all ſides.

VI. Ivy is the only plant, and perhaps the only body, with which they do not ſympathize: they are not fond of uniting with it, or twiſting themſelves around it. If any external power unites them to that plant, and forces them to deſcribe around it a certain number of circumvolutions, they endeavour to avoid it, to diſembarraſs themſelves from it, and they remove from it as ſoon as circumſtances will permit.

VII. If a branch of the plants in queſtion be ſuſpended in the air, it continues to deſcend in a perpendicular direction, unleſs it be very ſlender. In that caſe, it abandons the ſtraight line which it followed, and bends itſelf contrary to the laws of gravitation, raiſes itſelf up, and returns to its own ſtem, that it may deſcribe around it the uſual ſpirals.

VIII. Sometimes the extremity of theſe plants, after having twiſted itſelf ſeveral times around different neighbouring bodies, abandons the ſpirals it had begun to form, and recedes ſeveral inches; eſpecially if deranged in its progreſs, either on purpoſe or by the effect of ſome local circumſtances: but, by a very wonderful mechaniſm, it almoſt always falls back towards thoſe bodies which it embraced, ſearches for them as if by natural inſtinct, and again twiſts itſelf around them, following the ſpirals it deſcribed before. Theſe ſpirals are ſituated in a direction from the top to the bottom, or from the bottom to the top, according to the poſition of the point which terminates them. Indifferent to every kind of direction, they conſtantly follow that given to them, without obeying either the laws of gravity, or thoſe by which other plants ſeem to be regulated.

IX. All theſe phænomena cannot be explained by ſimple mechaniſm: they ſeem to be the effect of a principle of ſenſation and life; a principle which I diſcovered ſeveral years ago in the *tremellæ* of Dillon, and ſome other ſmaller plants, as yet little known, of the family of *liſerons*, and of which I have demonſtrated the exiſtence by evident proofs, as may be ſeen in my different works. Theſe proofs have never yet been conteſted by any obſerver or philoſopher.

X. Life and ſenſation are found to be obſcured among certain animals which have very little analogy with man and with common animals, and to become the leſs apparent as their organization differs from ours. Sometimes, even, it is difficult to believe that they enjoy real life; and this diſcovery can be made only by the laborious reſearches of the philoſophic obſerver. Several of theſe animals are deſtitute of the

the organs of hearing, ſight, taſte, and ſmell. There are whole families which have not the organ of voice, that powerful mean for diſcovering ſenſation; and the ſenſe of touch itſelf is ſometimes ſo obſcure by the abſence of thoſe ſenſible movements and violent agitations manifeſted by other animals in conſequence of any ſtroke, that it cannot be known whether it is produced by ſenſation, or by any other principle unknown, and merely mechanical. Even the organs of digeſtion, which in other animals are internal, can be ſupplied in the family of the polypes by the external organs, and even by the ſkin itſelf. It is well known that theſe animals form a kind of elongated bag, if they are turned, like the finger of a glove; the ſtomach becomes ſkin, and performs the functions of it, while the ſkin becomes ſtomach, and digeſtion continues.

XI. It was formerly thought that, to aſcertain whether an organized being belonged to the animal or vegetable kingdom, it was ſufficient to divide it into ſeveral parts; but at preſent we are acquainted with a multitude of animals which may be divided without deſtroying them, and which even multiply, like ſome plants, in proportion as they are divided.

XII. The little analogy, therefore, which exiſts between our organs and thoſe of plants, and even the abſolute privation of certain organs, will not permit us to obtain certain and evident proofs of the life and ſenſation of vegetables. For, if we ſuppoſe that all plants are endowed with ſenſation, and that it is even much more exquiſite than our own, how can we aſcertain whether their mode of living and feeling be different from ours, if their organs have no relation to our organs, and if they are entirely deprived of thoſe which could act upon ours? But though we cannot prove directly, and in an evident manner, that plants feel, it by no means thence follows that plants are incapable of feeling as well as animals, and even in a much more exquiſite manner. We cannot without temerity, and without expoſing ourſelves to the danger of falling into error, refuſe to nature a power which ſhe exerciſes, perhaps, over all organized beings.

XIII. Motion, perhaps, is the moſt certain character by which animals are diſtinguiſhed, and that which rarely abandons them: without it we ſhould be obliged to conſider as inanimate an infinite number of beings in whom life is manifeſted in the moſt evident manner by the rapid movements of their different parts; movements which can in no manner be aſcribed to mere mechaniſm, but which are neceſſarily produced by ſenſation; and yet theſe movements ſometimes become ſo ſlow, and ſo obſcure, that they are capable of exhausting the patience, the sagacity, and the talents of the most indefatigable philosophic observer.

XIV. But if the vital movements are obscure in a very great number of animals, they become almost imperceptible in plants. I do not here speak of those movements, so well known, of the sensitive plant, the *hedysarum gyrans*, and the stamina of several others, which, as soon as touched, move and contract themselves. It would be difficult to prove that these movements are really vital; and that they arise from sensation: they appear to me to be purely mechanical, though we may be ignorant of the real mechanism which produces them; and they have a great analogy to those which depend on the elasticity or spring of certain machines which, when once wound up, unbend themselves, and continue to move as long as the force and principle by which they were put in action exists.

XV. The indefatigable botanist Dillon studied with great care the *small tremella*, but he did not discover those movements which it constantly manifests to the eye of an attentive philosophic observer. Adanson went still further, but without observing in it any thing that depends on a real principle of life and sensation. Having become the object of my researches, it exhibited to me a great number of movements still unknown, and of the greatest importance. After having carefully examined, and, as I may say, analysed them, it appeared to me, as may be seen in my works, that they might conduct to a real principle of life altogether different from mere mechanism. But I must confess there are only two plants the life and sensation of which I was able to demonstrate in an evident manner, and so as to leave no doubt; and these two plants are very small, and even microscopic. They have little if any analogy with other plants; and though they exhibit all the properties by which vegetables are characterized, some even may class them in the animal kingdom. But whatever may be the case, and however important the question may appear, it cannot destroy truths founded on facts and observations. It is much to be wished, for the progress of science, that the life and sensation of common plants could be proved, and that it were possible to show that these qualities, so wonderful, and at the same time so obscure, do not exclusively belong to animals, but that they are extended to all organized beings.

XVI. I opened the entrance into this laborious and difficult path by some observations, which were indeed very imperfect, on the *convolvuli*; a kind of plants which are very large, and well known to every body. Animated solely by a desire a deſire to be uſeful, obſervers abler than myſelf, and more favoured by circumſtances, will no doubt finiſh what I have only ſketched out.

XVII. To encourage them, and ſhow them the probability of the moſt brilliant ſucceſs, let me be permitted to ſay a few words reſpecting two properties equally common to animals and vegetables, from which life and ſenſation ſeem to flow—I mean generation and reſpiration, ſo neceſſary to theſe two kinds of organized beings.

XVIII. Air is an element ſo neceſſary to animal life, that it becomes weakened, and is at length extinguiſhed, when entirely deprived of it; and from my own obſervations I am induced to believe that no animal can live without air, and without a quantity, more or leſs conſiderable, of oxygen; though we are but imperfectly acquainted with the manner in which it acts, how it is reſpired by the different animals, and by what means they are able to ſeparate it from the different ſubſtances with which it is united, and which can more or leſs alter its purity.

XIX. What is certain is, that ſmall eels, ſuſceptible of being recalled to life, die if deprived of air, or if care be not taken to renew it; and it would be loſt labour to attempt to reanimate them, if the water in which they are immerſed is not in contact with reſpirable air. They live only a very ſhort time in every kind of water deprived of that air. They die in hydrogen and nitrogen; though theſe gaſes are not prejudicial to them of themſelves; and do not occaſion to them ſudden death, like the greater part of the other gaſes, without even excepting carbonic acid gas. It appears, then, that oxygen contains the vital principle; and the principle it has of maintaining and preſerving life, induces us to believe that if it does not communicate it directly, it can at leaſt awaken it, and put it in action. The ſmall animals above mentioned are an irrefragable proof of this aſſertion. They remain dead as long as they are deprived of oxygen, but its preſence immediately reſtores them to life and motion. It may therefore be ſaid that it contains the direct principle of life, which it infuſes into them; or that their vital organs, in order to be reanimated, ſtood in need only of this aliment, of which they were deprived.

XX. The abſolute need which plants have of air in order to vegetate and be preſerved, renders it highly probable that it ſerves for the ſame purpoſes among animals; and that it ought to be conſidered in theſe two claſſes of beings as a principle from which life flows, in the ſame manner as an effect from its cauſe. Beſides, vegetables are organized as

 well

well as animals; they increaſe alſo by nutrition; and though this function in both follows laws different in their details and application, we may however aſſert, that plants have in general more reſemblance to our ſmall eels, than the latter have to common animals. A plant, indeed, may be dried to a certain degree, ſome even may be reduced to a ſtate of perfect deſiccation, without loſing the property of vegetating as before; whereas the ſlighteſt deſiccation of animals is, in general, ſufficient to deprive them of life for ever. The diverſity of theſe phænomena is, no doubt, owing to the ſimplicity of the organs of plants and of our microſcopic eels, and, above all, to the facility with which the latter can be dried, which prevents the corruption and diſſolution of their organs; while this diſſolution is unavoidable among common animals which have a very complex organization, and whoſe humours naturally tend to corruption. This, however, in no manner deſtroys the great analogy which exiſts between theſe two ſpecies of beings; an analogy founded in particular on the circumſtance of both having equal need of oxygen for their exiſtence.

XXI. Generation, that myſtery, ſo obſcure in its principles, which belongs no leſs to plants than to animals, forms the ſecond kind of analogy between the two claſſes of organized beings, and induces us to believe, that where the organs are the ſame, and have the ſame uſes, we ought to find alſo an identity of wants. Vegetables and animals have diſtinct genital parts of ſeparate ſexes, male and female organs in different individuals, and exhibit efforts in theſe different ſexual parts to conſummate the act of fecundation. Since, the ſexual organs, and the manner of reproducing themſelves, are common to the two grand families of organized beings, do they differ in ſenſation? And this difference, ſo improbable between beings ſo like in other reſpects, and the only one of the kind, perhaps, that can be found in nature, on what is it founded? Is it on our organs and mode of ſenſation not being in harmony or uniſon with that of plants? Was there ever any reaſon weaker, or leſs philoſophical?

XXII. The movements of the male organs of plants which perpetuate their ſpecies, and the manner in which they prepare for that grand work, ſeem to me to deſerve particular conſideration, and further reſearch. I propoſe to undertake this labour, or rather to continue it; for I have already been employed on this object under various circumſtances, and I am of opinion that the ſum of my obſervations is ſufficiently deciſive to enable me to aſſert, with ſome foundation, that the movements by which different beings reproduce themſelves, are not the reſult of mere mechaniſm, but that the grand act of generation is executed by a principle of ſenſation which regulates and directs it: it, however, appears that this ſenſation abandons plants as ſoon as the end of nature is accompliſhed. Thus we ſee different kinds of animals die when they have enſured the reproduction of their ſpecies.

XXIII. It is then highly probable that plants as well as animals enjoy a principle of life and ſenſation, whatever be the nature of that principle, and notwithſtanding the difference it exhibits in theſe two families of organized beings; a difference which is neceſſary, ſince their ſtructure is not the ſame. The abſolute need of oxygen, which is equally felt by both; the ſimilitude of their organs; the análogous movements in the uſe of theſe organs; and, in the laſt place, the inconvenience there would be to refuſe life and ſenſation to other plants, while we are obliged to acknowledge it in the two microſcopic plants which I examined, beſides thoſe alluded to in this memoir; are all proofs on which the life and ſenſibility of plants are founded. It muſt, nevertheleſs, be confeſſed, that the little analogy which there is between their organs and ours, will not permit us to eſtabliſh theſe proofs on facts, and to give them the evidence of demonſtration. It appears, however, very ſurpriſing that nature grants to vegetables a force and an energy which it ſeems to have refuſed to animals. I allude to the property which the former poſſeſs, not only of decompoſing every kind of bodies, but of collecting the elements to form new compounds, and thus to produce foſſils and even metals. Animals are deſtitute of this property, or at leaſt enjoy it only in a weak degree.

XXIV. Life and ſenſibility, however, in different animals exhibit degrees ſo various and different, become weaker, and decreaſe by gradations ſo inſenſible, that the philoſophic obſerver can ſcarcely fix their limits, and aſcertain where they begin and where they end. It even appears that ſenſibility, to judge at leaſt by its effects, is not altogether of the ſame nature in the different kinds of animals. The numerous obſervations and experiments, which I have made for years, on the ſenſibility of animals, particularly the cold-blooded, and on the return of ſome of them from a ſtate of death to that of life, leave no doubt that there are a great many of them in which the ſevereſt wounds occaſion neither ſenſation nor pain. For example, if the head of a fly* be

* The naturaliſt and real philoſopher will not be ſurpriſed to ſee the author fix his attention on a fly, an inſect conſidered by the vulgar as contemptible. The ſmalleſt inſect enjoys life and ſenſation as well as the largeſt

be cut off, it continues to fly about, to walk, to leap, and to climb as before; its regular and uniform motions, ſometimes ſlow and tranquil, ſometimes quick and animated, exhibit nothing convulſive, nothing that reſembles thoſe violent agitations which accompany pain. In a word, the number and rapidity of theſe movements, as well as the time that theſe flies can live, are ſubject to no fixed rule.

Let us ſtop for a moment to conſider the movement of their legs; they are equally various and wonderful as thoſe by which they can move the whole of their body.

XXV. To appreciate properly the different movements performed by an animal in a ſtate of health or of diſeaſe, it is neceſſary to have an exact idea of the circumſtances in which it is placed, of the motives and impulſes which in general induce it to move; in a word, we muſt be acquainted with its nature and character. This being premiſed, it is eaſy to obſerve, that of the nine kinds of movement to which thoſe executed by flies in a ſtate of health may be reduced, eight ſtill ſubſiſt even after they have been deprived of the head. Scarcely has a fly been decapitated when it is ſeen to move its hind legs for a long time, and at different periods to croſs them, either ſtanding up or flying, to rub them along its body, to hook the one into the other in the form of a wheel, and to agitate them in a thouſand ways with wonderful rapidity and regularity. All theſe movements, which conſtitute the firſt kind here treated of, are abſolutely ſimilar to thoſe performed by a fly, when, poſſeſſing perfect health, and being haraſſed by no fear or want, it appears to move only for its own pleaſure. Theſe movements, then, ſeem to be produced in theſe two ſtates, ſo oppoſite, by the ſame principle, that is, ſenſation and volition.

XXVI. The ſecond kind of movement reſults from their hind legs, which they raiſe up towards the interior edge of their wings; ſo that by theſe means they are elevated, and placed in different directions. The object of theſe movements, exceedingly varied, though conſtant, ſeems to be, to rub, beat, and poliſh their wings. Their hind legs can execute a third kind of movement, no leſs varied, when, conveying them above their wings, which they keep extended, they rub them, beat them, and poliſh them, as in the preceding caſe. Their middle legs alſo are often obſerved to be twiſted around thoſe behind, and to rub each other mutually; which forms two new kinds of motion different from thoſe we have examined.

largeſt animals: theſe properties belong as much to microſcopic and infuſion animals, as to the elephant and whale. Nothing is contemptible in the eyes of the philoſopher; every thing in nature is great and wonderful.

XXVII.

XXVII. They can alfo move their fore legs, and twift them in the form of a whirl, which forms a fixth kind of movement, fo various and multiplied that the eye can fcarcely follow them. The feventh and eighth kinds refult from the union and interlacement of their middle legs with thefe before. What feems furprifing is, that thefe eight kinds of movement are perfectly fimilar to thofe voluntarily performed by the animal, and as it were for pleafure, before its head is cut off, and when it enjoys perfect health and tranquillity. In the laft place, flies perform a ninth kind of movement by conveying their legs and thighs around their head, which they rub and polifh in various directions. It is evident that this movement cannot take place after they have been decapitated.

XXVIII. Flies certainly would not exhibit thefe phænomena if they experienced as acute pain as warm-blooded animals when fubjected to decapitation, which is conftantly followed by fpeedy death. Will it be faid that thefe different movements fuppofe, on their part, neither pleafure nor recreation, but rather a need which they experience of rubbing and polifhing their legs, their wings, and different parts of their bodies? But if this be the cafe, we muft be forced to confefs that decapitation does not prevent them from being fenfible to fmall privations and old habits, which are by no means neceffary to their exiftence; a manifeft proof that it occafions no pain to them, or at leaft that it is exceedingly flight; otherwife they would not attend to things of fo little importance.

XXIX. Cold-blooded animals could fupply me with many examples of a fimilar kind; but I fhall content myfelf with mentioning only one, furnifhed by the tortoife. If the head of this animal be cut off, or, what is ftill better, if its brains be fcooped out with dexterity, it feems to be fcarcely fenfible of the operation; it continues to walk as before, it afcends and defcends, avoids thofe obftacles which it meets with, turns itfelf to different fides, ftands up on its hind paws, and performs movements of every kind; and though its paws are covered with a hard and fcaly fkin, if they are flightly touched even with a feather when it is walking, or has been a little irritated, it immediately draws them back, and remains fome time at reft: in a word, its fenfibility feems to be fo little altered, that it feels the fmalleft impreffion made on its fhell. Tortoifes live in this manner five or fix months without any change being obferved in their movements or habits: there are even certain movements which they execute with more readinefs and facility than in their ftate of integrity. Such is that

that by which they turn themſelves on their belly, and ſtand up on their hind paws. They at length die at the end of ſix months; but it appears that their death ought to be aſcribed rather to hunger than to the operation to which they have been ſubjected, ſince thoſe on whom it has not been performed die in the ſame time if deprived of nouriſhment. If the tortoiſe in this ſtate experiences real pain, were it even very ſlight, it would certainly not feel the weak impreſſions made on the ſhell which covers it; for it is well known that a very ſtrong impreſſion entirely effaces that which is very weak.

XXX. The facts I have eſtabliſhed will, no doubt, find many oppoſers. This muſt be the caſe. They are ſurpriſing; have even ſomething of the marvellous; and tend, in particular, to deſtroy errors hitherto conſidered as inconteſtable truths.

XXXI. We, however, know that ſenſibility is very weak in animals at the time of their birth; that it is gradually expanded with their organs; and, when come to its higheſt degree, it begins to decreaſe by inſenſible gradations until it is at length entirely extinguiſhed, when the animal dies. It is eaſy to obſerve theſe different degrees of ſenſibility in animals attacked by diſeaſes, or ſubjected to experiments of this kind.

XXXII. I might here add, that no naturaliſt is now ignorant that the ſmall eels which have been the object of my obſervations, and which are found in different kinds of the gall-nut, can paſs inſenſibly from the ſtate of death to that of life; die again, to be afterwards revived, if circumſtances admit; and that the number of theſe different reſurrections is ſtill indetermined. It is more than twenty-ſix years ſince I publiſhed, in different journals, the obſervations which confirm theſe truths; and I have demonſtrated, in the moſt evident manner, that theſe ſmall animals may be revived at pleaſure; that they enjoy this ſingular faculty as long as they are hermaphrodites, but that at the moment when they aſſume a ſex they return to the common law, and, when once dead, can no longer be revived. I have even found the means of giving them a ſex at pleaſure, and theſe means are thoſe employed by nature for perpetuating their ſpecies*.

V. *An*

* Theſe truths and ſeveral others are explained and illuſtrated in a work, which I intend to publiſh, on the life, death, and ſenſibility of animals. This work, ſeveral fragments of which have been communicated, for more than twenty years paſt, to different learned men in Europe, will be enriched with above 200 engravings, and will form two large volumes

V. *An Examination of* C. Clouet's *new Process for making Cast Steel from Bar Iron by means of the Decomposition of Carbonic Acid.* *By* David Mushet, *Esq. of the Calder Iron Works* *.

In the prefent extenfive ftate of the iron trade in Britain, which exceeds in that manufacture alone the collective exertions of all Europe, every difcovery which may lead to elucidate the principles of this art, and place them upon a permanent foundation, or which may remove thofe local difadvantages, the inevitable confequence of the ufe of pit-coal, become highly interefting. To counteract the inferior effects produced by this otherwife invaluable combuftible in the converfion of caft into malleable iron, and to produce Britifh bar iron equal in quality for all purpofes, fteel-making included, to the foreign fabrics, would be an attainment of much national importance. Multiplied beyond precedent have been the modes devifed within the laft fifty years; but as they have been merely modifications of each other, with no effential change of principle in the operation, the refults have been confequent to the unvaried nature of the means. Every new idea, therefore, ought to be feized with eagernefs, accurately examined, and put to the teft of experiment. Ultimately great improvement may be drawn from a hint loofely thrown out, or even derived from experiments, which, though at firft fight engaging, may neither carry with them conviction of principle, nor be productive of eventual utility. Impreffed with a full conviction of thefe truths, the procefs for converting iron into fteel, communicated by the French chemifts in the *Journal des Mines*, No. 45, appeared to me deferving of a fair examination. To this procefs I alluded in my laft communication, and to it I fhall now particularly attend.

The report of citizens Guyton and D'Arcet to the French National Inftitute, announcing this difcovery, fets forth, that C. Clouet, among other inventions, had devifed a method of converting iron into fteel by means of the decompofition of the carbonic acid in lime. To prove this,

lumes in quarto. I have need only of a few moments of philofophic leifure to put the laft hand to it; but, can I hope for it after having been perfecuted with as much injuftice as barbarity?

* Communicated by the Author.—In Mr. Mufhet's paper given in our laft Number, Vol. xi. p. 269, line 16, for *ftate* of fteel, read *ftate* of fteel.

feveral

feveral experiments are detailed with a degree of minutenefs bordering upon demonftration.

The report ftates, that a $\frac{1}{32}$d part of carbon is fufficient to convert iron into fteel: a quantity of it, equal to $\frac{1}{6}$th of the weight of the iron, gives a quality of fteel more fufible but ftill malleable; but beyond that term it approaches to caft iron, and has no longer fufficient tenacity. By increafing the dofe of carbon you increafe the fufibility, and it paffes at length into the ftate of gray caft iron.

In purfuance of the proportions here laid down, we find the report prefcribes, that a mixture of carbonate of lime and argil, comprifing 2-3ds the weight of the iron, muft be employed, viz. 1-3d the weight of the iron of carbonate, and the fame proportion of argil. Thefe are to be introduced into a crucible along with fragments of iron, expofed to heat for a time fufficient to effect fufion, and that the refult will be found fteel.

That C. Clouet applied the proportion of 1-3d of carbonate of lime, from a calculation of the quantity of carbonic acid it contained, and thence the quantity of carbon difengaged by its probable decompofition, will appear very evident from an analyfis of the experiments performed by citizens Guyton, D'Arcet, and Vauquelin.

The quantity of iron operated upon in this experiment was 6914 grains. Admitting, according to Clouet, that $\frac{1}{32}$d part of carbon was fufficient to convert this quantity into fteel, then $216\frac{6}{32}$ grains of carbon are requifite for this purpofe. We find that one-half of the flux ufed amounted to 2304 grains of carbonate of lime. If we fuppofe this united with 36 *per cent.* of carbonic acid, then the refulting quantity of acid will be 829·44 grains. Granting, according to Lavoifier, that the component parts of carbonic acid are 72 of oxygen and 28 of carbon, then by the following formula we have $\frac{829{\cdot}44 \times 28}{100} = 232{\cdot}5$ grains of carbon, nearly.

	Grains.
Contained in this portion of lime - -	$232\cdot\frac{16}{32}$
Quantity of carbon neceffary to fteelify the iron at the rate of $\frac{1}{32}$d part - - -	$216\cdot\frac{6}{32}$
	$16\cdot\frac{10}{32}$

This fmall difference is eafily accounted for by fuppofing the lefs difference to have exifted as to the quantity of carbonic acid contained in the lime ufed by Clouet.

I think it probable, from the coincidence here pointed out, that the other proportions of Clouet were fixed by the fame ftandard of calculation; at leaft, the detail of experiments which

which I ſhall bring forward relative to the various parts of this theory, as formerly ſtated, will bear certain evidence that the whole had been founded upon one ſeries of experiments, the juſtneſs of which may be gathered from what follows. Grains.

Exp. I. Four pieces of malleable iron, weighing 1457 were introduced into a Stourbridge clay crucible, and the following mixture added, being the exact proportions pointed out by Clouet:

1-3d of Balgrochan raw lime, or 486 grains;

1-3d of Stourbridge clay (old pot), or 486 grains.

The pieces of iron were imbedded in, and completely covered by, the mixture of earths. The crucible was expoſed for 35 minutes to a high heat, when the mixture was ſuppoſed to be reduced. The reſult was a fine dark green glaſs, tranſparent towards the edges, covering a perfect regulus of metal. Upon cloſe examination, a few traces of imperfect cryſtallization were viſible upon the upper ſurface. The under ſurface was marked with deep pits or honey-combs, but nothing which betokened any great degree of diviſion having taken place while the metal was fluid.—This product weighed - - - - - 1437

Loſt in fuſion 20

equal to $\frac{1}{72}$d part the weight of the iron introduced. From the dark colour of the glaſs I inferred that a quantity of iron equal to the deficient weight might eaſily be diffuſed throughout the maſs.

The quality of this metallic button was minutely examined, and the following characteriſtics rigidly aſcertained:

The maſs was partially cut acroſs with a chiſel, and with difficulty broken cold over an anvil. The fracture diſplayed a conſiderable quantity of fibre; a few imperfect granulations, of no determinate form, were alſo viſible, but poſſeſſed of a ſmall degree of comparative luſtre. One-half of the button was forged at a cherry heat. It drew into a ſquare ſhape with facility; but the action of the hammer, even at this low heat, expreſſed a conſiderable number of ſparks. The ſolidity of the bar, notwithſtanding the fuſion, was far from being complete. At a high red heat, a ſlight hammering diſſipated the particles of metal like caſt iron.

One end of the bar was plunged into water at the ordinary heat for ſteel. This produced no alteration, and the bar bent ſeveral times without breaking. When again hammered, it was plunged at a bright red: the ſurface then ſhaled a little,

 and,

and, upon being touched by the file, felt perceptibly harder. The fracture of the piece thus hardened was of an open regular grain, approaching somewhat, in point of colour, to malleable iron. A fracture from the unhardened part of the bar was precisely that of bar iron made in the puddling process; a dark blueish fibre breaking short across, without tearing out in groups. But before I allowed myself to draw any conclusion from this experiment, I deemed it necessary to repeat it as follows:

	Grains.
Exp. II. The quantity of iron now operated upon was - - - - -	1331
To which were added 1-3d of the same carbonate of lime, or 443 grains; 1-3d of Stourbridge clay (old pot), 443 grains.	
This mixture was arranged as in No. 1. The crucible was introduced into the furnace, and in 30 minutes I judged the fusion entire. A metallic button, as in No. 1, was found, beneath a dark green glass, which weighed - - - -	1312
Lost in fusion	19

equal to $\frac{1}{70}$th part of the original quantity of iron introduced.

The glass obtained in this experiment, upon comparing fragments of equal thickness, was found one shade lighter than in No. 1. The upper surface of the metallic button exhibited a more perfect approach to crystallization than in No. 1. This supposes a more perfect fusion had been effected. The under surface contained one large pit, the interior surface of which was strongly marked with a streaky crystallization. The fracture was also brighter, and the grain more distinctly formed than in No. 1.

A bar forged from one-half of the button drew into a shape at a low red heat. At a white heat it stood a few blows; but, in returning through the intermediate heats, dissipated like hot sand. A fragment well hammered, and plunged into water at a low red heat, exhibited no shale, and bent easily over the anvil. When plunged at a bright red heat, a fair proportion of shale turned up. The piece then felt similarly hard, under the file, with No. 1. The hardened fracture was open, distinctly granulated, and bright. A fracture of the unhardened part of the bar was blueish, tough, without grain; nearly two shades lighter in the colour than No. 1. This circumstance, along with others formerly mentioned, was most probably derived from the superior degree of fusion which the product underwent.

The same experiment was several times repeated with nearly similar results as to loss of iron. The quality of the metal was found invariably the same. I therefore deemed myself warranted to draw the following conclusion, that with the quantities of mixture prescribed by the French chemists for converting iron into steel, I had not been able to obtain a product which, under the various tests, exhibited the properties of cast steel. But, from the result of the foregoing experiments, I did not think myself at liberty to infer, that although the usual marks of steel had not been perceptible, steel could not be formed in this way. Aware that carbonates of lime differ much in their respective quantities of carbonic acid, and proceeding upon the supposition that the carbonate used by C. Clouet might afford a greater quantity of carbonaceous matter, I resolved to make up in quantity of earth what probable deficiency might have arisen from an inferior quantity of carbonic acid.—To ascertain this point, I made the following experiment:

	Grains.
Exp. III. I used of small pieces of iron -	$1342\frac{1}{2}$
To which were added $\frac{1}{2}$ of carbonate of lime, or $671\frac{1}{4}$ grains; $\frac{1}{2}$ of Stourbridge clay pot, $671\frac{1}{4}$ grains. The quantity of carbonate here used exceeded that employed by Clouet in the proportion of 9 to 6, and ought to have given out to the iron, every thing else being alike, a portion of carbonaceous matter equal to $\frac{1}{21\frac{1}{8}}$ths the weight of the iron employed. This mixture was fused in 38 minutes, and, as usual, a metallic button found under the fused earths which weighed -	$1325\frac{1}{2}$
Loss equal to $\frac{1}{79}$th part of the weight of the iron fused	17

The glass appeared to be a minute shade lighter than No. 2. The upper surface possessed a few imperfect radii, a commencement of the usual crystallization. The under surface was similarly pitted with No. 1 and 2; and no alteration of quality could be inferred from the external character of this button. The fracture of it when broken was entirely fibrous, and tore asunder somewhat like lead. Under the hammer it cracked less than did the former two. It was next double-welded, and drawn a second time; cracking a little, and throwing off a few sparks. In hardening, and in every stage of the subsequent examination, I could detect no circumstance that would warrant the supposition of one additional atom of carbon being in combination with this iron beyond that possessed by Nos. 1 and 2. The property of its standing a higher heat, and exhibiting a tougher fracture, are circumstances rather the reverse, but which a more attenuated

ated or less perfect fusion will, I conceive, satisfactorily account for.

In order further to elucidate this subject, now pregnant with doubts as to the efficacy of the carbonic acid,

Exp. IV. was performed as follows: Grains.

Iron introduced - - - 1468

Carbonate of lime - 930 grains,

Stourbridge clay pot - 930 grains:

this proportion being of each of the mixtures 5-8ths the weight of the iron, and to C. Clouet's proportion as 6·25 to 3·33; nearly double. From this mixture, still following his calculation, should have been obtained steel, approaching to cast iron in point of saturation of carbon.

The fusion was completed in 32 minutes, and a very perfect crystallized button of metal was produced, which weighed - - - - 1434

Lost in fusion 34

equal to $\frac{1}{44}$th part of the original weight of iron introduced.

The glass now obtained was darker by a marked shade than that obtained in experiment No 1. This circumstance, and the increased quantity of glass, may in some measure account for this experiment losing more iron than any of the former. The external figure and density of this metallic button was in no way altered from the former results. It underwent the same minute examination as did buttons Nos. 1 and 2, and in no instance afforded the least perceptive difference in point of quality.

Exp. V. was performed as follows: Grains.

Iron introduced - - - 1307

Carbonate of lime - 1960 grains,

Stourbridge clay pot - 1960 grains.

This quantity exceeded that of C. Clouet's in the proportion of 3 to ·666, and of course contained nearly five times the quantity of carbonic acid. This ought to have afforded a result in gray cast iron.

The fusion was completed in 30 minutes, and a metallic button, as usual, obtained decumbent to a very ponderous mass of glass, and weighed - 1275

Lost in fusion, equal to $\frac{1}{40}$th part of the weight of the iron, 32

The glass obtained in this experiment, upon comparison, was found to be exactly the same with No. 1. The great deficiency of weight, in the proportions of 40 to 73, may be accounted

accounted for by taking into confideration the extra quantity of earths introduced, and fufed in contact with the iron. The vitrid mafs had imbibed as large a fhare of the colouring principle, from whatever fource derived, as that in the firft experiment, where not above 1-5th part the weight of the earths were ufed to a nearly fimilar portion of iron. The metallic button now produced was fubjected to the fame tefts with the former, and refults exactly fimilar in every ftage of the examination were obtained.

The refults of the foregoing experiments, I conceive, will with fafety admit of the following deductions:

That in treating malleable iron with any of the foregoing proportions, of an equal mixture of carbonate of lime and fome vitrified Stourbridge clay-pot, refults in fufion are obtained the qualities of which differ fo little from each other, that no change feems to be derived from altering the proportions of the earths prefcribed: That in no fhape nor form does iron in this ftate approach to what is commonly underftood to be fteel; nor can it, in any cafe where elafticity, edge, and polifh, are requifite, be ufed as a fubftitute for it.

It might, however, prove a curious fubject of inquiry, and in the end be productive of confiderable utility, to inveftigate the principle of alteration that takes place in this procefs. The iron lofes in weight from $\frac{1}{40}$th to $\frac{1}{70}$th part, depending moft probably, in fome meafure, upon the quantity of earths ufed. If the deficiency confifts of iron alone, there can be no reafon affigned why the refulting button of metal fhould be fo completely altered in its properties. This becomes deprived of its malleability when heated beyond a bright red, and particularly of the property of welding. It acquires additional foftnefs when cold; and in that ftate will flatten aftonifhingly, without rending, or exhibiting the fmalleft crack.

C. Clouet, in fufing iron and glafs, attributes to the latter the property of combining with the former. Were a combination of earths with iron fuppofed in the prefent cafe, this would naturally add to the weight of the product, unlefs a greater quantity of iron united to the glafs than left the glafs to unite with the iron. Again, this fuppofition involves the exertion of a double affinity, wherein a preference of predifpofing caufe cannot be attributed to the one more than to the other. It will therefore remain for experiment to decide whether the alteration in quality of the iron is occafioned by an affumption or a lofs of principle.

Exp. VI. Before I finifhed this part of the examination of

 C. Clouet's

C. Clouet's proceſs, I deemed it of importance to make the experiment that follows:

	Grains.
Iron introduced - - -	1399
Carbonate of lime, the ſame as formerly uſed, 1399 grs. From this mixture, by fuſion, was obtained a very denſe maſs of iron, poſſeſſed of a ſmooth radiated ſurface, different in every external feature from the reguli obtained in the former experiments. It weighed	1317
Loſt in fuſion, equal to $\frac{1}{17}$th part the original weight,	82

This extra loſs of metal, however great, appeared ſtill more ſo upon breaking the button, and detecting in its interior a conſiderable portion of incorporated glaſs. From the apparent quantity found in this ſtate it may be fairly preſumed that $\frac{1}{15}$th part the weight of the iron employed had diſappeared in the fuſion. The glaſs, in point of colour, was green, approaching to black; its fracture, in point of luſtre, reſembled the poliſh of fine black marble.

Upon ſubjecting part of the metal now obtained to the hammer, it was found ſoft in the extreme when cold, but in no ſtate would ſtand the effect of ſlight hammering beyond a low red heat. A piece beat out, heated pretty high, and plunged into water, turned up a rich and uniform ſhale, ſo as to make its ſurface reſemble tin; but the ſame, when attempted to be broken, bent ſeven times before it parted. The grain then, far from reſembling the cloſe uniform grain of caſt ſteel, preſented a ſilky fibrous appearance. In this reſpect it differed materially from the reſults obtained in the five former experiments. A bar, $\frac{5}{16}$ths ſquare, plunged hot into water, and ſuffered to cool ſlowly, had aſſumed a partially cryſtallized fracture, reſembling ſome varieties of American red-ſhort bar iron. In ſeeking for a ſolution of the phænomena which appeared in this experiment beyond thoſe in the former, it may be ſuppoſed, upon the principles of the theory advanced by C. Clouet, that the ſuperior denſity of the button was in conſequence of a greater degree of diviſion in the fluid: that this, in part, might be occaſioned by the oxygen ſet free by the decompoſition of the carbonic acid: that part of this oxygen might ſeize upon the iron, and, while theſe conveyed an unuſual degree of colouring principle to the glaſs, might at the ſame time account for the great deficiency of metal.

This ſubject, of the greateſt importance to a juſt theory of the component parts of the various modifications of iron, will meet a copious inveſtigation hereafter. Suffice it for the preſent prefent to draw the following conclufion, warranted by the foregoing experiment: That when equal portions of iron and carbonate of lime are fufed together, a metallic button is the refult, even lefs allied to fteel than the products of experiments Nos. 1, 2, 3, 4, 5.

The iron ufed in thefe experiments was drawn down from a bar of excellent Swedifh iron, a fragment of which was always put in competition with the products obtained by fufion. In thefe, the only appearances that indicated fteel were, fhaling, and flightly hardening when plunged hot into water. Both of thefe properties were, however, much more confpicuous when a piece of the unfufed bar was fubjected to the fame tefts. In fhort, all good foreign bar iron, when fimilarly treated, is fufceptible of the fame difplay of properties.

It may be here proper to obferve, that the carbonate of lime ufed in thefe experiments was the fame throughout; that, after being dried in a temperature of 180° of F. it yielded in diftillation 39 *per cent.* of carbonic acid.

VI. *On the Strength of Acids, as indicated by the Specific Gravity, and by the Areometer of M. Baumé.* By ROBERT BINGLEY, *Efq. King's Affay Mafter, Mint, London.*

To Mr. Tilloch.

SIR, Tower, Jan. 23, 1802.

AS I do not remember to have feen any table fhowing the correfpondence between the fpecific gravities of acids and the degrees of the *pefe-liqueur* (areometer) of Baumé, and as I am poffeffed of a good inftrument of Baumé's, and have examined the nitric acid of different fpecific gravities, and made a comparifon with the above inftrument, I am enabled to give a fufficiently accurate idea of what ftrength of acid is meant when Baumé's areometer is quoted. I remember, a few years ago, being much at a lofs to underftand the power of acids expreffed by the French chemifts according to Baumé, and made many inquiries amongft our chemifts without obtaining the information wifhed for; and as the experiments made on the continent are generally defcribed according to the *areometre de Baumé,* and much read in this country, I have annexed a table of thofe various degrees of nitric acid which have been examined and compared with the French inftrument; and if you think them worthy a place in your Philofophical Magazine, you are welcome to infert them. It is not pretended that this fcale is accuracy itfelf, though I am confident it will be found fufficiently fo for moft purpofes; and it it is ſurely very eſſential to have a clear idea of the power and ſtrength of an acid uſed in various metallic ſolutions.

This compariſon was made in September laſt, and the temperature about 60. Having a ſtock of acid of the ſpecific gravity 1435, a little diſtilled water was daily added, and the gravity taken the day after each addition of the water; then the *areometre de Baumé* was immerſed with the following correſpondence. It will be eaſy to underſtand and calculate the degrees of Baumé both above and below thoſe already compared and examined. I had no acid of higher gravity than 1435, nor did I deſcend lower than 1150; which is a weaker acid than uſed in moſt metallurgic operations. The areometer, or *peſe-liqueur*, is graduated from 0 up to 50.

Specific Gravity. Water taken as 1000.	Strength by the Areometer.
1435	45
1416	43
1400	42
1383	41
1367	40
1358	39
1350	38
1342	37
1333	36
1312	35
1300	34
1283	32
1275	31
1267	30
1250	29
1233	28
1216	26
1167	20
1150	18

VII. *Obſervations on the Plant by which the Indians of America preſerve themſelves from the Bite of venomous Serpents. Communicated to Count* RUMFORD *by Don* PEDRO D'ORBIES Y VARGAS *.

THE abundance of venomous ſerpents found in the warm diſtricts of America has rendered it neceſſary for the

* From the *Bibliothèque Britannique,* by Profeſſor Pictet.

unfortunate

unfortunate Indians and negroes, who traverse the woods almost always barefooted, to search out the most efficacious remedies for the disagreeable effects produced by the bite of these animals. Of the remedies hitherto discovered, none is equal to the juice of a plant of the creeping kind called *vejuco du guaco*; for it not only cures the maladies arising from the bite of serpents, but preserves from these effects those who have drunk of it before they are bitten; so that the negroes and Indians acquainted with this plant, lay hold, with their naked hands, of the most venomous serpents without sustaining any injury from them. This knowledge, of which they formerly made a great mystery, gave them much importance in the country; and there is no doubt that they gained a great deal of money, both from those who were bitten by serpents, and from those who were desirous, through curiosity, to see them handle these dangerous animals.

Being born in the kingdom of Santa-Fé, belonging to South America, I had often heard the inhabitants boasting of the great ability of these negroes, whom my countrymen call *empirics*. But as in the capital, where I was educated, which lies in a cold district, there are no venomous serpents, I had no opportunity of seeing any till the year 1788, when, being at Margerita, I heard of a slave who had a great reputation as being invulnerable to serpents, and who belonged to a gentleman of that place. As I was resolved to examine him myself, I begged his master to send for him, with a sufficient provision of serpents; which he readily consented to do.

On the 30th of May, the same year, the negro came to the house where I resided with one of the most venomous serpents of the country, which he had put into a calabash; a kind of vessel employed by these people for the same purposes as bottles are employed in Europe. Having informed him that I was desirous of seeing a specimen of his talents, he replied that he was ready to gratify my curiosity, and, taking the serpent from the calabash, handled it with so much confidence and composure, that I imagined he had previously deprived it of its teeth that contained the poison. I therefore caused him to open its mouth: but I saw that it still had its teeth; and was convinced that the negro possessed some secret for soothing it, for it appeared as tame and harmless as the most innocent animal could have been. After a long conversation with the negro, of whom I asked several questions, to which he gave the most pertinent answers, I informed him how much I should be gratified if I could be enabled to handle serpents with the same security; and, finding that he was not averse to procure me that satisfaction, I offered him a recompense, with which he seemed satisfied. Next morning he returned with the leaves of the plant in question, which he moistened, and, having bruised them in my presence, made me drink two large spoonfuls of the juice.

He then made three incisions between my fingers in each hand, in which he inoculated me with the same juice: he performed a similar operation on each foot, and on each side of my breast. When these operations were finished he informed me that I might lay hold of the serpent. I made several observations to him in regard to the disagreeable consequences to be apprehended in case I should be bit by the animal; but finding that he seemed confident in his skill, I resolved to take it into my hands without any fear; which I did several times, the animal never making the least attempt to do me any injury. One of the individuals, however, who were in the house, being desirous to run the same risk, was bit by the serpent the second time he took it in his hand; but without any further inconvenience than a slight inflammation in the part.

Two of my domestics who had been also inoculated, encouraged by this first attempt, went out into the fields and soon brought with them another kind of serpent, equally venomous, without sustaining any hurt from it. In a word, I have caught several since that time without any other preparation than that of having drunk a little juice of the *vejuco du guaco*; and after repeating these trials, either on myself or my domestics, and always with the completest success, I resolved, in 1791, to give a memoir on this remarkable antidote in a periodical paper published every week at Santa-Fé. I added a description of the plant, and every thing that appeared to me necessary for rendering public and general this discovery so useful to mankind. An account of all my experiments, and of the persons who were present, will be found in that paper, dated Sept. 30, 1791.

I shall here only observe, that the tradition current among the Indians and negroes of the vice-royship of Santa-Fé, respecting the manner in which the virtue of this plant was discovered, is as follows:—A bird of the kite kind, described by Catesby under the name of the *serpent-hawk*, feeds chiefly upon snakes in the hot and temperate regions of that part of America. This bird has a monotonous cry, sometimes very disagreeable by its repetition, which imitates the articulated word *guaco*, on which account the inhabitants have given it that name; and these people say, that when it cries it is to call forth the serpents, over which it exercises a certain kind of of authority. They add other extravagant fables: but it is certain that the *guaco* purſues them wherever it finds them; and that the Indians and negroes, who ſpend the greater part of their time in the foreſts and open fields, aſſert, that to take them with more ſafety they prepare themſelves by eating ſome leaves of the plant in queſtion. This may be true; they may have diſcovered the virtue of it, and experienced it with ſucceſs. In this caſe, as in many others, the inſtinct of animals has been of uſe to us.

In regard to the plant, its genus has not yet been claſſed in any book of botany I have ever ſeen; and for that reaſon I ſhall venture to give a deſcription of it as well as I can, taking advantage of the memoir above mentioned. The root is fibrous, and extends in every direction. The ſtem is ſtraight, perfectly cylindric when the plant is tender, but when old becomes pentagonal, that is to ſay, acquires ſalient angles. The leaves which grow on the ſtem ſtand oppoſite to each other; are ſhaped like a heart; have a dark green colour intermixed with violet; are ſmooth on the lower ſide, rough on the upper, and ſomewhat velvety: its corymbiferous flowers are yellow, floſculous, and have four fleurons in each common calyx. The corolla is monopetalous, infundibuliform, with five indentations; and contains five ſtamina, united by antheræ in the form of cylinders, which embrace the ſtyle. The ſtyle has a ſtigma deeply divided, and the calyx contains ſeveral broad ſeeds, each with a ſilky aigrette.

The plant is vivacious, and is found in the hot and temperate regions of the vice-royſhip of Santa-Fé; it is, in general, fond of growing on the borders of rivulets and in ſhady places, rather than in the open plains. Nature has not produced it in the elevated or cold diſtricts of this continent; and for this reaſon, no doubt, that its virtue would be uſeleſs, as there are no venomous ſerpents but in the countries where it grows.

VIII. *Account of a ſingular Method of hunting Wild Swine in the Iſland of Sumatra. By Mr.* JOHN, *Miſſionary at Tranquebar* *.

Tranquebar, Feb. 10, 1797.

IN the kingdom of Siak, in the iſland of Sumatra, which lies oppoſite to Malacca, there are two kinds of wild ſwine: one kind live in the foreſts on roots and fruit; their fleſh is

* From *Der Geſellſchaft Naturforſchender Freunde zu Berlin Neue Schriften*, vol. ii.

exceedingly well tasted, but they are not numerous. The other kind frequent the impenetrable bushes and marshes on the sea coast, where they live on crabs and roots. They are not so large as the European swine, are of a grayer colour, and keep in large herds. The latter, at certain periods of the year, in herds of sometimes a thousand, swim from the one side of the river Siak to the other, at its mouth, which is three or four miles in breadth, and return to the former at stated periods. This passage takes place also in the small islands, by their swimming from the one to the other. On such occasions, these swine are hunted by a tribe of the Malays, who are not Mahometans but heathens, and who are held in great contempt by the other Malays, who will neither suffer them to inhabit among them nor to approach them. On this account they were obliged formerly to live more at sea than on land, in small boats covered with mats, until the king of Siak lately permitted them to establish themselves in the uninhabited districts on the sea coast. They are called in the Malay language the *Salettians*, and by the Dutch the *Lard Malays*. They are better made and of a brighter colour than the other Malays; have only one wife each, whereas it is well known that the other Malays admit of polygamy.

The women are said to be remarkably well made, and to retain their charms much longer than those among the other Malays and inhabitants of the sea coast; so that at the age of forty they look as well as the latter at twenty. They go half naked, and wear only a piece of cloth wrapped round their loins, which hangs down to their knee. They are much addicted to strong liquor, particularly arrac; and when they see an European vessel, their first inquiry is after that beverage, for which and tobacco they give every thing they have, and sometimes even their daughters and nearest connections, to be kept for a certain time as servants, but not slaves. They are much more attached to the Dutch than to any other nation, and have the greatest intercourse with them. They are much afraid of other nations, and particularly the Malay pirates, because the latter carry them away and sell them as slaves.

On such occasions they defend themselves with great bravery by means of slings, with which they hit a mark very accurately at a great distance. They can use also, with great dexterity, a sort of javelins made of very hard wood, with which they can strike fish at a considerable distance at the surface of the sea. When these javelins are to be used against robbers, they harden the points of them in the fire, and dip them in lemon juice, by which means the wounds they inflict prove highly dangerous, and often mortal.

Some of the Salettians inhabit alfo feveral fmall iflands, not frequented by the other Malays, which lie in the ftraits of Sinkapoor; but thefe are not fo civilized as thofe who live near Siak, and often while out fifhing, or hunting fwine, carry off many Portuguefe and other fifhermen, whom they fell as flaves.

They hunt the fwine at fea at different periods, when thefe animals, through natural inftinct, emigrate as above mentioned. The Salettians, on thefe occafions, fmell the fwine long before they fee them, and make ready their boats. They then fend out their dogs, which are trained to this kind of hunting, along the ftrand, where, by their barking, they prevent the fwine from getting on fhore to conceal themfelves in the bufhes. During the paffage, the boars go firft, and are followed by the female fwine and the young ones, all in regular rows, each refting its fnout on the rump of the one before it. Thefe animals, fwimming in long rows clofe to each other, form a very fingular fpectacle.

The Salettians, men and women, meet thefe fwine in their fmall flat boats. The former row, and throw large mats, made of the long leaves of the *pandamus odoratiffima* interwoven through each other, before the leader of each row of fwine, which ftill continues to fwim with great ftrength; but, foon pufhing his feet into the mat, gets fo entangled that he can no longer move them, or moves them only in a very flow manner. The reft of the row, however, are neither alarmed nor difconcerted, but keep clofe to each other; and none of them leave the row, or the pofition in which they are placed. The Salettians then endeavour to row towards them in a lateral direction; and the bufinefs of piercing the fwine is configned chiefly to the women, each of whom is armed with a long large javelin, in the form of a fpontoon, headed with iron, and with which they ftab as many of the fwine as they can reach, always drawing back the weapon. Befides this inftrument, they have alfo a number of fmaller javelins, about fix feet in length, without iron points, which they carry in their arms, and which they throw to the diftance of 30 or 40 paces, in fuch a manner as to kill thofe which they cannot reach with the long javelin. As it is impoffible for them to throw mats before all the rows, or to kill all the fwine in fo fhort a time, the reft of thefe animals fwim off in good order, and in regular rows, towards the place where they are taught by inftinct that they fhall find better nourifhment, and for this time efcape the danger

till

till they return to their ufual place of refidence. As the fwine which have been pierced float every where around, they are picked up and put into larger boats, which follow for that purpofe.

If thefe hunters fall in with any veffels belonging to the Chinefe, of whom a great many are fettled at Malacca, and who on fuch occafions become ready purchafers of their booty, they fell to them their fwine, and receive for thofe that have an inch and a half thicknefs of lard on the breaft at the rate of a piafter a-piece. It may readily be believed that none of them are purchafed by the Malays; for, being Mahometans, they abhor fwine's flefh, and, confequently, thefe pagan fwine-hunters; to whom they do all the mifchief they can, by deftroying their huts, robbing them of their property, and often affaffinating them, without ever being called to an account by any one for their depredations.

Of the fwine which they cannot fell, they firft cut off the head; they then fkin them and cut them up, chiefly for the fake of the lard, but fave as much of the flefh as may be neceffary to feed their dogs, and throw the reft into the fea. They then row their boats, laden with lard, to a folitary place on the coaft unfrequented by the Mahometan Malays, where they melt the lard, and preferve the greafe in large earthen veffels, or *bojans*, which are manufactured in Siam. This greafe they fell to the Maki Chinefe; and it is ufed not only by the common people inftead of butter, as long as it is not rancid, which is feldom the cafe in Malacca, but alfo for burning in their lamps inftead of coco-nut oil.

IX. *Inquiries into Coloured Light, by a Collation of the Experiments and Obfervations made by Sir Ifaac Newton on that Subject; together with fome additional ones. By Governor* POWNALL*.

To Mr. Tilloch.

SIR,

I SEND you my paper on *coloured light*; an hypothetic theorem inftituted to prove, not affuming to have proved, that there is but one *primary colour* in our folar fyftem, and that all the reft in the prifmatic image are, on one fide of the fcale, only gradations of that colour towards pure light; and, on the other fide the fcale, merely degradations from light towards the actual abfence of it; and that green exifts only as an intermediate compound between the two.

* Communicated by the Author.

The

The procefs of this theorem is founded on the experiments actually made by Sir Ifaac Newton, and on fome further experiments and reafoning, which his quæries fuggeft.

I wifh this imperfect attempt may excite fome real philofophers to take up this point of inquiry.

IT appearing from Dr. Herfchel's obfervations, publifhed in the Philofophical Tranfactions 1796, that the folar light does not proceed from the fun as an igneous body, but apparently from inflammable and inflamed vapours floating on the furface of its atmofphere; and it having been long known that this white lucid light could be decompofed into a number of coloured rays, each homogeneous, as has been fuppofed; each "originally endued," as Sir Ifaac Newton fays, "with its refpective colour;" or, as he otherwife expreffes the fame fact *, "with that which each is originally difpofed to exhibit;" it occurred to my mind, on reading this in the year 1797, that thefe obfervations of Dr. Herfchel, if finally found to be true in nature, and followed up by an induction of combining facts, might lead to a difcovery of that property of light which hitherto appeared to me to be inexplicable, at leaft unexplained, viz. that every the minuteft ray of which it confifts is a compound of feven, or three, principal primary homogeneous coloured rays, each originally endued with its refpective colour, as conftituent elements of this lucid white light.

It occurred to me, that if *fimilar* coloured lights, arifing from the aërial inflamed vapours, or flames of bodies combured in our terreftrial atmofphere, fhould be found to obferve the fame refractions, and by thefe refractions, as they never are purely homogeneous, fhould in their decompofition obferve and be affected by the fame laws as operate on the folar light; the folar light may, by direct analogy, be fuppofed to be only a compound of inflamed vapours taking the refpective colours of their flame, and being endued with their refpective refractions, according to fuch attractions as the refpective fubftances from which they arife are liable to. Whilft I confidered this, I recollected that I had read in, and learned from, Sir Ifaac Newton, fpeaking of flame, according to an expreffion which he ufed at that time, "as fmoke red hot," that, "according to the nature of the fmoke, the flame is of

* There are fome phænomena in the different colours of the fixt ftars which feem to indicate that the coloured light of their fyftem may differ from that of the folar one.

various

various colours, as that of ſulphur, blue; that of copper opened with ſublimate, green," &c.

To inveſtigate this analogy according as the various lights which come to our ſenſes preſent themſelves to us, I here directed my inquiry, 1ſt, To learn what colours the various inflamed vapours of terreſtrial bodies combured in our atmoſphere throw out: 2dly, Whether theſe colours were homogeneous; and if not, 3dly, How far they obſerved ſuch ſimilar refractions in their decompoſition as the ſolar light doth. From the ground of a ſpeculative opinion, which I have long entertained in my own mind, that, although the white lucid light of the ſun is only a compound, yet light as light exiſts in a lucid, homogeneous, elementary, white and uncompounded ſtate, and only becomes otherwiſe by mixing or paſſing through oxygenous and other airs, or ſuch vapours as reſult from combuſtion, and, by being ſuffuſed with the colours furniſhed by theſe, becomes leſs and leſs pure, and, conſequently, tainted with colours. I applied my obſervations firſt to the electric ſpark. From the inſtantaneous and tranſient nature of this light, the decompoſition could not be examined: it is known, however, that electric light paſſed through an exhauſted receiver exhibits tremulous rays of various colours ſomewhat ſimilar to the *aurora borealis*; ſo that it comes out that this light is a compound. The next light which met my attention was that of the ſparks of iron deflagrated in vital air. Although I could not examine this by experiment for the ſame reaſon as before, yet I may venture to pronounce that, this being nothing but the white heat of fuſed iron, it muſt be a compound light. The next was the very brilliant light of phoſphorus burnt in vital air, ſo blazingly bright, that no eye can ſtand the firſt ſtroke of it. The next was the lucid light of camphor inflamed by ſpirit of wine, whoſe blue may be ſuppoſed, in ſome degree, to tinge the brilliancy of the white: theſe decompoſed into a faint ſpectrum of the principal colours. The next was the light of ſpermaceti burnt in Argand's lamp, wherein the ſmoke is in great meaſure conſumed. This was ſaid by Dr. Franklin to come the neareſt to day-light of any other terreſtrial light. This decompoſes into red and yellow, ſomewhat greeniſh as it runs into the blue. The flame of wax was the next light: this has a ſuffuſion of yellow, and decompoſes into a red and greeniſh yellow and blue. The flame of tallow was the next, which Sir Iſaac Newton calls yellow: this decompoſed into a red, a blueiſh green, and blue.

From the ground of a reaſon which will be given below, I began

I began in examining coloured lights by that of red-hot iron *. This viewed through a prifm decompofed into a deeply tinged red, and a deep blueifh green only; the gradations of blue, if they are not abforbed in the deep green, are fo rapidly abforbed in darknefs as to be hardly if at all vifible.

I had read in Lavoifier of a red light, but was not able to afcertain the fact: fome time after, I met with a paffage in a tranflated edition of Lavoifier, inferted by the editor, wherein he mentions an example of a red light produced by the vapour of ftrontian inflamed by fpirits of wine, and reprefents it of a deep blood colour. In trials which I had an opportunity of feeing, and of making myfelf, this light, which, mixed with the inflamed vapour, appeared to my eye to give out a light by intermitting flafhes tinged with rather a carmine red; and I learn that nitrate of ftrontian gives to the flame of fpirit of wine a bright carmine red. Dr. Gibbes † fhowed me another inftance of muriate of ftrontian giving a fuffufion of red to the flame of fpirit of wine. Nitrate of *lime*, formed into the falt called Baldwin's phofphorus, does the fame.

I am taught that nitre, diftilled with half its weight of fulphur, gives a *yellow* ‡ acid liquor yielding *red fumes*.

Hydrogen and carbon, united with oxygen in the conftituent parts of animals and with azote in vegetables, form oil and fat: the flame of thefe is yellow or yellowifh, as the flame of fulphuric and bituminous bodies is blue fuffufed with green; the yellow of the former runs into reddifh fumes, the blue of the latter into greenifh fumes. The oleaginous parts of bitumens being yellow, explains the reafon why their blue flame is difpofed to green. Verdegris inflamed with fpirit of wine gives out a green flame; but this decompofes into a red and blue, with an intermediate blue-green.

Although thefe coloured flames and vapours are not homogeneous, yet they obferve, according to various attractions which the aëriform fubftances, their refpective bafes, are liable to, the fame laws of refraction and decompofition as the folar light doth, the primary colours of which are not themfelves homogeneous.

* Although there is no decided flame in this cafe, yet whoever views a piece of red-hot iron along its fides will fee a waving tremulous vapour.

† I beg here to acknowledge the obligations I received from Dr. Gibbes, in the arrangements of any matters connected with chemiftry which I had occafion to refer to.

‡ It may be worth noticing, by the by, that as this yellow liquor gives, in its aëriform ftate, a red flame; fo fulphur, yellow in its folid form, gives out, in its aëriform ftate, a blue flame.

As

As fubftances acidified by oxygen "*differ* from each other *only* according to the feveral natures of the oxygenated or acidified bafes," why may not this be the cafe of coloured flame or vapour? Their colour *differs only* according to the nature of the bafes thus illuminated by their combination with light.

In examining and obferving upon thefe facts *, both by prifms and lenses, in this comparative view, it appeared thefe terreftrial coloured flames and aëriform vapours combured in our atmofpheric air, obferve the fame laws of refraction and decompofition as the folar light in every inftance which can be brought under experiment. And that, as the folar light proceeds from inflamed vapour on the furface of the fun's atmofphere, as Dr. Herfchel ftates, this folar light muft arife from vapours having fimilar bafes as thefe terreftrial coloured lights have, allowing for an apparent different purity; and muft, having the fame properties, be of the fame nature. So that, although the folar light, coming to our fight in its compound ftate, is a bright lucid light; yet, being decompofed by the different refractions, which the feveral fubftances or bafes from which it proceeds are liable to, it is refolved, exactly as the terreftrial lights above examined are, into the conftituent coloured lights which the feveral bafes give out.

I will not venture, on the ground of the obfervations ftated, and on the reafoning attached to them as above, to lay it down as a propofition abfolutely proved; but will hazard † it as a quære, open to further inveftigation.

May not the fun, an unignited body, and fimilar to our terreftrial globe in all its external appearances, (but poffibly of variety in its mineralogy,) produce an atmofphere of more

* It is hardly worth while to defcribe the method I obferved to do this, yet it may not be amifs. I had prepared a tall long box, painted black on the infide, with a fliding door in the front, in which was bored a hole about half an inch diameter: the ufe of this fliding door was to raife or lower the hole to the height of the coloured light which I had occafion to try. There was alfo a door on the fide by which to put in the lights, and a valve at the top to let out the fmoke when I fhould obferve that it affected the light. By applying a prifm to this hole, I obtained from all the lights, except the blue, a prifmatic fpectrum: the light of the blue was fo weak, that I could not obtain a fpectrum from it coming through the hole; I therefore drew up the fliding-door entirely, and fo turning the prifm as to direct the refracted rays down upon the ground near under the table on which the box ftood, and into the dark, obtained, yet imperfectly, the fpectrum above noticed. By lenses applied to the hole, the differences of the different focuses were as the refractions inverfely.

† This I ftated to Dr. Herfchel by letter, dated March 17, 1797; and had afterward a converfation with him at Slough on this fubject of the coloured rays of the folar light.

ſublimed and purer vapours than our earth does; and ſo exhibit purer and more homogeneous coloured lights than the terreſtrial inflamed vapours do, although in every other reſpect of a ſimilar nature? May not this explain the reaſon why ſome of the fixed ſtars, alſo ſuns, appear of a green or red colour? for, if their bodies conſiſt of a different mineralogy, and the vapours of their atmoſpheres be thus diſpoſed to throw out different, as for inſtance green and red lights, in a greater proportion than the other colours, they will thus, of courſe, have this or that appearance of being green or red; which is a known phænomenon. Nay, even the white lucid light of the ſun, apart all conſideration of circumſtances which may affect it as it comes through our atmoſphere, gives out at times a ſomewhat differently ſuffuſed white, according as this or that conſtituent of its compound light may be in a greater or leſs proportion.

May not the moon, producing an atmoſphere viſible where no light of the ſun can reach, and therefore not a reflected light, poſſeſs, as a planet, an atmoſphere very different from what thoſe ſuns produce, perhaps of a phoſphoreſcent nature, which exiſts with little heat? And may not the viſible atmoſphere of Venus, conſiſting of enlightened vapours, be of the ſame ſort? I will ſtrengthen the propoſition of this quære from the opinion of Dr. Herſchel on this planet: "It cannot ſhine (he ſays) by a borrowed light, ſo that this faint illumination muſt denote ſome phoſphoric quality in the atmoſphere of Venus."

Thus far I proceed in my reſearches on the ground of the ſolar light being a compound of various rays, ſuppoſed to be originally diſpoſed to exhibit ſeven or three primary colours; which colours are alſo ſuppoſed to be homogeneous. A doubt, however, has always occurred to me, firſt, whether this opinion of the homogeneity of the ſeven primary colours be founded in fact: and, in the courſe of examining this firſt doubt, a further doubt ariſes, whether there are *ſeven* or only *three* principal colours, as Sir Iſaac Newton expreſſes it; or whether there be more than *two*, and perhaps not more than *one*. Stating then what follows as a doubt followed up by inveſtigation, it appears to me, that the ſpace which each priſmatic primary colour (as it is called) takes in the priſmatic ſpectrum, has not one only angle of refraction within its extent, but a ſucceſſive ſeries of angles of refraction, and a ſucceſſive ſeries * of innumerable circular or orbicular images of the ſun (as Sir Iſaac Newton ſtates the fact), ſo cloſely inter-

* Optics, experiment 5, p. 32. *Vide* fig. 15. plate 3. of book i. part i.

fecting

fecting each other, that the point of interfection is hardly perceptible to a common eye unaffifted, if at all. Although, therefore, the centres of thefe circles may approach to each other at an infinitely fmall diftance; yet they are placed under different refractions, and muft, of courfe, give a fucceeding tint more refracted than the preceding: and as thus throughout the fpace of the commonly called homogeneous colour, fo muft the indifcriminate feries of refractions, and of the interfecting images of the fun, proceed *gradatim*, and not *per faltum*, into the next fpace of the next primary colour; and fo through that into the next; and in like manner throughout the whole. The angle of refraction at the point where the fpace in the fpectrum, which contains the *red*, is fuppofed to end, exceeds that which is on the outfide of the fame fpace, where it commences, in a much greater degree than it doth the next contiguous and continuous feries of angles of refraction of the fpace which contains the *orange*. And the fame holds good of the laft angle of refraction at the point where the orange ends, or is fuppofed to end, and where the yellow commences, or is fuppofed to commence; and fo through all the gradations of colours. Sir Ifaac Newton, in the hypothefis of his theorem inftituted for experiment, fo ftates it. As therefore the angles of refraction at the centres of the innumerable * circles or orbicular images of the fun in the prifmatic fpectrum, as ftated by Sir Ifaac Newton, which form any one of the principal colours, homogeneous as they have been called, thus differ in their degrees, fo muft the tints in each fuch colour differ in a feries of gradations; and cannot therefore, however they may to a tranfient or undifcriminating view appear, be abfolutely homogeneous: "for, the light which I here call homogeneous (fays Sir Ifaac Newton) being not homogeneal, there ought to arife fome little change of colours from its heterogeneity †." The *red*, by fuch a fucceffive feries of tints, is heightened towards and expanded into the *orange*; the orange, by a like fucceffion of tints, into the yellow; and the yellow, in like manner, into the white.

As thefe coloured lights on this end of the fpectrum heighten by gradations of tints more and more illumined, as if purging themfelves from the fuffufion of colour into pure light: fo the blues at the other end of the fpectrum, by a like gradation of tints, or rather hues, deeper and deeper, weaker and weaker, go off into darknefs, in "a continued feries, according to their degree of refrangibility ‡."

* *Vide* fig. 15. of plate 3. book i. part i. Newton's Optics.
† Newton's Optics, book i. part xii. prop. 11. p. 107.
‡ Newton's Optics, p. 32.

Having thus stated the *idea* of the successive gradations of the prismatic colours, contrary to what is commonly supposed, of each being homogeneous; let us now refer to the *fact*, as it actually exists. But before we examine the two gradations here stated of the red and blue (if blue be a colour), we will examine the *intermediate green*.

[To be continued.]

X. *On a new Combination discovered in Zaffer, which Brugnatelli considered as the Cobaltic Acid. By* C. DARRACQ *.

BRUGNATELLI, in a paper published in the *Annales de Chimie* for the month of Pluviose, year 8, mentions several experiments on zaffer, or the gray oxide of cobalt, in which he thought he had discovered a new acid. I shall give an account of some of these experiments, and also of the properties ascribed to his cobaltic acid.

Having suffered zaffer to remain some time with ammonia, after a few days digestion in the sun, he obtained a red liquor known under the name of *ammoniure of cobalt*, which he filtered, and evaporated to dryness. The concrete residuum obtained appeared to him to be composed of two distinct substances; one of a dark red, and the other of a pale yellowish colour. The red part dissolved in water, and the yellowish remained in the filter. This residuum he found to be pure oxide of cobalt. The part soluble in water was evaporated, and deposited, on cooling, some small crystals, which Brugnatelli found to be a combination of the new cobaltic acid with ammonia. The supernatant liquor still possessed an evident character of acidity.

The author observes, that his acid could be obtained coloured or without colour, according to the means employed to obtain it. When the evaporations are effected by means of fire, they leave a residuum, which, when dissolved in water, gives cobaltic acid almost colourless; while those effected in the sun give it always more or less red. Brugnatelli, desirous to ascertain whether his acid was formed during the operation of which I here speak, or whether it existed quite formed in the zaffer, boiled for twenty-four hours six pounds of this substance in eight pounds of water. He filtered the liquor while warm, and then caused it to evaporate. When

* From the *Annales de Chimie*, No. 121.

reduced to one-half it became turbid: the evaporation being continued till there remained only one-third of the liquid, it was taken from the fire, and it then deposited a white matter, which was collected on the filter. The liquor which passed through had a yellow colour and a taste sensibly acid, and exhibited the same phænomena in every thing as the cobaltic acid obtained by the process before indicated.

The following are some of its properties, which Brugnatelli considers as characteristic:

1st, It precipitates a solution of silver.

2d, It precipitates lime water in a white coagulum, insoluble in water and in excess of acid.

3d, It can be separated by alcohol from its aqueous solution.

4th, It precipitates acetite and muriate of barytes.

I shall now describe the experiments I made on this subject, and I submit the consequences I have deduced from them to the examination of the Institute.

Exp. I. I formed ammoniure of cobalt by suffering ammonia to macerate, in the sun, over zaffer. I took care to stir the mixture, which was contained in a matrass: it soon assumed a red colour, and, forty-eight hours after, a bright red colour. I remarked in this liquid a pretty abundant crystallization under the form of white and brilliant needles. This crystallization was permanent until the sun had again heated the liquid. It then disappeared, and dissolved in the liquor. In this state the ammoniure of cobalt was filtered, and put into a retort: as it was heated it assumed a violet red colour, which became darker till it assumed a beautiful red wine colour. When the greater part of the ammonia was evaporated the mixture acquired a greenish colour, and, by rest, there was precipitated a matter of the same colour as the solution. The liquid being then filtered while warm, and evaporated to dryness, there were obtained the rudiments of crystals, the form of which could not be determined. White and brilliant particles were remarked. The rest of the matter had a yellowish colour. I poured water over this residuum, stirring it with a platina spatula. The part least coloured dissolved entirely, and communicated to the water a straw colour. This liquor was acid, and possessed some of the properties announced by Brugnatelli. I shall describe hereafter the experiments to which I subjected it, and the results of which, carefully examined, prove that it was not an acid formed by cobalt. The two residuums obtained in this experiment, one of a green and the other of a yellowish colour, were found not to be pure oxide of cobalt, as Brugnatelli says, but a combination of that oxide with arsenic acid.

Exp.

Exp. II. Having prepared a new quantity of ammoniure of cobalt, as in the firft experiment, I fubjected it to fpontaneous evaporation in a place where the fun accelerated the volatilization of the ammonia. In proportion as the liquid evaporated, there was precipitated a flaky matter of a whitifh rofe colour, which fenfibly increafed till the ammonia was evaporated. The liquor emitted no odour, but it had retained a fufficiently beautiful rofe colour. The liquor was again evaporated to drynefs, and the remaining matter was diffolved in diftilled water. This folution, of a light rofe colour, was acid, and poffeffed the properties of the acid obtained in the firft experiment.

The refiduum left on the filter was carefully examined, and found to be arfeniate of cobalt.

Exp. III. I took, as the author of the memoir did himfelf, a kilogramme of zaffer, which was reduced to an impalpable powder. I boiled it, for half an hour, in three litres of diftilled water. The liquor, which I filtered while warm, had a light colour and a fenfible favour; and, being evaporated in a porcelain capfule, the liquid, towards the end of the operation, became turbid. I then continued till there remained only about a hectogramme of the liquor. By cooling I obtained a cryftallization in needles. Having filtered the mixtures, ftirring them, the cryftals remained on the filter, and the fupernatant liquor paffed through, very tranfparent, of a bright yellow colour. Brugnatelli, in making this experiment, did not remark the cryftallization; he only obferved a white matter, which affumed a rofe colour by coming into contact with the air, and which he found to be oxide of cobalt. The needle-formed cryftallization which I obtained produced, as foon as it was heated, a confiderable difengagement of arfenic. When carefully examined it was found to be arfeniate of cobalt, and not pure oxide, as had been announced. The fact is of little importance, no doubt, in regard to the difcovery of Brugnatelli; but it may ferve to fupport my opinion, if the refults of my experiments were not more than fufficient to prove that the cobaltic acid does not exift.

The liquors of the firft, fecond, and third experiments, found to be acid, and of the fame nature, were fubjected to the following proofs, and comparatively with the arfenic acid:

1ft, This liquor was precipitated of a yellow colour, like that of orpiment or fulphuret of arfenic, by fulphurated hydrogen and alkaline hydrofulphurets. This precipitate was indeed fulphuret of arfenic, and not fulphur precipitated, as believed by Brugnatelli.

2d, It precipitates ammoniure of copper of a blueifh green.

 This

This property is that of the arsenic acid. This combination is known under the name of *arseniate of copper.*

3d, Sulphate of copper mixed with this liquor gives a precipitate of the same colour as the ammoniure of that metal. The arsenious acid possesses this property also, though not in so high a degree as the pretended cobaltic acid; but a similar precipitate may be obtained by making use of artificial arseniate of cobalt. The results then are the same: no difference can be perceived between them.

4th, It precipitates nitrate of silver white. The arsenic acid possesses this property.

5th, It precipitates nitrate of mercury of a straw colour. It is well known that the arsenic acid exhibits the same phænomenon.

6th, It precipitates lime water in a white coagulum insoluble in water, but not insoluble in excess of acid, as Brugnatelli says; since it, indeed, redissolves with the same facility in the pretended cobaltic acid as in the arsenic acid. The error of Brugnatelli, in my opinion, arose from his not having employed a sufficient quantity of acid.

7th, It precipitates acetite and muriate of barytes. I assured myself that this precipitation arose from some atoms of sulphuric acid contained in the acid called cobaltic. The arseniate of cobalt also disguises its solutions.

8th, With tincture of galls newly made it forms an abundant yellowish precipitate. It is well known that the arsenic acid produces the same phænomenon.

9th, This last experiment seems to have been considered by Brugnatelli as the most characteristic of his cobaltic acid: he observed that alcohol separated his solution from its aqueous solution, and the arsenic acid, dissolved in alcohol, possessed the same property: by these processes he obtained his concrete acid. This property of the cobaltic acid at first astonished me; but, presuming that I might find an explanation of it by examining the acid thus precipitated, I subjected it to the following experiments:

I. When heated on charcoal at the flame of the blowpipe it emitted white vapours, which were evidently arsenic.

II. A small portion, heated with borax, communicated to it a violet colour.

III. This acid, being precipitated only by alcohol, I examined the liquor after having separated it by the filter. Sulphurated hydrogen produced in it an abundant yellow precipitate, found to be sulphuret of arsenic.

IV. The acid, when separated, was scarcely soluble in water.

V. When

V. When mixed with ſome drops of arſenic acid, it diſſolved completely, and reappeared with its firſt properties.

After theſe trials I concluded that the acid of Brugnatelli could be only arſeniate of cobalt diſſolved in exceſs of acid.

To aſcertain this, I diſſolved pure oxide of cobalt in arſenic acid. I evaporated the ſolution to drvneſs, and added to the reſiduum diſtilled water. After ſtirring the mixture, it was filtered. The liquid obtained had a light roſe colour, and poſſeſſed all the properties of the cobaltic acid.

This liquor, mixed with alcohol, was abundantly precipitated: this precipitate, when collected, exhibited all the phænomena obſerved in concrete cobaltic acid.

It appears then certain, from the reſults here given, that no cobaltic acid exiſts: that the combination of the arſenic acid and the oxide of cobalt led Brugnatelli into an error, ſince it is this combination with exceſs of acid that is extracted from zaffer by the means deſcribed in that memoir.

XI. *Method of making Lime cryſtallize.* *By* TROMMSDORFF *.

THE cryſtallizable quality of lime was firſt diſcovered by Schaub. Bucholz obtained alſo very beautiful cryſtals of this earth by boiling it with its muriate. I have verified this diſcovery, and have found that cryſtallized lime may be obtained as well in winter as in ſummer, only that the ſalt prepared during the former ſeaſon is in larger and longer cryſtals. To obtain theſe cryſtals, boil any quantity, at pleaſure, of muriate of lime with a fourth part, or leſs, of cauſtic lime, and concentrate it, if in winter, till a drop of the ley, made to fall on a cold ſtone, aſſumes the conſiſtence of ſyrup without cryſtallizing or becoming fixed. The matter is then to be filtered through a piece of cloſe cloth extended above an earthen or porcelain capſule, which muſt be covered after filtration by a ſimilar capſule or a wooden lid, in order that the matter may cool as ſlowly as poſſible. In this manner may be obtained very long but very fine cryſtals of lime, which it is neceſſary to waſh in alcohol in order to free them from the adhering muriate. This operation cannot be undertaken with leſs than ſome pounds of the muriate of lime. It is well known that, when muriate of ammonia is diſtilled with exceſs of lime to decompoſition, a part of the reſiduum adheres ſo ſtrongly to the veſſel that it is almoſt

* From *Journal de Chimie*, by J. B. Van Mons, No. 2.

impoffible to detach it by foftening it. This hard mafs is formed, in a great meafure, by the lime cryftallized in a confufed manner; which it is more difficult to dilute in the water than the fame earth in powder, or very much divided.

XII. *Refult of the Obfervations of the new Star, difcovered on the 1ft of January* 1801, *at the Royal Obfervatory of Palermo. By* JOSEPH PIAZZI, *Director of the Obfervatory* *.

I. HAVING been engaged for nine years in verifying the pofitions of the ftars as collected in the catalogues of various aftronomers, I was fearching, on the 1ft of January 1801, among many others, for the 87th in the catalogue of the zodiacal ftars of the abbé De la Caille, when I obferved that this ftar was preceded by another, which, according to my ufual cuftom, I wifhed to obferve alfo, efpecially as it did not interrupt the principal obfervation. Its light was fomewhat faint, and in colour refembled that of Jupiter, but like that of many others which in regard to their magnitude are ufually placed in the eighth clafs. At that time no doubt arofe in regard to the nature of it; but on the evening of the 2d, having repeated my obfervations, and finding that they did not correfpond either in time or zenith diftance, I fufpected that fome error had been committed in my obfervations on the preceding day. I then began to entertain fome idea that it might perhaps be a new ftar. In the evening of the 3d my conjecture was confirmed, as I affured myfelf that it was not a fixed ftar. However, before I would fpeak of it, I waited till the evening of the 4th, on which I had the fatisfaction of finding that it had moved according to the fame laws which it had obferved on the preceding days. From the 4th to the 10th the fky was overcaft: on the evening of the 10th it appeared to me in the telefcope, accompanied by four others nearly of the fame magnitude. Being ftill uncertain in regard to the nature of the new ftar, I obferved them all as well as poffible, and, comparing thefe obfervations with others which I made on the evening of the 11th, I eafily diftinguifhed the new one from the reft by its motion. I was, however, exceedingly anxious to fee it when not on the meridian, and therefore employed myfelf in

* From *Rifultati delle Offervazioni della nuova Stella fcoperta il dì 1 Gennaio all' Offervatorio Reale di Palermo da Giufeppe Piazzi, Ch. Reg. Direttore del medefimo.*

in examining and contemplating it with great care; but, notwithſtanding all my exertions, and thoſe made by my aſſiſtants Cacciatore and Carioti, both poſſeſſing excellent ſight, and accuſtomed to viewing the heavens with a night teleſcope and an achromatic of four inches aperture, it was not poſſible to diſtinguiſh it from many others by which it was ſurrounded. I was therefore obliged to be contented with ſeeing it on the meridian for the ſhort ſpace of about two minutes, that is, during the time it employed to paſs over the field of the teleſcope; the other obſervations, made at the ſame time, not permitting me to remove the inſtrument from its poſition. To render theſe obſervations, however, more certain, while it was obſerved by myſelf with a circle, it was obſerved at the ſame time by Carioti with a tranſit inſtrument. In this manner, the heavens being conſtantly nebulous, and often obſcured by thick clouds, our obſervations were interrupted till the 11th of February; after which the ſtar having approached very near to the ſun, it was no longer poſſible to ſee it at the time of its paſſing the meridian. I reſolved then to ſearch for it beyond the meridian by the means of azimuths; but having fallen ill on the 13th of February, I was not able to make any further obſervations. Thoſe, however, which have been made, though not at the neceſſary diſtance to aſcertain the real path which this ſtar purſues in the heavens, are ſufficient, as far as I am able to judge, to make known, with certainty, the nature of it; as may be ſeen by the reſults I have deduced from them.

The magnifying power of the teleſcope of the circle was 50, and that of the teleſcope of the tranſit inſtrument 80; whence it was judged by Carioti that the new ſtar is of the 7th or 8th magnitude.

II. Combining in a parabola the two obſervations of the 1ſt and 19th of January with the third of the 11th of February, which were very good, I find the following reſults:

Perihelion - - -	$4^s\ 5^\circ\ 28'\ 36''$
Long. ☊ - - -	2 19 43 0
Inclination - - -	10 34 0
Log. diſt. of the perihelion -	0·3713077
Paſſage of the perihelion July 1801 -	3·6985

As theſe elements, however, did not agree with other obſervations, I tried another parabola combining other obſervations, and experienced the ſame difficulties. Reflecting afterwards, that to make the two obſervations, that of the 1ſt of January and that of the 11th of February, agree, it was neceſſary to ſuppoſe 0·26 of difference between the radius vector correſponding to the firſt, and the other correſponding to the

 ſecond,

fecond, I defcribed mechanically my firft parabola; and applying to it the longitudes and latitudes thus calculated and obferved, it was eafy for me to afcertain that the motion of this ftar could not, indeed, be reprefented in the arc of a parabola fuch as is nearly defcribed by comets.

III. From parabolic hypothefes I proceeded to circular, and, having made a few fuppofitions, I found two radii, viz. 2·7067 and 2·6862, with each of which the obfervations could be reprefented much better than with any parabola; for, as the planets defcribe ellipfes more or lefs eccentric, and not circles, it is to be fuppofed that our planet will not deviate from this law. I ought therefore to have continued my calculations on an ellipfis; but as the obferved arc was very fmall, the refults would have been uncertain, and the labour tedious and painful. On this account I preferred the circle, efpecially as the elements obtained from the ellipfe to determine the place of this ftar do not appear to me to be more certain than thofe afforded by the circle.

IV. On the 10th of January the ftar, from being retrograde, became direct. Setting out, therefore, from the obfervations of this day, I endeavoured to find its elongation while ftationary, which was 4ˢ 4°; whence we have for mean radius of its orbit 2·9352. But the difference between this radius and the other, obtained by the motion of the ftar between the 1ft of January and the 11th of February, feems to indicate a great eccentricity, whereas the feries of the obfervations feems rather to indicate that the eccentricity ought to be fmall: on the other hand, the diameter, deduced from the elongation of a planet while ftationary, can never be very exact, and particularly in the prefent cafe.

V. During the firft obfervations, bringing the ftar under the horizontal thread of the telefcope, it remained, as it were, entirely covered; and, as the thread fubtends to the eye an angle of about 6″, I judged the diameter of the ftar to be fomewhat greater, that is to fay, 7″. During the laft obfervations I was not able to form any judgment of its diameter, on account of the darkened ftate of the atmofphere.

VI. RESULTS.

Radius of the orbit - - -				2·6862
Motion in the orbit from January 1ft to February 11th - -		9°	2′	29·7″
Epoch 1801 - -	2ˢ	8	46	22
Motion in 100 days - -	0	22	6	33·7
Longitude ☊ - -	2	20	46	48
Inclination of the orbit - -		10	51	12

Mean

Mean distance deduced from the time of being stationary - - 2·9352
Tropical revolution, deduced from the mean distance according to the law of Kepler - - years 5·03
Sidereal revolution from the motion in the orbit - - days 1628·27
Apparent diameter at the distance of the earth from the sun - - 19″
Magnitude, $1\frac{1}{3}$d that of our earth.
Opposition, about the 1st of March 1802.

VII. Table of the Mean Time, Right Ascension, and Declination of the new Star as observed; together with the Longitude of the Sun, and the Logarithm of its Distance from the Earth.

	Days of the Month.	Ten Thousandths of the Day in mean Time.	Right Ascension.	Declination.	Sun's Place.	Log. Distance of ⊕ from ☉
January.	1	3635	51° 47′ 48·7″	15° 37′ 43·5″ N	9s 11° 1′ 33·1″	9,992617
	2	3606	43 27·7:	41 5·5	12 2 31·7	9,992629
	3	3577	39 36::	44 31·6	13 3 30·2	9,992641
	4	3547	35 47·2	47 57·6	14 4 29·0	9,992652
	10	3378	23 1·5:	16 10 32·0	20 10 29·5	9,992768
	11	3350	22 26·0	14 30, est.	21 11 29·5	9,992794
	13	3295	22 34·5	22 49·5	23 14 28·0	9,992848
	14	3268	22 55·8	27 5·7	24 14 27·3	9,992882
	17	—	27 35::	40 13·0	—	—
	18	—	28 45::	—	—	—
	19	3136	32 2·2	49 16·1	29 19 14·1	9,993060
	21	3084	38 34·0	58 35·9	10 1 21 2·5	9,993151
	22	3059	42 21·3	17 3 18·5	2 21 55·1	9,993196
	23	3033	46 43·5	8 5·5	3 22 46·4	9,993242
	24	—	51 45::	—	—	—
	28	2909	52 13 38·3	32 54·1	8 26 45·5	9,993522
	30	2860	27 2·1	43 11·0	10 28 10·6	9,993645
	31	2837	34 18·8	48 21·5	11 28 55·5	9,993708
February.	1	2813	41 48·0:	53 36·5	12 29 36·6	9,993773
	2	2789	47 45·9	58 57·5	13 30 17·0	9,993851
	4	—	53 7 45::	—	—	—
	5	2719	15 40·5	18 15 1·0	16 32 13·9	9,994083
	8	2650	44 37·5	31 23·2	19 35 2·2	9,994328
	11	2583	54 16 23·1	47 58·8 N	22 35 41·3	9,994588

N. B. The observations marked with two points (:) are a little doubtful; those marked with (::) very uncertain.

VIII.

VIII. Table of the Geocentric Longitudes and Latitudes of the new Star, both by Observation and Calculation; together with their Differences.

	Days of the Month.	Geocentric Longitude. Observat.	Geocentric Longitude. Calculat.	Differ.	Geocentric Latitude. Observat.	Geocentric Latitude. Calculat.	Differ.
January.	1	1^{s} 23° 22′ 58·5″	1^{s} 23° 21′ 59·2″	−59·3″	3° 6′ 32·4″	3° 6′ 50·2″	+17·8″
	2	19 44·8	18 40·2	−64·6	2 13·1	2 27·7	+16·6
	3	16 49·3	15 47·1	−62·2	2 57 58·9	2 58 6·3	+ 7·4
	4	14 16·5	13 18·9	−57·6	53 44·5	53 48·4	+ 3·9
	10	7 59·4	7 19·5	−39·9	28 50·9	28 31·8	−19·1
	11	8 25·7	7 43·4	−42·3	—	—	—
	13	9 58·0	9 38·9	−19·1	16 49·0	16 21·0	−28·0
	14	12 1·6	11 32·6	−29·0	12 47·1	12 23·2	−23·9
	19	25 49·4	25 51·5	+ 2·1	1 53 28·3	53 1·3	−27·0
	21	34 21·8	34 23·4	+ 1·6	45 58·9	45 31·6	−27·3
	22	39 1·8	39 6·7	+ 4·9	42 18·7	41 51·3	−27·4
	23	44 15·6	44 17·4	+ 1·8	38 39·2	38 12·3	−26·9
	28	24 15 16·0	24 15 28·1	+12·1	20 58·7	20 32·0	−26·7
	30	30 5·4	30 23·0	+17·6	14 5·3	13 43·5	−21·8
	31	38 8·6	38 20·5	+11·9	10 45·0	10 22·4	−22·6
February.	1	46 19·6	46 38·0	+18·4	7 23·8	7 3·6	−20·2
	2	54 55·6	55 12·8	+17·2	4 0·7	3 47·0	−13·7
	5	25 22 43·5	25 22 52·8	+ 9·3	0 54 19·0	0 54 9·6	− 9·3
	8	53 17·9	53 15·6	− 2·3	44 42·7	44 50·9	+ 8·2
	11	26 26 26·1	26 26 24·8	− 1·3	35 47·9	35 50·4	+ 2·5

IX. The correspondence of the observed longitudes of this star with those calculated according to the circular hypothesis; its motion in the zodiac, from which it deviates only a little in its greatest latitudes; and its position between Mars and Jupiter; seem to leave no doubt that it is a real planet, and, in all probability, the very same deduced from calculation in the year 1772 by professor Bode, and announced to the Academy of Sciences of Berlin. That it was not then observed, though the zodiac was diligently examined by the best astronomers, ought to be ascribed chiefly, in my opinion, to its smallness compared with its distance from the earth and with its greatest latitudes; though it is not improbable that it was seen by the abbé De la Caille or Tobias Meyer. In the catalogues of the stars made by these two astronomers there are some observed only once; which I have not been able to find, though I searched for them repeatedly and at different times. If the original observations of Meyer are preserved at Göttingen, and those of the abbé De la Caille at Paris, they may possibly be yet discovered. In the end of my work on the position of the fixed stars, the printing of which, through the munificence of our gracious sovereign, is already far advanced, there will be given a catalogue of the lost stars, by which this research will be greatly facilitated.

X. It

X. It is the opinion of many astronomers, and I am inclined to agree with them, that there are several other planets of the like kind still undiscovered. But as astronomers either seldom observe stars beyond the 7th magnitude, or are satisfied with observing them once, or at most twice, there is reason to doubt whether these planets will be easily discovered. Had I not been accustomed to observe stars four, five, or six times, and even more, I should certainly not have discovered the one in question. Re examining, as was the case, after a long time, the observations of the 1st and 2d of January, and finding that they did not agree, I should have searched in the same place of the heavens for this new star, and, not finding it, should have classed it among the number of the doubtful ones; a thing I have been too often obliged to do with others, the observations of which I was prevented from continuing by the inclemency of the weather.

XI. Oriani, Bode, and Von Zach, had scarcely seen the observations of the 1st and 23d of January, which I communicated to them on the 24th, adding at the same time that on the 10th the star from being retrograde had become direct, when they were of opinion that it was a new planet; and they next concluded that its elements were the same as those which I had conjectured. But as the star after the 23d began to decrease sensibly in magnitude and in light, being uncertain whether this ought to be ascribed to its receding rapidly from the earth, or rather to the state of the atmosphere, which after that period became more overcast and obscure; I began to entertain some doubts respecting its nature, and to believe that, in all probability, it was a comet and not a planet. It was only by calculating all the observations that my doubts were at length dissipated; but, on account of other avocations and the bad state of my health, I was not then able to pay proper attention to them. Finding myself somewhat better in April, I resolved to calculate my observations; but having brought on a second indisposition while fixing the meridian of the metropolitan church, and being reduced to a weaker state than before; being uncertain at what time I should be able to resume my studies, and anxious to communicate my observations to the above-mentioned meritorious astronomers, I transmitted them to Lalande at Paris, Oriani at Milan, and Bode at Berlin. As yet, however, I have received no answer but from Bode, who, on seeing my further observations, is more and more confirmed in his first opinion, and only seems a little surprised that, as I was much inclined in my first letter to Oriani to consider my new star as a planet, I should

I ſhould afterwards have conſidered it rather to be a comet. But had I communicated to him alſo the circumſtance reſpecting the diminution of its light, he would, perhaps, have entertained ſome doubts of the ſame kind.

XII. As this ſtar has not been lately ſeen, ſome doubt will remain in regard to its nature; and it may be difficult to diſcover it again on account of the uncertainty of the elements of the orbit which it deſcribes, and more particularly its very ſmall ſize. At preſent it remains above our horizon the greater part of the night, being in the ſign of Cancer; but it is at too conſiderable a diſtance for me to hope that I ſhall be able to diſtinguiſh it: and, on the other hand, I am deſtitute of the inſtruments neceſſary to ſearch for it, with any certainty, out of the meridian. About the beginning of November it will be much eaſier to diſcover it; and as the firſt days of March, the time of its oppoſition, will be the moſt convenient period for obſerving it with advantage, I entertain great hope that it will not eſcape the reſearch of aſtronomers.

XIII. However, whether this new planet be found again by myſelf or by any other aſtronomer, I ſhall be the more gratified, as, after the example of a Halley, a Hevelius, a Bode, and a Herſchel, who have inſcribed in the heavens the glorious names of a Charles II. a Poniatowſki, a Frederic, and a George III. all illuſtrious patrons of aſtronomy, I think I have an equal, and perhaps ſtronger reaſon, for inſcribing in indelible characters that of the auguſt and magnanimous founder of this obſervatory, our ſovereign Ferdinand, conjoined with that of the indigenous deity of this part of his kingdom, which he renders more lively and happy by his preſence. I have therefore informed thoſe aſtronomers who are my correſpondents that I have given to this new ſtar the name of *Ceres Ferdinandea.*

Telluris patriæ ductura à Principe nomen
Aſtra inter, Siculis fulſit ab axe Ceres.

Michael Angelus Monti, Scol. Piar.

SUPPLEMENT.

This ſhort memoir not being yet publiſhed, I ſhall here ſubjoin the ſubſtance of a letter which I received from my eſteemed friend Oriani, dated Milan, July 25, 1801, in which he communicates to me the reſult of his calculations, accompanied with thoſe of ſome other aſtronomers who have done me the honour to employ their talents on my obſervations.

Oriani, who calculated in a parabola, found

☊ - - - - -	$2^s\ 21^\circ\ 48'$
Inclination - - -	$9\ \ 33.$

Perihelion

Perihelion - - - -	$4^s\ 10^\circ\ 16'$
Diſtance of the perihelion - -	2·1045
Paſſage of the perihelion June 1801 -	21·07

Mr. ——— found alſo in a paraboia,

☊ - - - -	$2^s\ 20^\circ\ 50'$
Inclination - - -	$9\ \ 41$
Perihelion - - -	$4\ \ 8\ \ 38\ \ 25''$
Diſtance from the ſun - -	2·21883
Paſſage of the perihelion June 1801	$30^d\ 19^h\ 1^m$

The difference of the three parabolas calculated by Oriani, Mr. ———, and myſelf, none of which repreſent all the obſervations, confirm ſtill more that the motion of this ſtar cannot indeed be repreſented in a parabolic arc, as I have ſaid in my preceding memoir.

The ſame perſon who calculated the ſecond parabola tried to make the obſervations agree with a circle. His elements are:

Radius - - - -	2·74
Epoch 1801 - - -	$2^s\ \ 8^\circ\ 16'\ 20''$
☊ - - -	$2\ \ 20\ \ 15\ \ 0$
Inclination - -	$0\ \ 11\ \ 21\ \ 0$
Sidereal revolution - -	years $4\frac{1}{2}$.

In this circle the errors in exceſs amount to $2'\ 30''$, and thoſe in defect to as much.

Burckhardt calculated in an ellipſe, and found:

☊ - - -	$2^s\ 20^\circ\ 58'\ 30''$
Inclination - -	$0\ \ 10\ \ 47\ \ 0$
Aphelion - -	$2\ \ 8\ \ 59\ \ 37$
Paſſage of the aphelion Jan. 1801 -	1·3328
Eccentricity - - -	0·0364
Log. of the greater ſemi-axis -	0·4106586
Sidereal revolution - -	years 4·13

In this ellipſis the longitudes and latitudes of five obſervations are repreſented pretty well, there being only the difference of a few ſeconds between calculation and obſervation.

The aſtronomer who calculated the parabola and circle above mentioned, and whoſe name I was not able to make out, in the few leaves printed in German with which Oriani favoured me, ſuſpected that there might have been ſome error in the copy of my obſervations tranſmitted to him. This was really the caſe in the firſt; but I afterwards ſent another corrected copy, as well as to Lalande, Oriani, and Bode, all correſponding exactly with that on which I founded my own calculations.

However, that my exertions may correſpond with the intereſt generally taken in my diſcovery, whatever the reſult may be, and to leave no doubt in regard to the obfervations, I have again examined them; and in fome places which were rather uncertain, and which I employed in my firft calculations, I fubftituted others. I have alfo made an allowance for the deviation of the inftruments, and employed all thofe precautions which are ufual when great precifion is required. The refult of all thefe has been only a fmall difference in fome of the right afcenfions, which can have little or no influence on the ultimate refults of the calculation, and for which reafon I did not think great exactnefs neceffary in the firft reductions. According to this laft rigorous examination, 1·5″ are to be deducted from the firft four right afcenfions, and 1·5″ to be added to thofe of the 10th, 11th, 14th, 19th, 21ft, 23d, 28th, 30th, and 31ft of January, and the 1ft of February; and 3″ muft be taken from thofe of the 5th and 8th of February. With the tranfit inftrument the obfervations were made, different times, with all the five threads; and thefe I always preferred to thofe made with the circle. I ufed thofe by the circle when I failed with the tranfit inftrument; as on the firft four days; and on the 13th of January, when the obfervations were not made with all the five threads, I took a mean of the obfervations made with the circle, and another of thofe made with the tranfit inftrument. The difference between thofe with the former inftrument and thofe with the latter, was never greater than 0·2 in time, except on the 19th of January, when I found 1″ in time more by the circle. In regard to the declinations, I have found no corrections to be made. Should any aftronomer, for his greater fatisfaction, be defirous to fee the original obfervations, I fhall be happy to communicate them to him. They will be publifhed in the fixth book of the *Specola Aftronomica*, together with all my other obfervations made fince 1794.

XIII. *Reflections on the new Primary Planet fuppofed to exift between Mars and Jupiter, and now in all Probability difcovered. By Baron* VON ZACH, *Lieutenant-Colonel in the Service of the Duke of Saxe-Gotha, and Director of the Ducal Obfervatory at Seeberg* *.

THE exiftence of a primary planet, which, on account of the faintnefs of its light and fmall fize has hitherto remained undifcovered, was, as far as I know, firft conjectured, or at

* From *Monatlich Correfpondenz zur Beförderung der Erd-und Himmelfkunde*, June 1801.

least

least publicly mentioned, about forty years ago by the immortal Lambert. In his Cofmological Letters on the Univerfe, which appeared at Augfburg in 1761 *, the following remarkable paffage occurs towards the end of the firft letter: "And who knows whether fome planets may not be loft, from the wide fpace between Mars and Jupiter?" This idea was no doubt fuggefted to Lambert by comparing the different diftances of the planets; and he muft foon have found that the diftance between Mars and Jupiter is too great in proportion to the reft. To fill up this vacancy he placed in it a new primary planet; and as it had not been obferved during the courfe of two centuries, that is, fince the invention of the telefcope, he fuppofes it to have been torn from the powerful influence of the fun by a deftructive comet, round which it revolves in infinite fpace as a fatellite; and concludes with this obfervation: "Is the cafe the fame among the celeftial bodies as on the earth, that the ftronger exterminate the weaker; and, are Jupiter and Saturn deftined merely to prey upon others?"

What might tend more and more to confirm aftronomers in the idea of the exiftence of fuch a planet, was a certain relation which they had obferved in regard to the diftances of the fix primary planets from the fun, and which was confirmed, in an unexpected manner, by a feventh primary planet beyond the orbit of Saturn difcovered by Dr. Herfchel in 1781. This remarkable relation was firft adopted by profeffor Bode in the fecond edition of his Introduction to the Knowledge of the Starry Heavens, publifhed in 1772, from profeffor Tilius's tranflation of Bonnet's *Contemplation de la Nature*, Amft. 1764. To reprefent this relation by fmall numbers, which can be eafily compared if the diftance of Saturn from the fun be divided into 100 equal parts, the diftances of the planets from the fun in fuch parts will be as follows:

1. Mercury	-	4 fuch parts from the ☉
2. Venus	-	$4 + 3 = 7$
3. Earth	- -	$4 + 2.3 = 10$
4. Mars	- -	$4 + 2.2.3 = 16$
5. Hera or Juno		$4 + 2.2.2.3 = 28$
6. Jupiter	-	$4 + 2.2.2.2.3 = 52$
7. Saturn	- -	$4 + 2.2.2.2.2.3 = 100$
8. Uranus	-	$4 + 2.2.2.2.2.2.3 = 196$, &c. &c.

or more generally the n^{th} planet, reckoning from the fun, is

* A French tranflation of thefe letters, by Darquier of Touloufe, was printed at Amfterdam in 1801 by Hulft van Keulen, with notes refpecting the lateft difcoveries by J. M. C. Utenhove, of Utrecht. This edition was undertaken in confequence of my recommendation.

diftant

diſtant from that body $4 + \left(2^{n-2} \cdot 3\right)$: or, if we expreſs, as profeſſor Würm has done*, the mean diſtance of the firſt planet by a, the difference between the diſtance of the firſt and ſecond by b, and the mean diſtance of the earth from the ſun by 1, the mean diſtance of the n^{th} planet from the ſun will be $= a + \left(2^{n-2} \cdot b\right)$.

This law is founded on no known theory, at leaſt it has never yet been demonſtrated *mathematically*, and is merely deduced *empirically* from *analogical concluſions*. In no ſcience have the powers of the human mind, merely by mathematical reaſoning and the acuteneſs of geometrical reflection, produced ſo many, ſo certain, and ſo pure truths as in aſtronomy. When we conſider the immenſity and exalted nature of the objects on which this ſcience is employed, and the inſignificance of man and his terreſtrial habitation, together with the endleſs variety and concatenation of the celeſtial phænomena, which all take place in conſequence of one general very ſimple law of nature, that of gravity, diffuſed throughout the whole creation; and when we reflect what abſtruſe mathematical formulæ and methods muſt be invented to ſubject theſe ſo variouſly combined phænomena to calculation, and to obtain a uniform and continual correſpondence of theſe calculations with what actually takes place in the heavens, it muſt be acknowledged, that no ſcience does more honour to the human genius; that in no ſcience have more diſcoveries been made *à priori*; and that no ſcience is founded on more incontrovertible proofs than the exalted ſcience of aſtronomy†.

The mathematical aſtronomers, for there are ſome who are not ſo, do not readily adopt any thing that cannot be mathematically proved. However great, therefore, might be the probability that the above-mentioned relation, in regard to the diſtances of the planets, taking it at leaſt as an approximation, exiſted in nature, there were aſtronomers who doubted the conſequences of this undemonſtrated law, and the exiſtence of an inviſible planet between Mars and Jupiter.

* Berlin Aſtron. Jahre Buche, 1790, p. 168.

† La Place, that eminent analyſt, in the preface to his excellent work, ſpeaking of the progreſs of the modern chemiſtry, ſays: Et ce ne ſera qu'au moment où les loix des affinités chimiques ſeront ſuffiſamment obſervées, pour y appliquer l'analyſe, que cette ſcience (la chimie) aura le degré de perfection auquel l'aſtronomie s'eſt elevée par la decouverte de la gravitation univerſelle. *Théorie du Mouvement et de la Figure elliptique des Planètes*, 1784.

It

It is worthy of remark, that the aftronomers of no nation but Germany adopted this conjecture in their aftronomical books, or wrote any thing on the fubject. How is this to be accounted for? Does the fpirit of an eminent German, the fpirit of a Kepler, ftill hover over Germany? I do not here mean to affert that the Germans believed unconditionally in the exiftence of this planet, or acknowledged it as proved. Profeffor Bode has mentioned it in all his valuable works on aftronomy, and in all the editions of them publifhed fince 1772; but he always fpeaks of its exiftence as founded on conjecture or analogy, and not as a demonftrated truth.

About fixteen years ago I employed myfelf in analogical calculations of the elements of the orbit of this latent planet, as may be feen by the letters I wrote to profeffor Bode from Drefden, in September 1785, which were printed in the Berlin Aftronomical Almanac for 1789. But I fpoke of thefe refearches as mere *dreams*, and called my calculations *chimerical*. I even ridiculed them, and compared them to the refearches of the adepts who endeavour to find the art of gold-making. In the year 1798, when, in confequence of Lalande's vifit to Gotha, I had the pleafure of feeing my highly efteemed friend Bode, this fubject formed part of our converfation. That worthy veteran Lalande had no great faith in the exiftence of this planet; he has not faid a word of it in any of the three editions of his Aftronomy which have been publifhed: and I ufed to call thofe who entertained a ftrong belief of its exiftence, *aftronomical adepts*.

In the year 1787 profeffor Wurm was engaged with fimilar thoughts on the poffibility of other planets and comets exifting in our fyftem. Thefe ideas he communicated to the public in the Berlin Aftronomical Almanac for 1790, and in that of the following year, where he extended them to the fyftem of the fatellites. But he obferved at the fame time, that he was far from wifhing to make any one adopt his aftronomical *vifions*, as he exprefsly terms them.

Aftronomers had the greater reafon for being on their guard againft analogical conclufions, as the remarkable inftance of our great mafter may ferve to fhow that too much confidence muft not be placed in a lively imagination. Poets may be allowed fome latitude in this refpect; but we muft refufe the fame indulgence to a certain clafs of naturalifts and philofophers, who think they can catch nature in the flippery paths of myfticifm; who fubftitute an unintelligible jargon for a language generally underftood; and who attempt to explain a difficulty by obfcurity.

Kepler, who often indulged in fuch aftronomical dreams, and gave full fcope to his ardent imagination, thought he had made a great difcovery by fuppofing that the five regular bodies would fit exactly into the vacuity between the fix planets known at that time; and their diftances indeed, according to the lateft obfervations, correfpond very well with this rule. But unfortunately, as has been remarked by profeffor Wurm, Euclid and Nature left no regular body for Uranus; and I may add, that none remains for Hera: fo that Kepler's ingenious idea falls at once to the ground.

Should the queftion propofed on the difcovery of Uranus be here afked—Why was not this planet difcovered long ago? we might give the fame anfwer to it as given by Lichtenberg *, who confidered it of the fame kind as that of Lelio's fervant in Leffing's Treafure, who wifhed to know why his father had returned on a certain day, and not a year fooner or later; which he thought would have been much lefs incomprehenfible.

The moft natural way is, as profeffor Bode has done in his Illuftration of Aftronomy, to conjecture that this planet, being fmaller than Mars, and at a confiderable diftance beyond it, reflects too little light from its furface; and hence it has efcaped the keeneft-fighted obfervers. But who knows what the nature of its furface may be? We are acquainted with celeftial bodies which exhibit different colours, fhades of red and green; as for example Mars, and the double ftar γ in Andromeda, the light of which increafes and decreafes; and others which even difappear from our view.

Kant and Wunfch, in their Cofmological papers, affert therefore that this planet does not exift by itfelf, but that it is incorporated with Jupiter; which is therefore of greater fize than it ought to be according to the fuppofed rule, and confequently fupplies the place of two planets. Kant afcribes the fmall fize of Mars, and its want of fatellites, to the fame caufe. But this hypothefis was not neceffary to explain the invifibility of this planet, as it can be done in a much better manner, and more agreeable to the laws of nature. How long did Uranus remain concealed from our fight! And yet it was not only in the heavens, but, as we now know, was feen and obferved 20, 30, and 90 years before Herfchel's difcovery of it, by a French, a German, and an Englifh aftronomer. How then could profeffor Wunfch, in the fecond edition of his Cofmological Converfations, publifhed in 1791 †, confequently [illegible] years after the difcovery of Uranus, afk the followi[illegible] tion: "What kind of a body muft that be which, [illegible]

* Götting. Tafchenbuch, 1783.

† Vol. i. p. 599.

fo

ſo near, cannot be found by the beſt teleſcopes, though every ſmall ſpot in the heavens is every night examined with the greateſt diligence? But if this planet ſhould once be diſcovered, or if it be really diſcovered, it will be eaſily comprehended why, as a teleſcopic ſtar, it could have remained ſo long concealed among the innumerable multitude with which the heavens are covered *. Profeſſor Wunſch thinks, that as the ſatellites of Saturn and Uranus, which ſhine only with a pale faint light, can be ſeen by means of good teleſcopes, this concealed planet might alſo have been ſeen. But profeſſor Wunſch does not reflect, that to ſearch for an exceedingly ſmall moveable luminous point, that changes its place in the immenſity of the heavens, is much more difficult than to ſearch for a ſatellite, which always remains in the neighbourhood of its primary planet, and muſt even be found in the field of the obſerver's teleſcope. As profeſſor Wunſch is a good mathematician, he may calculate the probability and poſſibility of the diſcovery of a ſatellite and of ſuch a planet. It is poſſible that this planet, as was the caſe with Uranus, may have been ſeen ſeveral times; but it is alſo poſſible that it is not always viſible. For, ſince it appears only as a teleſcopic ſtar during the time of its neareſt approach to the earth, at its greateſt diſtance from us it may eſcape the beſt inſtruments and diſappear entirely; which renders the finding of it ſtill more difficult and uncertain.

All theſe impediments might in part have been foreſeen; and it was only by an accident, or ſyſtematic regulation, that the diſcovery of this planet amongſt the immenſe number of teleſcopic ſtars was poſſible. In the year 1787, when I undertook, at Gotha, a new reviſion of the ſtarry heavens, my view was to ſearch for this planet, to which I was particularly encouraged by the auguſt founder of the obſervatory of Gotha. I therefore confined myſelf merely to the zodiacal ſtars, and made a catalogue containing the right aſcenſion of theſe ſtars, under a conviction that it was poſſible by theſe means alone to fall upon this concealed planet.

In the autumn of laſt year, when I had the pleaſure of undertaking a ſhort aſtronomical tour to Celle, Bremen, and Lilienthal, and of ſpending a few agreeable weeks in the company of ſome of the ableſt German aſtronomers, theſe

* On the diſcovery of Uranus Lalande made the following reflection: Le nombre des etoiles de ſeptieme grandeur eſt ſi prodigieux qu'on auroit regardé comme impoſſible et inutile de les obſerver toutes, et à pluſieurs repriſes; cela étoit cependant néceſſaire pour ſavoir s'il n'y en avoit pas quelqu'un qui eut un mouvement. *Ephemer. des Nouv. obſ.* vol. viii. p. [illegible].—Lalande here alludes to ſtars of the 7th magnitude, but Hera appears as a ſtar of the 8th or 9th.

eminent

eminent men were of opinion that it was not poſſible for one or two aſtronomers to examine all the teleſcopic ſtars of the whole zodiac in ſearch of this concealed planet. Six aſtronomers then aſſembled at Lilienthal reſolved therefore, on the 21ſt of September 1800, to eſtabliſh a ſociety of twenty-four practical aſtronomers diſperſed throughout different parts of Europe, for the expreſs purpoſe of ſearching out this planet ſuppoſed to exiſt between Mars and Jupiter. They elected Mr. Schröter as their preſident; and I had the honour of being choſen their perpetual ſecretary. The plan of the ſociety was to divide the whole zodiac among the twenty-four members, each of whom was to have aſſigned to him, by lot, a zone of 15° of longitude, and of from 7 to 8° of north and ſouth latitude. Each member was to conſtruct a new celeſtial chart of his department, comprehending the ſmalleſt ſtars, and, by repeated examination of the heavens, to aſcertain the true ſtate of his diſtrict, and whether it contained that wandering body. By this inſtitution we entertained hopes that this ſtar, which had ſo long eluded our reſearches, would be at length traced out. In the name of the ſociety I wrote to ſeveral of the moſt celebrated practical aſtronomers in Europe, inviting them to concur in this general object; and almoſt all of them agreed to the invitation with great readineſs *. Some of the members, who have already begun their labours with great diligence, have tranſmitted, for our inſpection, very intereſting information; and though our infant ſociety may be deprived of the honour of firſt diſcovering this planet, not only is the ſuppoſed diſcoverer of it among the number of its members, though the uncertain ſtate of conveyance by poſt or by ſea, in conſequence of the war, has hitherto prevented us from tranſmitting our invitation to Palermo, but the ſociety has already contributed a great deal, and will ſtill contribute more, towards correcting our catalogues of the ſtars: and as this is not the only object of the ſociety in the extenſive province of aſtronomy, its labours, by being continued, muſt be of conſiderable utility.

In the month of February, this year (1801), I received a letter from Lalande at Paris, in which he informed me, that Piazzi, aſtronomer at Palermo, had diſcovered, on the 1ſt of January, a ſmall comet in the ſhoulder of Taurus; it appear-

* The only aſtronomers who begged to be excuſed were profeſſor Sniadecki of Cracow, and profeſſor Wurm of Blaubeuern. The former, becauſe, the univerſity of Cracow being ſuppreſſed, he was conſequently obliged to leave the obſervatory: the latter, on account of the want of inſtruments. But this difficulty was removed by princely munificence. The duke of Gotha ſent profeſſor Wurm a 7 feet reflector by Herſchel, and the ducheſs an aſtronomical clock.

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ed like a ſtar of the 8th or 9th magnitude, without any nebula or tail: but as nothing elſe was ſaid of its poſition or courſe, it was not poſſible to find it; and therefore, in expectation of more correct information, I paid no further attention to it.

In April I received a letter from Bode of Berlin, dated the 14th of that month, in which he told me that he had received a letter from Piazzi, dated Palermo, January 24, ſtating, that on the 1ſt of January he had diſcovered a ſmall comet in 51° 47′ right aſcenſion, and 16° 8′ north declination. On the 11th of January, from being retrograde, it had become direct; on the 23d, its right aſcenſion was 51° 46′, and its northern declination 17° 8′. He hoped he ſhould be able to obſerve it during the whole of February; it was very ſmall, like a ſtar of the 8th magnitude, and without any nebulous light. Profeſſor Bode added: "On reading Piazzi's letter, I was much ſtruck with the appearance and motion of this ſuppoſed comet; and I immediately wrote to him to ſend me the ſequel of his obſervations. In the mean time I cannot help mentioning that I have found, by a well-known and eaſy calculation, that the two obſervations of the 1ſt and 23d of January, and the ſtationary ſtate of the ſtar on the 11th of the ſame month, agree excellently with the ſuppoſition that it is not a comet, but perhaps the hitherto unknown planet between Mars and Jupiter, taking its diſtance at from 2·75 to 2·80. What do you ſay of it? It is much to be regretted that we have not a third obſervation. But, as the ſtationary ſtate agrees very well with the given places, it has become to me a matter of great importance. Send me a few words by the next poſt reſpecting your opinion. I may err, and wiſh for information; but the correſpondence is very remarkable. Do you know any more obſervations reſpecting this ſingular comet?"

On reading this letter, I immediately had recourſe to my old calculations of the years 1784 and 1785, and ſhowed to profeſſor Paſquich, who was preſent when I received the letter, that my elements of the orbit of this planet, calculated from analogy in the year 1785, and inſerted in the Berlin Aſtronomical Almanac for 1789, gave as its diſtance from the ſun 2·82, and as its period of revolution 4·74 years, or 4 years 9 months. Profeſſor Bode, from Piazzi's obſervations, had found the diſtance 2·75, and the revolution the ſame as I had deduced from analogy, viz. 4 years 9 months. I immediately ſent an anſwer to profeſſor Bode's letter, and informed him, that my two elements of the orbit of this ſo long concealed planet, calculated, proviſionally, ſixteen years before, amidſt my analogical dreams, and which I had depoſited in his hands in a ſealed note in October 1785 *, when I had the pleasure of forming a personal acquaintance with this worthy friend at Berlin, corresponded perfectly with his own, and consequently with those of Piazzi. I was therefore not only of opinion that the supposed comet might be the invisible planet so long sought for in vain, but found that Oriani of Milan, from whom I received a letter two days after, entertained the same opinion, that this star was the planet supposed to exist between Mars and Jupiter. Piazzi himself, even in January, (Piazzi's letter to Oriani is dated on the same day as that to Bode, viz. the 24th,) informed Oriani that he considered this star, which he first supposed to be a comet, as a real planet. The honour, therefore, not only of having first discovered this star, but that also of having first ascertained it to be a planet, cannot be refused to Piazzi; and one might almost suspect (though we can hardly ascribe such a motive to him) that he wished to retain to himself likewise the honour of having first calculated its orbit, as he communicated his observations in so sparing and imperfect a manner.

Professor Bode announced this discovery and conjecture to the Royal Academy of Sciences at Berlin, and caused it to be published in the Berlin Gazette of May 12, the Jena Literary Gazette of May 6, and in the Hamburgh paper of the 13th, from which it was copied into other journals.

Two days after I received professor Bode's letter, and before I could return an answer I received one from Oriani, dated April 7, in which he says: "I have received a letter from Piazzi at Palermo, which contains information highly worthy of your attention and of all astronomers. He writes me, that on the 1st of January 1801 he had observed a star, of the 8th or 9th magnitude, in the shoulder of Taurus. On the 2d of January he found this star had advanced *about* 3′ 30″ further towards the north, and about 4 min. towards o Aries. The two following days, the 3d and 4th, he found nearly the same motion. On the 5th, 6th, 7th, 8th, and 9th, the heavens were so obscured that he could make no observation. On the 10th and 11th he again saw the star; and afterwards on the 13th, 14th, 17th, 18th, 19th, 21st, 22d, and 23d of January. From the 10th to the 11th its motion, from being retrograde, had become direct. He adds, that on the first day of observation (Jan. 1st) its right ascension was 51° 47′, and its declination 16° 8′ north. On the 23d of January he found R. A. 51° 46′, and N. D. 17° 8′. He writes also, that he first announced this star as a comet; but, as he always

* These elements had been communicated also to the duke of Gotha, to count Bruhl at London, and Köhler at Dresden.

obferved it to be without any nebulous ring, and to have a very flow motion, he had feveral times conjectured it might be a planet. This letter, written on the 24th of January, was unfortunately 71 days on the road; it was therefore difficult, from the only two pofitions given by Piazzi, to conjecture the place of this wandering ftar after fo long a period. I have, however, endeavoured to take advantage of the circumftance, that on the 10th of January the ftar from being retrograde had become direct, and, on the fuppofition of a circular orbit, have found that its diftance from the fun muft be three femidiameters of the earth's orbit; fo that this ftar may be a new planet, the orbit of which will fall between thofe of Mars and Jupiter. There is reafon to think that the orbit of this planet, like thofe of all the reft, will have a perceptible eccentricity *, and confequently the hypothefis of a circular orbit, which I fuppofed, muft be improper for reprefenting its motion and geocentric place after fo long a period: we muft therefore wait for the remainder of Piazzi's obfervations, which he no doubt has continued. The heavens with us, fince the receipt of Piazzi's letter, have been always overcaft: you, perhaps, have a fky more favourable for aftronomical obfervations than ours †. In this perfuafion, I fend you my elements of the orbit, calculated, as you will fee, from thefe imperfect obfervations, by which you will be able to calculate the planet's place nearly. Heliocent. long. of the ftar at noon, Dec. 31, 1800, $2^s\ 6^\circ\ 54'$; heliocent. motion in long. in 100 days $18^\circ\ 19'$. Longitude of the afcending node, $3^s\ 8^\circ\ 32'$. Inclination of the orbit, $3^\circ\ 50'$. But, as already faid, thefe refults are fubject to great doubts; for they are founded only on two very imperfect obfervations, and on the very infufficient hypothefis of a circular orbit. I, however, flatter myfelf that this letter will foon reach you; and, before the ftar be loft in the fun's beams, you will perhaps be fo fortunate with your fuperior inftruments as to find it, and to be able to give me more correct information refpecting it."

Immediately after I received Oriani's letter, I calculated the place of the planet with its elements, and fearched for it in the heavens on feveral ferene evenings; but, unfortunately, the information had arrived too late. This fmall ftar had already advanced too near to the fun, fo as to be loft in its

* According to my *conjectural calculation*, a very great one, 0·14; the greateft next to that of Mercury. Time will fhow whether I was right.

† At the bottom of the Thuringian forefts, two hundred per cent. worfe than Milan! How badly my friend was informed refpecting our April weather! It is a common proverb at Gotha, that during the brighteft and fineft weather one muft not go abroad without a great coat or umbrella.

rays and the vapour of the horizon. I afterwards found that Oriani, in confequence of his great hafte and anxiety to communicate to me the intelligence as foon as poffible, muft have committed an error in the calculation of thefe preliminary elements, particularly in the ☊, and in the inclination of the orbit. But even had there been no error I fhould not have found this fmall wandering ftar; for when I received the information the obfcurity was too great, and the ftar too near the horizon. Profeffor Bode alfo, as he informed me in a letter dated May 12, had fearched for it feveral evenings, but in vain.

As no hope now remained of being able to obferve this fingular and remarkable body till its return from the fun in Auguft or September, I made a calculation of its orbit as well as poffible from fo imperfect and fcanty obfervations, not with a view of finding it again in the courfe of two or three months (for before that time I hoped Piazzi, by continuing his obfervations, would give us better and more determinate data and conclufions), but merely that I might have by me fome conjectures to enable me with better grounds to believe in the actual exiftence of a planet between Mars and Jupiter.

The obfervations given by Piazzi for calculating the orbit are partly imperfect and partly infufficient. 1ft, His two obfervations which are known, are only for minutes, and announced as *nearly*. 2d, For calculating the orbit of a planet or a comet, *three* obfervations at leaft are neceffary. 3d, The times of the obfervations are not affigned. In regard to the firft, we may admit, that at leaft the *neareft* minutes of the obfervation are given right. In regard to the fecond difficulty, Piazzi perhaps, as already faid, may have purpofely withheld the *third* obfervation, that he might calculate firft himfelf the orbit of this planet *; for he confidered it as fuch before the 24th of January. But if this were the cafe, he betrayed, in fome meafure, his having made a third obfervation; as he mentions that the planet from the 10th to the 11th of January was ftationary. Oriani and Bode took advantage of this circumftance, and I employed it myfelf to calculate an approximate orbit of this planet from thefe fcanty obfervations. The third difficulty was obviated by the following conjectures:

Piazzi, as is well known, is employed in the conftruction of

* The celebrated French aftronomer J. N. de l'Ifle went ftill further. Having difcovered, at the obfervatory of Paris, the long announced, long expected comet of 1759, he kept it a long time fecret, obferved it in private, and ordered his pupil Meffier to fay nothing of his difcovery.

of a large celestial chart *. He had also, in part, the good fortune which I hoped to enjoy while preparing my catalogue of the stars, and with the view of which the Lilienthal Astronomical Society was formed; and the planet would certainly not have escaped them, even if it had not been discovered by Piazzi †. Piazzi is furnished at his observatory with an excellent transit instrument, and a complete meridian circle by Ramsden, by means of which he no doubt fell upon this small wandering star. This star must have culminated on the 1st of January, the day when discovered, about nine in the evening. At that time of the year and hour it is perfect night at Palermo; and therefore Piazzi could very well observe this small star, of the 8th or ninth magnitude, with a day telescope. It was, however, full moon; and as the moon had risen three hours above the horizon of Palermo, the night must have been very bright. But the moon was four hours, or about 68° distant from the star; and this circumstance alone converts my conjecture into certainty, that Piazzi must have found this supposed comet not with a comet-searcher, but with his meridian instrument. The above-mentioned right ascensions of the wandering star, converted into time, were consequently the time of its culmination in sidereal time: these I again converted into mean solar time, and thus brought out the true moments of the observations of the star. By these, and the obliquity of the ecliptic 23° 28′ 10″, I have obtained the following data for calculating the orbit:

1801.	Mean Time at Palermo.	Geocent. Long. of the Plan.	Geocent. Lat. South.	Place of ☉ + 20″	Log. Dist. ☉ ⊕
Jan. 1st	8h 43′ 15″	1s 23° 29′ 40″	2° 37′ 5″	9s 11° 1′ 40″	9·9926158
23d	7 16 41	1 23 43 40	1 38 50	10 3 22 28	9·0932251

As the star on the 10th of January became stationary, I found its elongation for that time 7s 26° 41′ 41″, and, by means of Keil's proposition, that the tangent of the elongation is equal to the semidiameter of the orbit divided by the square root of that semidiameter, + 1. I found this semidiameter = 3·071, and, by Kepler's principle, the revolution

* Last year he published his large catalogue, comprehending 5500 stars.

† It was really fortunate that Piazzi discovered his star on this day; for, had he fallen upon it eight days later, it is possible it might have escaped him; for, as this wandering star about that time became stationary, he would not, from the observations of next day, have remarked its motion. It is very singular that this new planet, if its being such be really confirmed, was discovered and seen in the shoulder of Taurus, as Uranus was 20 and 110 years ago.

$(3{\cdot}071)^{\frac{3}{2}}$

$(3{\cdot}071)^{\frac{3}{2}} = 5{\cdot}3817$ years. By the usual methods I obtained the following approximate elements of the orbit:

Epoch of the mean heliocentric longitude for the commencement of 1801 - -	2^s	6°	$55'$	$40''$
Long. of the ascending node - - -	2	22	0	50
Mean annual motion - - -	2	6	54	25
Inclination of the orbit - - -		7	47	40
Semidiameter of the orbit 3·071				
Synodical revolution - 5·3806				

Those who attentively consider and compare these elements will no doubt remark, that the distance and periodical time of Piazzi's star are exactly the same as those of the celebrated comet of 1770, the calculation of which gave so much employment to astronomers, and the orbit of which Lexel could no otherwise represent than in an ellipse of $5\frac{1}{2}$ years. Pingré also found its distance = 3·09, and the period of revolution 5·4 years *. Burckhardt, who lately obtained the second prize proposed on this subject by the National Institute, could not reduce this singular body from new observations into any other than an elliptical orbit of $5\frac{1}{2}$ years. Was the comet of 1770 the so long concealed planet? or was Piazzi's star the comet of 1770? In either case, whether it was a comet or planet, Why was it not oftener seen, or before discovered? The causes, some of which we have already mentioned, may have been various. But to confine myself to a probable way of explaining this point, let the reader only look into Schröter's Observations on the Comet of 1799, published in the third volume of his Collections in regard to the latest Astronomical Discoveries †. The most singular phænomena of the accidental variable modifications of the nebulous light surrounding the comet, and also the well-known atmospheres of the planets, and even the sun, will be found there explained. In the nebulous light which surrounded the comet of 1799, Mr. Schröter observed not only occasional, but sudden and disproportionably great changes both in the extent and strength of the photosphere. Dr. Herschel not long ago presented to the Royal Society a paper on the structure of the sun, in which this eminent astronomer explains the solar spots by supposing an elastic and not luminous gas disengaged from the opake surface of the sun's body, which divides the surrounding luminous fluid or photosphere, by which means we then see dark parts of the body, which we consider as spots.

Is it not therefore possible that we see stars at one time

* Cometographie, part ii. p. 89.

† Allgem. Geograph. Ephemerid. vol. iii. p. 432.

which

which diſappear at another? We are acquainted with many which have periodical changes of light of greater or longer duration; as, for example, thoſe numerous changeable ſtars called *Stellæ mirabiles.* What has become of the celebrated Tycho Brahe's ſtar in Caſſiopeia of the year 1572, which at one time ſhone as bright as Sirius, and even exceeded Venus and Jupiter when in their perigeum; ſo that it could be ſeen in the day with the naked eye; ſince which time not the ſmalleſt traces of it have been found? We learn by the old chronicles, that in the time of the emperor Otho I. about the year 945, and alſo in 1264 *, a new and motionleſs ſtar appeared between Caſſiopeia and Cepheus. Some aſtronomers, therefore, conjectured that this might be the ſtar of 1572, which appeared periodically in the courſe of about 300 years †. Had it any affinity with the celebrated ſtar diſcovered by Kepler in 1604 in the foot of Serpentarius, which ſeemed at one time ſo bright that it exceeded in ſplendour ſtars of the firſt magnitude, became inviſible next year, and has never ſince appeared? What are Herſchel's planetary nebulæ? Did not Schröter ‡ find that whole tracts of luminous nebulæ diſappeared from the heavens? What are our large fire-balls, which move with a velocity a hundred times greater perhaps than that of a cannon bullet, emit a bright light, burſt, and then diſappear? It is proved that they are bodies belonging to the univerſe, and not to our atmoſphere. This was the opinion of Montanari § in 1676, who, in his *Fiamma volante* ||, calculates that one of theſe fire-balls, ſeen throughout all Italy in the above year, was at the height of forty Italian miles. All our readers muſt remember the large fire-ball ſeen in the year 1783 in every part of Europe, and pretty well obſerved. It was calculated by ſome of the Engliſh aſtronomers that the height of this immenſe body of fire was 60 Engliſh miles, and that its greateſt diameter was 1 1/8th mile. In ſeven minutes it would have paſſed over a ſpace equal to the ſemidiameter of the earth

Why may not comets appear ſometimes luminous and at

* Leovit. apud Tychon. lib. i. p. 441. Licet. lib. v. cap. 12.

† See Laplace's hypotheſis on this ſubject in his *Expoſit. du Syſtème du Monde*, liv. v. p. 347. an. 7. the only real edition in quarto acknowledged by the author.

‡ Beytrage, p. 232.

§ Halley and Hevelius conſidered them alſo as coſmical bodies.

|| La Fiamma volante, gran Meteora veduta ſopra l'Italia la ſera de 31 Marzo 1676, Speculazioni fiſiche et aſtronomiche eſpreſſe dal Dott. Geminiano Montanari ..... in una Lettera all, Ill. Ecc. Sgr. Marcheſe Federico Gonzaga, Bologna 1676. 95 pages quarto: a very ſcarce work, for which I am indebted to the kindneſs of profeſſor Blumenbach, and which Dr. Chladni, who has collected on this ſubject with ſo much care, was never able to obtain.

others

others dark? The comet of 1770, therefore, might exist sometimes in an opake, and sometimes in a phosphorescent state; and hence perhaps, and from the perturbative power of the larger and more dense bodies, the unfrequency of their return may be explained. They come back, and we do not see them; they are present, and we do not observe them. To draw conclusions, from the identity of the elements of the orbit of a body, respecting the identity of the body itself, gives, as all astronomers know, but a high degree of probability *, among the ninety-one comets, confirmed by only one hypothesis †, which certainly may be true taken *directly* but not *inversely*. Two comets, however, whose orbits have not the identical same elements, may be one and the same body. This, as far as I know, has never been explicitly asserted by any one: but Lexel mentioned it implicitly, when the following objection was made to him on account of the five years elliptical orbit of the comet of 1770: Since the period of the revolution of this comet is so short, why was it not seen oftener, and long ago? Lexel was of opinion, and perhaps so was the great Euler his master, under whose inspection he laboured, that the influence and perturbation of the large body of Jupiter, to which this comet approached very near on the 27th of May 1767, and the 23d of August 1779, might have entirely changed its orbit ‡. Burckhardt was of the same opinion in his memoir written on the subject of the prize already mentioned.

But how frightful is the mere idea of calculating the perturbation of such a body! Would not such a calculation exceed the powers of our analysis? The difficult theory of the moon would be mere elementary calculation compared with the variable orbit of such a body. It were to be wished that so striking a phænomenon would produce the necessity of giving to our calculation, in regard to perturbation, a new direction, that the theory of so complex an approximation might be more improved, and that the influence of the successive integrations on neglected quantities might be better and more accurately determined. But the geometrician, who can represent all the co-ordinates of the motion of each celestial body in speedily approximating series of sines and cosines of the angle depending on its real motion, does not, perhaps, exist on our earth.

* Monat. Correspond. vol. iii. p. 414

† The so called Halley's comet, the fifth and last observed return of which took place in 1759, and which will appear again in 1835.

‡ "M. Lexel pense, que son orbite peut avoir été *totalement* changée par l'action de Jupiter." Pingré Cometograph. part ii. p. 90. See also Mém. de Paris 1776. p. 648.

Many no doubt will confider the tail which, according to Meffier's exprefs obfervation *, attended the comet of 1770, as a proof that it could not be a planet. But, is it proved that planets cannot have a tail? We have planets with fatellites, and others without thefe attendants; why therefore fhould there not be planets with tails, to prove, in a vifible manner, that they are nothing elfe than planets? This difference in the appellation originated in the periods of ignorance, and muft now be admitted into our language to diftinguifh thofe bodies, the appearance of which is of fhorter duration, and which do not remain vifible throughout their whole orbit †, from thofe which are always vifible, except when they approach too near to the fun. The circumftance which feems to be peculiar only to comets, that fome of them move retrograde, while all the planets move direct, is only apparent. The reader is referred to the explanation which Laplace and Lalande have given of this point in regard to the retrograde fatellites of Uranus ‡. Lalande fays the word *retrograde* impofes by its expreffion, but in reality is nothing. Kant, in his *Allg. Nat. und Theorie des Himmels*, original edition, publifhed in 1755, conjectured that the retrograde motion of fome comets might be only an optical illufion, like that of the geocentric motion of the planets.

Every newly difcovered object muft have a new appellation. Though a name in itfelf is of no great importance, we have feen in regard to Uranus how difficult it is for all mankind, from the Thames to the Neva, to be unanimous in this refpect. If the ftar lately difcovered by Piazzi be really the fuppofed planet between Mars and Jupiter, a great and auguft patron of aftronomy, the founder of the obfervatory of Seeberg, gave it, in my opinion, a very appropriate name fifteen years ago. Uranus has afforded us a ftrong right, on the fcore of uniformity, to affign to this new planet, as has been the cafe with the old ones, a name borrowed from the heathen mythology. The duke of Gotha propofed, therefore, that of *Hera*, Ἥρα, or of the deity ftyled by the Romans Juno, the daughter of Saturn, and the fifter and wife of Jupiter §: Jupiter therefore would have his father and grandfather above him, and his wife and children below him.

* Mém. de Paris 1776. p. 597.

† The new planet, perhaps, is not vifible in apogeum, as already obferved.

‡ Allgem. Geograph. Ephemerid. vol. ii. p. 170 and 259. See alfo Laplace's *Expofit. du Syftème du Monde*, p. 342.

§ Called alfo *Saturnia*. The duke at firft propofed *Rhea*, the wife of Saturn; but this name has already been applied to the Earth.

The

The Greek name *Hera* is to be preferred to the Latin name Juno, 1ſt, Becauſe the latter has been already applied to the planet Venus. Pliny ſays *, "Infra ſolem ambit ingens ſidus, appellatum Veneris . . . . Alii enim *Junonis*, alii *Iſidis*, alii matris Deûm appellavere." St. Auguſtine † calls Venus, Stellam *Junonis*; and Apuleius ‡ ſays, "*Junonia*, immo Veneris ſtella cenſetur." *Hera* is always involved in clouds, and our planet kept itſelf a long time concealed. This name, therefore, will ſtill be appropriate even if the new ſtar ſhould not be the ſuppoſed planet: in that caſe, inſtead of the deity we have embraced a cloud §. 2d, *Hera* is at the ſame time the name of a city in Sicily, by which means the diſcovery made on that iſland, and the celebrated name of the diſcoverer of this eighth primary planet, will be eternized and preſerved as long as tradition and hiſtory exiſt on the earth. The city of *Hera*, ſituated, as well as Palermo, on the coaſt of Sicily, was called alſo *Hybla Minor*, and is the ſame mentioned by Pauſanias, Cicero in his Letters to Atticus, and Antoninus in his Itinerary. In the laſt place, *Hera* is the mother of Vulcan, who has his workſhop in the burning mountain of Ætna in that iſland.

The objection already made in regard to Uranus, that all the planets have Roman and not Greek names, muſt be of leſs importance, as the Greek name Οὐρανὸς has been retained to that diſcovered by Herſchel, though it would have been more agreeable to analogy to have called it *Cœlus*. All the old planets, the diſcovery of which is loſt in the obſcurity of time, may retain their Latin names; but the new planets, the hiſtory of the diſcovery of which will be handed down to the lateſt poſterity, with the names of the diſcoverers, ought, by way of diſtinction, to have Greek names. What ſeems here an interruption of analogy will be only harmony. Since the creation, like the Creator, has no bounds, ſhould another planet be diſcovered beyond Uranus, its hieroglyphic appellation ought to be Greek.

It will be neceſſary alſo to invent an appropriate character for this planet. To a new planet we may aſſign the character of a new metal. This idea has been followed in regard to Uranus; but we have thereby committed an error, or rather eſtabliſhed a monument of our ignorance reſpecting the component parts of platina. It was a much better propoſal to diſtinguiſh this planet by ♅, the under part of which repre-

* Hiſt. Nat. lib. ii. cap. 6.

† De Civitat. Dei, lib. vii. cap. 15.

‡ De Mundo, p. 252. edit. Bipont.

§ Nubes et inania captare.

fents a planet, and the upper a fixed ftar: it fhould, indeed, denote a fixed ftar become a planet. But as this character has been already introduced in the *Vienna Ephemerides*, in order to prevent mifconception it ought to be inverted. The characters of the Earth ♁, and Venus ♀, have the fame affinity, as well as thofe of Mars ♂ and Uranus ♅.

POSTSCRIPT.

While the laft fheet of this paper was printing we received from our worthy friend Dr. Olbers, of Bremen, the following elements of the orbit of Hera, which he calculated in a circle from the obfervations communicated to him by Piazzi:

Semidiameter of the orbit	- -	2·951
Longitude of the afcending ☊	-	$2^s\ 21°\ 55'$
Inclination of the orbit	- -	7 56
Heliocent. long. Jan. 1ft, 1801	-	2 7 45
Sidereal revolution	-	1851·6 days = 5·0694 years.
Daily heliocent. motion	- -	$11'\ 39{\cdot}95''$
Yearly motion	- - -	$71°\ 1'$

This orbit is very infufficient, as it is calculated on the hypothefis of a circle: the very imperfect obfervations were only 22 days diftant from each other; and, as Dr. Olbers very well obferves, the lines of fight do not lie advantageoufly. Dr. Olbers is alfo of opinion that thefe elements are not fufficient to calculate a planet fo long before, in order to be able to find it again on its reappearance in the morning in Auguft. In the month of Auguft it will pafs through the whole fign of Cancer, and muft be fought for in the equinoctial point. From the 1ft to the 31ft of Auguft, a zone of the heavens from 115° to 130° of right afcenfion, and from 22° to 24° north declination, muft be carefully fearched; for this will be the diftrict of the heavens where the planet will, in all probability, be found in that month.

In the oldeft periods Latin verfes have been invented to denote the order of the planets in regard to their diftance from the fun. Thus, for example, we have the old well-known verfes:

> Saturni atque Jovis Sidus, Mars, Sol, Venus alma,
> Mercurius, claudit ultima Luna chorum.

On the difcovery of Herfchel's planet, Poinfinet de Sivry wifhed to have it called *Cybèle* or *Cybelle*, after the wife of Saturn, the neareft planet below it: and expreffed the order of the feven planets in the following three Latin verfes:

> Ambit Solem Hermes, Venus hunc, mox Terra, Diana,
> Mars fequitur. Pergit rex Jupiter. Hunc Saturnus.
> Omnes hos orbes amplectitur alma Cubelle.

One

One of my friends, poſſeſſed of a poetical vein, has expreſſed the order of the planets, now increaſed to eight, in the following four lines, and not unhappily:

Mercurius primus: Venus altera: Terra deinde,
Mars poſthac: quintam ſedem ſibi vindicat Hera.
Jupiter hanc ultra eſt. Sequitur Saturnus: at illum
Uranus egreditur, non auſim dicere ſummus.

XIV. *Some Particulars reſpecting the new Planet* Ceres Ferdinandea.

In the two preceding papers our readers are put in poſſeſſion of every thing relative to the diſcovery of this planet. The following are ſome of the obſerved places of the Ceres Ferdinandea, upon its being rediſcovered lately, and the times noted are mean time.

1801.

Dec. 7. Obſerved by Dr. Zach of Saxe Gotha, under ſome uncertainty of its being the planet.

		M. T.	R. A.	Dec. N.
		at 18^h 48′ 10″	178° 33′ 31″	11° 41′ 30″

1802.

Jan. 5. By Dr. Olbers, at Bremen,

		17 30	185 43	11 8 0

11. Again, by Zach,

		at 17 3 17	186 45 50	11 10 0

26. By Mechain, at Paris,

		at 16 10 48	188 24 49	12 0 43

Feb. 3. By the aſtronomer royal, Greenwich,

		at 16 11 15	188 43 0	12 39 0
	4.	at 17 25 46	188 43 0	12 44 0
	12.	at 15 4 12	188 30 0	13 33 0

On the 7th of February and ſubſequent days, it was obſerved by Dr. Herſchel at Slough, and alſo by Alex. Aubert, eſq. at his obſervatory at Highbury Houſe.

The ſtate of the weather at Slough did not admit of Doctor Herſchel's ſeeing the new planet till Monday night the 8th current, he having previouſly received diſtinct notice as to its place from his friend Dr. Maſkelyne, the aſtronomer royal, who for the firſt time obſerved it, like a ſtar of the eighth magnitude, on the 4th current, in the morning. On the 9th and 10th of February at night, Dr. Herſchel again traced the planet, and perceived its motion. It was not however till the morning of the ſucceeding day, that through a much clearer air, and at a more favourable altitude, with leſs obſtruction

obstruction from the moon-light, he obtained a distinct view of it for a short time. Through his ten feet reflector, with a magnifying power of six hundred, and higher powers, he perceived its disc, though very small, yet distinctly round and well defined; but saw nothing further which denoted a ring or a coma, or a satellite. The favourable state of the air appearing then very precarious, Dr. Herschel did not attempt to ascertain the apparent diameter of the planet by means of his lamp-micrometer, as some preparations and adjustments would have been necessary before he could have availed himself of that curious apparatus. But in order to form some estimate as to a point of so much importance, he adopted a happy expedient, and very suitable to the urgency of the moment. The Georgian planet being situated near at hand, in the same region of the heavens, he directed his telescope first to it, and then to the other, with his attention fixed upon making a comparative estimate of the apparent diameter of each disc. In order to this, and to avoid as much as possible certain fallacies to which this method is more or less liable, he was careful to form estimates over and over again, according as his telescope was last turned from the greater disc to the smaller; and *vice versa*. From such observations, frequently repeated, he concludes that the apparent diameter of the *Ceres Ferdinandea* is about a fourth part only of the apparent diameter of the Georgium Sidus. By applying therefore the proper calculation, Dr. Herschel has inferred that the real diameter of this newly found primary planet, called *Ceres Ferdinandea*, is only a little more than half of the diameter of our moon and less than 5-8ths of it. The smallness of the planet, together with the great inclination of its orbit, are peculiarities which may probably lead to other discoveries in the solar system.

The following are some places of the planet calculated forward in foreign journals, but corrected by the latest observations, showing nearly where the planet may be expected to appear: computed for 12^h or 15^h Greenwich time.

1802.	R. A.	Dec. N.
Feb. 17.	188° 15′	14° 8′
23.	187 39	14 49
March 1.	186 50	15 31

The planet will be in opposition to the sun about the 13th of March.

The following are the elements of the planet as calculated by M. Gauss, of Brunswick, for an elliptic orbit.

Epoch 1801, Jan. 0, or Jan. 1, which of the two is uncertain - - - - 2^s 17° 36′ 34″

Aphelium

Aphelium - - -	10^s	$26°$	$27'$	$38''$
Ascending node - - -	2	21	0	44
Inclination of orbit - -	0	10	36	57

Eccentricity 0,0825017 to its mean distance unity.
Mean distance from the sun 2·7673.
Mean diurnal heliocentric and tropical motion, say, $12'$ $50.914''$.

Periodic time 1681 days, or 4 years 7 months.

Baron Von Zach, director of the observatory of Gotha, writes as follows to C. Mechain, administrator of the observatory of Paris:

"M. Schröter of Lilienthal has seen, with his large telescopes, the new planet Ceres, under a disc of nearly $2''$. He suspects that it has two satellites. The planet is enveloped in a very thick atmosphere, for it appears to be surrounded with much nebulosity. I am very anxious to learn what Dr. Herschel will tell us respecting it: in the mean time I thought it my duty to write you this in haste."

Elliptic elements of the new planet, corrected by M. Gauss from his last observations:
Diurnal tropical heliocentric motion $770·7376''$.
Tropical revolution, $1681^d\ 12^h\ 9^m$.

It is a curious circumstance that the discovery of this planet has been long expected, and even in some measure predicted. Professor Bode, of Berlin, in his *Kurzer Entwurf der Astronomischen Wissenchaften*, Berlin 1794, § 387, has the following passage:

"Is it probable that Uranus, or the Georgian planet, is really situated at the utmost limit of our solar world? This appears to be very doubtful, considering the immense space interposed between it and the nearest fixed stars. Other planets perhaps may be still more remotely situated, and may perform their revolutions unseen by human eyes. We can scarcely suppose that any planet exists nearer to the sun than Mercury: but considering the proportions of the distances of the planets from the sun, we observe between Mars and Jupiter, a distance far greater than a comparison of the other distances would lead us to expect, and this space may perhaps be occupied by a planet yet unknown."

Similar ideas seem to have been entertained in this country, even long anterior to the conjectures of Lambert and the German astronomers, as appears by the following, which is given as a note in a work lately published on astronomy, by Mr. Olinthus Gregory, teacher of mathematics, Cambridge.

"Mr. Maclaurin and other philosophers expected nearly one hundred years ago, that such a discovery as this of

6 Piazzi

Piazzi would be made by ſome diligent aſtronomer; and the opinion has been lately revived by Mr. Capel Lofft, a gentleman well known for his attachment both to the ſciences and the muſes. In the New London Review for March 1800, this gentleman, in a critique on the Athenian Letters, ventured to offer ſome conjectures reſpecting an intermediate planet between Mars and Jupiter, the coincidence of which with the new diſcovery is very remarkable. He ſuppoſed that the diſtance of the intermediate planet from the Sun would be to that of Mars, either as 33 to 15, or as 20 to 15, the midway between which correſponds nearly with the fact. With reſpect to its diameter, he conceived it might be to that of Mars, as that of Mars to the diameter of the earth; and then being not much more than half the diameter of Mars, and at five times the perigean diſtance, it would be ſeen from the Earth under an angle of 2½″ or 3″, while Georgium Sidus would appear under an angle of 4″. Theſe lucky conjectures were drawn from a certain kind of *Pythagorean harmony*, and are ingeniouſly defended in the Review juſt mentioned."

XV. *Deſcription of a new-invented Aſtronomical Inſtrument, for placing Globes in a proper Situation, by means of the Sun, without the Help of a Magnetic Compaſs, or other Inſtrument.* By Mr. B. M. Forster *.

It conſiſts of a braſs circle, C,C, (Plate II. fig. 2 and 3.) made to fit the ſurface of a twelve-inch globe. Acroſs this circle is a braſs bar, B, B, arched on the under part, to fit the curve of the globe, and flat at the top. The ſides of this bar are graduated, anſwering to ten degrees of the ecliptic; and the under part is chamfered ſo, that the edge may be very narrow which lies over the ecliptic, when in uſe. On the flat top of the bar is a ſmall hole, with a metal pin S, ſerving as a *ſtyle* to caſt a ſhadow. The part which receives the ſhadow is painted a dead-white, to prevent the dazzling which the braſs alone would have occaſioned.

This little inſtrument, which may be called a ſolar ſtyle, was made under my direction by Mr. Blunt of Cornhill.

Method of uſing the Inſtrument.

Find the ſun's place in the ecliptic, by an almanac, (or on the wooden horizon of the globe, if marked there,) then place the inſtrument in ſuch a manner on the ſurface of the globe that the pin or *ſtyle* be exactly over that place on the ecliptic. Having done this, turn the globe into ſuch a

* Communicated by the author.

 ſituation

ſituation that the *ſtyle* ſhall caſt no ſhadow: when this is the caſe the globe will be properly placed; or, as it is uſually called, rectified for the time of day.

This inſtrument may alſo be uſed for taking diſtances of places or ſtars, on different parts of the globe, which are not more than ten degrees aſunder.

XVI. *A few Particulars reſpecting Mr.* BOAZ'S *Patent Telegraph*.*

THIS Telegraph exhibits the alphabetic or other characters, or ſignals, by means of lamps rendered viſible and inviſible by a mechaniſm ſimple in its conſtruction and immediate in its effects, as alſo by boards or other opaque bodies, which may, in like manner, be rendered viſible or inviſible at pleaſure, by alternately preſenting their fronts or their edges to the eye. Or by lamps and boards ſo combined together, that both or either may be uſed: but perhaps the modes eaſieſt in fitting up and application are the following, viz.;

Twenty-five lamps are placed in a frame at equal diſtances from each other, as repreſented by the white dots in fig. 1, No. 1, (plate III) each having a moveable cover, or blind, fitted to it, in ſuch a manner that, when they fall down, (which they do by their own weight) the lamps, or rather the flames of the lamps, are intercepted and rendered inviſible, ſo preſenting a mere blank to the eye placed in front of the frame, as in fig. 2; No. 1.

Another frame is provided having grooves cut in it: and for each letter of the alphabet or other ſign wiſhed to be exhibited, an oblong piece of wood is alſo provided, made ſo as to ſlide backward and forward in theſe grooves, having an alphabetical letter or other ſign, conſpicuouſly marked on the outer end of it. Any one of theſe pieces of wood, by being puſhed in or drawn out, pulls *all at once* from before the lamps, or lets down (by means of ropes or wires) as many lamp-covers or blinds as are neceſſary to exhibit that particular letter or ſign marked on that particular piece of wood, ſo puſhed in or drawn out. The letters of the alphabet ſo exhibited, are repreſented by figures 3 to 28, No. 1.

The ſame thing, however, may be accompliſhed by means of nine lamps only. Fig. 1, No. 2, repreſents the nine lamps, rendered viſible by having their covers or blinds removed as mentioned above. Fig. 2, No. 2, repreſents the whole covered. Figs. 3 to 28, No. 2, repreſent the whole

* Communicated by Mr. Boaz.

alphabet.

alphabet. The letters are perhaps lefs diftinct than thofe produced by means of twenty-five lamps; but this fpecies of Telegraph comes cheaper, and is eafier fitted up: and although the alphabetical letters reprefented by it appear at firft fight imperfect, yet, after having feen them twice or thrice, they will always after be eafily recognifed by the intelligent mariner or foldier, and may, even at firft fight, be read by one quite unacquainted with the invention. With twenty-five lamps, *capital* letters are exhibited: with nine lamps, *fmall* letters muft fometimes be fhown.

Forms of Signals fuggefted by the Patentee.

As only one letter appears vifible at a time, to divide thefe letters therefore into words, a fign is requifite; for which purpofe fig. 29, No. 1 or 2, in the refpective Telegraphs, is exhibited.

When a veffel approaches another, and wifhes to fpeak the ftranger, fig. 30, No. 1 or 2, is exhibited, being as much as to fay "I wifh to fpeak you."

When the ftrange veffel fees this, fhe fhows fig. 31, No. 1 or 2, as much as to fay "I perceive you wifh to fpeak me, am ready to hear you;" that is, to mark down the letters you exhibit. When the veffel has fpoke, the ftranger, in her turn, exhibits fig. 30, No. 1 or 2, and replies.

Fig. 32, No. 1 or 2, reprefents a fign which the captain of the Eliza throws up, fhould he wifh to fpeak the captain of the James, *without what he fays being known to the crews of either veffel.* For this purpofe two circular flat rings of wood or metal may be put in a box, the one moveable on its centre, the other fixed. On one of thefe rings let the twenty-fix letters of the alphabet be engraven, painted or written, at equal diftances from each other, *progreffively as they occur in the alphabet.* On the other ring let thefe characters be engraven, painted or written, *but irregularly.* Let each captain be poffeffed of a box as above, fimilar in every refpect.

When the captain of the Eliza throws up fignal marked fig. 32, No. 1 or 2, and immediately thereafter the letter G, this fhows he wifhes to fpeak privately to the captain of the James, on this letter (or key, as it may properly be called). On feeing this, the latter turns round the moveable ring in his private fignal-box, until the two G's are over againft or clofe to each other, as in the figure annexed; (the white circle with black letters reprefenting the moveable ring;) it being underftood that the captain of the Eliza has previoufly done the fame thing in his private fignal-box.

Every letter the Eliza's captain exhibits is taken down and deciphered or tranſpoſed by the private ſignal-box of the other. Thus, making a ſuppoſition that the arrangement of the alphabetical characters is as in the annexed repreſentation of the ſignal-box; if one exhibits the letters *drmw—ub—irpqr*, when tranſpoſed they expreſs the words *news of peace*; the letters ſo exhibited being divided into words, by fig. 29, No. 1 or 2, as above mentioned.

Fig. 33, No. 1 or 2, ſignifies a contraction of the conjunction *and*.

Fig. 34, No. 1 or 2, an apoſtrophe, neceſſary in writing French.

Fig. 35, No. 1 or 2, as much as to ſay, "I am in diſtreſs," or "I did not underſtand your laſt ſignal or ſentence, I pray you ſpeak again."

Fig. 36, No. 1 or 2, a period.

Fig. 37, No. 1 or 2, a final ſtop.

Or both 36 and 37, No. 1, may be given at once, ſignifying "I have finiſhed what I intended to ſay, you have now an opportunity to ſpeak without confuſing me."

Any two or more ſigns or characters can be exhibited at once, thus increaſing the number of appearances, which may be uſed as different ſignals.

Fig. 38. a ſalutation when the veſſels part, q. d. "farewell."

Figs. 39 to 48, Nos. 1 and 2, exhibit the numerical figures neceſſary to tell the latitudes, longitudes, &c. at ſea. With the nine-lamp Telegraph, however, the numerical figures are more liable to be miſtaken, and it might perhaps be as well rather to *ſpell the number* wiſhed to be expreſſed. This lies in the option of the ſpeaker.

Theſe are a few hints concerning this invention, which may be applied to a great variety of purpoſes. By uſing lenſes before, or reflectors behind each of the lamps, to increaſe the glare and render the light more vivid, they become viſible even during the brighteſt ſun-ſhine, at a very great diſtance.

At ſea the Telegraph hangs on pivots, to yield to the ſwing of the veſſel; ſo remains always perpendicular. For land ſervice this is unneceſſary, but in either caſe it may be turned round ſo as to face in any direction.

Thus a complete ſyſtem of communication may be eſtabliſhed,—ſimple, eaſy, and unſhackled;—not liable to be miſtaken, but legible by day or night to all;—capable of being the vehicle of ideas in any language, and ſo quick, that, if neceſſary, ſixty ſigns can be exhibited, and diſtinctly read at a great diſtance, in the ſpace of one minute.

This

This mode of communicating thoughts, information, and intelligence would confequently be of much ufe on light-houfes, dangerous coafts, harbours, fortifications,—at head-quarters, and out-pofts,—in fhips of war, revenue cutters, merchant veffels, &c. Correfpondence in this manner might alfo be carried on between gentlemen's country feats, if in fight of each other, though far diftant.

Orders for thefe Telegraphs, or offers from tradefmen to make them, may be addreffed per poft "James Boaz, Glafgow, North Britain;" or to Mr. Wyatt, No. 182, Fleet-ftreet, London.

LONDON,
February 1802.

XVI. *Notices refpecting New Publications.*

A Treatife on Aftronomy, in which the Elements of the Science are deduced in a natural Order, from the Appearances of the Heavens to an Obferver on the Earth, demonftrated on Mathematical Principles, and explained by an Application to the various Phænomena. By OLINTHUS GREGORY, Teacher of Mathematics, Cambridge. London, Kearfley 1802. 8vo.

ASTRONOMY is a fcience not only noble and exalted in itfelf, but attended with fo many advantages that little needs be faid in its favour. Its utility in navigation, by which means that art has been brought to a ftate of very great perfection, is alone fufficient to fhow its importance, and confequently the benefits that muft arife to fociety by encouraging and promoting it. Moft of the civilized countries in Europe, indeed, have lately paid confiderable attention to this important object. And the great improvement made in the conftruction of telefcopes, and other aftronomical inftruments, has produced difcoveries, and an accuracy in obfervations of the celeftial bodies, which otherwife could not have been expected. By the munificence and liberal protection of our moft gracious fovereign, who has always fhown himfelf a friend to fcience, thefe improvements have been carried to a very great extent in this country: and it muft fill the breaft of every patriotic Briton with honeft pride, to reflect that there is fcarcely an obfervatory in Europe which is not furnifhed with telefcopes by Herfchel or Dollond, and with quadrants, equatorial fectors, tranſit inſtruments, &c. by Ramſden and other eminent Engliſh artiſts. But though aſtronomy has been ſo much improved in this country, we were far behind ſome of our neighbours on the continent, in regard to good works on the ſubject. We do not here mean to throw out any reflections againſt thoſe authors who have written on aſtronomy, many of whom are juſtly entitled to praiſe. But we are of opinion that none of theſe works, the ſyſtem of profeſſor Vince excepted, is ſuited to the preſent improved ſtate of the ſcience; for works of aſtronomy, from the nature of the ſubject on which they treat, the neceſſity of frequently correcting the tables, and other circumſtances, muſt have a very limited period of utility. As profeſſor Vince's work is too bulky and expenſive for the great maſs of the public, it gives us pleaſure to find that a gentleman ſo well qualified for the taſk as Mr. Gregory ſeems to be, has turned his attention to this deficiency, and ſupplied the public with a comprehenſive, clear, and well arranged elementary treatiſe on this noble and uſeful ſcience. We have no heſitation in ſaying, that we conſider it as the beſt practical work on the ſubject publiſhed ſince the time of Leadbetter. The author's rules are ſimple and eaſy; and the whole rendered ſo familiar, by a variety of examples, that any perſon initiated in the principles of the mathematical ſciences muſt readily comprehend them. It will be of great utility to young perſons in particular, who are ſtudying aſtronomy; and thoſe who have made conſiderable proficiency will find it exceedingly convenient to refer to. The author has omitted none of the modern diſcoveries; and the tables he has given at the end are taken from the beſt ſources, and improved by the lateſt corrections.

XVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

DR. GIBBES, in a paper on galvaniſm, read before the Society on the 21ſt of January, aſſumes an opinion contrary to that of Dr. Wollaſton, reſpecting the origin of electricity in chemical changes, and maintains, that the electrical changes are to be conſidered as preceding and favouring the chemical. He imagines that the ſimple contact of various ſubſtances produces changes of electrical equilibrium, and that the action of acids is effectual in promoting theſe changes by bringing their ſurfaces into contact. Dr. Gibbes obſerves obferves upon Dr. Wollafton's experiment of immerfing zinc and filver in an acid folution, that if they are placed in two feparate portions of the fluid, and the parts not immerfed are brought into contact, there is no emiffion of gas from the filver; but that it is copioufly produced when the contact takes place in the fame fluid. He proceeds to relate fome experiments which feem to fhow a difference between galvanifm and electricity, particularly that galvanifm does not appear to be attracted by metallic points. He alfo ftates an experiment in which a piece of paper is placed on tinfoil, and rubbed with elaftic gum; and although the tinfoil is not infulated, fparks are produced on raifing the paper. Dr. Gibbes concludes with fome arguments againft the doctrine of the decompofition of water; and advances as a probable opinion that oxygen and hydrogen gas are compofed of water as a bafis united with two other elements, which combined form heat.

The meetings of the 28th January, the 4th and 11th February, were principally occupied by a paper on the hyperoxygenized muriatic acid, by Richard Chenevix, Efq. F.R.S.

After a fhort account of the experiments that had been made before him, and particularly of the ingenious conjecture of Mr. Berthollet *, Mr. Chenevix proceeds to ftate the means by which he has afcertained, that the acid contained in hyperoxygenized muriate of potafh is muriatic acid in a particular ftate of combination with oxygen, and the experiments by which he determined the proportion of thefe elements.

From the quantity of oxygen and of fimple muriatic acid contained in the falt, he proves that hyperoxygenized muriatic acid is compofed of

oxygen - -	65
muriatic acid - -	35
	100

From the proportions of the falt which is formed when a current of oxygenized muriatic acid is paffed through a folution of potafh, and which he found to be compofed of the fame elements, in the fame proportion as oxygenized muriate of potafh would be, if, at the very moment of its formation, it had not been refolved into fimple and hyperoxygenized muriate, he concludes that oxygenized muriatic acid is compofed of oxygen - - 16
muriatic acid - - 84

* *Journal de Phyfique*, 1788, page 217.

From

From a number of experiments which are ſtated at length, Mr. Chenevix imagines that the ſalts of the genus oxygenized muriate do virtually exiſt; but that, by ſuperior affinities, they are all reſolved into muriates and hyperoxygenized muriates at the very moment the acid comes in contact with the baſes.

He deſcribes and analyſes all the ſalts of the genus hyperoxygenized muriate; and mentions the hyperoxygenized muriate of ammonia as an extraordinary inſtance of diſpering affinities. He obtained the alkaline ſalts pure by repeated cryſtallizations; and the earthy ſalts, by boiling them with phoſphate of ſilver.

He has examined likewiſe ſeveral of the metallic ſalts of this genus, and mentions hyperoxygenized muriate of ſilver as ſtrongly marking the difference between muriatic and hyperoxygenized muriatic acids. He aſcribes to this ſalt, when mixed with any combuſtible body, an expanſive force, which he thinks he will be within bounds if he ſtates to exceed five times that of any known detonating ſalt.

He concludes with an appeal to the chemical world, whether, in the preſent ſtate of the ſcience, it would not be more philoſophical to ſay,

Muriatic radical, or ſome one word of the ſame import,	inſtead of	Muriatic acid;
Muriatous acid,		Oxygenized muriatic acid;
Muriatic acid,		Hyperoxygenized muriatic acid;

and ſtates the arguments in favour of each appellation.

As a warning to thoſe who would repeat his experiments, he relates, in the courſe of his paper, an accident that happened in his laboratory, to himſelf, and to Mr. Vandeer, by which the latter gentleman had almoſt loſt his ſight, and was wounded in the moſt dreadful manner.

On the 4th and 11th of February, letters from Dr. Maſkelyne, Mr. Von Zach, Alex. Aubert, Eſq. and Mr. Gilpin, clerk to the Royal Society, were read, ſtating that they had obſerved the new planet. As we have already given its obſerved places, (ſee p. 54, &c.) we need not here repeat them.

On the 18th of February, a paper by Dr. Herſchel on the ſame ſubject was read. His reſults are given in the preceding pages.

ROYAL ACADEMY OF SCIENCES, BERLIN.

The laſt ſitting of the Royal Academy of Sciences was occupied

cupied a good deal with galvanic experiments. Mr. Herhard has found that nickel in contact with zinc produces the fame effect as filver and copper. Klaproth read fome account of the galvanic experiments, made on a large fcale, by Van Marum, of Haarlem, and of his counter-trials with Tyler's large machine. Thefe trials confirm Volta's theory refpecting the identity of galvanifm and the electric fluid.

FRENCH NATIONAL INSTITUTE.

[Continued from p. 381, vol. xi.]

C. Meffier having in vain fpent the night of the 3d of October in fearching for the new ftar; to confole himfelf for his want of fuccefs, he began to confider the very uncommon fpectacle exhibited by the conftellation of the Lion, in which Saturn, Jupiter, Venus, and the Moon were collected around the beautiful ftar called Regulus or the Lion's heart.

This was not a real conjunction, fince there was between all thefe ftars the diftance of feveral degrees. Thefe affemblages, refpecting which aftrologers formerly made fo much noife, and which, according to them, were to be followed by dreadful cataftrophes, have always paffed over in a very tranquil manner, and have produced no other effect than that of covering with confufion thefe filly prophets. They are alfo attended with no other benefit than that of affording to the aftronomer an opportunity of obferving feveral of the planets at the fame time. C. Meffier took advantage of this circumftance to fix the refpective fituations of thofe which were then in the neighbourhood of Regulus. The conjunction of feveral planets being a circumftance very uncommon may ferve alfo to fix the epoch of any event, and in fuch an invariable manner that no change of æra or in the calendar can render it doubtful.

C. Prony read to the clafs a notice of fome experiments made with an Englifh rule, conftructed according to the ftandard of thofe employed in the grand trigonometrical operation of general Roy, and brought to Paris by profeffor Pictet of Geneva, and which was compared with the metre of the Inftitute, and the toife called the Peruvian.

C. Flaguergues fent fome obfervations of the planets, and of different eclipfes.

Chemifts were acquainted with two metallic combinations, which have the remarkable property of producing a violent detonation, when fubjected to a flight compreffion, or when expofed to a flight degree of heat. Thefe combinations are fulminating gold and filver. Mr. Howard has lately made known a third, which poffeffes the fame property, and to which

which he has given the name of *fulminating mercury*. It is attended, however, with this difference, that it is not produced under circumſtances ſimilar to thoſe under which gold and ſilver ſuſceptible of detonation are obtained. It is formed by the ebullition of nitrate of mercury with alcohol, and depoſits itſelf in a powder, the colour of which varies from white to a gray more or leſs dark. It was neceſſary to find in the analyſis of this ſubſtance an explanation of its production, and that of its reſemblance to fulminating gold and ſilver; and alſo of its differences from them. Mr. Howard concluded from his experiments, that it was compoſed of oxalic acid, oxide of mercury, and ethereal nitrous gas.

C. Berthollet has ſhown by his, that it contains no oxalic acid; but that it contains ammonia; that it forms in this reſpect a combination analogous to fulminating gold and ſilver; and that, conſequently, its effects ought to be explained in the ſame manner as thoſe of theſe compounds. Fulminating mercury differs from theſe two detonating metallic ſubſtances, by a portion of altered alcohol, which enters into the combination, and which, when decompoſed, produces carbonic acid. Fulminating mercury then is a triple combination, while fulminating gold and ſilver are only binary combinations of the oxides of theſe metals with ammonia.

C. Vauquelin read a memoir on the analyſis he made of a copper ore diſcovered in Derbyſhire, ſpecimens of which had been tranſmitted to the Council of Mines by Count de Beurnon. Vauquelin ſtates, that this ore is compoſed of 62 parts oxide of copper, 24 parts of arſenic acid, 8 parts arſeniate of iron, and 8 parts water of cryſtallization.

Vauquelin having been requeſted alſo by the Council of Mines to examine a mineral ſent to it by M. Karſtein of Berlin, under the name of arſeniated copper, aſcertained that this mineral is formed of 60 parts oxide of iron, 22 parts arſenic acid, and 18 parts water of cryſtalliſation; but that it contains not an atom of copper. Neither copper nor iron had been before found united to the arſenic acid; and this is the reaſon why the mineralogiſts had made no mention in their works of the arſeniate of iron or arſeniate of copper. The labours of C. Vauquelin have therefore enriched with two diſtinct ſpecies the catalogue of mineral ſubſtances already known *.

C. Vauquelin read alſo a memoir on a mineral found in the environs of Limoges, which C. Alluau, director of the porcelain manufactory in that city, had tranſmitted to him

* This is a *miſtake*. See Philoſophical Tranſ. 1801, part I.—EDIT.

under

under the name of *tin ore*. He has diſcovered that this ore is compoſed of 42 parts of the oxide of manganeſe, 27 phoſphoric acid, and 31 oxide of iron. He is of opinion that theſe three ſubſtances are intimately united, and form a kind of triple ſalt with a double baſe. This combination is abſolutely new to naturaliſts. No one had ſpoken of it before C. Vauquelin. It muſt hold a particular place, in the ſyſtems of mineralogy, either in the genus of iron or the genus of copper.

C. Gillet-Laumont read to the claſs an account of a mineral already known to naturaliſts, though they were too little acquainted with its ſituation in the earth. This ſubſtance, which contains a metal lately diſcovered by Vauquelin, is *chromated iron*. It had been ſeen in an inſulated maſs by C. Pontier, near Goſſin, in the department of the Var. But this mineralogiſt has ſince found it, in very great abundance, in the middle of a quarry of ſerpentine near the borders of the ſea, at a ſmall diſtance from the harbour of Cavalaire: and this diſcovery is valuable to the arts not only on account of the nature of the mineral, but alſo on account of the eaſe with which it can be procured in conſequence of the ſituation of the quarry.

C. Juſſieu read a notice of ſeveral kinds of Indian plants deſcribed by various authors, and which, in his opinion, ought all to be referred to the genus known under the name of *litſee*, the native country of which is China.

C. Delille, member of the Inſtitute of Cairo, tranſmitted to the claſs a memoir in which he gave an exact deſcription of the *doum* or palm of the Thebaid, which was before but imperfectly known.

The deſert plains by which Egypt is ſurrounded have not at all times been deſtitute of vegetable productions. At very remote periods they have been covered with trees, and particularly palms; and this may ſerve to account for the petrified trunk of a palm-tree found in the deſert near the iſthmus of Suez, and tranſmitted to the claſs by general Regnier, member of the Inſtitute of Egypt.

C. Regnier, his brother, added to this ſpecimen a manuſcript, written by himſelf, containing general conſiderations on the agriculture of Egypt, as well as the ameliorations of which it is ſuſceptible.

C. Ventenat preſented the 5th and 6th numbers of his deſcription of the new or little known plants cultivated in the garden of C. Cels.

C. Lamark preſented his *Annuaire Météorologique pour l'an* 10.

C. Van Mons, aſſociate, tranſmitted to the claſs the firſt volume

volume of a *Journal de Chimie*, which he has undertaken, and which is intended as a supplement to the *Annales de Chimie*, and other periodical works published on that science.

He transmitted at the same time a translation of an Italian work by Brugnatelli, professor at Pavia, containing a chemical nomenclature by him, and a synonyme of the different modern chemical nomenclatures.

Lacepede transmitted to the class the third volume of *Histoire Naturelle des Poissons*, 4to.

XIX. *Intelligence and Miscellaneous Articles.* *February* 1802.

MINERALOGY.

THE *Journal de Physique* (Brumaire, an 10.) contains a notice of the discovery of corundum, at Chesnut Hill, near Philadelphia, by Dr. Seybert. From a few experiments upon the most obvious properties of a specimen received from the discoverer, made by Mr. Richard Phillips, to whom we are indebted for the present article, he is induced to believe that it is not corundum: it has for matrix decomposed felspar, accompanied by white felspar and mica; only one crystal was contained in the specimen, whose form was sufficiently determinate for description; it appeared to be a compressed hexaedral prism, having one of its sides interrupted in crystallizing; its colour inclining to green; its hardness sufficient to give fire with steel, and to scratch glass; but it is itself easily scratched by corundum, and is not hard enough to mark the beryl; whereas the latter is readily scratched by corundum. It is easily reduced to powder. Its specific gravity only 2,70, that of corundum being 3,87: the quantity was too small to allow of accurate analysis; upon examination it would probably be found to be quartz, which it nearly resembled in fracture, deriving its colour from oxide of iron.

The following account of the discovery of a mine of native sulphur in the Lower Pyrenees has just appeared in one of the French journals, in a letter from Dr. Thore. "Permit me to announce, through the medium of your journal, the discovery made some time ago of a mine of native sulphur, by a proprietor in the commune of Saint Boué, department of the Lower Pyrenees. Accident, the grand author of discoveries, was the author of the one in question.

"Some works undertaken for repairing a water-mill laid open at first a quarry of gypsum, and soon after the mine to which I here allude. When informed of it, I repaired to the spot to obtain the necessary information respecting it; I at

I at the fame time wrote to my friend Darracq, preparer general at the School of Mines, to requeft his opinion of it. He fent me notice that he would accompany me to vifit it; which we accordingly did, and I can verify the difcovery, till we give a detailed memoir on the advantages or inconveniences likely to refult from it.

"In the mean time, I think it my duty to fay, 1ft, That it is fituated in a very deep ravine, and to the fouth of a redoubt, known under the name of the redoubt of Hillot, which commands it by about 150 toifes at leaft.

"2d, That the fulphur prefents itfelf in two very diftinct ftates. In the firft, it has for matrix fometimes earth of a gray afh colour, which evidently ferments with acids without being converted into lime, and fometimes gypfum, at the fide of which the fulphur is found. In both thefe cafes it is of a beautiful lemon yellow colour, and tranfparent as glafs; fometimes it is detached in blocks of feveral pounds, or enveloped in a foliaceous mixture, which we have not yet been able to determine. In the laft cafe, the fulphur is of the colour of colophonium, and appears to have undergone an evident degree of fufion, whatever may have been the agent that occafioned it.

"3d, If the matrix, which is very hard, be divided, it emits a very fenfible bituminous odour, and the fractures fhow geodes of different forts. Some contain only petroleum, and others very curious cryftallizations.

"4th, This mine, during great heats, exhales fo ftrong an odour, that the proprietor was obliged to cover it with feveral feet of earth to fhelter himfelf from it."

SUGAR FROM THE BEET-ROOT.

Doctor Achard has publifhed, at Berlin, the refults of a fecond trial, made on a large fcale, to extract fugar from the beet-root, under the direction of a commiffion appointed for that purpofe by his Pruffian majefty. By thefe refults it appears that 1500 quintals of beet-root gave 5952 pounds of raw fugar; 450 quintals of refufe, and 100 ounces of fyrup. Thirty quintals of beet-root, cultivated according to the procefs of Achard, gave each fix pounds three ounces of raw fugar. The refufe may be employed as coffee, or to diftill fpirit; and is more profitable for feeding cattle than beets themfelves. The raw fugar may be refined for every purpofe whatever. According to a calculation made by the commiffion charged to examine this difcovery, it will produce to Pruffia an annual faving, or rather an advantage, of two millions and a half of rix-dollars.

MUSIC.

MUSIC.

M. Horſtig, counſellor of the conſiſtory of Minden, has invented an ingenious method of marking the muſic of an air or ſong without employing the common muſical notes. Inſtead of notes he makes uſe of figures, which expreſs the notes equally well: thus 1 always ſignifies the tone of the piece, whatever it may be; 2, the ſecond; 3, the third; 4, the fourth, &c.: but if the piece is played or ſung in *fa*, the cipher 1 denotes *fa*, 3 *la*, &c. The following is given as an example:

Cœurs ſenſibles, cœurs fidèles,
Qui blâmez l'amour leger.

The muſic of theſe lines is thus repreſented:

5 3 1 3 2 4 4 3
3 4 5 5 6 5 4 3 3 2

If the piece then is played in *fa*, we have

re ſi ſol ſi la ut ut ſi

If any other tone be aſſumed, the ſame air will always be preſented.

VACCINE INOCULATION.

We are informed by a member of the Vaccine Inſtitution, that cow-pock matter ſent upon glaſs, from Milan, by M. Saccho, taken from the Milaneſe cows, as communicated to Dr. Pearſon, has been employed on ſeveral patients at the Inſtitution; on all of which it has produced preciſely the ſame diſeaſe as that from the Engliſh.

On the 23d inſt. Mr. Blair began a courſe of popular lectures on anatomy and the animal œconomy: wherein the ſtructure and functions of the human body are to be familiarly explained and illuſtrated; for the information of ſcientific perſons, amateurs of natural hiſtory, and ſtudents in the liberal arts.

The plan of the courſe is as follows:

Component parts of the body.—Bones, cartilages, and ligaments.—Muſcles and muſcular action.—The integuments and membranes.—Brain, nerves, and ſenſation.—The heart and vaſcular ſyſtem.—Blood, circulation, and abſorption.—The glands, ſecretions, and excretions.—Reſpiration and animal heat.—Digeſtion, nutrition, and growth.—Utero-geſtation, and parturition.—The eye, and phænomena of viſion.—Functions of the ear, noſe, and mouth.—Phyſiognomy, beauty, and the paſſions.

XX. *An Examination of* C. Clouet's *new Procefs for making Caft Steel from Bar Iron by means of the Decompofition of Carbonic Acid, continued*.* *By* David Mushet, *Efq. of the Calder Iron Works.*

However conclufive the experiments communicated in my laft appeared, in oppofition to the theory adopted by the Parifian chemifts, that the decompofition of carbonic acid afforded the fteely principle to iron, I deemed the examination imperfect, unlefs experiments were made with various carbonates, and particularly with thofe foreign to this country. With a view therefore to give every degree of juftice and candour to the inveftigation, the experiments which follow were carefully and accurately performed.

Exp. VII. Fufion of the fame fragments of Swedifh iron with equal portions of Italian marble and Stourbridge old clay pot.

		Grains.
Pieces of Swedifh iron	- -	916
White Italian marble, 1-3d	305·3 grs.	
Stourbridge old clay pot, 1-3d	305·3 grs.	
This is the exact proportion prefcribed by Clouet for the formation of his beft caft fteel. From the mixture, I obtained in 40 minutes a very perfect fufion. The metallic button poffeffed a denfe fmooth furface, free from pits and honeycombs. It weighed	-	897
	Loft in fufion	19

equal to $\frac{1}{48\frac{3}{10}}$th part of the original weight of the iron. The incumbent glafs was very perfect, and of a fmooth denfe furface. Its colour was light bottle green, fome fhades purer than the glafs of experiment No. 1. afforded with Balgrochan limeftone.

The button now obtained was cut, and broken acrofs an anvil. Its fracture was regularly granulated, of a dark blue grain, very prominent, but pervaded with a dull lead colour betwixt the cryftals. One-half of this button was drawn into a fmall drilling tool, and hardened with a degree of heat beyond that of common fteel. The file, at firft, met with a flight refiftance; but a few ftrokes removed the partial hardnefs, and an uncommon foft iron fucceeded. This drill was introduced below a drilling frame loaded with only 7 lbs. upon the end of the lever. The brace was not turned half round

* For the former part of this Examination fee p. 27 of our laft Number.

round before the point had ſet up all round like lead. The ſame piece was afterwards ſubjected to ſeveral trials of hardening, bending, and filing, in all which it manifeſted properties peculiar to the ſofteſt malleable iron. The denſity conveyed to the metal by fuſion, ſeems to form an univerſal characteriſtic feature of this ſpecies of metal. When the file is applied to the melted maſs, it meets with a much greater degree of reſiſtance than from common malleable iron; but this exiſts no longer after being forged, and the file then acts with the greateſt poſſible effect.

		Grains.
Exp. VIII. Pieces of Swediſh iron weighing		1090
Italian marble -	2180 grains,	
Stourbridge clay, old pot	2180 grains.	

This proportion is ſix to one of that preſcribed by C. Clouet, and from which gray caſt iron ſhould have been obtained. In 45 minutes I deemed the fuſion complete, and withdrew the crucible. When cold, I found a very perfect metallic button reſting beneath a great thickneſs of glaſs. The external appearance, in every point, reſembled that of No. 1. It weighed - - - 1070

Loſt in fuſion 20

equal to $\frac{1}{54\frac{1}{2}}$ part the original weight of iron. The glaſs in this experiment was ſeveral ſhades lighter than that obtained in No. 7. The reaſon for this ſeems obvious, as ſeven times the quantity of glaſs poſſeſſed only a quantity of colouring principle ſimilar to the former, if by this be underſtood the ſpecific quantity of iron loſt in fuſion. The fracture of this button was preciſely that of No. 7. One-half of it was drawn into a ſmall cutting chiſel, and hardened at a pretty bright red heat. It ſhaled a little, and felt, for a few ſtrokes, perceptibly harder under the file. Upon applying the chiſel to cut a piece of newly hammered Swediſh iron, it made a conſiderable impreſſion with a moderate blow from a 3 lb. hammer; but the whole edge was flattened, and at a ſecond blow the edge ſtaved completely, and meaſured nearly 1-8th part of an inch acroſs.

A fragment of the ſame experiment was ſubjected to a variety of teſts, in none of which the moſt diſtant ſymptom of ſteel was manifeſt: on the contrary, in point of ſtrength and durability it was found inferior to pieces of the original bar.

		Grains
Exp. IX. Swediſh iron, in pieces -		1099
Italian marble -	1099 grains.	

This

	Grains.
Brought over	1099
This mixture was expoſed to a violent degree of heat, and in one hour I judged the fuſion complete. The button obtained was finely poliſhed upon its upper ſurface, and remarkably denſe. It weighed -	1059
Loſt in fuſion	40

equal to $\frac{1}{27\frac{4}{10}}$th part of the original weight of the iron. The fracture of the button now obtained was of a ſmaller grain than the former, equally regular but leſs brilliant. It drew into ſhape equally well under ſimilar circumſtances with the former, and exhibited the very ſame appearances.

One half of this button was forged into a point, and hardened at a high heat for common ſteel. I endeavoured to pierce a piece of thick rolled plate iron; but at the firſt blow the point bent upwards, throwing a white ſhale and leaving the ſurface like tin.

The glaſs obtained by the fuſion of lime alone in this experiment was very different from that in Nos. 7 and 8. It poſſeſſed a rich graſſy green colour, free from air bubbles and very tranſparent. The extra loſs of metal in this experiment beyond that in Nos. 7 and 8. may be accounted for from the infuſibility of carbonate without the uſual addition of clay. I have repeatedly found that lime alone, when pure, gradually contracts its bulk, and remains unfuſed a conſiderable time after the iron has entered into fuſion. This naturally expoſes the latter to a greater degree of oxidation than when covered by a fuſible glaſs during the greateſt part of the expoſure.

The following experiments were performed with Kilkenny blue marble:

	Grains.
Exp. X. Pieces of Swediſh iron weighing -	1196
1-3d of Kilkenny marble, or 398 grains.	
1-3d of Stourbridge clay pot, or 398 grains.	
There reſulted from this mixture, after half an hour's expoſure, a very fine fuſion accompanied by a very perfect metallic button of - -	1173
Loſt in fuſion	23

equal to $\frac{1}{52}$th part of the firſt weight of the iron. The colour of the glaſs now formed was ſome ſhades lighter than in experiment No. 7. The green was ſlightly tinged with an amber colour, which difference, moſt probably, aroſe from the nature of the lime.

The fracture of this button differed little from Nos. 7 and 8, and in forging no difference could be detected, which implied a marked distinction as to quality.

		Grains.
Exp. XI. Swedish iron, in pieces	-	1012
Kilkenny marble -	2024 grains,	
Stourbridge clay, old pot	2024 grains.	

This proportion of earths is 6 to 1 of that prescribed by C. Clouet. The mixture was exposed for nearly 50 minutes, when I judged from the degree of heat that the fusion was complete. When cold, I found a large mass of glass resting upon a very perfect smooth-skinned metallic button, which was found to weigh 989

Lost in fusion 23

equal to $\frac{1}{44}$th part of the original weight. The fracture was so like that of the former, that they were not easily distinguishable from each other. The same similarity in point of quality, when forged and put to various tests, was also evident.

The colour of the glass was as much alike to No. 10 as were the qualities of the respective metallic products.

		Grains.
Exp. XII. Swedish iron	- -	878
Kilkenny marble	1756 grains.	

From the purity of the Kilkenny marble I judged it requisite to expose this mixture to a very high degree of heat, which continued nearly one hour. I found a perfect fusion of the lime, and a very fine button of metal. The latter weighed - - 828

Lost in fusion 50

equal to $\frac{1}{17\frac{28}{50}}$th part of the original weight. The form, appearance, and density of this button exactly corresponded with that of No. 9, when Italian marble was fused along with iron. Its fracture was in most respects similar, and the subsequent trials proved that its quality was also the same. The loss in metal was greater in this experiment than in No. 9, probably arising from the iron being longer exposed under a high temperature before the carbonate, by reason of excessive purity, entered into fusion. The glass produced by the fusion of this marble in contact with iron was of a black colour, and possessed of no great degree of lustre in fracture. When pounded small, it assumed a shade of dirty green, differing materially from any of the former.

I have avoided entering into a detail of the various appearances which these three last products assumed in forging. Their

Their similarity and identity with those described in Nos. 7, 8, and 9, rendered such a description unnecessary. Suffice it to say, they were applied in the same shapes to the same species of labour, and found equally inapplicable to the purposes for which they were intended.

Neither in the experiments recorded in this paper, nor in those which accompanied the former communications, was there found any product resembling steel, or possessed of any of those properties which iron derives from its combination with carbon: on the contrary, the resulting product in all of them was found debased, in point of strength and elasticity, below the standard of common malleable iron. The uniform quality of the various metallic masses, though fused in contact with widely different proportions of earths, leaves little room to doubt that the latter are entirely neutral; nor does it appear, upon a review of the experiments performed with a mixture of carbonate and argil, that the loss of metal is dependent upon the quantity of earths fused in contact with it, as the following table will show: the same review exhibits a triple loss of metal in those experiments wherein the carbonate of lime was fused alone with the iron.

Experiment No. 1.	loss was one	73d part.
2.	- -	70th
3.	- -	79th
4.	- -	44th
5.	- -	40th
7.	- -	48th
8.	- -	54th
10.	- -	53d nearly
11.	- -	44th

Average loss in fusion = one 56th part.

The three first fusions seem to have experienced a smaller loss in metal than the subsequent ones. This I am apt to attribute to a comparatively imperfect fusion, as the buttons obtained in Nos. 1, 2, and 3, were considerably pitted, and every way less dense than those afterwards produced, when I had learnt to regulate the necessary heat to a certainty.

Experiment No. 6.	loss one	17th part
9.	-	27th
12.	-	17th

Average loss in fusion one 20⅓d part.

It appears also that the quantity of carbonate used, and the loss of iron sustained, were not analogous to each other. To verify this, No. 6 and 12 may be compared.

In the courfe of making the feries of experiments from which the foregoing are felected, I had occafion to remark that the mixture of argil and carbonate, when moderate proportions were ufed, entered into fufion in 10 to 15 minutes from the period of being introduced into the furnace, and from 15 to 20 minutes before the iron melted. A few crucibles were withdrawn at this early ftage of the procefs; the glafs was found perfectly pure and tranfparent. The pieces of iron were more or lefs welded together, but poffeffed their original fhape and quality. And though it may feem fomewhat foreign to the prefent fubject, I fhall further remark, that fo long as the pieces of iron remained unfufed or fufing, a continual bubbling took place upon the furface of the liquid glafs refembling the difcharge of an elaftic body. I foon learned to prejudge the degree of perfection in the fufion by the furface of the glafs. In imperfect reductions this was completely covered with various fized bubbles, many of which were concentric: on the contrary, when the metallic button was found poffeffed of fmoothnefs and uniform denfity, the furface of the glafs was either entirely fmooth, or at moft prefented one folitary bubble exactly in the centre, through which it is prefumable, at the time of its fixing, had been difcharging the laft portions of the gafeous fubftance formerly alluded to.

In purfuing the inveftigation of this fubject, I had prepared a variety of other experiments. The fufion of the fame Swedifh iron with deacidified carbonate and argil, in various degrees of mixture, and with the former alone, formed a diftinct head. The fimilarity of the refults, excepting an alteration in the colour of the glaffes, renders thefe experiments no longer interefting. Others, however, performed with Britifh cold fhort iron, made in the ftamping procefs, are more worthy of notice.

Various weights of iron were fufed with mixtures of argil and carbonate, and with the latter alone. In every cafe, a much greater quantity of metal difappeared than in any of the former experiments, and a fimilar difference in the appearance of the glafs.

1ft, In two experiments performed with cold fhort iron and an equal mixture of 1-3d each of Kilkenny marble and Stourbridge clay (old pot) in the one, and double the weights of the iron of each in the other, the lofs of metal was $\frac{1}{11}$th and $\frac{1}{16}$th: average, $\frac{1}{13\frac{1}{2}}$. The accompanying glafs was poffeffed of an uncommon degree of luftre and denfity. Its fracture prefented a polifh and colour fuperior to black marble:

very

very thin splinters, however, displayed a rich amber-green colour.

2d, 1000 grains of Kilkenny marble and 500 grains of cold short iron were fused, with a loss of $\frac{1}{10}$th of metal. The glass was of a deep grass-green colour.

3d, Equal mixtures of 1-3d each of Italian marble and clay pot fused with 1000 grains cold short iron, a loss of $\frac{1}{15\frac{4}{10}}$th part was sustained.

4th, 2000 grains of Italian marble and 1000 grains of cold short iron were fused together: a very black, smooth, shining glass was obtained, and a loss of 1-9th of iron was experienced.

5th, I welded several pieces of this cold short iron; drew it into a bar; cut the bar across; welded the pieces, and again drew it into shape. The iron seemed considerably improved. 1000 grains of this iron were fused with 666 grains of an equal mixture of Italian marble and Stourbridge clay pot: the loss in metal only amounted to $\frac{1}{25\frac{1}{2}}$ part of the original weight of the iron.

The foregoing experiments are, I conceive, in point of variety and accuracy, sufficient to demonstrate, that in no proportion, either alone or in mixture, does carbonate of lime, by the decomposition of its acid, or otherwise, communicate carbonaceous matter to iron fused in contact with it. In seeking, therefore, for an explanation of the phænomena which have so completely deceived the Parisian chemists, the two following reasons occur as being most probable. The crucibles used may have contained a portion of carbonaceous matter, or the reporters have been deceived by the ignorance or intention of their artists. If the latter has been the case, then the concluding paragraph of my communication in January will apply with peculiar force: if the former, the following experiments will show how easily the deception might have taken place.

1st Experiment with black lead crucibles. Grains.

Swedish iron - - -		6959
Calcined carbonate of lime	1750 grains,	
Stourbridge clay pot -	1750 grains.	

This mixture was exposed to a degree of heat capable of fusing a small portion of cast steel, and in an hour I found the contents completely reduced. When the crucible was withdrawn, and cooled, I found an ingot of metal; the upper surface of which was beautifully

	Grains.
Brought over	6959

coloured, and cryſtallized in the moſt perfect radii, feathered tranſverſely. The under ſurface was conſiderably honeycombed, and the concaves poſſeſſed minutely cryſtallized interiors. The maſs weighed 7058

Gained in fuſion	99

equal to $\frac{1}{70}$th part nearly of the original weight of the iron employed.

The glaſs in this experiment was milky blue inclining to azure; denſe, and very tranſparent.

The ingot was cut into three pieces, each of which was drawn into a neat bar. The caution neceſſary was great, as the product upon the firſt operation diſplayed an intimate connection with ſteel, and proved to be ſteel very highly ſaturated with carbon. A razor and ſeveral penknives were made from this reſult, all of which hardened to exceſs with a low ſhade of red.

A fair inference from this experiment is, that the carburet of iron which entered into the compoſition of the crucible, here united in part with the iron, and formed very highly ſaturated ſteel. The precaution uſed by introducing deacidified lime leaves no doubt upon the ſubject of the carbonic acid.

2d Experiment with black lead crucibles.	Grains.
In this experiment I uſed of the ſame iron -	3514
Of calcined lime - 440 grains,	
Old clay pot - 440 grains.	

A very beautiful button of ſteel was obtained in this fuſion, the cryſtallized ſurface of which was ſo prominent as to leave a very perfect impreſſion upon the under ſurface of the incumbent glaſs. The weight was found to be - - - 3567

Gained in weight	53

equal to $\frac{1}{66\frac{3}{10}}$th part of the original weight of the iron. The quality of the ſteel now obtained was ſofter than that produced in laſt experiment, and more ſuitable for the ordinary purpoſes to which caſt ſteel is applied.

3d Experiment with black lead crucibles.	Grains.
Swediſh iron - - -	3630
Calcined lime - 220 grains,	
Stourbridge clay pot - 220 grains.	

From this mixture another elegant button of metal

		Grains.
	Brought over	3630
covered by a thin layer of glafs was obtained, which weighed - - -		3623
	Loft in fufion	7

equal to $\frac{1}{518\frac{4}{10}}$th part of the weight of the iron employed.

This fteel was, in point of quality, fofter than any of the former; and from this circumftance I inferred that the earths fo far contributed to the fteelification of the iron by fufing upon the furface of the metal, and preventing the efcape of the carbon. It appeared alfo further adducible, that if the iron was prefented with a fufficient dofe of carbon to make allowance for wafte, fteel fhould be formed without the addition of any portion of earth.

4th Experiment will juftify the firft inference.

	Grains.
Into a black lead crucible was introduced -	3710
of the iron employed in former experiments. A tight lid was fitted to the crucible, and the iron expofed to as high a heat as I judged the crucible capable of fuftaining. This continued, from firft to laft, for 38 minutes, when the crucible was withdrawn. When cold, I found that the iron had funk to the bottom, forming an imperfect ingot of fteel of a proper degree of faturation of carbon at the under part, but almoft of the nature of malleable iron towards the upper furface. It weighed - - -	3692
Loft in fufion	18

equal to $\frac{1}{207}$th part of the weight of the iron.

The latter inference was juftified in the courfe of fome hundreds of experiments with various carbonaceous fubftances and iron, the ultimate refult of which proved that excellent caft fteel could be formed in crucibles deftitute of carbonaceous mixture by the fingle addition of carbon.

XXI. *Experiment made with a Galvanic Belt, or Chain, by Mr.* RICHARD TEED, *Jeweller, Lancafter Court, Strand.*

To Mr. Tilloch.

SIR, Feb. 4, 1802.

I HAVE not obferved, in any publication on galvanic electricity, that material benefit has been as yet attained by this new difcovery, and therefore I fhall beg leave, through

 the

the medium of your valuable Magazine, to submit the following experiment to the consideration of your readers, with the hope that others will be induced to try it, and also that it may afford relief to some labouring under similar complaints.

For a considerable time past I have been much troubled with a constant pain in the small of my back and loins, and, although it resembled the lumbago, it was scarce ever so violent; but I always felt it most after sitting long in a reclined or writing posture; so that, if I arose suddenly, it was with much difficulty. This complaint continued for eight or ten months, and lately with increasing inconvenience. The idea of a galvanic belt or chain, composed of zinc and copper, had often occurred to me, and I waited only in hopes of hearing it had been applied in similar cases: but the experiments of scientific men taking a different direction, I made a belt consisting of fifteen small square plates of zinc, and connected each with two links of plated copper wire, fastening the two ends with a common hook and eye, so that there was a perfect chain or circle of metal round my body, and by that means no interruption of the electric fluid could take place. I also covered about three parts of the chain with leather, leaving the remainder to come into contact with the part where I felt the most pain. I had not worn this belt twelve hours before I found sensible relief, and the pain gradually left me. In three weeks I had not the least return, and after wearing the belt three months I concluded it had answered all my expectations. But, to put the experiment beyond the possibility of doubt, I discontinued it, and had no pain whatever in my back for two months, when at times I perceived the same pain return. I again had recourse to the belt, and am now wearing it, which, as at first, has removed all pains in that part, and I feel no inconvenience whatever.

A short time after using the belt I observed a considerable oxidation on the zinc, which, I suppose, was occasioned by the perspiring matter from my skin, and which, I conclude, was the medium or exciting agent, as is the case with the diluted nitric acid in the galvanic pile. By scraping off the oxide, which I constantly did once in three or four days, I believe the effect became greater.

In giving you this plain but circumstantial account, I trust, a discovery so valuable will not be slighted, but that unprejudiced persons will also make the trial; and I particularly recommend it to the attention of those who have complaints of the same kind.

XXII. *In-*

XXII. *Inquiries into Coloured Light, by a Collation of the Experiments and Obfervations made by Sir Ifaac Newton on that Subject; together with fome additional ones.* By *Governor* POWNALL.

[Concluded from p. 49.]

THE green prifmatic tint is fuppofed to be homogeneous, becaufe confifting of angles of refrangibility in the yellow and in the blue fo exactly connected in degree at the point where they coalefce, that no prifm nor any fucceffion of prifms have as yet decompofed it. It is, however, in various other inftances decompofed; in the circles of colours arifing from the refractions of light paffing between two lenfes, as alfo in the rings of colours exhibited on bubbles of water. In thefe inftances its conftituent parts, which combined form the compound green, emerge as yellow on one fide and blue on the other.

Sir Ifaac Newton, in Obferv. iv. part 1. book 2. marking the order of colours in the circles produced by the refraction of light between two lenfes, fays, "the green was very copious and lively, inclining on one fide to blue, on the other to yellow:" and again, in part ii. book 2. p. 202. he fays, "the green exhibited is principally conftituted of *original green*, but not without a mixture of blue and yellow:" again, in part i. book 2. p. 190. he fays, "then after a lively orange followed an *intenfe bright* and copious yellow, which was alfo the beft of all the yellows; this changed firft to a greenifh yellow, then to a greenifh blue; but the green between the yellow and the blue was very little and dilute, feeming rather a *greenifh white* than a green: the blue which fucceeded became very good." In this obfervation made on the rings of colours on bubbles, notice the increafing intenfenefs of the light; alfo fucceffive gradations; alfo the gradual procefs of the compofition of green and of its decompofition, which is given in the next obfervation, p. 189: "After the red fucceeded little or no yellow, but a copious green, which at firft inclined a little to yellow, then became a pretty brifk and good *willow-green*, and afterwards changed to a blueifh colour; but there fucceeded neither blue nor violet." In the firft inftance, the very little and dilute greenifh white, as well as the willow-green in the fecond, was the dominancy of an intervening white, as will come out hereafter; and in both inftances the yellowifh green and blueifh green mark the compound, partaking on each fide of the yellow or of the blue,

blue, as the one or the other predominated in the compofition.

But to put this matter out of all doubt, the *fact* comes out on experiment, that when the gradations of the colour take fuch an arrangement that there is no interfection of the yellow orbicular images with thofe of the blue, and fo no coalefcence of thefe colours, *there is no green.*

Obferv. iv. part i. book ii. p. 174. Sir Ifaac Newton fays, "the colours reckoned in their order from the centre were, black, blue, *white*, yellow, red; here the blue and yellow not interfecting or coalefcing, but feparated by an intervening white, there was *no green* in this gradation of colours: if they had approached fomewhat near to an interfection of their circles, there might have been, as above, a whitifh or willow-green bordered with yellowifh and bluifh green. The fame fact is ftated in Obfervation ix. p. 181: the order of this gradation was, "violet, blue, white, yellow, red." Here again an intervening fpace of white excludes the exiftence of green. The fame fact of an *intervening white* between the yellows and the blues, excluding the green, comes out in fome experiments and obfervations made by G. W. J.* in addition to thofe made by Sir Ifaac Newton, where, in p. 99. he ftates, that "diluted purple and blue hues appear completing, with the external yellow and red, and an *intervening white*, thefe formations of thefe fringes." I will venture to add, in confirmation of all this, one amongft feveral obfervations which I made on thefe facts. Viewing, through a prifm, the folar light as it came through a fafh window, and taking the fpectrum in the line of refraction as it came from above, the following circumftances appeared:— The frames of the fafh were dark, and gradations of colours proceeded from them. The gradations of blue on the upper fide; thofe of the red, orange, yellow, from the lower fide. The gradations proceeded from the upper fide in this order— deep blue, and, in fucceffion, the feveral lighter tints or hues of blue till they vanifhed in the folar light. From the under part they proceeded in this order—deep red, then a brighter, going into orange, and the orange fucceffively brightening into yellow, which melted off into the folar light: no gradations of blue fucceeded either order, becaufe there was no going off of light into fhade when the fpectrum was thus tranfparent to the folar light; and as there was no blue to intermix with the laft yellow, there was *no green*. In proof that this is the reafon, it may be obferved, that if the light itfelf

* A very ingenious work publifhed by Cadell 1799.

through which the ſpectrum is viewed, decreaſes, in any conſiderable degree of its brightneſs, there will come on blueiſh hues, and a very faint intervening greeniſh one. To try this circumſtance more decidedly, I threw up the ſaſh, and made an aſſiſtant hold a broad rule, ſo that I might view the ſpectrum as it proceeded from the two edges in the ſame manner as from the frames of the ſaſh: I then directed my aſſiſtant to lower the rule down to the bottom of the window, till the yellow in the under gradations interſected the bottom. The yellow of this poſition not going off into tranſparent light, but into ſhade, the blue hues appeared in very decided tints through all their gradations; and where blue, ſucceeding to the yellow, interſected it, a decided green intervened.

Although no common eye, unpractiſed in the uſe and arrangement of colours and their various tints, can diſcover in the ſpaces of the priſmatic colours called homogeneous, when thrown on an opaque ſurface, any gradations of tints; yet when theſe colours are viewed by the priſm through an open light ſo as to be tranſparent, the gradation of ſucceſſive tints in each homogeneous ſpace becomes evidently obſervable to the moſt common eye. The obſerver may diſcern the fact, that no one of thoſe priſmatic colours which are called homogeneous is abſolutely ſo, but that, as according to the theorem ſtated by Sir Iſaac Newton himſelf, they conſiſt of a ſucceſſive ſeries of innumerable circular or orbicular images of the ſun; the tints of the commonly called homogeneous colour, red, vary according to the innumerable ſucceeding angles of refraction, heightening gradually towards *orange*, and going gradatim into the orange; and that the orange heightens in the ſame gradual ſucceſſion of tints going into yellow, and the yellow, in like manner, into pure light: alſo that the blues of the other gradation of colours are an indiſcriminate ſucceſſion of tints or hues.

Conformably to what is here ſtated of the heightening of the gradations of the colours at one end of the ſpectrum into light; of the blues going off, by a like gradation at the other end, into ſhade or dark; Sir Iſaac Newton (Optics, book i. prop. v. exper. 16. p. 85.) ſtates, that "the moſt luminous of the priſmatic colours are the orange and the yellow; next the green; and that blue is a faint and dark colour, and the indigo and violet the weakeſt and darkeſt of all colours."

Now from a more deciſive examination of the fact by actual experiment we ſhall be led to ſee demonſtrably that theſe apparent hues, which are called blue colours, are *not colour*, but a mere modification of light going off into ſhade, *a partial and ſucceſſive deprivation of light.*

In

In the firſt place, it is known by Sir Iſaac Newton's experiment, (Optics, book ii. part iii. prop. 7. p. 236.) "that blacks are inclined to a blueiſh colour may be ſeen from illuminating white paper by light reflected from black ſubſtances, for the paper will appear of a *blueiſh white.*"

In conſidering this obſervation on a fact as made by Sir Iſaac Newton, the method to try the effect of throwing a partial portion of light on a ſhade occurred to me. I darkened a room partially, letting in a beam of light of five or ſix inches breadth by opening one of the ſhutters. I then threw the ſhade of a ruler, by the intercepted light of a candle, on a ſheet of paper: I then held this paper, with this ſhade ſo formed on it, in the beam of light. The effect of this portion of daylight thus thrown on the ſhade of the ruler was, that the ſhade exhibited a pale but decided blue. As I removed this paper, with this ſhade of the ruler thus thrown on it, further back from the entrance of the light, but ſtill in the ſame beam, this blue, without the alteration of any one circumſtance but a further deprivation of light, became of a ſtronger, more decided, deeper blue; and ſo on gradually as I removed this paper and ſhade ſtill further back, making a further deprivation of light of a deeper and deeper but decided blue till it went off into a black. This ſhade, ſuffering this gradual deprivation of light, exhibited all the gradations called blue which are ſeen in the priſmatic ſpectrum. Here, then, the fact ſtands aſcertained, that blue is the effect of a partial light, and that a gradual deprivation of light gives all the gradations of blue as they go off into dark.

Mountains and hills, and even unelevated land, in the horizon exhibit, by the going off or deprivation of light, blue tints in the moſt diſtant, though ſuch are not the natural tints of theſe objects; and as theſe objects ſuffer more and greater deprivations of light in the evening, or from any other incidental circumſtance, theſe hues go through all the gradations of blue. This any one may ſee every day; and the painter, if he is an artiſt, knows how to copy this phænomenon in the arrangement of his colouring.

Another circumſtance in the landſcape of nature is ſtill further to be obſerved. As the ſhade of evening comes on, the blue in the diſtant horizon not only becomes deeper, but, when the departing light ſkirts the line of the horizon, this blue, without any change in the ſtate of the refractions, or of the actual colour of the objects, becomes a ſtrong vivid purple. The dark blue of the clouds juſt above this ſkirting of departing light, changes in like manner. The rays of the reddiſh-yellow light and thoſe of this blue coming in coincidence

cidence at the ſame time to the eye, cauſe that ſenſation in the optic nerve.

The ſky alſo, the leſs that clouds and reflecting vapours are mixed with it, abſorbs the light, and gives out only this blue hue, which is a reflection deprived in part of light *: the more clear the ſky, the deeper the blue: in the higher regions of the atmoſphere this blue becomes of a deep hue going off into dark.

This circumſtance takes place and prevails in the claro-obſcuro of all figures. A true artiſt knows, that if he would copy the *real fact* in nature, he muſt tint the going off of light not with gray and black but with gradations of blues, and give a faint ſuffuſion of green in the firſt going off. Zincke, the famous painter in enamel, underſtood and copied this fact: he worked an almoſt inſenſible green into the blues of the firſt going off of his lights.

If one may then aſſume the fact here grounded on the experiments and obſervations of Sir Iſaac Newton, with addition of ſome further ones on the caſe as exiſting in nature, that the gradations of blue are mere effects of the privation of light as it ſinks into ſhade or darkneſs; and then, from the teſtimony of the viſual ſenſes, which are the only teſt in the experiments above related, that the gradations of the red even in the priſmatic ſpace, wherein it is ſuppoſed to be homogeneous, of the orange likewiſe, and ſo of the yellow, are only gradual and ſucceſſive tints of the *one primary colour, red*, heightening, by an innumerable and infinite ſeries of angles of refraction, into a more and more brilliant approximation of pure light, as they purify from the ſuffuſion of colour which they had received in their combination with ſolar or terreſtrial gaſes and vapours; it will, on this aſſumption thus grounded on facts, ſtand demonſtrably proved (although I do not venture to announce it but with every diffidence) *that there are not ſeven, nor three, nor two, but* ONLY ONE PRIMARY COLOUR—*a compound baſis of all the tints of red, orange, and yellow: that green, as an actual and uncompounded primary colour, has no exiſtence: and that the blues are only hues ariſing from a partial deprivation of light as it goes off into darkneſs.*

That all the phænomena of colours produced by the re-

* This circumſtance, alſo, makes the difference of the blue of diſtant parts of the landſcape as reflected from valleys and lower parts, or from the higher: the deprivation of light, by its being abſorbed by the clearer air in the latter caſe, gives a deeper blue than that which is reflected from the air hanging over the lower parts. This a true artiſt, who paints from nature, knows.

fraction

fraction of prisms or of lenses, or by thin plates of air or water between lenses made to approach each other, or by the rings on bubbles, or by reflections or inflections of light, may be explained from the facts contained in the above proposition.

All the experiments, observations, and reasoning above, respect only *coloured light*. I dare not venture, on theory, to assert any thing as to the existence of an elementary, pure, uncoloured, uncompounded white light; yet that some such elementary substance exists, combined with one or more of the principles of our atmosphere, and also in combustible and incombustible, and perhaps animal and vegetable substances, seems to be probable; for it can be separated from such in various operations of nature and art.

However, although light may principally exist, combined with caloric and other substances, in a gaseous form, yet it may, and it is very generally admitted, that it does also exist, independent of caloric, in solid bodies and fluids of various kinds; and heat, it cannot be denied, exists sensibly in both combustible and incombustible bodies which exhibit no visible light. Indeed, that light and heat are two distinct existing principles is now finally and demonstrably ascertained and proved by Dr. Herschel's curious experiments of the fact, showing that heat as well as light are each liable to different and peculiar refractions, by which they are separated from each other, and take a separate and distinct existence in different space.

Quod restat desideratum est.

XXIII. *History of Astronomy for the Year* 1801. *By* JEROME LALANDE.

THE commencement of the 19th century was distinguished by an astronomical event, the discovery of a planet at Palermo in Sicily, by M. Piazzi, on the 1st of January*. It was as small as a star of the 8th magnitude: he observed it during forty days. The observations he sent me arrived too late for us to be able to follow it, and we were obliged to calculate its orbit from his own observations alone. Burckhardt, Olbers, Bode, Piazzi, and Gauss, have found that to represent these observations it is necessary to suppose that it

* I here employ the calendar of all nations, being persuaded that the French government will soon renounce the new calendar, which is not understood, and cannot be adopted either by our neighbours or by the majority of the French themselves.—L.

it revolves in four years. The following are the elements found by Burckhardt and Gaufs:

Inclination - - -	$10° 47'$	Inclination -	$10° 36' 57''$
Node - -	$2^s 20° 58$	Node - -	$2^s 21 0 44$
Aphelion -	$2 9 0$	Epoch - - -	$2^s 16° 28'$
Paffage of the aphelion Jan. 1, 1801	8 hours	Mean anom. -	$3 15 55$
		Aphelion	$10^s 26° 27' 38''$
Eccentricity -	0·0364	Eccentricity -	0·0825017
Semi-axis - -	2·574	Equation - - -	$9° 28'$
Revolution -	4·13 years	Diftance - - -	2·7355

The difference of thefe elements appeared to me to throw fome doubt on the reality of this orbit of four years; but in the beginning of the year 1802 it was perfectly confirmed, and we now have a ninth planet.

On the 25th of October we received a printed memoir of Piazzi, with his obfervations and calculations. As he hopes that this ftar will be acknowledged to be a planet, he has given it the name of Ceres Ferdinandea, in honour of the king of Naples; and Bode withes it to be called Juno: as for my part, I fhall call it Piazzi, as I gave the name of Herfchel to the planet difcovered in 1781. The pagan deities are no longer interefting, and adulation pleafes only the perfon who is the object of it.

On the 12th of July, in the evening, Meffier, Mechain and Bouvard, each found a fmall comet near the head of the Great Bear; and it appears that it was feen the preceding night by Pons, concierge of the obfervatory at Marfeilles. The Board of Longitude has given him the 600 francs which I depofited in the hands of a notary for the perfon who fhould difcover a comet; the three able aftronomers of Paris having themfelves judged that the artift ought to be encouraged.

John Louis Pons was born at Peyre, a village in the department of the Upper Alps, on the 24th of December 1761, and has refided at the obfervatory of Marfeilles fince the 3d of February 1789: his conduct, addrefs, and intelligence, have procured him much efteem from the director of the obfervatory. He conftructed the night-telefcope, with which he difcovered the comet of the 11th of July 1801, on the model of a telefcope by George Adams, which is at the School of Navigation, Marfeilles. The Board of Longitude have fent him a better one.

I have had a new proof of the utility of the fifty thoufand ftars which I have procured to aftronomy, and of the exact pofitions which my nephew Lefrançois has fixed for the ftars formerly obferved. Several of them have ferved to determine the places of the new comet, which Meffier, Mechain and

Bouvard followed with affiduity, and the orbit of which will be very well determined, though it appeared only ten days.

Thulis has fent me feven obfervations, from the 12th to the 21ft of July, deduced only from the azimuths and altitudes, without having been able to compare them with ftars; but we were more fortunate at Paris, and Mechain had fufficient data to calculate the elements in the following manner by a firft fketch:

Inclination -	25°	Perihelion	-	6^s	11°
Node -	0^s 8	Diftance	-	0	3
		Paffage Auguft 7, 15 hours.			

This fmall comet, found almoft at the fame time by four perfons, proves that it is not difficult to difcover comets. Three or four have been feen in the courfe of a year; and if a few amateurs would employ themfelves in fearching for them, it is probable that the number would rapidly increafe. This is ftill a defideratum in aftronomy; it is humiliating for us, that we do not know whether it is by thoufands or tens of thoufands that comets ought to be counted, and whether they return, or lofe themfelves in the immenfity of the univerfe.

Nothing is neceffary but a common telefcope to fearch for and find comets, and to point out their fituation to aftronomers. I fuppofe that the obferver is provided with a wooden quadrant of two feet radius, which any carpenter can make, and that a meridian has been traced out with a large circle on the floor; that the circle is divided into degrees, and that the inftrument is directed towards the place where the comet is. Both the altitude and diftance from the meridian will be thus found for the time of obfervation. Nothing more will be neceffary to enable aftronomers to find a comet which may have been announced. To find comets, it is not neceffary, therefore, to know the ftars. But there are a hundred nebulæ which have fome refemblance to fmall comets. Thofe who wifh to diftinguifh them muft have recourfe to the Celeftial Atlas *, where they are all marked. This ftudy will neither be long nor difficult. The Atlas of Berlin is much more complete: we fhall give an account of it hereafter.

The night-telefcope employed by Meffier, and with which he has already found twenty comets, is two feet in length, and has an aperture of 2 [illegible] inches: it has three eye-glaffes. The firft next the eye has a focus of $2\frac{1}{2}$ inches, and 10 lines of aperture; the fecond 9, and the third $9\frac{1}{3}$ inches. There are 10 lines between the two, and 5 inches between the pre-

* A Paris, chez Lamarche, rue du Foin.

 ceding

ceding and the second. There is a diaphragm of 14 lines between the first and the second eye-glass, at the distance of 2 inches from the former, and 3 from the second. This telescope magnifies only five times, but it has a field of four degrees. One of the same kind may be constructed for 70 or 80 francs.

Burckhardt also has calculated the orbits of the comets of 1763, 1771, and 1773; and for the second he has found a hyperbolic orbit.

The comet which Messier discovered on the 14th of June 1770, respecting which Burckhardt made long and learned calculations, seems to have a small circular orbit of five years seven months. However, this comet was not seen before 1770, nor has it appeared since. This can be ascribed only to great changes in its orbit.

Must we then, after having asserted, in the 18th century, that all the comets return, assert in the 19th that comets do not return, that of 1759 excepted?

On this account I no longer think but of comets; I speak of nothing but comets; and the only thing I recommend to my correspondents is to search for them, as I write to them that the only thing wanting to astronomy is the knowledge of these bodies.

On the 15th of May I had the pleasure of receiving the first copy of my *Histoire Céleste Françoise*, the fruit of twelve years labour, which terminates the 50,000 stars, on which my nephew Michel Lefrançois has employed the best part of his youth. It contains also observations made by D'Agelet before he set out on his voyage round the world; and those by which Darquier, aged 83 years, terminated his glorious astronomical career.

The observations of Tycho, Flamsteed, Picard, Lacaille, and Maskelyne, have been the foundation of all the progress made in astronomy. The most profound theories and the most learned calculations cannot do without them, and cannot dispute with them in regard to importance or duration. Observations alone will survive us; and observers, whom mankind too often affect to undervalue, may console themselves, that they will be the only astronomers to whom, long after their death, the praises and gratitude of our successors and of posterity will be addressed.

Lefrançois Lalande, my nephew, continues his observations, together with the calculation of 9000 declinations and 1000 right ascensions of the principal stars, each observed several times. These long and painful labours have obtained for this able observer a place in the National Institute. On the 26th of December madame Lefrançois Lalande continued the reduction of the 50,000 ſtars; an immenſe labour, to which ſhe has devoted herſelf with courage, and which her pregnancy even has not interrupted. Their ſon is preparing to tread in their ſteps, and already calculates with ſome ſucceſs. I hope that Iſaac Lalande will be the third aſtronomer of his name.

Delambre has obſerved ſeveral declinations with the multiplying circle. Piazzi has announced to us a catalogue of 700 ſtars, which he obſerved at Palermo; and Cagnoli is preparing a catalogue of 500 ſtars, which he obſerved at Paris and Verona with particular care.

Vidal, whoſe courage and exactneſs I have ſo often celebrated, has ſent me the continuation of the auſtral ſtars, which are not well ſeen at Paris; the circum-polar ſtars which were wanting, and a very ſingular *triduum*. On the 23d of April and the following days he obſerved all the planets every day. He has joined to them obſervations of Mercury and the ſun at the two ſolſtices with an ingenious compaſs, which he employed to make a great number of obſervations on the declination of the magnetic needle.

Bourg, an aſtronomer of Vienna, who gained the prize propoſed by the Inſtitute on the inequalities of the moon, continues to employ himſelf on that ſubject. He has recalculated, with 3000 obſervations, the 24 inequalities of the moon; and has added new ones, which were pointed out to him by Laplace according to his theory. Theſe tables arrived on the 8th of December; the errors do not amount to 15″; and the prize of 6000 francs, propoſed by the Board of Longitude to the firſt who ſhould make good lunar tables, will be well merited by this able and courageous aſtronomer. The Board of Longitude is ſtill employed in verifying them; but all the obſervations lately made at Gotha confirm the exactneſs of theſe tables. For it was at the obſervatory of Gotha, the ſanctuary of aſtronomy in Germany, that M. Bourg finiſhed his labour. Baron von Zach had invited him thither, that he might enjoy all the comforts and conveniences he could deſire.

What remains to be done in regard to the theory of the moon depends, perhaps, on ſome terms in which the higher powers of the eccentricities and forces muſt be employed. Burckhardt is now engaged in reſearches on that ſubject.

Arabian obſervations of the 10th century had been employed for the motions of the moon. The manuſcript which I fortunately found among the papers of Joſeph Deliſle made us deſirous of obtaining the original, which was at Leyden; and

and the Batavian minifter fent it to us. C. Cauffin examined this manufcript: but it is not complete; it contains only obfervations already known. We found in it none of the information fo much wifhed for refpecting the inftruments of the Arabians, and their method of obferving; but it has furnifhed us with fome interefting corrections for our copy, which is now printing in Arabic and French at the printing-office of the Republic by order of the minifter of the interior. The obfervations of the fummer folftice have again affured to us that the obliquity of the ecliptic is 23° 28′ 6½″, being 5″ more than in my tables. The multiplying circles give us the truth within a fecond; and I think we can with certainty fay that the diminution, which has occafioned fo much difpute, is 33″ per century; very far from that which C. Caffini affigned from bad obfervations made at the obfervatory with bad inftruments.

The winter folftice gives us 8″ lefs; but there is reafon to believe that this arifes from the refraction, which is not yet well enough known for fmall altitudes. Though the obliquity is nearly decided, the Academy of Berlin has ftill propofed this variation as the fubject of a prize for the year 1802. It requires the moft interefting refearches and explanations in regard to this fubject, where feveral points ftill remain to be cleared up.

All the planets have been eclipfed by the moon in the courfe of this year, as has been obferved by Reggio in the Ephemerides of Milan. This phænomenon is rare. We were not able to make a good obfervation but of the eclipfe of Venus on the 13th of May. We were, however, indemnified by the eclipfes of that beautiful ftar the Virgin's Spike, obferved in many places on the 30th of March and the 24th of May, which enabled me to verify the longitudes of feveral countries. Eclipfes of four ftars of the firft magnitude are phænomena of very great importance for all determinations of this kind.

I have continued to difcharge the tafk, which I impofed upon myfelf forty years ago, of calculating all the eclipfes of the fun and ftars hitherto obferved, and from which aftronomers had neglected to draw conclufions on account of the length of the calculations. I have corrected the longitudes of Rome and Middlebourg, and of the new city of Wafhington in America.

M. Leduc of Sermonetta, Gaëtani, and Conti have fent me obfervations from Rome. M. Ciccolini has fent me fome from Florence; and the day on which the king of Etruria came to the Inftitute, I had the pleafure of prefenting to him a determination of the longitude of Florence, which had been very badly determined, notwithstanding the celebrity of that capital, and the great number of eminent men it has produced.

M. Kautsch, a piarist of Leutomischel in Bohemia, has finished an immense labour on the eclipses of the sun. He has calculated, for the whole of the 19th century, charts in which are seen all the circumstances of these eclipses for every country of the earth where they are visible, in the same manner as they have been inserted in our Ephemerides since 1750, and at present in the *Connoissance des Temps* by the care of C. Duvaucel. I wished to have the means of publishing the labour of M. Kautsch, whose zeal and ability deserve every praise.

C. Goudin has also published an analytical method for eclipses: he has applied it to the eclipse of 1847, which will be the most considerable of this century, and has calculated all its circumstances for the whole surface of the earth.

The conjunctions of the planets are not interesting to astronomers, but they afford a spectacle to the public, especially when connected with other events. Messier, therefore, remarked, that when the cannon announced to us the happiness of peace on the 3d of October, the Moon, Venus, Jupiter, and Saturn, were near that beautiful star in the Lion's heart.

We no longer live in times when such phænomena are considered as of importance, but in 1186 the astronomers announced terrible revolutions in consequence of the conjunction of all the planets. I engaged C. Flauguergues to calculate this phænomenon exactly by our new tables, and he has found that on the 15th of September at 5 h. 2 m. all the planets were between 6 signs, and 6 signs 10 degrees of longitude.

These are not complete conjunctions; exact conjunctions of all the planets are incalculable; a sketch of these returns, in which I employed only days for the duration of the revolutions, gave me 17 millions of millions of years as the interval between one conjunction and another. What would the case have been, had I taken into account the hours and minutes?

As the tables of Mars were the most incorrect, C. Lefrançois has been employed on them six months; he has calculated all the observations, and has employed all the new perturbations. He has carried his precision to tenths of seconds, and has at length constructed tables of Mars, which will leave very little to be wished for, and which have appeared

appeared in the *Connoissance des Temps* for the year 12. I have seen, with pleasure, my immediate successor and dearest pupil pursue the labour which my master Lemonnier made me undertake, fifty years ago, in imitation of Tycho Brahe, who began his researches on the planet Mars, and who put Kepler in the way of making his discoveries by means of the same planet. He will soon employ himself on tables of Venus, taking into account her perturbations.

For Saturn, the error has been found + 1″ in longitude and — 9″ in latitude. Delambre has made new researches to correct the error of 30″ in the tables of Jupiter, but it has been found in the observations made for 60 years: we must therefore search for the cause of it in the theory, and in some new irregularities.

Bouvard has terminated his calculations of all the perturbations of the planets, each by the action of all the rest, according to the theory of Laplace. The result will be new tables, which will be still more correct.

C. Burckhardt has made an analytical and numerical calculation of the terms of the fifth order, which were not before taken into account in consequence of the length of the calculations. He has found that these terms increase the great irregularity of Saturn by one minute.

C. Chabrol has calculated observations of the sun, and has found that 7″ must be deducted from the longitudes given by our tables. But Delambre has undertaken to calculate 7 or 800 observations of Bradley, applying 8 or 10 new equations furnished by calculations of the attraction. The eccentricity of Jupiter and the earth give equations for the sun which amount to from 8 to 9″. We shall therefore soon have new tables of the sun still more correct than those published by Delambre ten years ago, and to which it appeared that nothing could be added.

For mercury, the error in my tables did not exceed 10″. An observation of Venus on the 24th of May gave me an error in the tables of + 30″. This seems to indicate that we ought to deduct 12″ from the epoch, but that the equation of the orbit is good.

The *Connoissance des Temps* for the year 12 (1804), which has just appeared, contains every thing most interesting that has occurred in regard to astronomy during the last year. Curious researches respecting the theory of the moon, by Laplace; new tables of Mars, by Lefrançois-Lalande; a new catalogue of stars reduced, making the number 11,300, being a continuation of those in the preceding volumes; important observations, tables, and calculations, by Mechain,

chain, Delambre, Chabrol, Vidal, Thulis, Flauguergues, Ciccolini, Duc Lachapelle, Burckhardt, Bernier, Humboldt, Quenot, and several calculations by myself; with a notice of the most important works that have appeared in the course of a year.

The Ephemerides of Vienna for 1802 contain a fourth series of longitudes determined by Triesnecker, who has calculated all the eclipses of the sun and stars which have been observed; a considerable and important labour, which remained to be undertaken. M. Triesnecker has given us at the same time a table of all the preceding results in regard to the position of towns where eclipses have been observed.

The third volume of the Memoirs of the Institute, the eighth volume of the Memoirs of the Italian Society, the Ephemerides of Berlin for 1803 and 1804, those of Milan for 1801, and the Journal of Baron von Zach during the whole year, have continued to furnish interesting observations and new memoirs. Baron von Zach has procured the observations made by Liesganig at Vienna from the year 1755 to 1774; and those which Niebuhr made in the Levant in 1761, and which he did not publish. We have received the Memoirs of the Academy of Berlin for 1796 and 1797, and the sixth volume of the Memoirs of Turin; but they contain nothing on astronomy.

The observatory of Paris, having acquired new instruments, has been put into activity by Mechain and Bouvard; and the Board of Longitude intends to print the observations of 1801 in the same form as those of Greenwich.

Caroché having finished the telescope of 22 feet, Tremel is employed in constructing a stand to support it; and the platform on which it is to be placed is already in a state of great forwardness. On all occasions we have enjoyed the happiness of having for minister a man long celebrated by his attachment to the sciences, and who, in order to be useful to them, wanted only that influence of which he is so worthy.

The transit telescope which Joseph Delisle had, in 1748, erected at the hotel de Cluny, and with which I as well as Messier made my first observations, had become almost useless by rust. The Board of Longitude wished that it should be reconstructed with platina, and our colleague Messier will have new assistance for his useful observations.

Lenoir has shown at Paris, during the public exhibition of the year 9, that the French industry is not inferior to the English; he received from government one of the twelve gold medals distributed to the most meritorious of our artists.

The

The Board of Longitude has ſent a quadrant to Flauguergues at Viviers, and one to Dangos at Tarbe, to enable them to make more correct and more continued obſervations.

Flauguergues has already employed his obſervations to determine the latitude of his obſervatory 44° 29′ 22″, greater by 18″ than what was given by the triangles; he has aſſiduouſly continued to obſerve the eclipſes of Jupiter's ſatellites; he has obſerved the ſpots of the ſun, which were frequent this year, and has calculated a great many poſitions of the ſtars.

Chabrol has communicated to us a new analytical method for eclipſes, and has calculated ſeveral; he has alſo verified the tables of Mars and Mercury by obſervations in the preſent year. He has reduced 600 obſervations of the ſtars, and has calculated 600 longitudes in the fundamental catalogue: in a word, he ſhows himſelf a young zealous co-operater, curious and void of ambition, who is entitled to our grateful acknowledgments.

C. Mougin, curé of La Grande-Combe-des-Bois, in the mountains of the department of Doubs, has ſent us a large table of preceſſions; that is to ſay, of the annual changes of the ſtars in right aſcenſion, according to the data with which I furniſhed him. For thirty years paſt we have received from this worthy paſtor marks of zeal, application, curioſity, and courage, very rare, eſpecially in the deſerts.

Dr. Maſkelyne has ſent us his obſervations of 1800, a continuation of the valuable collection he has been making for 36 years; and he has announced to us the Nautical Almanac of 1806.

[To be concluded in our next.]

XXIV. *Some Account of the Poiſonous and Injurious Honey of North America.* *By* BENJAMIN SMITH BARTON, *M.D.**

IN the year 1785 I had an opportunity of obſerving ſome of the diſagreeable effects of our wild honey upon ſeveral perſons who had eaten of it, in the weſtern parts of Pennſylvania, near the river Ohio. From theſe effects I was perſuaded, that a ſubſtance which is generally conſidered as entirely innocent, is capable of doing much injury to the conſtitution. I was, therefore, induced to pay ſome

* Read before the American Philoſophical Society, and communicated to the Editor of the Philoſophical Magazine by the author. It will form part of the fifth volume of the American Tranſactions.

attention

attention to the subject. The result of my inquiries I now communicate to the Philosophical Society.

It is not necessary to make any remarks on the fabric of honey. It may be sufficient to observe, that the honey will always partake, in a greater or a lesser degree, of the smell, the taste, and general properties, of the flowers from which it is obtained. This obvious fact should have solicited more of the attention of those whose employment it is to raise large numbers of bees for the purpose of obtaining the valuable product of these little insects. But, in this country at least, hardly any attention has been paid to the subject. Perhaps, the following loose hints, by pointing out some of the sources from which an ill-flavoured or pernicious honey is obtained, may be of some service to the new or remote settlers of our country.

I must observe, that in these hints I do not mean to include among the disagreeable consequences of the eating of honey, the occasional effect of its purging: for although, as I shall presently observe, a purging is one of the common effects of the poisonous honey, yet the most innocent honey will often induce the same state of the body, when it is eaten in large quantities, or when it meets with an irritable state of the bowels.

The honey which I call deleterious or poisonous honey, produces, as far as I have learned, the following symptoms, or effects: viz. in the beginning, a dimness of sight or vertigo, succeeded by a delirium *, which is sometimes mild and pleasant, and sometimes ferocious; ebriety, pain in the stomach and intestines, convulsions, profuse perspiration, foaming at the mouth, vomiting, and purging; and, in a few instances, death. In some persons, a vomiting is the first effect of the poison. When this is the case, it is probable that the persons suffer much less from the honey than when no vomiting is induced. Sometimes the honey has been observed to produce a temporary palsy of the limbs; an effect which I have remarked in animals that have eaten of one of those very vegetables † from whose flowers the bees obtain a pernicious honey.

Death is very seldom the consequence of the eating of this

* An intelligent friend of mine related to me the case of a person who, for a short time, was severely affected from the eating of wild honey, in Virginia. He imagined that a person seized him rudely by one arm, and then by the other. After this he fell into convulsions, from which, however, he recovered in about an hour. It was imagined that this honey was obtained from a kind of poisonous mushroom.

† The Kalmia latifolia.

this kind of honey*. The violent impreffion which it makes upon the ftomach and inteftines often induces an early vomiting or purging, which are both favourable to the fpeedy recovery of the fufferer. The fever which it excites is frequently relieved, in a fhort time, by the profufe perfpiration, and perhaps by the foaming at the mouth. I may add, that as the human conftitution refifts, to an aftonifhing degree, the effects of the narcotic and other poifonous vegetables that are beft known to us, fo we need not wonder that it alfo refifts the effects of the deleterious honey which is procured from fuch vegetables.

It deferves to be mentioned, that the honey which is formed by two different hives of bees in the fame tree, or at a little diftance from each other, often poffeffes the moft oppofite properties. Nay, the honey from the fame individual comb is fometimes not lefs different in tafte, in colour, and in its effects. Thus, one ftratum or portion of it may be eaten without the leaft inconvenience, whilft that which is immediately adjacent to it fhall occafion the feveral effects which I have juft enumerated.

I have taken fome pains to learn what are the figns by which the deleterious honey may, at firft view, be diftinguifhed from innocent honey. I am informed that there is no difficulty in the matter.

The poifonous honey is faid by fome to be of a crimfon colour; by others, it is faid to be of a reddifh-brown colour, and of a thicker confiftence than common innocent honey.

Thefe are the figns by which, I am told, the moft experienced hunters in the fouthern parts of North America are enabled to diftinguifh pernicious from innocent honey.

On a fubject fuch as this, I feel every difpofition to pay a good deal of deference to the experience of an American hunter. Even philofophers may obtain much ufeful information from hunters, however wandering their life, however rude their manners. It is in the power of our hunters to enrich natural hiftory with many important facts. But we ought not, I prefume, to confide implicitly in every thing they tell us.

I have good reafons for doubting whether the figns which I have mentioned will enable us, in every inftance, to determine whether honey be poifonous or innocent.

The honey of the bee, undoubtedly, fometimes partakes of the colour of the flowers from which it is gathered. The bees gather honey from many flowers of a crimfon colour,

* We fhall afterwards fee that not one of Xenophon's men died from the deleterious honey which they had eaten, in large quantities, on the fhores of the Euxine fea.

and

and from many flowers whose colour is a reddish brown. In these cases, it is probable that the honey will sometimes borrow, in some degree, the colour of the flowers. Yet there are many crimson-coloured and reddish-brown coloured flowers that are perfectly innocent. The honey obtained from them will, I presume, be innocent also. Mr. Bruce says he was surprised to see, at Dixan, in Abyssinia, "the honey red like blood; and nothing," he remarks, "can have an appearance more disgusting than this, when mixed with melted butter *." Nothing is said, by this author, that can lead us to suppose that the Dixan honey was poisonous. From the manner in which it is mentioned, it is pretty evident that it was not poisonous. Linnæus informs us, that in Sweden the honey, in the autumn, is principally gathered from the flowers of the erica, or heath, and that this honey is of a somewhat reddish colour; and accordingly, he observes, those provinces of the country that are destitute of the heath, such as the province of Oelandia, furnish a white honey †. The great naturalist says nothing concerning the properties of the heath honey. However, we may presume, when we recollect the minute accuracy of Linnæus, that this honey did not possess any dangerous properties, otherwise he would have noticed the circumstance. Whilst I resided in Edinburgh I had the honey from the Highlands frequently brought to my table. I often remarked that this honey had a dirty brownish colour; and I was told that it was chiefly procured from the different species of erica, perhaps principally from the "blooming hather ‡," which abound in the Highlands. I never heard the people in Edinburgh, although they consume large quantities of this honey, complain that it possesses any noxious property. If it were actively poisonous or injurious, the quality would have been long since observed. I well remember, however, that, for two years that I used it, it almost always rendered me drowsy. Sometimes, indeed, it composed me to sleep as effectually as a moderate dose of laudanum would have done. A foreigner, who had not been accustomed to eat anodyne honey, was better capable of remarking the effect which I have mentioned than the natives, who had been in the habit of using it from their infancy. I do not find that this singular property of the Scots honey has been noticed by any writer §. I have therefore related it, though

* Travels to discover the Source of the Nile, vol. v. or Appendix, p. 151. quarto edition.

† Fauna Suecica.

‡ Burns.

§ Dr. Withering says, bees extract a great deal of honey from the flowers though it rather opposes any objection to the signs employed by our hunters to distinguish poisonous from innocent honey. But he who is studious of truth should relate useful facts as they are, without regarding what is their connection with a favourite system or opinion.

The learned Joseph Acosta speaks of a gray-coloured honey-comb which he saw in the province of Charcas in South America. The honey of this comb, he says, is "sharp and black." He says nothing further of its properties *.

An ingenious friend of mine †, to whom the public are indebted for a variety of valuable information concerning the natural productions of various parts of North America, informs me, that, in the Carolinas and Floridas, the poisonous honey is often so similar in colour, taste, and odour, to the common or innocent honey, that the former cannot be distinguished from the latter. It is owing, he says, to this circumstance that so many accidents daily happen from the use of the wild honey. He was informed that it is experience alone which enables the hunters and others to determine whether the honey which they find in the woods be poisonous or innocent. They have observed that the injurious effects manifest themselves in a short time after the honey is taken into the stomach. They are accustomed, therefore, to eat a small quantity before they venture to satisfy their appetite. Should this produce *any* disagreeable effects, they do not think it prudent to continue the use of it. But if, in a short time, it should occasion no inconvenience, they think they may, with perfect safety, indulge their appetite to the full.

I have been informed that the poisonous honey, by boiling and by straining, may be rendered as innocent as any honey whatever. It is likewise said, that by long keeping it becomes harmless.

The honey of which I am treating is poisonous to dogs as well as to men.

Hitherto I have not been able to obtain any certain information concerning the means to be pursued in the treatment of persons labouring under the effects of the poisonous honey. It is said that the Indians, and some of the whites, use cold bathing with advantage. It is probable that this

flowers of the *erica vulgaris*, or common heath; and he remarks that, "where heath abounds, the honey has a reddish cast." A Botanical Arrangement of British Plants, &c. vol. i.

* The Naturall and Morall Historie of the East and West Indies, &c. p. 303.

† Mr. William Bartram.

practice has been useful. As the effects produced by this honey are so similar to those produced by several narcotic vegetables that are well known to us, such as opium, henbane *, thorn-apple †, &c. it is probable that the same means of treatment will be found useful in both cases. Of those means it is not necessary to make particular mention in this place.

It would be curious to ascertain whether the bees are ever injured or destroyed by the quaffing of the nectar of the flowers from which they prepare the poisonous honey. It is probable that they are; and, perhaps, some of the diseases of these little insects may arise from this source ‡. It is true, indeed, that there are some poisonous plants the nectar of which the bees will not touch. This is the case with the *fritillaria imperialis*, or crown imperial §. I do not remember to have seen bees in or immediately about the flowers of the common rosebay or oleander ||, in the tube of which there is a fluid which destroys thousands of the common house flies. But what is called instinct is not always sure. The bees may prepare an honey from plants that are very injurious to them. The excellent Mr. Evelyn, speaking of the elm, says, "But I hear an ill report of this tree for bees, that, surfeiting of the blooming seeds, they are obnoxious to the lask ¶, at their first going abroad in spring, which endangers whole flocks if remedies be not timely exhibited; therefore it is said, in great elm countries they do not thrive; but the truth of which I am yet to learn **."

In South Carolina, in Georgia, and in the two Floridas, but more especially in East Florida, the instances of injuries from the eating of wild honey are more numerous than in any other parts of North America that are known to us.

There is a tract of country included between the rivers

* Hyoscyamus niger.

† Datura stramonium.

‡ Dr. James E. Smith asserts that the honey or nectar of plants is not poisonous to bees. Syllabus to a Course of Lectures on Botany, p. 23. I have some good reason to believe that, sometimes at least, the contrary is the case.

§ Linnæus, speaking of this plant, says: "Nulla, excepto Melianthο, copiosiori melle scatet planta, quam hæc; sed apes id non colligunt!" Prælectiones in Ordines Naturales Plantarum, edidit Giseke, p. 287. Hamburgi 1792.

|| Nerium oleander.

¶ This is one of the most mortal diseases of bees. It is beautifully described, and the remedies for it mentioned, by Virgil, Georgic. lib. iv. l. 251—280.

** Silva, or a Discourse on Forest Trees, &c. p. 133 and 134. Doctor Hunter's edition.

St.

St. Illa and St. Mary's, in East Florida, that is remarkable for abounding in vast numbers of bees. These insects, which were originally introduced into Florida by the Spaniards *, have increased into innumerable swarms, from the facility with which they procure their food, in perhaps the richest flowered country of North America. In this tract of country the alarming effects of the wild honey are often experienced by the settlers, by wandering hunters, and by savages.

It is highly probable that this poisonous honey is procured from a considerable number of the flowers of the countries which I have mentioned. A complete list of these flowers would be acceptable; but such a list it will be difficult to procure at present. Perhaps my hints may induce some intelligent native of the country to favour us with his observations on the subject. Meanwhile, I am happy to have it in my power to mention some of the vegetables from whose flowers the bees extract a deleterious honey, not only in the country between the St. Illa and St. Mary's, but also in some other parts of North America.

These vegetables are the *kalmia angustifolia* and *latifolia* of Linnæus, the *kalmia hirsuta* of Walter †, the *andromeda mariana*, and some other species of this genus.

I. Every American has heard of the poisonous properties of the kalmia angustifolia and latifolia. The former of these plants is known, in the United States, by the names of dwarf-laurel, ivy, lambkill, &c. It has long been known, that its leaves, when eaten by sheep, prove fatal to them. The following fact will show that the flowers likewise are endued with a poisonous property.

About twenty years since, a party of young men, solicited by the prospect of gain, moved, with a few hives of bees, from Pennsylvania into the Jerseys. They were induced to believe that the savannas of this latter country were very favourable to the increase of their bees, and, consequently, to the making of honey. They accordingly placed their hives in the midst of these savannas, which were finely painted with the flowers of the kalmia angustifolia. The bees increased prodigiously, and it was evident that the principal part of the honey which they made was obtained from the flowers of the plant which I have just mentioned. I cannot learn that there was any thing uncommon in the appearance of the honey: but all the adventurers who ate of it became intoxicated to a great degree. From this experiment, they were sensible

* See Transactions of the American Philosophical Society, vol. iii. no. 31.

† Flora Caroliniana, p. 138.

that

that it would not be prudent to ſell their honey; but, unwilling to loſe all their labour, they made the honey into the drink well known by the name of metheglin, ſuppoſing that the intoxicating quality which had reſided in the honey would be loſt in the metheglin. In this reſpect, however, they were miſtaken. The drink alſo intoxicated them; after which they removed their hives.

In North-Carolina, this ſpecies of kalmia and the andromeda mariana are ſuppoſed to be the principal vegetables from which the bees prepare the poiſonous honey, that is common in that part of the United States.

II. The kalmia latifolia, known in the United States by the names of laurel, great-laurel, wintergreen, ſpoon-haunch, ſpoon-wood, &c. is alſo a poiſon. Its leaves, indeed, are eaten with impunity by the deer*, and by the round-horned elk†. But they are poiſonous to ſheep, to horned-cattle and to horſes. In the former of theſe animals, they produce convulſions, foaming at the mouth, and death. Many of General Bradock's horſes were deſtroyed by eating the leaves and the twigs of this ſhrub, in the month of June 1755, a few days before this unfortunate general's defeat and death. In the ſevere winter of the years 1790 and 1791, there appeared to be ſuch unequivocal reaſons for believing that ſeveral perſons, in Philadelphia, had died in conſequence of their eating our pheaſant‡, in whoſe crops the leaves and buds of the kalmia latifolia were found, that the mayor of the city thought it prudent, and his duty, to warn the people againſt the uſe of this bird, by a public proclamation. I know that by many perſons, eſpecially by ſome lovers of pheaſant-fleſh, the circumſtance juſt mentioned was ſuppoſed to be deſtitute of foundation. But the foundation was a ſolid one. This might be ſhown by ſeveral well-authenticated facts. It is ſufficient for my preſent purpoſe to obſerve, that the collection of a deleterious honey from the flowers of this ſpecies of kalmia gives ſome countenance to the opinion, that the fleſh of pheaſants that had eaten of the leaves and buds of this plant may have been impregnated with a pernicious quality§.

* Cervus Virginianus of Gmelin.

† Cervus Wapiti, *mihi*.

‡ Tetrao Cupido of Linnæus.

§ It is not a new ſuſpicion, that the fleſh of animals that have eaten of the leaves, &c. of deleterious vegetables is ſometimes endued with a poiſonous property. Georg. H. Welſchius, a very learned German writer, quoted by Dr. Haller, (ſee *Hiſtoria Stirpium Indigenarum Helvetiæ Inchoata*, tom. i. p. 443.) ſays, that the fleſh of a hare which was fed with the leaves of the rhododendron ferrugineum proved mortal to the gueſts. This ſpecies of rhododendron is a native of Switzerland, Siberia, and other parts of the Old World.

I have

I have been informed, that our Indians ſometimes intentionally poiſon themſelves with a decoction of the leaves of this kalmia. The powder of the leaves has been employed (but I ſuſpect with little advantage) in the inflammatory ſtage of certain fevers. From experiments made upon myſelf, I find that this powder is ſternutatory.

To ſome conſtitutions the flowers of the kalmia latifolia, even externally applied, are found to prove injurious.

III. The kalmia hirſuta appears to poſſeſs nearly the ſame properties as the two ſpecies which I have juſt mentioned. This pretty little ſhrub is a native of South-Carolina, Georgia, and Florida.

In Georgia and in Florida, this ſpecies of kalmia is ſuppoſed to be the principal vegetable from which the deleterious honey in thoſe parts of our continent is procured.

IV. The andromeda mariana, or broad-leafed moorwort, is a very common plant in many parts of North America. The leaves are poiſonous to ſheep. The petioli, or foot-ſtalks of the leaves, and the ſeeds within the ſeed-veſſel, are covered with a brown powder, ſimilar to that of the kalmiæ. This powder applied to the noſtrils occaſions violent ſneezing*. From the flowers of this plant the bees extract conſiderable quantities of honey; and it deſerves to be mentioned that this honey, as well as that obtained from ſome other American ſpecies of andromeda, has frequently the very ſmell of the flowers from which it is obtained†.

I have already obſerved, that it is highly probable, that the American poiſonous honey is procured from the flowers of a conſiderable number of the plants of the country. I have mentioned but a few of them. But there are many others which I have ſome reaſons for ſuſpecting are alſo capable of affording an injurious honey. Indeed, every flower that is poiſonous to man, and is capable of affording honey, may

* For ſome information relative to the properties of the andromeda mariana, ſee Collections for an Eſſay towards a Materia Medica of the United States, pages 19, 20, 47. Philadelphia, 1798.

† In juſtice to the fine genus of andromeda, I muſt obſerve, that all the ſpecies do not furniſh a pernicious honey. The andromeda nitida or lucida of Bartram affords an abundance of nectar, or honey. The flowers of this ſpecies are called by the country people of Carolina and Georgia, "honey-flowers," not, however, merely from the circumſtance juſt mentioned, but from the regular poſition of the flowers on the peduncle, which open like the cells of a honey-comb, and from the odour of theſe flowers, which greatly reſembles that of honey. This ſpecies grows abundantly in the ſwamps called bay-galls. The inhabitants of Carolina are univerſally of opinion, that it affords the greateſt quantity of honey, and that of the beſt quality.

produce an honey injurious to man; ſince the properties of this fluid are ſo dependent upon the properties of the plants from which it is procured. There is, therefore, more poetry than philoſophy in the following lines of Mr. Pope:

" In the nice bee, what ſenſe ſo ſubtly true
" From pois'nous herbs extracts the healing dew?"
Eſſay on Man, Epiſtle i. lines 211 & 212.

I have been informed that in the ſouthern parts of our continent, there is a plant, called hemlock, from the flowers of which the bees prepare a honey that is poiſonous. The flowers are ſaid to be yellow, and the root a deadly poiſon. I do not know what plant this is. Moſt probably, it is ſome umbelliferous plant, perhaps a cicuta, an angelica, or a ſcandix.

Some ſpecies of agaricus, at leaſt ſome fungous vegetables, that grow in the ſouthern ſtates, are extremely poiſonous. As accidents from the uſe of deleterious honey have happened in the ſame countries in which theſe poiſonous fungi grow, it has been ſuppoſed, and aſſerted, that the poiſonous honey is prepared from a dew that collects upon theſe fungi. Perhaps, this ſuppoſition is not entirely devoid of foundation*.

I ſhall now mention a few vegetables from the flowers of which, I think, it will be found, that the bees collect a poiſonous or injurious honey. Theſe are:

* If the celebrated author of the *Recherches Philoſophiques ſur les Americains* be ſtill living, this account of our poiſonous and injurious honey (ſhould my memoir fall into his hands) would afford him ſome entertainment. I would adviſe him to connect the facts, which I here communicate, with the remarks concerning our inſects contained in the firſt volume of the *Recherches* (ſee p. 169 and 170.) I hope, however, that Mr. De Pauw, who, notwithſtanding his love of ſyſtem and his many errors, is certainly a man of great reading, will recollect, that the Greek and Roman writers (as we ſhall afterwards ſee) have ſaid much concerning the poiſonous honey of various parts of the Old World. And now let me add, that in America there is as good honey as in any other parts of the world; and there is not a ſcarcity of this good honey. The honey which is collected from the flowers of the tulip-tree (liriodendron tulipifera), the buckwheat (polygonum fagopyrum), the red maple (acer rubrum), the clover (trifolium), and many other plants, is excellent. The Abbe Clavigero ſays the bee of Yucatan and Chiapa makes " the fine clear honey of Eſtabentun, of an aromatic flavour, ſuperior to that of all the other kinds of honey with which we are acquainted." *The Hiſtory of Mexico,* vol. i. p. 68.† Perhaps on ſome future occaſion I may communicate to the Philoſophical Society a liſt of thoſe indigenous vegetables which, as furniſhing an innocent and excellent honey, are worthy of preſervation in the neighbourhood of apiaries. The liſt is an extenſive one.

† This fine honey, according to the Mexican hiſtorian, is " made from a fragrant white flower like jeſſamine, which blows in September."

I. The

I. The rhododendron maximum, or Pennſylvania mountain laurel. This belongs to a very active genus of plants. We have already ſeen, that one of the ſpecies, the rhododendron ferrugineum, was, long ago, obſerved to produce the ſame effects which have been aſcribed to the kalmia latifolia. Another ſpecies, the rhododendron cryſanthum, has been found a powerful medicine, and has been uſed, in Ruſſia, with much advantage, in the iſchias, in chronic rheumatiſm, and in other diſeaſes; and we ſhall immediately ſee that from another ſpecies a poiſonous honey has been procured in the neighbourhood of the Euxine Sea. The footſtalks of the leaves, and alſo the ſeeds, of our rhododendron maximum are covered with the ſame brown powder as I obſerved covered the leaf-footſtalks and the ſeeds of ſeveral of the andromedæ, and the kalmiæ. This powder in the rhododendron, as well as in the andromedæ and kalmiæ, excites ſneezing, and it is curious to obſerve that a ſneezing is mentioned by Dioſcorides among the ſymptoms produced by the honey about Heraclea Pontica. That honey, as will be preſently ſhown, is procured from the rhododendron ponticum.

II. The azalea nudiflora. This fine ſhrub, is well known in Pennſylvania, and other parts of the United States, by the name of wild honeyſuckle. Of its properties I know nothing certain. It has, however, too much of the family face, and is too frequently found in company with the rhododendron maximum, and the kalmiæ, not to make me ſuſpicious that it partakes alſo of the characters of theſe deleterious vegetables. Moreover, a ſpecies of this genus, the azalea pontica of Linnæus, is ſuppoſed to be the ægolethron of Pliny, who mentions it as the plant from which the poiſonous honey about Heraclea Pontica is prepared. The tube of the flower of our azalea is perforated by the large bee, called bumble-bee.

III. Datura ſtramonium. This plant is known by a variety of names, ſuch as Jameſtown-weed, gymſin, ſtinkweed, French-cheſnut. Its active and poiſonous properties are now pretty generally known. Children have often been injured by eating the ſeeds. The tube of the flower contains a conſiderable quantity of honey. This honey is bitter, and has much of the poiſonous ſmell. Bees quaff it. But admitting that it is of a poiſonous nature, it does not follow that our *cultivated* bees (if I may be allowed to uſe this expreſſion) will collect ſo much of this honey as to prove injurious to thoſe who eat of it. But, in particular places, where this plant has been permitted to increaſe to a great degree, large quantities of honey may be collected from it:

and I cannot help suspecting that the use of this honey may prove injurious*.

Some of the ancient writers of Greece and Rome have related instances of the deleterious properties of the honey of certain countries. The botanist Dioscorides, speaking of the rhododendron ponticum, a species of the same genus to which our mountain laurel belongs, has the following words: "About Heraclea Pontica, at certain seasons of the year, the honey occasions madness in those who eat of it; and this is undoubtedly owing to the quality of the flowers from which the honey is distilled. This honey occasions an abundant sweating; but the patients are eased by giving them rue, salt-meats, and metheglin, in proportion as they vomit. This honey," continues the Greek botanist, "is very acid, and causes sneezing. It takes away redness from the face, when pounded with costus. Mixed with salt or aloes, it disperses the black spots which remain after bruises. If dogs or swine swallow the excrements of persons who have eaten of this honey, they fall into the same accidents†."

Pliny has also taken notice of this poisonous honey. "In some years," says the Roman naturalist, "the honey is very dangerous about Heraclea Pontica. It is not known to authors from what flowers the bees extract this honey. Here is what we have learned of the matter. In those parts, there is a plant called ægolethron, whose flowers in a wet spring acquire a very dangerous quality, when they fade. The honey which the bees make of them is more liquid than usual, more heavy, and redder. Its smell causes sneezing. Those who have eaten of it sweat excessively, lie upon the ground, and call for nothing but cool drinks‡." He then makes the very remarks which I have quoted from Dioscorides, whose words, indeed, as Mr. Tournefort observes, he seems to have merely translated. The following remark, however, appears to belong to Pliny. "Upon the same coast of the Pontus, there is found another sort of honey, which is called mænomenon§, because those who eat of it are rendered mad. It is supposed, the bees collect it from the flowers of the rhododendros, which is common among the forests. The people of those parts, although they pay the Romans a part of their tribute in wax, are very cautious how they offer them their honey‡."

* See the late Dr. Samuel Cooper's Inaugural Dissertation on the Properties and Effects of the Datura Stramonium, p. 33. Philadelphia, 1797.

† Dioscorides, as quoted by Mr. Tournefort.

‡ C. Plinii Secundi Naturalis Historiæ Lib. xxi. cap. 13.

§ From the Greek verb μαινομαι, insanio.

‡ Ibid.

The

The Greeks and the Romans have often defcribed the various plants that were known to them, in fuch dark and obfcure terms, that the botanifts of modern times are frequently at a lofs to determine, not merely the fpecies but alfo the genus the ancient writers have mentioned. With refpect, however, to the plants which I have juft mentioned, the difficulty does not feem to be great. Mr. Tournefort has, I think, fhown, in a very fatisfactory manner, that the ægolethron of Pliny is the chamærhododendros pontica maxima, Mefpili folio, flore luteo of his *Inftitutiones*, a plant fince defcribed by Linnæus and by other botanifts by the name of azalea pontica. Mr. Tournefort has likewife fhown, that the other plant called by Pliny rhododendros is his chamærhododendros pontica maxima, folio laurocerafi, flore cæruleo purpurafcente*. This is the rhododendron ponticum of Linnæus. It is confiderably allied to the azalea pontica.

Xenophon has recorded the remarkable effects of fome poifonous honey, in his celebrated work called *Memorabilia*.

When the army of the ten thoufand had arrived near Trebifond, on the coaft of the Euxine or Black Sea, an accident befel the troops, which was a caufe of great confternation. "As there were a great many bee-hives," fays the illuftrious general and hiftorian, "the foldiers did not fpare the honey. They were taken with a vomiting and purging, attended with a delirium, fo that the leaft affected feemed like men drunk, and others like mad men, or people on the point of death. The earth was ftrewed with bodies, as after a battle; not a perfon, however, died, and the diforder ceafed the next day, about the fame hour that it began. On the third and fourth days, the foldiers rofe, but in the condition people are in after taking a ftrong potion†."

The fame fact is recorded by Diodorus Siculus.

Mr. Tournefort thinks there is every probability that this poifonous honey was fucked from the flowers of fome fpecies of chamærhododendros, or rhododendron. He obferves that all the country about Trebifond is full of the fpecies of this plant, and he remarks that Father Lambert, Theatin miffionary, agrees that the honey which the bees extract from a certain fhrub in Colchis or Mingrelia, is dangerous, and caufes vomiting. Lambert calls this fhrub oleandro giallo, or the yellow rofe-laurel, which Mr. Tournefort fays is, without difpute, his chamærhododendros pontica maxima,

* Inftitutiones, &c.

† Thefe are nearly the words of Mr. Tournefort's tranflation. I am forry that I have not the original work of Xenophon at hand.

Mespili folio, flore luteo*; the azalea pontica, already mentioned.

There are several passages in the Roman poets, which plainly show, that they were no strangers to the poisonous properties of certain kinds of honey. It is not necessary to mention all these passages. But the following are worthy of notice.

Virgil cautions us not to suffer a yew tree to grow about bee-hives:

> Neu propius tectis taxum sine.————
>
> Georgic. Lib. iv. l. 47.

In his 9th Eclogue, the same philosophic poet speaks of the yews of Corsica as being particularly injurious to bees.

> Sic tua Cyrnæas fugiant examina taxos. l. 30.

The honey of Corsica was, as Dr. Martyn strongly expresses it, "infamous for its evil qualities†."

The raising of bees, for the purposes of procuring their honey and their wax, may, at some future period, become an object of great importance to the United-States. Surely then it would be a matter of consequence to attend to the cultivation or preservation of those vegetables which furnish an innocent and a well-flavoured honey, and a good wax. But even in a more limited view of the subject, some knowledge of these vegetables seems to be indispensably necessary. And in the new settlement, whither the settler has carried his bees,

* See Tournefort's Voyage into the Levant, vol. iii. p. 68. English translation. London, 1741.

† See his translation of the Georgics of Virgil, note to line 47, in book iv. Dr. Martyn's criticisms and annotations always demand attention. I greatly doubt, however, if the taxus of Virgil be the common yew, or any species of that genus. Martyn himself allows, that "it does not appear from other writers (beside Virgil), that Corsica abounded in yews." I have been assured, that the yew is not an indigenous vegetable in that island, and that it is even rare among the foreign vegetables. It may, indeed, be said, perhaps it was common in the time of Virgil. I would observe, that the yew is much less poisonous than has been commonly supposed. I know not that any modern writer has pretended that the bees procure a pernicious honey from its flowers. These facts give rise to my suspicion, that the taxus of Virgil was not the yew, or taxus of the modern botanists. If not the yew, what vegetable was it? Perhaps, the buxus virens, or box. This vegetable abounds in Corsica, where to this day it is known by the name of *taxo*. The gentleman from whom I received this information assured me, that the bees of Corsica are very fond of the flowers of the box, and that the honey from this source is reputed poisonous. The box is, unquestionably, a poisonous vegetable. But there is still a difficulty in the case. Virgil mentions both taxus and buxus. I think there can be no doubt that his buxus (see Georgic. lib. ii. l. 449.) is the buxus of the modern botanists.

where

where improvements are ftill very imperfect, it cannot be deemed a trivial tafk to have pointed out fome of thofe vegetables from which an injurious honey is obtained.

The ancients, who, in fome refpects at leaft, were equal to the moderns, appear to have paid much attention to this fubject. Virgil* and Columella have both told us what plants ought to grow about apiaries. It is unneceffary to repeat, in this place, what the two Roman writers have faid on the fubject. The *Georgics* of the Mantuan poet are in the hands of every man of tafte; and the work of Columella† *fhould* be read, wherever agriculture engages the attention of gentlemen.

The proper management of bees may be confidered as a fcience. It is not fufficient that bees merely make honey and wax. Their honey may be injurious or poifonous, and their wax may be nearly ufelefs. To affift and to direct the labours of thefe little infects, the knowledge and the hand of man are required. Let, then, this interefted being be at leaft attentive to his own benefits and pleafures. Let him carefully remove from about the habitations of his bees every fetid or poifonous vegetable, however comely its colour or its form. In particular, let him be careful to remove thofe vegetables which are noxious to himfelf. In place of thefe, let him fpread the "marjoram and thyme," and other plants, "the love of bees‡," and his labours will be rewarded. He may, then, furnifh his table with an honey not inferior to that of Mount Hermettus, or of Athens; nor to that of Sicily, to which Virgil has fo handfomely alluded in the feventh Eclogue:

> Nerine Galatea, thymo mihi dulcior Hyblæ,
> Candidior cycnis, hederâ formofior albâ. L. 37, 38.

XXV. *Defcription of the Arfeniates of Copper and of Iron. By the Count de* BOURNON.

[Concluded from p. 12.]

THE modification which we have juft feen the primitive cryftal affume at one of its folid angles, and only on one fide, fometimes takes place alfo (only on one fide) at its two other angles. Then, if the act of cryftallization has continued fo long, under the fame mode of increafe, that the new

* See Georgicorum lib. iv. l. 30—32.
† De Re Ruftica, libri xii.
‡ Armftrong.

edges, as AB, (fig. 15.) produced by the replacing of the solid angles, unite together, and give birth to a new equilateral triangle, placed in a direction contrary to the primitive one; and if, at the same time, the crystal has such a length that the secondary planes terminate at the opposite base, and are very acute isosceles triangles, the crystal will present the appearance of a kind of truncated hexaedral pyramid, the base and apex of which will be equilateral triangles. (fig. 18. Pl. IV.) The six triangular planes which compose the pyramid of this crystal, are always acute isosceles triangles; but three of them have their acute angle much smaller than the three others. The sides of the base of this kind of pyramid are opposite to the least acute angles; and its truncated apex is opposite to the most acute ones; the triangular planes being placed alternately in an opposite direction. I have seen several instances of this form; but I never saw such intermediate varieties as the secondary plane, represented by the dotted lines in fig. 15. would give, if it existed at the same time in the three angles.

By a longer duration of the act of crystallization, under the same modification, the plane corresponding to the truncated apex of the pyramid (fig. 18.) becomes progressively smaller; the most acute isosceles triangular planes, which answer to the secondary ones, encroach on the least acute, all which are the primitive planes of the crystal, and the pyramid becomes truly triedral at its upper extremity, whilst it remains hexaedral at the base, on account of those parts of the three planes of the primitive crystal which are still preserved. (Fig. 19.)

By a still more considerable duration of the act of crystallization, the pyramid would become completely triedral, and would not be truncated at its apex. I have never met with this modification so complete; but I have seen the variety represented in fig. 19; which, however, as well as fig. 18. is very uncommon.

The triedral prism is subject to a fourth modification, which takes place at the three edges of one of its two bases or terminal surfaces only, and replaces each of those edges by a plane, much more inclined on the side of the prism on which it is placed than on the terminal surface. (Fig. 20.) I have not been able to determine, in these crystals, the angles formed by these new planes, either with the sides of the prism or with the terminal surfaces; but the varieties belonging to this modification demonstrate that these angles are the same as those which the secondary planes of the solid angles make, either with the terminal surfaces, or with the edges

edges of the prifm on which they are inclined. When thefe new planes have acquired an increafe of fufficient extent to make the primitive planes of the prifm totally difappear, and to replace them, the cryftal is changed to a triedral pyramid with a truncated apex; the bafe and truncated apex of which are equilateral triangles. (Fig. 21.) When it happens that the cryftal has, at the fame time, gone through this modification and that which replaces the folid angles of its other extremity, and thefe two modifications have commenced at the very origin of the formation of the cryftal, there is a particular period of its progrefs, in which the cryftal is lengthened into a hexaedral prifm, with acute triangular ifofceles planes, having for their bafes two equilateral triangular planes, perfectly equal. (Fig. 22.) After this period, if the act of cryftallization continues, the cryftal affumes the appearance of an extremely acute rhomboid, the acute folid angles of which are replaced, more or lefs completely, by an equilateral triangular plane, (fig. 23.) and finifhes at laft by taking the form of a perfect rhomboid. (Fig. 24.)

All thefe varieties, though lefs common than thofe of the firft modification, are yet frequently to be met with, excepting that of fig. 22. which is extremely rare, and of which I have feen only two or three cryftals: in general, however, the cryftals of thefe varieties are very fmall, and their form cannot be well feen without the affiftance of a magnifying glafs.

It frequently happens that two of the elongated triedral prifms (fig. 10.) are clofely united to each other by one of the fides of the prifm; whence refults a kind of macle (fig. 25.) the form of which is a rhomboidal tetraedral prifm of 60° and 120°; but there is always difcernible, on the terminal furfaces of thefe prifms, a very fine tranfverfe line, AB, on the fmall diagonal of the rhomboidal plane of thefe furfaces; this line fhows the place of union of the two cryftals of which the macle confifts.

Sometimes the two component cryftals of this kind of macle belong to the triedral prifm, which has a fecondary plane in the place of one of its edges; it then has the form either of a hexaedral prifm that has four of its fides (two and two in oppofition) broader than the others (fig. 26.), or of one that has only two oppofite fides broader (fig. 27.), or of a regular one, according to the width of the fecondary planes: in all thefe forms, the line AB, indicating the place of union of the two cryftals, is perceptible.

It is not very common, as I have already obferved, to meet with ſpecimens of this ſpecies in which the cryſtals are ſufficiently detached to let their form be diſtinctly ſeen. In general, the cryſtals are grouped together in great numbers, and ſeem to penetrate each other, ſo as to form mamillæ, more or leſs round; or they form a kind of indented cylinders, which have ſome reſemblance to the trundle of a mill. In that caſe, the part of the cryſtals which appears at the ſurface of theſe aggregations, commonly belongs to one of the ſides of their priſm, either the broad or the narrow one. But, when theſe aggregations form either a kind of cylinders, or of mamillæ in cluſters diverging like a fan, there may be ſeen, at the two edges of the cylinder, or at the ſummit of the cluſters, the whole of the equilateral triangular terminal planes, or trapezia, of one part of the component cryſtals.

The ſpecific gravity of this ſpecies of arſeniate of copper is nearly the ſame as that of the preceding one; I found it to be 4,280. Its hardneſs, however, is not ſo great; it is with difficulty that it can be made to ſcratch calcareous ſpar.

The cryſtals of this ſpecies, when they have not undergone any change, are tranſparent, and of a very beautiful blueiſh-green colour, or deep verdigriſe; but their ſurface eaſily becomes decompoſed, and turns black; the cryſtals are then totally opaque. It is indeed very ſeldom, and only in cavities recently expoſed, that cryſtals can be found which retain their tranſparency and colour. Yet, as the change they undergo commonly takes place only at the ſurface, rarely penetrating to any great depth, their original colour may eaſily be reſtored, merely by ſlightly ſcraping the ſurface with a ſharp inſtrument.

The above is the only change I have had occaſion to remark in this ſpecies.

Sometimes, but very rarely, this ſpecies is found in the orm of ſmall hair pencils, with very delicate fibres; and as, in the ſpecimens in which I obſerved this variety, the little fibrous tuft had preſerved its beautiful verdigriſe colour, nothing could exceed the beauty of their appearance.

I have likewiſe obſerved this ſpecies in a mamillary form, with a compact texture; but this variety, like the preceding, is extremely rare.

The matrix of this arſeniate of copper is the ſame as that of all the preceding ſpecies; and that ſpecies which is moſt frequently found with it, is the arſeniate in obtuſe octaedra. It is alſo frequently accompanied with that kind of ore which is known by the name of *azure copper ore*.

SECTION

SECTION II.

Arseniates of Iron.

Muttrell mine, which is immediately contiguous to Huel Gorland mine, in the county of Cornwall, has produced some specimens of arseniates of copper exactly similar to those described in the former part of this paper. But this mine is still more interesting to mineralogists, on account of a combination found therein, of arsenic acid with iron, and also a double combination of that acid with both iron and copper.

The first-mentioned of these arseniates seems analogous to those crystals, or cubes, of a fine green colour, of which some specimens had already been found in Carrarach and Tincroft mines, and which Klaproth, in his Memoir upon the Mineralogy of Cornwall, considered as belonging to the arseniates of copper; but, according to the analysis made by Mr. Chenevix, with all the care which his extensive knowledge and extreme zeal for science would naturally lead him to employ, it appears to be a true arseniate of iron, containing only a small quantity of copper; and even that quantity seems to be merely an accidental mixture. As, in the specimens from the old mines of Tincroft and Carrarach, the greatest part of the crystals adhered to vitreous gray copper ore, it is possible that some particles of that ore remained attached to the crystals; or, as I have frequently found to be the case, that some such particles had penetrated into the crystals, and that Mr. Klaproth had been thereby deceived, by finding in the button left by the blowpipe a much greater proportion of copper than this ore really contains. The natural decomposition of this arseniate, which produces an oxide of iron of a fine reddish yellow colour, strongly confirms the result of Mr. Chenevix's analysis.

Gmelin, in his Principles of Mineralogy, printed at Göttingen in the year 1790, had already supposed that these crystals could not belong to the substance which, in mineralogical publications, had been called *arsenical copper ore*. He had consequently separated them, leaving them, however, among the ores of copper, under the name of *würfel ertz*.

The double combination of the arsenic acid with iron and copper, although it had appeared to exist in the arseniate just spoken of, in the mines of Tincroft and Carrarach, had not excited the attention of mineralogists. It is however possible that the transparency, the brilliancy, and the pale blue colour of its crystals, might occasion them to be mistaken for crystals of a stony nature. Besides, their smallness might easily

eaſily cauſe them to eſcape the notice of common obſervation, particularly when they are not in pretty large groups.

The matrix of theſe two arſeniates is exactly the ſame as that of the arſeniates of copper; conſiſting, like that, of quartz, mixed with yellow, gray, and vitreous ores of copper, with oxides of iron, and with miſpickel. The mines of Huel Gorland and Muttrell, although not ſituated in the diſtrict of the tin mines, have yet produced ſome ſpecimens of tin, the cryſtals of which are covered with thoſe of the arſeniate here ſpoken of. Two ſpecimens of this kind are in the collection of Sir John St. Aubyn.

Species I. *Simple Arſeniate of Iron.*

This ſpecies cryſtallizes in perfect cubes, (fig. 28.); ſometimes, though rarely, they are a little flattened; their ſides are ſmooth and brilliant.

The only modification I have obſerved in this form is, that four of the eight ſolid angles of the cube are replaced by an equal number of equilateral triangular planes, ſituated in ſuch a manner, that every one of the ſides of the cube becomes an elongated hexagon, having two angles of 90° each, and four of 135°. (Fig. 29.) Cryſtals modified in this way are very ſcarce. I have never ſeen but one ſuch ſpecimen, which is in the collection of Sir John St. Aubyn. The cryſtals of it are pretty large, and very well defined.

The ſpecific gravity of this ſpecies is 3,000. Its hardneſs is juſt ſufficient to ſcratch calcareous ſpar. Its cryſtals, which are tolerably tranſparent, are of a dark green colour, with a browniſh tinge; ſometimes they are rather yellowiſh; and there exiſt ſome ſpecimens of a brown yellow colour like reſin. I have never ſeen this ſpecies in any other ſtate than that of perfect cryſtallization.

Sometimes, indeed, a decompoſition takes place, which cauſes the cryſtals to paſs into the ſtate of a pulverulent oxide, of a fine reddiſh yellow colour. In this caſe, as the bulk of the cryſtals is conſiderably diminiſhed, there is perceived, upon breaking them, a conſiderable number of ſmall cavities in their ſubſtance. Theſe cavities are analogous to thoſe which appear in the cryſtals of the ſpathoſe ores of iron when they have paſſed into a ſimilar ſtate of decompoſition.

Species II. *Cupreous Arſeniate of Iron.*

The cryſtals of this ſpecies are of uncommon brilliancy, and are perfectly tranſparent. Their form is a rhomboidal tetraedral priſm, having two of its edges very obtuſe, and the other two very acute: but, owing to the minute ſize of

theſe

theſe cryſtals, I have not yet been able to determine the meaſure of their angles. The priſm is terminated at each of its extremities by a tetraedral pyramid, which is pretty ſharp; and its planes, which are ſcalene triangles, unite by pairs, forming elongated ridges, which join the acute edges of the priſm: in the other direction, they unite, alſo by pairs, ſo as to form a ridge which is leſs elongated, and joins the obtuſe edges. Very often the obtuſe edges of the priſm are replaced by planes (of greater or leſs extent) equally inclined upon the adjacent ones. (Fig. 31.) Sometimes the acute edges are alſo replaced in the ſame manner, but always by planes of leſs extent. (Fig. 32.)

The above are the only varieties I have obſerved of this arſeniate. Its cryſtals ſeldom occur ſingly, being generally grouped together, in a very irregular manner; ſometimes, however, they are ſo united as to aſſume a mamillated form, having the pyramids of the cryſtals which compoſe the mamillæ all placed upon the ſurface thereof.

The ſpecific gravity of this arſeniate is 3,400.

Its hardneſs is rather greater than that of the ſimple arſeniate of iron: it ſcratches calcareous ſpar with greater facility, but does not ſcratch fluor ſpar, or heavy ſpar.

Its colour is that of a very faint ſky-blue; ſometimes the blue colour is a little deeper. I have ſeen ſome cryſtals which had the ſame brown reſin colour as the preceding ſpecies; but they are very rare.

Hitherto I have never met with this ſpecies in any other form than that of a perfect cryſtal.

XXVI. *Analyſis of the Arſeniates of Copper and of Iron.* *By* Richard Chenevix, *Eſq. F.R.S. M.R.I.A.**

SECTION I.

Arſeniates of Copper.

THE endleſs diverſity which the hand of Nature has diffuſed through all her works, even when ſhe makes uſe of the ſame primitive materials, muſt ſufficiently convince us, that, whatever accuracy we may attain in the knowledge of the latter, the means which ſhe employs to form her combinations are ſtill ſecret. The intellectual eye may indulge in the contemplation of hypothetic ſyſtems, which itſelf has created, and which it alone can behold; but how far removed

* From *Tranſactions of the Royal Society of London for* 1801.

must

must they ever be from truths evident to our senses, and supported by palpable experiments!

To follow Nature through the minutiæ of her labours, and behold her reproducing the same primitive materials in many different shapes, has always been deemed a less splendid achievement of science, than to discover one more of those simple substances, by the union of which she forms the complicated effects we daily admire. Yet to me it appears, that in no instance is she more truly wonderful than in the unbounded variety which she has sometimes produced from a small fund of original resources, and when we can fairly follow a few primitive substances through a series of combinations infinitely multiplied.

In addition to the two chemists who, as is mentioned in the preceding paper by the Count de Bournon, appear to have had some knowledge of the existence of a natural arseniate of copper, I must name M. Vauquelin. In a letter to me last year, he communicated the discovery of such a substance in France. Of the different varieties which these gentlemen, Messrs. Klaproth, Proust, and Vauquelin, have examined, I shall have occasion to speak in the course of these experiments: but it was reserved for the Count de Bournon to state, in the said paper, with his usual talent and perspicuity, the scientific detail of the external characters, particularly of the crystalline forms, by which he had identified their nature. The free access to the extensive collections of the Right Hon. Charles Greville and of Sir John St. Aubyn, also the easy communication with the native soil of this mineral, were the peculiar advantages, which enabled the Count de Bournon and myself to pursue the mineralogical and chemical researches which are stated in these communications to the Society.

When the Count de Bournon had completed what appeared to him to be the mineralogical classification of these copper ores, he gave me some specimens of each kind, numbered indiscriminately, for the very purpose of excluding prejudice; and it was not till my task was ended that we compared our observations. If I had been admitted into any previous knowledge of the arrangement dictated to him by the principles of crystallography, I should have been afraid that I had merely thought true what I wished to be so. But I can, most conscientiously, indulge in the satisfaction which the according results of different means to prove the same proposition naturally excite; and which is justly due to the truth of the outward marks, however delicate, yet still to be perceived,

perceived, that Nature has left visible to those who will observe her.

I shall now proceed to offer the result of a chemical analysis, undertaken with a view to determine what confidence the crystallographical arrangement, adopted in the preceding paper, might merit; and to show how far sciences so nearly allied may receive new light and confirmation from reciprocal aid.

I shall confine myself to detail only those general processes which, upon frequent trial, have been found preferable. By reducing to powder any of the arseniates of copper here spoken of, and then exposing them to heat in a platina crucible, the water of crystallization was quickly dissipated. But, as too great a degree of heat volatilized some portion of the arsenic* acid, it was found necessary to moderate the heat; and, in order that every particle of water might be finally expelled, to prolong it. When the diminution of weight was ascertained, the residuum was dissolved in acetous, or, still better, in dilute nitric acid, and nitrate of lead was poured in. Arseniate of lead and nitrate of copper were thus formed, by double decomposition; but, when more nitric acid had been used than was strictly necessary to dissolve the arseniate of copper, no precipitate appeared till the liquor had been evaporated. When the evaporation was pushed too far, part of the nitric acid, contained in the soluble nitrate of copper, flew off; and that nearly insoluble cupreous nitrate, first

* There is no doubt that philologists, who do not consider the principles of the new methodical nomenclature, may, at first sight, think the term *arsenic* objectionable; particularly as previous custom and analogy had given another denomination, *arsenical*, which is the natural adjective of the substantive *arsenic*. They may say that the difference of accentuation alone marks the distinction between the substantive and the new adjective. But every chemist will set the weightier considerations of method and order before such objections. In French, the termination in *ic*, for the substantive, and in *ique*, for the adjective, obviates all confusion. One remark I shall beg leave to offer to the consideration of those chemists who have laboured to adapt to the English language a literal translation of the French nomenclature. It is the genius of the former language to throw the accent as far back as possible; so that, in trisyllabical nouns, the first or second syllable is usually accented; while, in the French language, the accent is generally thrown upon the last; thus, we say *súlphuric acid*, but they say *acide sulphuríque*. It was very natural therefore, as in the latter case, to make the accented syllable be that which should denote the particular state of the substance of which they speak. Thus, *sulphuríque, sulphureúx*; *nitríque, nitreúx*; *sulphâte, sulphíte*; *nitráte, nitríte*. But, without offending the radical orthoepy of our language, we cannot make the same method subservient to that purpose; for, when we wish to mark the distinction in that manner, we are obliged to wrest the word from its proper pronunciation, and to say, *nitríc, nitróus, sulphureoús*, &c.

mentioned

mentioned by Mr. Prouſt *, was produced. To obviate both inconveniences, alcohol was added, immediately before the liquor was quite evaporated, and long after the precipitate had begun to appear; in a few minutes, all the arſeniate of lead fell to the bottom, while the nitrate of copper was held in ſolution. Theſe new products being ſeparated by filtration, the ſpirituous liquor was diſtilled; and, from the nitrate of copper, the quantity of that metal contained in the ore was obtained by boiling the ſolution with potaſh or ſoda †.

To

* *Annales de Chimie*, vol. xxxii. p. 26.

† By potaſh and ſoda, I mean thoſe alkalies pure, obtained according to the method preſcribed by Berthollet. I know of no other. It is not that I have any predilection for thoſe identical terms; yet, whatever melioration ſubſequent improvement may introduce in particular caſes, if principles are to be adopted, they ſhould, in general, be ſtrictly adhered to. But it muſt be a violation of them to apply a word, appropriated by common conſent to deſign a pure, and as yet a ſimple ſubſtance, to ſuch heterogeneous mixtures as lapis cauſticus, carbonates of potaſh and ſoda, &c. It is indeed much to be deſired, that the epithets *cauſtic*, *pure*, *ſaturate*, &c. ſhould be regarded as tautology, which they really are. There is no potaſh purer than potaſh. When it is not pure, we ſhould ſay, inſtead of "I took ſo much potaſh," "I took ſo much of a mixture of potaſh, and whatever other ſubſtance is mixed with it." Thus, inſtead of calling lapis cauſticus, cauſtic potaſh, or potaſh, as is often done, we ſhould ſay, "I took ſo much of the mixture of potaſh, ſulphate, muriate, carbonate, and ſulphuret of potaſh; ſiliceous and aluminous earths; iron and mangane ſe;" for ſuch I find, by analyſis, lapis cauſticus to be. To all this is added, by apothecaries, a little lime. Yet this is the ſubſtance ſometimes called potaſh.

M. Lowitz's manner does not give potaſh pure enough for delicate analyſes of ſtones. I have never ſeen any prepared by his method, in which I could not diſcover iron, ſilica, alumina, and carbonic acid. To the proofs given by Dr. Kennedy, (in his paper intituled "A Chemical Analyſis of three Species of Whinſtone and two of Lava," in the Edinburgh Tranſactions for 1799,) of the efficacy of his method, I propoſe the following objections: That chemiſt ſuperſaturates by nitric acid, and examines by nitrates of barytes and of ſilver. This will be a ſufficient teſt for ſulphuric and muriatic acids; but carbonic acid may have been preſent before ſaturation. He then evaporates, and, if all is rediſſolvable, concludes there is no ſilica or alumina; but, after ſaturation by an acid, ammonia is a more delicate teſt than evaporation for ſmall portions of thoſe earths.

By treating Dantzic potaſh, or, ſtill better, pearlaſh, with lime, and evaporating in a well plated copper veſſel, a white maſs is left. This maſs, diſſolved as far as it can be in alcohol, and the liquor diſtilled to dryneſs in a plated alembic, gives an alkali of a perfect whiteneſs. In this ſtate it is dangerous to touch it, its action on animal matter is ſo ſudden and ſo violent. It attacks all ſtones with the greateſt caſe and rapidity. Diſſolved in water, it makes not the leaſt cloud in barytes water, or in a ſolution of nitrate or muriate of that earth; and may be uſed as a very delicate and ſenſible reagent, to diſtinguiſh it from ſtrontian. By ſaturating with an acid, and then ſeeking ſilica or alumina, by ammonia, no trace of them can be found, nor indeed of any thing elſe. I do not ſay, however, that the potaſh is perfectly free from every other ſubſtance; I

believe

To the ufe of alcohol, in order to get rid of the excefs of acid, as mentioned above, there is not the fame objection that there might be to evaporation, or to an alkali: it can combine with that acid only which is free; and an excefs of it can, in no way, affect the metallic falts.

I have given the preference to lead, above every other method of combining arfenic, to determine its quantity in any other body, having found arfeniate of lime, which has been hitherto recommended, as well as all other earthy arfeniates, to be nearly as foluble in water as fulphate of lime. Lead prefented alfo much facility as to the proportions of its arfeniates; and a few experiments, inftituted to arrive at them, afforded fufficient accuracy. But, firft, it was neceffary to afcertain how much acid a given quantity of metallic arfenic could afford; and, finding that it was in vain to afpire at a greater degree of precifion than that which Mr. Prouft had obtained, I have adopted his refults. By them it appears, that 133 of white oxide and 153 of acid, contain each 100 of real arfenic, the reft being oxygen. But, 100 of metallic arfenic, acidified by nitric acid, neutralized by an alkali, and precipitated by nitrate of lead, gave 463 of arfeniate of lead; that is, 100 of arfeniate of lead contain 33 and a fraction of arfenic acid; and, on the other hand, my own experiments informed me, that lead, diffolved in nitric acid, and precipitated by arfeniate of ammonia, gave a proportion of 63; and 4 were expelled by heat from this falt. The compofition of arfeniate of lead, therefore, is,

Arfenic acid - - -	33
Oxide of lead - - -	63
Water - - - -	4
	100

This experiment, repeated feveral times, never gave 1 per cent. difference in the refults. Another method, which may be deemed fhorter, and perhaps even more accurate, to analyfe arfeniate of copper, is as follows:—After the quantity of water has been eftimated, the remainder may be treated by either of the fixed alkalis, which will combine with the acid, and leave the brown, the only real oxide of copper, in the fame ftate as that in which it exifted in the ore; the alkaline liquor may be neutralized as above, and the proportions determined in the fame manner.

believe it contains a little carbon, produced by the decompofition of the alcohol, and is therefore a fubcarburet of potafh; but carbon can be of no confequence, in the generality of experiments, in humid docimafia. The fame method, employed with carbonate of foda, is the only one to procure foda in a ftate of equal purity.

No. I. *Third species of the preceding paper.* One hundred parts, exposed to a low red heat, lost nothing of their weight. Dissolved in dilute nitric acid, decomposed by nitrate of lead, and precipitated by evaporation, and then by alcohol, they left a white powder, which, well washed and dried, weighed 121. But 121 of arseniate of lead contain 39,7 of arsenic acid. The nitrate of copper, boiled with potash, left a precipitate, which weighed 60. Therefore, there are in this variety,

Oxide of copper - - -	60
Arsenic acid - - -	39,7
	99,7

No. II. *Fourth species of the preceding paper.* One hundred parts, exposed to a low red heat, lost 16. Treated as above, they yielded a quantity of arseniate of lead, corresponding to 30 of arsenic acid; and I obtained 54 of oxide of copper. Therefore this variety contains,

Oxide of copper - -	54
Arsenic acid - - -	30
Water - - - -	16
	100

No. III. *Var. 2 of the third species.* One hundred parts, exposed to a low red heat, lost 18 of water. The 82 remaining, boiled with potash, left a residuum of a blackish brown colour, which weighed 51, and which, examined by the different reagents, was found to be oxide of copper, without mixture. The supernatant liquor, and the liquor which washed the 51 precipitated, being neutralized and evaporated together, left a precipitate, by nitrate of lead, which weighed 88, and, by the proportions of arseniate of lead, established above, indicated 29 of arsenic acid. The proportions in this variety are therefore as follows:

Oxide of copper - -	51
Arsenic acid - - -	29
Water - - - -	18
	98

No. IV. *Var. 5. of the third species.* This gave, by one or other of the two methods, already described and applied, as follows:

Oxide of copper - -	50
Arsenic acid - - -	29
Water - - - -	21
	100

N.

No. V. *Second species of the preceding paper.* This is the variety which, according to the description I received from M. Vauquelin, he had analysed. In his letter to me, he gave no particulars of the method he had employed, but merely stated his result *. By that, it appears to contain,

Oxide of copper - - -	59
Arsenic acid - - -	41
	100

Before the reception of his account, I had found,

Oxide of copper - -	58
Arsenic acid - - -	21
Water - - - -	21
	100

This induced me to repeat the analysis with the greatest care and attention; for I thought, that to differ from so great a master must be to differ from truth; but I constantly found 21 of water, and 21 of arsenic acid.

This apparent difference must, therefore, depend on the state of dryness in which he obtained his acid; or perhaps he estimated it with the water; and, if so, I am happy to find I agree with him so near as one per cent. A greater precision, as every person familiar with analysis well knows, is not within the power of chemical exactness.

No. VI. *First species of the preceding paper.* One hundred parts, exposed to a low red heat, lost much more than any of the other kinds; the deficit amounted to 35. The usual treatment gave 49 of oxide of copper, and only 14 of arsenic acid. I repeated this analysis with some portions which had not been exposed to heat, and never found more than 14 of arsenic acid. This arseniate contains,

Oxide of copper - -	49
Arsenic acid - - -	14
Water - - - -	35
	98

[To be continued.]

* J'ai analyzé, ces jours derniers, une mine de cuivre d'un vert clair cristallin, en lames hexaëdres, se divisant en lames menus, et legèrement flexibles, comme le mica; et c'est pour cela, que les naturalistes l'avoient nommé *mica vert*. J'ai trouvé que ce mineral étoit composé d'environ 59 d'oxide de cuivre, et de 41 d'acide arsénique; et c'est de véritable arséniate de cuivre. Paris, August 30, 1798.

XXVII. *On the apparent Elevation of Objects above the Horizon.* By *Professor* De Luc*.

It is well known, that on the borders of lakes, the banks of broad rivers, and the ſhore of bays, a certain optical illuſion often takes place when the ſpectator is ſtationed in a high point of view. The oppoſite bank, under certain circumſtances, is ſeen as it were floating in the air; and this phænomenon is aſcribed to refraction: but in my opinion it proceeds from a different cauſe.

About two years ago I obſerved the phænomenon in queſtion in one of thoſe extenſive turf moors that occur ſo often among the heights in Bremerlande, through which I was then travelling. The trees which bordered this moor, towards the horizon, appeared to me to be floating in the air. I at firſt conjectured, according to analogy, that there muſt be a lake or river at the end of the turf moor; but the poſtillion who drove the carriage aſſured me that this was not the caſe.

In the month of July this year, being in the ſame diſtrict, I ſaw a ſimilar phænomenon, and under circumſtances which induced me to examine it with more attention. I was on the road between Bremervörde and Brederkeſa, proceeding in a north-eaſt direction. It was about nine in the morning; the heavens were covered with light clouds, yet the ſun appeared through them a little behind me on the right, and over a turf moor which extended to the horizon, and was bordered with a row of trees. When the phænomenon firſt appeared, I was travelling down an eminence on a road adjacent to the edge of the moor, which entered it a little way, and then proceeded up an oppoſite hill. The variations which I obſerved in the phænomenon as I advanced ſlowly down the hill, induced me to diſmount, that I might be able every now and then to turn back. What I obſerved on this occaſion was as follows:

At the moment when I was about to deſcend, I could ſee and obſerve from the higheſt point of the hill a very large ſurface of the turf moor, which extended itſelf with increaſing breadth towards the horizon; the diſtance of which I eſtimated at a German mile. From this pretty high point of ſight, I could perceive that the trees at the extremity of the moor did not ſtand exactly in a line, but that they formed a ſort of wood, which extended backwards on the other ſide, and,

* From *Der Geſellſchaft Naturforſchender Freunde zu Berlin Neue Schriften*, vol. III. 1801.

and, on the fide next to me, had fome projecting curves, but in fuch a manner that the row feemed uninterrupted and continued.

The firft thing that ftruck me as I defcended, was, that at the bottom of this row of trees, and near the extremity which was oppofite to me, there appeared an aperture where there feemed to be no trees, and where I thought I could perceive the heavens under a line of trees which I confidered as further diftant, becaufe their colour was not fo dark, and becaufe they appeared fmaller than the trees of the firft row between which I faw them. This feparated part of the row feemed to be completely floating in the air; the horizon in the interval of the trees of the firft row was bounded by the furface of the turf moor, and I imagined that I faw the heavens between this extreme furface of the moor and the row of the moft diftant trees.

It was at this moment I alighted: I then turned back, afcended the hill a little, and faw the trees which attracted my attention gradually affume their former appearance. The whole feparated part again funk down to the earth, recovered their former dark colour, and alfo their former height; fo that I was fcarcely able to diftinguifh this part from the remainder of the row. Some traces, however, of the former transformation remained. I was therefore induced to examine the whole row throughout with the greateft attention; and, on doing fo, I imagined that I obferved fome parts which lay a little more in the back ground, and which, as I conjectured from certain figns, would appear transformed in the like manner when viewed from a fomewhat low point of fight. In regard to the loweft ground line of the row of trees, it appeared, on account of the diftance and thicknefs of the atmofphere, too undefined to perceive bendings in it: I was not able to obferve the trunks of the trees, which I diftinguifhed from the furface of the moor merely by its brown colour being rendered lefs pale by the intervening ftratum of air, by the trees having a higher pofition, and by their being feen through a purer ftratum of the atmofphere. On again defcending, I faw thofe parts of the row which I confidered as the moft remote, gradually raife themfelves above the horizontal plain; fo that they feparated themfelves from the row in which they ftood, and feemed to float in the air behind the row which now had the appearance of being broken. When I reached the valley, where I was however fomewhat elevated above the level of the moor, thefe phænomena began to appear in the foremoft row, where the firft breaking appeared. I again thought I faw the heavens below the trees at the extreme edge of the moor. These trees also appeared to be floating in the air, but they did not elevate themselves above the ground so much as those I first observed. As I advanced further in the valley, my horizon did not any more change in a perceptible manner; though my position in regard to certain parts of the row of trees was considerably altered, which produced several modifications of the phænomenon. The elevation of the trees above the horizontal surface increased the further I proceeded, but gradually decreased in those parts from which I receded, and where it had first begun, and at length the whole disappeared when I ascended the opposite hill.

This circumstance of the phænomenon led me to an observation, from which I concluded that refraction had no share in it. The masses of the distant trees which raised themselves from the ground, were situated between parts of the foremost row, which for a considerable time did not change their position; and these unchanged parts gave me fixed points, by which I was enabled to estimate the elevation of the objects; and it was this estimation which conducted me to a knowledge of the nature of the phænomenon.

The intervals where, in consequence of my progressive motion forwards, the heavens appeared under the most distant trees, raised themselves more the lower my station became on the hill, and the further I advanced in the valley: but the summits of the floating trees were not elevated; on the contrary, they seemed somewhat depressed in comparison of the foremost trees. The masses became shortened below, but at the top they remained on the same level. I had sufficient opportunity to confirm this circumstance, as, by changing my position in a vertical and horizontal direction, I saw three different masses so totally disappear, that, in the interval which they before occupied in the row of trees, nothing more was to be seen but the pure heavens. On one of these masses disappearing in this manner, when I retired back from my position, there first appeared towards the heavens some brown spots, which were the highest summits of the trees; and the more I receded, the more it seemed as if a curtain dropped down which concealed the part of the heavens before seen. But with whatever attention I looked towards the spot where this curtain fell down, and where the summits of the trees at length disappeared, I could observe nothing but the pure heavens. Had a person, without suspecting any thing of this optic transformation, seen from the valley the row of trees in the horizon evidently broken, and had again seen from the hill without observing the gradual change during the time of advancing, the ſame row once more in the horizon entirely whole, without any interval or break, he would certainly not have believed his own eyes.

Between this phænomenon and that where the coaſts appear elevated in the like manner, I could obſerve no difference in regard to the cauſes which produce them; and the ſame identity is applicable to thoſe phænomena which I obſerved in another moor in the ſame neighbourhood about two years before; only that the trees which formed the horizontal boundary ſtood actually in a line, ſo that the whole row ſeemed floating in the heavens at once, and not in portions at a time. All theſe phænomena then are identically the ſame, and it follows from the circumſtances here deſcribed, that they do not depend on refraction. The cauſe alone lies in the rays of light reflected from the vapours, which in conſequence of this reflection appear ſo dazzling to the eye, that they conceal the objects ſituated below, and preſent the ſame appearance as the heavens. The ſun, as already ſaid, was towards that ſide where I ſaw the phænomenon, and his altitude was conſiderable, as I obſerved the above transformation on the 18th of July, between the hours of 9 and 10. While I obſerved from the valley ſeveral of theſe broken parts of the firſt row of trees, the ſun was in a perpendicular direction over them. The elevation of the maſſes above the ground increaſed in general more and more as my ſituation became gradually lower, and according as each maſs of the objects to be changed approached nearer to a perpendicular poſition under the ſun, in conſequence of my advancing in a horizontal direction. As I deſcended, I had between me and the objects a ſtratum of the atmoſphere, which was always denſer, and conſequently more proper for reflecting the rays. When I advanced in the horizontal plain, this reflection took place more in a ſtraight line; and when I came into ſuch a poſition that the vapours threw back the rays of light to the level of the ſummits of the moſt diſtant trees, I ſaw nothing but this luminous vapour, which loſt itſelf in the heavens, and formed with them one continued field. When I had reached the loweſt point of the plain, where I found a favourable point of view, ſome of the foremoſt trees could then raiſe themſelves from the ground, becauſe they were nearer to me; and to compenſate for this greater proximity, the rays of light muſt have penetrated a much thicker ſtratum of the atmoſphere, and muſt have proceeded from a point ſo much nearer the perpendicular poſition under the ſun.

The preſence of the ſun is the principal condition of this

 phænomenon:

phænomenon: without the action of the sun's beams, the vapours would not be dense enough to conceal the objects: it is by means of the solar rays that they are made to disappear from the eye. These rays, however, can be transmitted to the eye only under a given angle, in the same manner as a certain angle is required in order to produce the rainbow by refraction. The stratum of air which is sufficiently dense to effect this reflection of the sun, rises to a certain height, but it can be perceptible to the eye only when the vapours actually throw back a great number of rays. As soon as the point of sight corresponding to this effect is obtained, the eye sees only the shining vapour, which it cannot distinguish from the heavens. This disappearance, however, takes place in regard to distant as well as to near objects, and rather at the lower than the upper part of the object, because, in the first case, the direction of the line of sight passes through a larger mass of vapour, and in the second through a denser stratum. The less dense part of the vapour, which did not reflect rays of the sun, but which always grew denser the lower my station became, acting like a veil, which interposed itself between my eye and the trees which had not yet disappeared, made me observe their distance, and contributed to cause the elevated trees to appear as if floating behind the row.

In regard to the circumstance that the tops of the distant trees did not raise themselves above those of the foremost, though their lower parts seemed to rise above the ground as I descended, and that these tops on the contrary seemed somewhat depressed; it arose from the decrease in the elevation of my point of station having a greater ratio to the less distant than to the most distant trees; by which means, the former were raised more in the same proportion above the horizon.

When this phænomenon is observed on the banks of large pieces of water, and when the objects are at different distances from the observer, the truth of the principles here advanced may be confirmed by remarking, that the phænomenon is modified by a change of position; but if the observer can move about in a horizontal direction, it will be of great use to observe the changes of the object in a vertical direction, supposing he has opportunity, on any gradual declivity, of ascending and descending. If the vertical measurement of the objects then changes, so that their total height decreases the more the objects seem to rise from the ground when the observer descends the declivity, this is a sufficient proof that the whole image is not elevated, but that its

its loweſt part diſappears from the eye, behind a maſs of vapour ſo condenſed that it cannot be diſtinguiſhed from the ſky above or the water below.

I entertain no doubt, that this is the real cauſe of all the phænomena of this kind, as I have never been able to comprehend how ſuch phænomena could be produced by refraction.

When the objects which are ſeen under water appear elevated in different degrees, it may be readily conceived that the rays of light proceed from the water, which is very denſe, into air, which is much rarer. When we ſee the celeſtial bodies above the horizon, while they are actually below it, we may eaſily conceive that the rays of light, proceeding in an oblique direction, ſometimes paſs through rarer and ſometimes through denſer ſtrata of the atmoſphere. But, in the preſent caſe, there is no difference in the ſpecific denſeneſs or rarity, and the correſponding power of refraction of the medium through which the ray of light is tranſmitted to the eye; and if an actual elevation takes place by refraction, the action muſt be equally great from water or from a lake, and on the objects oppoſite to them: conſequently the whole maſs of theſe objects muſt appear equally elevated, without any interruption being obſerved in them.

I ſhall here add, that I have been very often deceived in theſe moors, ſo that I thought I ſaw at a great diſtance lakes and rivers, where my drivers aſſured me there was nothing of the kind. This was always the caſe in the morning, and when the ſun by ſtanding over the object gave riſe to the illuſion. Had not the horizon on theſe occaſions been bordered with a row of ſmall hills, I ſhould certainly have believed that I ſaw not only a piece of water, but even the heavens. The hill prevented me from ſeeing at one view the heavens and the luminous vapour, and the vapour ſeen alone appeared to me as an extended piece of water illuminated by the reflection of the heavens.

XXVIII. *Deſcription and Natural Hiſtory of the Wild Goat of the Alps.* By M. Berthout Van Berchem *jun.**

THE wild goat of the Alps† is ſtill little known to naturaliſts, though mentioned by ſeveral authors. As it lives on the

* From *Mémoires de la Societe des Sciences phyſiques de Lauſanne*, vol. ii.

† *Capra ibex*, Erxleben Syſt. Reg. p. 261. Linn. Hiſt. Nat. p. 95. *Le Bouquetin*, Buffon, vol. xii. p. 136.

ſummits

fummits of the higheft mountains, it has been obferved only by thofe employed in hunting it, who in general are men fond of the marvellous, and therefore their relations are filled with abfurd tales.

Its figure, however, is better known: yet the defcription of Daubenton* was taken from a very young individual, and the engraving which accompanies it is not correct. We are indebted indeed to profeffor Pallas† for a very minute and very good defcription of the wild goat of Siberia, but this animal feems to differ from ours in fome of its characters. Befides, profeffor Pallas does not fpeak of the Siberian wild goat at its different ages, and has not defcribed the female, which is ftill unknown. All thefe reafons have induced me to make refearches refpecting this inhabitant of our lofty Alps, and I flatter myfelf that I fhall render a fervice to naturalifts by making it better known.

Having learned that M. de Watteville, governor of l'Aigle in the Pays de Vaud, had in his poffeffion a live young goat, I eagerly embraced that opportunity, and undertook a journey to l'Aigle to defcribe and make a drawing of it. (See Plate V.) M. de Watteville was fo kind as to give me every accommodation for the purpofe, and to him I was indebted for feveral interefting details refpecting the habits and manners of this animal in its ftate of captivity.

But to know it in its ftate of nature, as the domefticated ftate always occafions a confiderable change in the manners of all animals, it was neceffary to confult the wild goat hunters. With that view I proceeded to the country where they live, that I might obtain more certain information, and be able if poffible to feparate truth from falfehood; but I fhould have loft much time in fruitlefs refearch, had I not had the good fortune to addrefs myfelf to one of the ableft hunters, a man who to great veracity added the fpirit of a good obferver.

The wild goat of l'Aigle, the firft time I faw it, in the fpring of 1784, was two years of age. It came from the valley of Oft, and had been purchafed when about five or fix months old: it was fuckled by a common goat, to which it was ftrongly attached: its mild phyfiognomy announced the gentlenefs of its character and of its manners: its hind legs were higher than the fore ones, and its four cloven feet indicated its aptitude for climbing the rocks. Its large and extenfive horns, which fhaded its fmall head, and its large limbs

* Hift. Nat. de Buffon, vol. xii. p. 166.

† *Ibex Alpium Sibiricarum*, Pallas, Sp. Zool. p. 31.

and

and muscles, seemed to show its strength and its means of defence.

On the first view, it had some resemblance to the sheep in regard to the head, but in the whole of its figure it had more relation to the goat. In comparing it with the latter, it appeared larger and thicker; its forehead was narrower and more elevated. These last characters distinguished it from all animals of the same kind. Its head was proportionally smaller, the forehead was a little arched before, the eye was quick and of a moderate size, the pupil contracted in the sun like that of the sheep, forming a rectangle, the base of which stood in the direction of the greatest length of the eye; the forehead and upper part of the head were thickly covered with hair; it had no vestige of beard. But on my second visit six months after, that is to say, when it was two years and a half old, it had a small one at the distance of an inch and a half from the extremity of the muzzle; it was black, and turned backwards.

Its horns were very different from those of the goat; they were large, near each other at the roots, and at a considerable distance at the points; bent back and outwards in an arch; they were fourteen inches in length, and eight inches in circumference at the base; the two longitudinal ridges, between which was the anterior face of each horn in the old goats, were not both very sensible; the interior one was the most perceptible; it had two or three large and very prominent tubercles. These tubercles in the large horns of the wild goat terminate the transversal ridges of the anterior face; but here the transversal ridges were not yet distinctly marked, though they were perceptible. The posterior face of the horns was round and smooth; but the second time I saw it they had considerably increased. The anterior face and longitudinal ridges were well marked. Near the base there were four transversal ridges very prominent, and elevated nearly half an inch: on the interior longitudinal ridge there were six tubercles, and the others went on decreasing to more than half the length of the horn.

The animal was then three feet and a half in length: it appeared to me proportionally higher on the legs than the goat; but this was on account of its being not then well formed, for the old wild goat has short legs in proportion to its length: it had a thicker and shorter neck than the goat, the rump was lighter and rounder, the body more plump, and the legs thicker; it had no callosities on the knees, which in the common goats is one of the marks of its domesticity: its ears were large, almost bare in the inside; but the edges were furnished with whitish hairs: the animal alw[illegible]rected them backwards.

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It had two kinds of hair: one a white down, very thick, frizzled and woolly; and large hair, very coarſe and thick on the whole body. It was longer under the neck, and on the anterior parts in general; it had not, like the common goat, a ſort of mane of long hair on the back; the hair on the body was of a ſhining gray fawn colour, and that below the chin, darker than that on the body, inclined to brown. At the lower part of the flanks there was a ſtripe of browner hair which began about the thigh, and extended on the ſide as in ſome ſpecies of the antelope; the belly and inſide of the four legs were white or whitiſh. That black ſtripe on the ſpine found in all wild goats was not obſerved on this animal in the ſpring; but in the month of October it reappeared, beginning at the diſtance of about four inches from the root of the horns, and extending to the tail. The black ſtripe at the lower part of the flanks was alſo better marked, and the hair of the body was of a browniſh gray colour; the hair on the parts below the tail was white, as in the greater part of antelopes. The tail itſelf was brown above and white beneath; it was very ſhort, and covered with long hair: the hair of the body was directed towards the rump, and that on the thighs downwards. The hoofs of this animal were very long; the fore legs were ſenſibly thicker than thoſe behind, and the thighs were ſtrong and fleſhy; its feet were of a rounder form than thoſe of the goat, and better determined; each hoof had a ſort of talon very apparent; the lower face of the hoofs which touches the ground was concave, and terminated by a ſalient edge, principally on the outſide, as in the chamois. The horn of the hoofs was pliable and elaſtic, and equally durable as thoſe of the ſheep.

Dimenſions of the Wild Goat at the Age of Two Years.

	feet.	in.
Length of the whole body from the point of the muzzle to the root of the tail -	3	2
Height before - - -	2	6
Height behind - - -	2	9
Length of the horns according to their bending	1	2
Circumference of the horns at the baſe -	0	8
Length of the tail - - -	0	5

Dimenſions of the Wild Goat at the Age of Three Years.

	feet.	in.	lin.
Length of the whole body from the muzzle to the root of the tail - -	3	6	0
Height before - - -	2	8	3
Height behind - - -	2	11	3

Length

	feet.	in.	lin.
Length of the horns according to their bending	1	4	6
Circumference of the horns at the root -	9	0	0
Length of the beard - -	0	1	6

After having defcribed the wild goat of l'Aigle, I fhall now give fome remarks on wild goats in general.

The length of the horns varies. D'Aubenton gives the dimenfions of a pair of horns preferved in the king's collection, which were 2 feet 9 inches in length, and 9 inches in circumference at the roots; they had 20 rings or ridges: this is the greateft length known of the horns of this animal. Their diftance from each other varies alfo. Sometimes they diverge a great deal, at others very little; their colour is reddifh or blackifh, but when the animal becomes old they affume a dirty white colour.

It is to be obferved, that on the large horns of the wild goat the largeft and moft prominent tranfverfal ridges and tubercles are towards the roots. This may be eafily conceived, if attention be paid to the increafe of this kind of horns, which takes place at the bottom; that is to fay, the increafe of each year pufhes upwards that of the preceding year, fo that the young horns at the end of fome years form the upper part of the large ones: and as thefe horns always acquire more fize and breadth at the bottom, the tranfverfal ridge or ring of each year becomes always lefs prominent.

The young are brought forth covered with their firft woolly hair, but in autumn they begin to be clothed with their long and ftiff hair: the horns appear in the firft month, and the animal immediately acquires the black ftripe on the fpine.

In fpring, towards the month of May, all the young and old goats change their hair; that on the back they lofe firft, and that on the thighs laft: the black ftripe difappears in moulting time, and afterwards re-appears, as we have obferved: they then acquire red hair inclining to yellow, which, as the feafon advances, becomes dark brown mixed with gray; fo that in the month of September they are almoft entirely of that colour, or at leaft the upper parts of the body, but in winter and towards fpring their hair becomes reddifh gray. The belly remains white, and the black ftripe is always feen in the flanks: on growing old they become gray.

The hunters can tell the age of the wild goats by the number of tranfverfal ridges on the anterior face of the horns. Every year they fay one is produced; and it is by thefe alone that the age can be determined, and not by the other knots or wrinkles with which the horns are covered. But this method in my opinion is not very certain, ſince the wild goat of l'Aigle, when I ſaw it in the month of October 1784, had four tranſverſal ridges, and two large and extenſive knots, which were to form ridges. According to the calculation of the hunters, therefore, this animal ought to have been four or five years of age, and yet it is certain that it was only two years and a half. Hence, it is not by the horns that the age of the wild goats can be determined with certainty, but by means of the teeth, as is done in regard to ſheep.

It appears that the wild goat lives to a very great age, but not ſo long as might be ſuppoſed from the report of the hunters; for, according to them, it grows to the age of fourteen or fifteen; and as it is known that all animals live ſeven times the period of their growth, it would thence follow, that the wild goat might attain to the age of ninety-eight or one hundred and five years. We ſhall ſoon ſee that this calculation is very much exaggerated.

From what I have already ſaid, it is evident that the hunters are miſtaken when they ſay that the wild goat grows to the age of fourteen or fifteen: what has led them into an error is the number of tranſverſal ridges, which is always greater than the number of years, and conſequently has made the animal appear older than it really was.

Another method, therefore, muſt be employed to determine the period of their growth. The wild goat is not capable of producing its ſpecies before the age of two years, or two years and a half: if that of l'Aigle produced at the age of two years, this ought, perhaps, to be aſcribed to the more abundant nouriſhment with which it was ſupplied in its ſtate of ſervitude, which haſtened prematurely its procreative faculties. And, as it is known that quadrupeds cannot engender before the time when they have nearly attained to their full growth, we may preſume, with ſome ſort of foundation, that the wild goat grows to the age of four years, and perhaps more. This will give 28 or 30 years for the duration of its life, which, in my opinion, is much more natural than that aſſigned by the hunters.

The horns increaſe in length almoſt during the whole life of the animal, but in ſize it grows only till the age of 15 or 16 years. The largeſt have 20 or 30 tranſverſal ridges.

The female wild goat, called vulgarly *étagne*, is a third leſs than the male: it is alſo thinner, and leſs fleſhy. Its horns have very little relation to thoſe of the male wild goat; they are like thoſe of the common goat, and are very ſmall: I have ſeen ſome at Chamonix which were only eight inches in length. They have no anterior face; and, like thoſe of the

the common goat, are furnifhed only with one longitudinal ridge. They are fomewhat thicker than thofe of that animal, and have a few fmall knots on the ridge.

The female, in fhape, habit of body, and the figure of its horns, has a great refemblance to a common he goat that has been caftrated. When fuckling, it has two teats like the common fhe goat; and the feet are fmaller in proportion than thofe of the male; they are fharper, and not fo round. The female wild goat has no beard: according to the hunters, the males are not always provided with one; and, when they are, it is not more than two inches in length. This is one of the differences which appear to exift between our wild goats and thofe of Siberia; the males of which have always a very large beard, and the females a fmall one.

The wild goat has different cries; the moft common is a fhort fhrill whiftle, very like that of the chamois, but with this difference, that the whiftle of the latter is longer: fometimes it makes a noife by breathing through its noftrils; and, when young, it has a fmall kind of bleating, which it lofes as it grows old.

The wild goat which I faw at l'Aigle was exceedingly gentle and tame: its attachment to the common goat, which fuckled it, was very ftrong, though it no longer fucked: often when it was on the top of the houfe, or in other places inacceffible to the common goat, and when the latter feemed uneafy, and called to it by her bleating, it immediately returned.

This animal has not a lively and active look; its ftep is flow, and it feems to poffefs great curiofity; all its movements feem to be directed by a fpirit of reflection, and it appears never to decide till after mature deliberation. Has it this in common with mountaineers, who differ in fo many refpects from the inhabitants of the plains? Confidering the agility of this animal among the rocks, it appeared on the firft view as if oppreffed with the weight of the atmofphere of the level country, and aftonifhed to find itfelf on ground for which it was not formed. But, when it had to climb, that heavy and embarraffed air feemed entirely to difappear: it became agile, and all its movements were performed in a graceful manner; it leaped on the tops of the houfes and on walls with the greateft lightnefs and eafe. I beheld it with admiration, in one of the interior courts of the caftle of l'Aigle, at two leaps mount a wall, without any other fupport than the fmall projection of ftones left by the mortar having dropped off: and, at a third leap, jump upon another wall, which formed a right angle with the former. It began by placing itfelf oppofite to the fpot which

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it wiſhed to reach, parallel to the wall along which it intended to proceed, and examined it with great attention; then, as if it had renounced its deſign, it began to traverſe, with ſlow ſteps, the court in which it was confined; but, returning from time to time, it took its ſtation oppoſite to the place it had in view; and ſometimes balanced itſelf on its hind legs, as if to try their elaſticity: at laſt, having repeated this preparation for ſome time, it at once formed its reſolution, darted itſelf forwards, leaped along the wall as already mentioned, and ſoon reached the ſpot it intended. It has been ſeen ſtanding on the upper edge of a door. It choſe for its lodging a place under the roof of the higheſt tower in the caſtle.

This animal was not miſchievous; and if it often preſented its horns, it was only that they might be ſcratched, of which it ſeemed very fond. It knew thoſe who ſcratched it, and often preſented its head to them for that purpoſe. When it wiſhed to ſtrike with its horns, it never did ſo directly forwards, but by riſing up on its hind legs and ſtriking ſidewiſe.

As it occaſioned ſome damage by its leaping about, it was found neceſſary to tie its legs. This ſtate of reſtraint appeared to me to have had an influence on its phyſical conſtitution, as it afterwards became meagre. Born to enjoy liberty, this animal cannot endure confinement: I ſhould even have imagined that a part of its docility aroſe from its weakneſs, had I not known that this gentle and ſociable animal can be eaſily tamed. The following trait, which ſhows how ſuſceptible it is of being tamed, deſerves to be known:

A guide of Chamonix, being employed to conduct to Chantilli, for the menagerie of the prince of Conti, two wild goats which he had reared, they followed him merely through their attachment to him. When he arrived near the town of Beſançon, they were frightened by a herd of cows, immediately betook themſelves to flight, climbed up the ſteepeſt rocks in the neighbourhood, and abandoned their guide, who was under great embarraſſment: he, however, went after them, and, having called to them, they ſoon returned, and followed him as before.

I thought it neceſſary to enter into theſe details, to deſtroy that prejudice which repreſents this animal as wild and untractable. What I have ſaid proves, on the contrary, the docility, familiarity, and attachment, of which it is ſuſceptible.

[To be concluded in next Number.]

XXIX. *Ac-*

XXIX. *Account of the Reſults of ſome comparative Experiments made with the Teylerian Electrical Apparatus and Volta's Metallic Pile.* *By* VAN MARUM *and* PFAFF. *Communicated to Profeſſor* VOLTA *in a Letter dated November* 1801. *Abridged by* LUKE HOWARD, *Eſq. from* Journal de Chimie *by J. B. Van Mons* *.

A PILE of 200 pairs of metals was conſtructed in the uſual method of Volta. The metals employed were Dutch three florin pieces of nearly $1\frac{1}{2}$ inch in diameter, and correſpondent pieces of zinc. The firſt experiments were directed to charging the jars of a large battery containing $5\frac{1}{2}$ ſquare feet of coated ſurface a-piece. A ſingle jar was firſt charged, afterwards different numbers connected, and ſucceſſively augmented to 25. In fithe rſt, and in each ſucceeding experiment, the charge was effected by mere contact for as little time as poſſible with the pile. The diſcharge in every caſe produced a divergence of the gold leaves in Bennet's electrometer, amounting to ths of an inch, which was alſo found to be the degree of intenſity (*tenſion*) of the pile itſelf.

The whole coated ſurface of $137\frac{1}{2}$ ſquare feet was next charged, at ſeveral times, by a pile augmented at each charge. With 40 pairs of metals, the divergence of the electrometer was perceptible. The increaſed divergence cauſed by the pile itſelf at each addition, was equally producible by the battery when charged by it. No difference was perceived in the effects of the negative and poſitive ſtates of the battery in this reſpect.

The ſenſations producible by the diſcharges under the laſt-mentioned arrangements were as follow:

With 20 pairs of metals, the paſſage of electricity was ſenſibly felt in both hands, each being wetted, and graſping a copper conductor of 2 inches diameter. Sixty pairs affected the elbows. With 200, ſtrong ſhocks were received, extending to the ſhoulders. They concluded that the force of the battery in this reſpect was in all caſes only half that of the pile; which indeed was to be expected.

They next proceeded to a comparative examination of the charge communicable by momentary contact of the battery with the conductor of an electrical glaſs plate apparatus of 31 inches diameter. Precautions were uſed to ſecure the conductor from receiving more than a momentary charge from the plate. It was found, by repeated trials, that ſix

* Communicated by Mr. Howard.

repetitions of the contact were required to bring the charge of the battery to that degree of intenſity which the pile communicated at one.

They were of opinion that the ſenſation of the diſcharge differed nothing at equal intenſities, whether the battery were charged by the pile or the plate apparatus. In ſhort, they conſider the two as mere exciters of the ſame electricity; and conſequently, that the ſuppoſed galvanic fluid has no exiſtence diſtinct therefrom.

Van Marum obſerves, that the rapidity of the current of electricity in the pile muſt be inconceivable, ſince, by a contact laſting only 1-20th of a ſecond, it will charge completely 137 ſquare feet of ſurface; an effect exceeding, in his opinion, the power of every known electrical apparatus, except the large one in the Teylerian muſæum!

This pile was very carefully *inſulated* on a plate of gum lac, and ſupported in its vertical poſition by ſticks of ſealing-wax. Cloth ſoaked in ſolution of muriate of ammoniac was uſed to ſeparate the pairs of metals.

They conſtructed a ſecond pile with 5-inch ſquare plates of copper and zinc. This proved much more powerful when arranged in 32 pairs perpendicularly, than when, by laying down four pieces in contact at once, the height was reduced to eight pairs, and the ſurface in each quadrupled. The former pile melted five inches of No. 16. (iron wire) into globules, and reddened ſeven inches. In attempting to carry it higher, the weight of the ſuperior part, expreſſing the liquid from the inferior, deſtroyed the effect. They remedied this by connecting four ſhort piles, and thus obtained the joint action of 110 pairs of the latter metals.

In order to determine whether there exiſts any perceptible difference between the ſpark given by the pile to any conducting body, and that which it receives therefrom, "we employed (ſays Van Marum) a bowl of quickſilver ſuitably connected by a wire with the ſuperior plate of one of the outſide piles, and we brought to its ſurface one while the point of a needle attached to the wire of communication, then the blunted point of the latter. We next connected the quickſilver with the other extremity of the piles, and, repeating this ſeveral times, we could ſee, in one inſtance, the ſpark paſs from the conducting wire into the quickſilver, in the other from the quickſilver to the wire." The reſult ſatisfied them, that there is not the leaſt perceptible difference between the poſitive and the negative ſparks: but this trouble was compenſated by the view of a very beautiful and intereſting phænomenon. "When we touched (ſays Van Marum) the ſurface of the quickſilver with the end of the fine wire, the combuſtion was ſo ſtrong, that it diſperſed on all ſides ſparkles which had the appearance of thouſands of rays, conſtituting very fine ſuns of ſeveral inches diameter. By ſlowly lowering the wire as it conſumes, this ſpectacle may be continued at pleaſure.

The No. 16. wire, of $\frac{1}{250}$ inch diameter, ſucceeds moſt certainly; but wire of $\frac{1}{150}$ inch makes a more brilliant ſhow. When it is too thick to burn, the ſparks form ſpots of oxide on the bright ſurface of the quickſilver.

The end of a platina wire, of about $\frac{1}{175}$ inch diameter, was melted into a globule by the diſcharge. The ſparks given by the wire of communication, when the latter was large enough, exceeded $\frac{1}{15}$ inch diameter.

They thought much depended on a proper degree of humidity in the ſubſtances interpoſed between the metals; and they adviſe a *cold* ſaturated ſolution of muriate of ammoniac to be uſed, in preference, for this purpoſe.

The intenſities of two columns containing an equal number of plates appeared equal by the electrometer, although their diameters were ſo different as 1 and 5 inches. On taking ſeveral repeated ſhocks from each, their powers in this reſpect ſeemed alſo to be alike. In the fuſion of wire, however, the larger diameter had an evident advantage; which was attributed to the greater velocity of the current of electricity flowing through the metal.

By diminiſhing the diameter of the humid conductors between the plates, the force of the pile was leſſened in proportion. On ſubſtituting water, or ſolution of ſea ſalt, to that of ſal-ammoniac, the ſame pile which before melted four inches of wire could not be made to redden a ſingle line. Whence ariſes the ſuperior effect of the ammoniacal ſalt? Is it becauſe it oxidates the metals more? It appeared, indeed, that the action of this pile diminiſhed in proportion as the oxidation of the metals did ſo: but, on the other hand, from the reſult of ſeveral comparative experiments with the ſulphuric, nitric, and muriatic acids, they conclude that the prompt oxidation produced by theſe means is ſo far from increaſing, that it rather leſſens the power of the pile in all reſpects. Neither did ammoniac, which was alſo tried ſeparately, produce an effect at all comparable to that of the neutral ſalt *. The oxidation of the metals, therefore, although it

* The author does not ſay whether ſolution of pure ammoniac, or of the carbonate, was uſed. In either caſe, it does not appear to us that it could be applied in ſuch a degree of concentration as to afford a fair compariſon of

it muſt be allowed to contribute ſomething to the effect, does not exclude the cooperation of ſome other property which may reſide in the ammoniac. This opinion ſeemed to the authors to receive ſupport from a ſubſequent experiment, in which a ſaturated ſolution of potaſh exhibited a decided ſuperiority over water when both were ſucceſſively uſed in the ſame pile, although it did not at all tarniſh the metals.

Still further to elucidate this point, the authors proceeded to examine the comparative action of a pile in *vacuo* in oxygen and in other gaſes.

Having placed 60 pairs of zinc and ſilver, with a proper metallic communication, under the receiver of an air-pump, and exhauſted the latter to a preſſure of five lines, they found the action of the pile in no way changed. Air was admitted, and again exhauſted, and the pile left in *vacuo* for an hour, the effect ſtill continuing the ſame. Carbonated hydrogen was now admitted into the receiver, and, being afterwards exhauſted, was replaced by azote, without any perceptible variation, in either caſe, from the uſual appearances. Laſtly, having again exhauſted the receiver, it was filled with oxygen gas from manganeſe. The ſhocks now became ſtronger, and the ſparks much larger, brighter, and eaſier to obtain. On exhauſting the oxygen they were ſurpriſed to find the pile now really affected by the vacuum, and acting with much leſs force than in either of the preceding caſes. The atmoſphere of oxygen, and the vacuum after it, being once more repeated, left no doubt as to the reality of this increaſe and diminution of effect; the pile alſo returning nearly to its ordinary ſtrength on being reſtored to the atmoſpheric air.

The letter concludes with ſtating the effect of increaſing the 5-inch ſquare pile to 200 pairs; the diſcharge of which melted 23 inches of wire, and entirely reddened 33 inches: alſo three other experiments, which prove that, in order to augment the power of Volta's pile, it is neceſſary to increaſe the number, but not the ſurface, of the plates it is compoſed of.

of the effect of ammoniac, properly ſpeaking. It is not extraordinary that ſuch diverſity ſhould be found among liquids as among ſolids in their powers of conducting electricity.—H.

XXX. *Copy*

XXX. *Copy of a Letter from* M. DE DREE *, *Brother-in-Law to* M. DE DOLOMIEU, *to Professor* PICTET, *of Geneva* †.

YOU request me to give you some particulars respecting the valuable friend whom we have just now lost. I shall satisfy you with pleasure, for however afflicting may be the recollections which so recent a loss excites every moment, I experience a kind of satisfaction by communicating them to a friend filled with the same sentiments as myself for this worthy man.

Yes, Sir, it was at the moment when he enjoyed, in the highest degree, the tender affection of all his friends, when he called to mind, with pleasure, the particular marks of it which he had received at Geneva; and when he felt, in the liveliest manner, those testimonies of respect of which he had been, and still was, the object, that he was suddenly snatched from his numerous friends. It was at the moment when he seemed to have acquired a new zeal for geology, that science for which Nature makes us pay very dear, since she does not suffer her recesses to be explored but at the expense of the lives of those who have the courage to force their way to her. You can already reckon two friends among the number of her victims ‡. It was at the moment when he was preparing to undertake a journey which, as he told me, was to complete his knowledge, and enable him to render his labours and researches useful; in a word, at a moment when his existence became more and more interesting to the sciences, which he promoted with as much attachment as possible. He has left very numerous notes respecting his travels and various facts he had observed; together with some manuscripts on mineralogy, the fruits of his meditations and captivity. But these works, however interesting, had need of being arranged and illustrated by the same spirit which collected the observations.

During the three weeks he remained with us, we every day made a lithologic tour. I had discovered to him a kind of treasure in this way, which is found in a small circumference around my habitation, and which presents some very

* M. de Drée is a very intelligent amateur of mineralogy, who possesses, at Paris, one of the best collections of this kind belonging to an individual. He married a sister of Dolomieu; and it was in his house at Chateauneuf, in the department of Saone, that this celebrated naturalist breathed his last.—P.

† Communicated by professor Pictet.

‡ M. de Saussure and Dolomieu.

interesting geological facts; and he had made large collections for his friends. He possessed an excellent state of health, and we both enjoyed the strongest reciprocal friendship. He was cheerful, and foresaw no other chagrin than that of his speedy departure to Paris, when he was attacked by that dreadful malady, of which he had so little expectation, that, on the forenoon of the day on which he fell ill, he asked me where we should go a-mineralizing in the afternoon. The violence of the fever first attacked the brain; and we made haste to call in the assistance of art, but, unfortunately, without success. The first period of the malady produced agitation and pain: his head was sometimes confused. The second period, which may be dated from the 4th day, was marked with an almost continual dejection, which always increased till the last moment. During the last period he experienced no pain; he found himself easy; and an hour before he resigned his tender and sensible soul into the hands of his God, he still said he was well. In a word, he ceased to live by an insensible gradation, and like a lamp which becomes extinguished for want of aliment.

However, after he was attacked by the disease he foresaw the danger which threatened him; and he beheld his last moment with that coolness and tranquillity which belong to great minds, like his own. He was much more in pain for the grief he was about to occasion to his sister, than for his own situation; and the only object of regret which seemed to affect him in his last moments was that of not being able to bid adieu to all his friends. You, Sir, were among the number of those for whom he had the greatest affection; and he no doubt testified to you the regret he experienced on hearing that you was not to pass this way on your return from England.

If the course of his life has been tempestuous and agitated, by troubles occasioned to him in consequence of that jealousy which always attends merit, his last moments formed a striking contrast with that agitation; for he gave vent to no complaint against those who, by the most rigorous treatment, had attacked in him the principle of existence; and his soul seems to have taken its flight from the world with calmness and satisfaction, and as if it were smiling at the picture of life which it surveyed at the moment of its departure.

Madam de Drée and myself learned, with great sensibility, the proof of attachment which you as well as some of your countrymen have given to the memory of Dolomieu, in requesting to be at the expense of having a portrait of that celebrated man engraved from the excellent painting in the possession ceffion of La Metherie. This idea occurred alfo to that friend, and was at the fame time my own. I have written to him, that, your wifh in this refpect having been the firft expreffed, we are far from oppofing this impulfe of your hearts, fo flattering to the memory of our common friend.

In regard to myfelf, I expect, with the aid of that portrait, and another very like in the poffeffion of his fifter, to have a buft formed, which I fhall have the honour of fending you as a mark of my acknowledgement for the friendfhip you teftified towards him. And as his dear remains reft with me, the laft granite we broke together is deftined to be a tomb, which will fhow, till the lateft periods, the fpot where he employed himfelf in his laft mineralogical labours.

XXXI. *Notices relative to the Sciences and Arts in France.*

THE mufæum, the gallery of antiques, and that of Apollo, in Paris, are open to public view on the 8th, 9th, and 10th days of each decade. The other days are deftined for the ftudents. Foreigners, however, are admitted daily from ten in the morning to four in the afternoon, on prefenting their paffports; and this indulgence is extended to all perfons arriving from the provinces, and who are provided with paffports. Thefe exhibitions are gratuitous.

The gallery of Apollo contains the drawings which belonged to the late king. Five hundred of them, framed and glazed, are exhibited at a time. The collection confifts of about twelve thoufand. Among thefe are thirty by Raphael, and a confiderable number by Julio Romano, Titian, Paul Veronefe, Carachi, and in general by all the great mafters of the three fchools.

Such in Paris is the quantity of pictures brought from every part of France and Italy, from Flanders, Brabant, Germany, Piedmont, &c. &c. that, to get rid of a part of them, fifteen new mufæums are now eftablifhing in as many principal cities of France. In proportion as the buildings prepared for them are ready for their reception, the paintings, drawings, ftatues, &c. are forwarded.

The pictures in the grand mufæum in Paris confift, one half of the Italian fchool, one fourth of the Flemifh, and one fourth of the French. Befides this, there is a mufæum of the French fchool in the Palace at Verfailles. Twenty of the apartments in the palace are dedicated to this purpofe. In addition to the pictures of the French fchool, there are many others by the beft mafters of the Italian and Flemifh fchools.

 This

This musæum is open every day. The cielings of three new apartments, to be added to the galleries of antiques, are now painting. One of them is destined for the reception of Egyptian antiquities.—In this musæum will shortly be exhibited the tomb of the muses, several antique altars and chandeliers, two superb statues by Michael Angelo, one by Puget (Mars reposing), and several statues of the French school.

There is still a French academy at Rome. Three prizes for painting, sculpture, and architecture are awarded yearly; and the three successful candidates are sent to Rome to remain there four years at their government's expense. By this regulation there are constantly twelve French students at Rome, each of whom is obliged, in the course of his stay there, to send home one production at least. These productions form a part of the musæum of the French school at Versailles.

The Paris exhibition of the works of living painters, sculptors, and architects, is opened yearly in the months of September and October.

The national library in Paris, comprehending the cabinet of medals and antiques, is open to public view on the 3d, 6th, and 9th days of each decade from ten till two o'clock. The other days, with the exception of that of the decade, are set aside for the students. The library consists of about ninety thousand volumes, and upwards of eighteen thousand manuscripts, in all languages. There is, besides, a library of engraved antient and modern music.

The collection of medals is immensely rich. In the year 1800, the entire collection belonging to Pius VI. was added, together with a great part of his gems, the Isiac table, and several Egyptian statues in bronze, basaltes, granite, &c. Here are given annually and gratuitously, courses of public lectures on the Oriental and European languages, chemistry, natural history, botany, antiquities, mythology, &c. by the most celebrated professors.

The *Hotel des Monnoyes* (mint) contains a very fine cabinet of mineralogy. Public lectures on that science are given every other day, and the cabinet is daily open to the public.

The garden of plants is also open daily. During the summer, courses of practical botany are given by the most noted professors.—The cabinet of natural history is one of the completest in Europe. It is open to the public five days in the decade: the intermediate days are for study, and for the courses of public lectures.

A great collection of antiquities is expected in Paris from Italy. Upwards of five hundred cases filled with antiques of of every defcription, which had been feized by the Neapolitans on the banks of the Tiber, are to be reftored to the French by virtue of the laft article of the treaty of peace with the king of Naples: for this purpofe two commiffaries have been fent to Italy; one of them the fon of Chaptal the minifter for the home department, the other Dufourny, an architect.

The inftitute for deaf and dumb perfons, conducted by Sicard, is perhaps one of the moft ufeful eftablifhments which Paris can boaft. The progrefs made by the pupils under their able and benevolent profeffor excites an univerfal admiration. Maffieu, the favourite of the abbé Sicard, and the moft intelligent of his pupils, acts as fecond mafter. Some time ago a lady afked him in writing what was *gratitude*: to which he replied without hefitation, *The memory of the heart.* It was impoffible for any one of the fpectators, gifted with all his fenfes, to have replied with more juftnefs, truth, and delicacy.

The manufacture of porcelain at Seves, near Paris, is open daily to public view; as is alfo that of the Gobelins in Paris. In the latter, a number of artifts are now employed in copying in tapeftry the moft pleafing fubjects which can be felected;—landfcapes, cattle pieces, pieces of fruit, flowers, &c. Some of thefe are intended for prefents; the others are fold in proportion as they are finifhed.

The national inftitute of mufic, which was eftablifhed in 1793, and which is now ftyled the confervatory of mufic, is under the adminiftration of five perfons, who are lodged in the building, each with a falary of five thoufand livres. The profeffors, feventy in number, have each a falary of fifteen hundred livres. The number of pupils, males and females, amounts to about five hundred. The prizes are diftributed annually with great pomp; and the fuccefsful candidates fing or play publicly the pieces which have been crowned by fuccefs. From fix to eight concerts are given annually, to excite an emulation in the pupils, and to difplay their talents.

XXXII. *Ob-*

XXXII. *Obſervations on Maddering; together with a ſimple and certain Proceſs for obtaining, with great Beauty and Fixity, that Colour known under the name of the Turkey or Adrianople Red.* By J. M. HAUSSMANN*.

I HAVE already indicated, in the *Annales de Chimie*† and the *Journal de Phyſique*, that earths and metallic oxides have more or leſs the property of attracting and retaining the colouring parts of vegetable and animal ſubſtances; alumine and the oxide of iron poſſeſs it in a greater degree than the oxide of tin; but the attractive force of the latter far ſurpaſſes that of the other earths and metallic oxides in regard to the colouring parts of the ſaid ſubſtances.

Alumine and metallic oxides do not retain, with the ſame force of adheſion, the colouring parts of all animal and vegetable ſubſtances indiſcriminately; that of madder adheres much ſtronger than thoſe of the other colouring ſubſtances, which may be claſſed in the following order: kermes, cochineal, logwood, yellow India wood, woad, quercitron, Brazil wood, red India wood, yellow berries, &c. The gall-nut, ſhumac, and other aſtringent colouring ſubſtances, act principally by means of the gallic acid, and, in regard to their degree of fixity, may be placed immediately after madder: the caſe is not the ſame with the Pruſſic acid, which communicates a colour to different metallic oxides, from which it can be ſeparated cold by alkaline leys.

To judge of the fixity of colours ariſing from animal and vegetable ſubſtances, the beſt method is to employ a ley of oxygenated muriate of potaſh or ſoda, with exceſs of alkaline carbonate. The longer or ſhorter reſiſtance which the colours make in this ley, will indicate what they will make when acid, alkaline, ſaponaceous, and other reagents are employed.

In the art of dyeing, and that of cotton-printing, the name of maddering is given to that proceſs by which the colouring parts of madder are transferred, by means of water with the aid of heat, to alumine, or to the oxide of iron fixed in any kind of ſtuff.

* From the *Annales de Chimie*, No. 122.

† We muſt here mention, that C. Chaptal, miniſter of the interior, a good judge in matters of this kind, when he communicated to us theſe obſervations, wrote as follows: "C. Hauſſmann, manufacturer of printed cottons at Logelbach, near Colmar, in the department of the Upper Rhine, well known among thoſe chemiſts who apply the diſcoveries of ſcience to improvements in the arts, tranſmitted to me the annexed memoir. In my opinion it will be of utility to make it known in your Annals, and the author on my requeſt has conſented to its being publiſhed." *Note of the Editors of the* Annales de Chimie.

The

The brightnefs and fixity of the colours obtained from maddering depend not only on the procefs, but alfo on the ftate and purity of the water as well as of the madder. It is therefore abfolutely neceffary to avoid or to render inactive every acid, alkaline or faline fubftance that may be contained in the water, or in the madder itfelf. I have fhown that, by adding carbonate of lime, (pounded chalk,) madder which I fufpected to contain gallic acid was corrected; but that my friend Charles Bertholdi, profeffor in the central fchool of the Upper Rhine, afterwards found that it was fulphuric acid united to magnefia.

The important difcovery of this addition of chalk, which I made twenty-five years ago, has given birth to many manufactories, and improved all thofe eftablifhed near waters which do not run over or hold in folution this earthy falt, without which it is abfolutely impoffible to obtain beautiful and fixed madder colours. This chalk fince that time has become a new object of commerce; and as the price is very moderate, I have not yet determined the juft proportion to be employed: in general, I take one part for four, five, or fix, of madder.

In order to obtain the brighteft madder colours, it is not fufficient to attend to the quality of the water and of the madder: it is neceffary alfo to obferve the degree of the heat of the bath: a low temperature will check the attraction of the colouring parts, and prevent them from being extracted, while one too high will favour the adhefion of the yellow particles of the madder, which obfcure and tarnifh the fhades intended to be produced. The only colour which gains by increafing the heat is black. I have always obferved, that on withdrawing the fire from below the boilers, when the hand can no longer be held in the aqueous vehicle which they contain, if the maddering be then continued for two or three hours, the moft fatisfactory refults will be obtained, as the furnace ftill retains a fufficient quantity of heat to maintain the vehicle at the fame temperature, efpecially when, according to cuftom, large boilers are employed. Befides, it would be very difficult to fix a determinate degree of heat by the thermometer when the furnaces are large.

The yellow parts of the madder as well as of other colouring fubftances are, it is probable, nothing elfe than the colouring parts themfelves combined with oxygen. The product of this combination, by acquiring greater folubility, fuffers itfelf with more difficulty to be taken away by clearing, if the heat has not been properly regulated during the procefs of dyeing. I have often obferved that madder and other colouring

louring ſubſtances, when long expoſed to the atmoſpheric air, do not give colours of the ſame intenſity and the ſame brightneſs as before; either becauſe theſe ſubſtances abſorb the oxygen of the atmoſphere, or that they procure this radical from the water which they attract, or which they naturally contain as a conſtituent principle, and which is decompoſed by a ſlow and inſenſible fermentation. The expoſure, on the graſs, of cotton or linen dyed a dark madder red, might ſupport the idea of a change to a reddiſh yellow; for this dark colour becomes clearer but fainter by the expoſure, and then aſſumes a more agreeable ſhade of crimſon. I have ſhown, in a memoir on indigo, inſerted in the *Journal de Phyſique* for the year 1788, that nitric acid changes this blue fecula into a yellowiſh ſubſtance: a ſimilar change takes place by expoſing, on the meadow, the ſame fecula fixed on any ſtuff whatever; and the yellow reſulting in theſe two ways is more ſoluble in warm water than in the ſame liquid when cold. It however appears that the combination of oxygen is not the only cauſe of the change of colours, ſince curtains of any ſtuff dyed or coloured any ſhade whatever by vegetable or animal ſubſtances, and expoſed to the light, loſe their colour entirely in the courſe of time on the ſide expoſed to the ſolar rays, while the oppoſite ſide retains it for a conſiderable time. If the rays of the ſun then give more vigour to living bodies of the animal and vegetable kingdom by diſengaging from the latter oxygen gas, it appears that they act with deſtructive influence on the ſame bodies deprived of life, by decompoſing their conſtituent principles. In all caſes it will be proper to preſerve the colouring ingredients in dry places ſheltered from the light, which acts upon theſe bodies perhaps only by decompoſing the conſtituent aqueous part, the oxygen of which may join the carbon to form carbonic acid. Reſinous and oily ſubſtances ſhould be preſerved in the ſame way. Theſe conjectures prove at leaſt that the action of the ſun's rays, or of light, on theſe bodies, in general preſents a vaſt field for intereſting experiments to be undertaken.

If in maddering brighter colours are obtained by carefully regulating the heat, a ſacrifice is made at the ſame time of a ſmall portion of the colouring parts of the madder, which cannot be entirely exhauſted except by then increaſing the heat to ebullition; but as the colours thus obtained are degraded more or leſs in the ratio of the quantity of the madder, the gall-nut or ſhumac uſed, this method muſt be employed with caution, and principally for common effects, either in regard to cotton or linen. To avoid as much as poſſible the loſs of madder after the maddering of good articles

cles has been terminated, and before the common ones are put into the boiler, powdered gall-nut or ſhumac muſt be added, with a new but ſmall portion of madder: the proceſs muſt be managed alſo in ſuch a manner, that the ebullition ſhall not take place till two hours after.

I ſeveral times tried to exhauſt the madder by ſimple ebullition, and without adding any thing elſe than chalk; but I found that this was unfavourable to all colours, black excepted: it even appeared that the effect of the madder was much leſs than when the heat was moderately applied, and when the accumulated caloric eaſily decompoſed the colouring ſubſtance. It is this tendency to be decompoſed, and particularly by fermentation, however little it be moiſtened or diluted with water, which has hitherto prevented me from obtaining a ſubſtantial colour, pretty dark, and ſufficiently fixed to be applied on any kind of ſtuff. I obſerved alſo, that if the heat was carried too far the firſt time, in circumſtances when it was propoſed to madder a ſecond and third time, it prevented me not only from obtaining bright and agreeable ſhades, but alſo of the requiſite intenſity. The aqueous vehicle of the madder, at too high a temperature, never fails to weaken the adheſive force of the alumine and the oxide of iron to the ſtuff, and to take from it a portion, which an experienced eye may eaſily remark on examining the bath.

I ſhall here repeat, that for common and low-priced articles it is indiſpenſably neceſſary to employ gall-nuts or ſhumac, which will ſave one half and even two thirds of the madder; but the colours obtained are neither ſo fixed nor ſo bright. The addition of chalk, however, muſt not be omitted; otherwiſe the gallic acid will carry away a portion of the alumine and coloured oxide of iron, which will weaken the ſhades, and, by tarniſhing the ſtuffs, will alſo attack the white which may have been preſerved in them. Without the addition of gall-nuts or ſhumac, it ſeemed to me impoſſible to exhauſt the madder entirely of its colouring parts; which made me preſume that their adheſion is favoured by the viſcid nature of the tanning principle of theſe aſtringent ſubſtances, which carry away and combine with themſelves the colouring parts. I ſhall obſerve alſo, that gall-nuts as well as ſhumac loſe the property of dyeing black; and acquire, on the other hand, that of dyeing or colouring alumine yellow, oxide of iron olive green, by the addition of chalk, the calcareous baſe of which unites itſelf to the gallic acid. Do theſe yellow and olive-green colours ariſe from any peculiar ſubſtance contained in the gall-nuts and ſhumac, or are they indebted for their origin to the tanning principle? This remains to be examined.

The quantity of madder to be employed in dyeing ought not only to be proportioned to the extent of the surfaces to be maddered, but also to the concentration of the liquors of the *acetite of alumine and iron*, improperly called mordants; that is to say, to the greater or less quantity of alumine and oxide of iron which these saline liquors, either insulated or mixed together, when they dry on the articles to be dyed, may have left or deposited there by the evaporation of the acetic acid. If the objects to be dyed are not numerous, and, in particular, when bright shades only are to be produced, they may be maddered only once; but when they are numerous, and intended to have dark shades, the maddering must be repeated twice, and even thrice. Three quarters of a pound of madder of a good quality are sufficient for dyeing a piece of white Indian cloth of ten ells in length and three quarters broad, intended to exhibit only a few coloured objects: the quantity of the colouring substance must be increased in the ratio of the mass of alumine and oxide of iron, fixed on a piece of stuff of the above dimensions. It may be extended to 6, 8, 10, and even 12 pounds, for a ground well covered with a lively and very intense colour. Intelligence and practice in the management of a dye-house will not fail to indicate nearly the proper proportions.

Whatever care may be employed in maddering to avoid the adhesion of the yellow parts, the colours obtained will be far from having all the beauty and fixity which they might acquire by clearing, preceded by very long ebullition in exceedingly pure water. This ebullition alone, by the addition of bran, will serve to brighten the colour: more rosy reds will be obtained by employing soap with or without the addition of bran; carbonate of potash or of soda, substituted for bran, will make the reds incline to crimson; but I must observe, that unless the workman chooses to run the risk of making the reds entirely brown, and in such a manner that it will not be possible to restore them, it will be necessary, before soap and alkalies are applied to the stuffs, to expose them to the action of the strongest heat that can be communicated to water. This operation will be attended with success, if as little passage as possible be afforded to the steam, and if the boilers employed be converted into a sort of digesters. The fixity of the colours will be proportioned to the time employed in exposing them to the action of the boiling water. It is needless to observe, that there is no danger

danger of ſpoiling the colours by ſoap and alkaline carbonates, when the maddering, inſtead of being directed with a moderate heat, has been carried to ebullition, as is practiſed in many dye-houſes; but, in this caſe, the colours obtained are more difficult to be cleared.

As water charged with oxygenated muriatic acid eaſily carries away the colouring parts of madder, as well as other vegetable and animal ſubſtances, by decompoſing them; and as acids more concentrated may, in their turn, take from the ſtuffs the colourleſs alumine and the oxide of iron, it is impoſſible for me to adopt the idea of a chemical combination of the colouring parts with alumine and metallic oxides, which, in my opinion, when fixed and coloured on any ſtuff, form only compound aggregates.

The clearing of objects printed on a white ground requires modifications, which I ſhall detail on a future occaſion, when I find leiſure. It will therefore be ſufficient at preſent to ſtate, that after continuing for ſome time my experiments on the Turkey red, inſerted in the *Annales de Chimie* for the year 1792. I at laſt found a red much more beautiful and durable than that of the Levant, by fixing alumine on cotton, thread, and linen, by an alkaline ſolution of this earth mixed with linſeed oil. The following is the proceſs I employed.

[To be continued.]

XXXIII. *Notices respecting New Books.*

Ausführliche Geſchichte der Theoretiſch Praktiſchen Uhrmacherkunſt, &c. A Hiſtory of Clock- and Watch-making, both Theoretical and Practical, ſince the earlieſt Method of dividing the Day to the End of the 18th Century. By I. H. MORIZ POPPE, 1801. 8vo. 564. p. 8.

THE author of this work having publiſhed, in 1797, An Eſſay towards a Hiſtory of the Origin and Progreſs of Clock- and Watch-making, conſiſting of ſix ſheets; it met with ſuch a favourable reception, that he was induced to improve and enlarge it to its preſent ſize. He acknowledges the obligations he is under to profeſſors Kaſtner and Beckmann, who aſſiſted him with their advice and information; and who, on account of their learning and extenſive reading, were able to ſupply him with much uſeful information. The whole work is divided into ten chapters.

1. *The oldeſt method of dividing the day, and the invention of ſun-dials.*—The period when ſun-dials were invented is as little

little known as the country where, and the person by whom. The author, however, has collected from the oldest authors what is to be found on the subject, and examined it with critical accuracy. The earliest account of instruments destined to mark the lapse of time by the sun's shadow is to be found in the Bible and in Homer. A sun-dial constructed according to the description of Vitruvius was found in the year 1741, among the ruins of a villa on the Tusculan hill at Rome. A sun-dial of the same kind is still to be seen at Athens, standing on the summit of a rock, to the right of an edifice built by Thrasyllus.

II. *Use of sun-dials, and the division of the day into hours, among different antient nations; gradual improvement of the art of constructing and using them to the present period.*—The Indians, Siamese, Tartars, Persians, Chaldeans, Egyptians, and Chinese divided the day into 60 hours, and each hour into 60 minutes, &c. These people even employed a gnomon for the purpose of placing their temples according to the principal quarters of the globe. We know from history that the Chinese made use of gnomons 12 or 15 centuries perhaps before the birth of Christ. The old geographers, by examining the length of the shadows of gnomons of equal height, determined the latitude of places, and the obliquity of the ecliptic. Berosus brought to Greece from Asia the division of the day into twelve hours; and the first sun-dial. Anaximander, about six centuries before Christ, made an improvement in sun-dials, as did also Anaximenes. Eudoxus, four centuries before Christ, constructed a still more perfect sun-dial, under the name of *ar. chne*. Apollonius of Perga, about a century before Christ, invented the *pharetra*. Patrocles found out the *pelekinon*, Dionysiodorus the sun-dial in the form of a cone, Cleanthes the hemisphere or *scaph* , Parmenio the *prostapistorumena*, Theodosius and Andreas the *prospanklima*. Vitruvius makes mention of three other sun-dials; *gonarcha*, *engenaton*, and *antiboreum*, without describing them or naming the inventors. The author describes all these sun-dials with as much precision as the accounts of them remaining will allow, and with literary and technical accuracy. There were public sun-dials both at Athens and Sparta. Eratosthenes and Archimedes employed dials very accurately divided for astronomical purposes; and all the cities of Greece of any consideration soon had public instruments of this kind. Ring-dials soon began also to be used. A dial of this kind was suspended in the large ship of Hiero; but the Grecian navigators, for measuring the state of the sun, the time of the day, and the stars above the horizon, employed rather the hodometer

described

described by Vitruvius, which seems to have furnished the first hint for our measurers of time, constructed with wheel-work. It was very late before the Romans had real sun-dials. They employed in their stead obelisks, to which good gnomons were applied. The largest of all the gnomons of modern times was that of Ulug Beigh, erected at Constantinople in the 15th century, which was 183 feet in height. The Bononian obelisk, 83 feet high, erected by Cassini, that of Paris, and the one which Pope Clement XI. caused to be constructed, are also celebrated. In the antient gnomons, the hours were indicated by the shadow of a style; in the modern ones the same thing is done by the sun's rays passing through a hole. The Greeks and Romans employed persons of both sexes to announce to them the hours, as indicated by the public gnomons. Trimalchio caused the hours to be announced to him by a trumpeter. This was afterwards customary at the temples. It is not known, however, when portable sun-dials came into use. Some of these instruments, after being buried more than 1500 years, were found between 1730 and 1740 in the territories of Rome: of these remains of antiquity the author gives a description. That Purbach, an astronomer of Vienna, introduced sun-dials into Germany about 300 years ago is not probable, as the Germans, no doubt, obtained them by their intercourse with the Romans. In the 16th century artists took great pains to construct sun-dials in a great many ingenious ways; about the same period lunar and astral dials were invented. There is a sun-dial at Besançon which is seen only when the sun shines. This is also described, as well as the dial below the roof of the council-house at Ingoldstadt. Another at Alençon, still more ingenious, is connected with wheel-work, and indicates true and mean time. In the museum at Göttingen there is a collection of sun dials, and other works of art of a similar kind, preserved in a box.

III. *Oldest method of dividing the night. Invention of water-clocks and sand-glasses, and the progressive improvement of them to the present time.*—The circumstance of sun-dials being of no use in the night-time, and during cloudy weather, gave occasion to the invention and improvement of water-clocks; traces of which may be found among the oldest nations, the Chaldeans and Egyptians. They were in use also at an early period among the Chinese. They were employed by the Egyptian astronomers for measuring the diameter of the sun. In their first state they were called *clepsydræ*, because the water issued from them drop by drop. The defect, in regard to the inequality in the efflux was at first remedied by the small stick of the Indians having a hole bored in it, and

and which floats ſome time on the water, but which after a certain period, diſcovered by repeated trial, becomes immerſed. It was afterwards contrived to keep the water-clock always full, by ſuffering the ſame quantity of water to run in as that which flowed out, that equal portions of time might be indicated by an equal fall. A funnel or inverted pyramid was then employed, in which the water deſcended in unequal parts, but through equal degrees marked out on a ſcale. In the courſe of time the knowledge of aſtronomy was applied to theſe clocks, and ſome were conſtructed in a very ingenious manner: of this kind was the *anaphoricum*, which the author deſcribes; alſo the retrograding clock, and winter water-clock. Plato brought the firſt water-clock to Greece. The firſt that ever appeared at Rome was ſhown by Scipio Naſica, about the year 157 before Chriſt: but ſoon after theſe clocks were common, not only at Rome, but in other towns of the Roman empire. When Julius Cæſar invaded Britain, he found that the inhabitants had water-clocks. In general they were more prized than ſun-dials. Athenæus conſtructed a clock which indicated the hours by the hiſſing noiſe of the air forced through a narrow hole by the preſſure of water. Together with public ſun-dials, public water-clocks were alſo ſoon introduced. Theſe were eſtabliſhed even in the palaces of the great. That the hours as announced by theſe water-clocks may be more eaſily made known to the public, the Turks have criers poſted on high towers,—and the Chineſe, perſons who ſtrike on large bells. The Japaneſe employ burning matches to indicate the time, and announce the hours in the ſame manner as the Chineſe. The uſe which Hipparchus and Ptolemy made of water-clocks contributed to their improvement: they were much improved by Hero, but during the next ſeven centuries nothing was done in this reſpect. The clocks of Boethius, that which Harun al Raſchid ſent as a preſent to Charlemagne, and that of the philoſopher Leo of Conſtantinople, were much celebrated, though the invention of clocks with wheel-work rendered water-clocks unneceſſary. The latter, however, continued ſome time after in uſe; and about the year 1660 water-clocks in the form of a drum, which are thoſe called properly at preſent water-clocks, were invented it is probable in Italy, but rather for curioſity than for real uſe. The obſervation that the water ſoon evaporated, may have given occaſion to ſand being uſed in ſuch clocks inſtead of water. The period when this change was made is not known. There is reaſon only to conjecture that the Egyptians and Chaldeans had hour glaſſes. In the Monachal Laws the word *klepſammidia* occurs only in writings of the eighth century; and it was not till a much later

 period

period that attempts were made to construct hour-glasses in an elegant and ingenious manner. Many of them are described by Francis de Lanis, as well as by Schott and Ozanam. Rivault employed such sand-glasses for astronomical observations; and likewise Tycho Brahe, who used also clocks constructed with quicksilver.

IV. *Ingenious water-clocks and other machines, the moving principle of which was different from that of our common clocks that go by wheel-work.*—Under this head the author speaks of the machine which king Gonebaud received as a present from Theodoric king of the Goths. He gives also a description of the before-mentioned clock sent by Harun al Raschid to Charlemagne. At that period the eastern princes presented clocks to the western; at present this mode is reversed. This chapter contains also a description of some other ingenious clocks from Schott, Kircher, Ozanam, Martinelli and de Lanis; also Perrault's pendulum-clock, which was moved by water, and a Chinese one which Y-Hang caused to be constructed.

[To be continued.]

Traité Elémentaire du Calcul Differentiel et Integral, &c. An Elementary Treatise on the Differential and Integral Calculus, preceded by some Reflections on the Method of teaching the Mathematics, and appreciating in Examinations the Knowledge of those who have studied them. By S. F. Lacroix. Paris, An. 10.

THE author of this work is well known by his large treatise on the Differential and Integral Calculus, in three vols. quarto, in which he has given a complete view of every thing taught on the subject, and which ought to be studied by those who are desirous of enlarging the boundaries of this science. The time devoted, usually, to a course of analysis being far from sufficient to follow with the proper attention so many objects, the author found it necessary to make a selection. This publication, however, is not merely an extract from the above large work; it is a new treatise, which cannot fail of interesting those who are in the possession of the other. It is founded on the same principles, and will form an excellent introduction to the study of the more learned works.

Elements of Chemistry. By J. MURRAY, *Lecturer on Chemistry, Materia Medica, and Pharmacy.* 2 vols. 8vo. Edinburgh 1801.

THIS work will be found very useful to the chemical student. The author has given a correct view of the present state of the science of chemistry, and bestowed a proper degree of attention on the practical facts and applications, which serve to illustrate and establish its general principles.

A Statistical Account of the Population and Cultivation, Produce and Consumption of England and Wales, compiled from the Accounts laid before the House of Commons and the Reports of the Board of Agriculture; together with Observations thereupon, and Hints for the Prevention of a future Scarcity. By BENJAMIN PITTS CAPPER. 8vo. Kearsley. 1801.

THE title of the present work very well describes its nature, and renders it unnecessary for us to say any thing respecting it, further than that it is an interesting little work. The following summary is extracted from it:

The total number of population appears to be -		9,500,000
Number of inhabitants in the capital - -		715,002
Number of parishes, churches, and chapels		
in England -	10,500	
in Wales -	1,100	11,600
Number employed in agriculture - -		1,737,675
Number as tradesmen and mechanics - Wales, -	55,887	
England	1,840,598	1,896,485
Number of families - - - -		1,917,232
Number of persons to each family - -		$4\frac{3}{4}$
Number of houses inhabited - - -		1,598,278
Number of persons to each inhabited house		$5\frac{3}{5}$
Number of houses uninhabited - - -		58,962
So that there appear buildings sufficient to contain persons - - -		430,187
But by making an allowance of 45,000 houses, that we may suppose will be occupied by 250,000 persons, when the army and navy are reduced to a peace establishment, the number of uninhabited houses will then remain - - - - - -		13,962
The number of marriages appear to be annually - - - - 1 in		134

We understand that Dr. Gruber is at present engaged in translating C. Guyton's tract on the means of dis-infecting the air, preventing contagion, and checking its progress; which will appear in a few days.

XXXIV. *Pro-*

XXXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

THURSDAY, February 25. A letter from Mr. Schröter, of Lilienthal, refpecting the planet Ceres Ferdinandea, informed the Society that he had obferved a nebulofity round the new planet fomewhat refembling that of a comet: the diameter of the true difc being 1·8″, and that of the nebula 2·6″; but the diftinction was not always equally obfervable. Mr. Schröter confiders this body as of a hybrid nature, or a medium between a planet and a comet; but he imagines the apparent nebulofity to be owing to an atmofphere, and that, according to the different ftates of this atmofphere, the light reflected from the planet is either white, blueifh, or reddifh.

A table of obfervations of the fame planet was alfo communicated by Mr. Mechain, through Sir Henry Englefield.

In the meetings of the 25th of February, 4th and 11th of March, a paper which has been expected for fome time occupied the attention of the Society; namely, Mr. Howard's, on the analyfis of ftones that have fallen from the clouds.

Mr. Howard begins with a hiftorical detail of the various relations of this kind which are found on record, and particularly refers to the effays of Mr. King and profeffor Chladni, and to various authors quoted by them. But the firft inftances with which chemiftry has interfered are thofe of a ftone prefented to the French Academy by the abbé Bachelay in 1768; and another examined afterwards by profeffor Barthold. The ftones from Sienna in 1794; the large ftone of 56 lbs. weight which fell in Yorkfhire in 1795, and was exhibited foon after in London; and the fubftances which fell at Benares in 1798, are the immediate fubjects of Mr. Howard's inveftigation. All thefe agree in the general appearance of an afh-gray ftony fubftance, mixed with fpangles of pyrites and of native iron, and externally of a dark colour, covered with a femi-vitrified and bliftered cruft. The abbé Bachelay's was fuppofed to contain 8½ fulphur, 36 iron, and 55½ earth; and fome of the others were found to confift of fimilar ingredients. The ftone which fell near Mr. Topham's houfe in Yorkfhire, penetrated twelve inches deep into the earth, and fix more into a chalk rock: its fall was accompanied with noifes like a difcharge of artillery. A very particular and perfectly authenticated account is given, in the words of Mr. Williams, of feveral fubftances which fell about twelve miles from Benares, and penetrated ſome inches into the earth in ſeveral ſpots within the diſtance of 100 yards; their fall being accompanied by a very vivid light.

Mr. Howard proceeds to mention another ſpecimen from the Muſæum Bornianum, now in the poſſeſſion of Mr. Greville, ſaid to have fallen in Bohemia, which agrees with the reſt in its characters. A mineralogical deſcription of theſe ſtones by the Count de Bournon is ſubjoined. They appear to conſiſt principally of ſubſtances of four kinds, beſides the dark cruſt which ſurrounds them: the firſt of theſe ſubſtances is in the form of dark grains, of a conchoidal fracture, from the ſize of a pin's head to that of a pea; the ſecond is a kind of pyrites, the third is metallic iron, and the fourth a gray earthy ſubſtance, ſerving as a cement to the reſt. The proportions of theſe ſubſtances appear to differ in ſome meaſure in the different ſpecimens, the iron abounding moſt in the ſpecimens from Yorkſhire and from Bohemia. Mr. Howard has aſcertained, by a chemical analyſis, that ſilica, iron, magneſia, ſulphur, and nickel, are contained in the different parts of theſe ſubſtances. The globular bodies and the cementing earth each contained about 50 ſilex, 15 magneſia, 34 iron, and 2½ nickel.

From 150 grains of the earthy part of the ſtone from Sienna, Mr. Howard obtained about 70 ſilica, 34 magneſia, 52 oxide of iron, and 3 oxide of nickel; the contents of the ſpecimens from Yorkſhire and from Bohemia were not materially different. Mr. Howard proceeds to inquire into the cauſes of the difference in the reſults of his analyſis and thoſe of the foreign chemiſts, with reſpect to the ſpecies of the earths. After having ſhown the ſtriking analogy between theſe ſubſtances, and their total diſſimilarity to other mineral products, Mr. Howard examines into the form and contents of various ſpecimens of native iron: obſerving that Mr. Prouſt detected nickel in a large maſs of native iron found in South America; Mr. Howard diſcovers a portion of the ſame metal in every ſpecimen that he has examined from different parts of the world. A deſcription of theſe ſpecimens by the Count de Bournon is inſerted, and the large maſs diſcovered by profeſſor Pallas in Siberia, is particularly deſcribed. It is found to contain detached maſſes of ſemi-tranſparent ſubſtances conſiderably reſembling ſome of the conſtituent parts of the ſtones from Benares. Mr. Howard does not give a decided opinion reſpecting the origin of all theſe ſubſtances; he only obſerves that they agree in ſeveral remarkable properties, diſtinguiſhing them from all other bodies, that they all appear, from well authenticated accounts, to have fallen on the earth, attended

tended in most instances by meteors or lightning, and that it is remarkable that the native iron in all the stones contains nickel as well as the other native irons.

A letter was also read on the 11th of March from Mr. Von Zach, confirming Mr. Schröter's observation of the changeable light of the planet Ceres, which Mr. Von Zach had at first attributed to the haziness of our own atmosphere, until he found that MM. Olbers and Schröter were agreed in deriving it from a real change in the light reflected.

On the 18th, an appendix to Mr. Chenevix's paper on oxymuriates and hyperoxymuriates was read; and,

On the 25th, a paper on corundum, by Count Bournon, was begun; but it will take another night's reading to finish it. The Count classes along with corundum all the varieties of the ruby, sapphire, amethyst, emerald, &c. which, from their peculiar hardness, are classed by jewellers as oriental.

FRENCH NATIONAL INSTITUTE.

Some very interesting papers were read in the late sittings, particularly a very learned dissertation on the structure of primitive mountains, and of that of a part of the Pyrenees; a memoir, no less important, on the theory of the moon; and a description of some new experiments by C. Coulomb, to prove that all bodies are susceptible of being attracted by the magnet. In the sitting of March 17th, C. Gerard, engineer of bridges and causeways, read a continuation of his very learned dissertation on the pyramids of Ghizé and Saccara. Chef de brigade Grobert read a memoir on wheels with conical fellies, as used in England; and announced an extensive work on carriages of all kinds. Count de Dieth was present at this sitting.

SOCIETY OF FELIX MERITIS, AMSTERDAM.

The department of design in this Society, in the meeting of October 5, 1801, proposed the following prize subjects:

1st, A golden medal, of 14 ducats value, for the best historical painting of Abraham entertaining the angels: Genesis, xviii. 10—12.

2d, A medal, of the same value, for the best landscape in the Netherlands.

The paintings to remain the property of the artists.

The pieces must be transmitted to the Society before the 1st of August 1802. The paintings must be accompanied with a device, and be sent, carriage paid, to the president of the department of design, C. I. C. Linckers, at Amsterdam.

 XXXV. *In-*

XXXV. *Intelligence and Miscellaneous Articles.*

VACCINE INOCULATION.

In our laſt we mentioned that Dr. Sacco, of Milan, had ſent to Dr. Pearſon cow-pock matter taken from the Milaneſe cows. In a work publiſhed by Dr. Sacco, in Italian, entitled, Practical Obſervations on the Uſe of the Cow-pock, as a Preſervative againſt the Small-pox, he gives the following account of the manner in which he procured the pus for inoculation, and alſo a repreſentation of a cow's udder infected with the malady. (See Plate VI.)

"For ſome time I had been extremely deſirous to repeat the experiments of Jenner, and for this purpoſe made diligent ſearch to diſcover the cow-pox in Lombardy, it being extremely difficult, eſpecially in the preſent circumſtances, to obtain the pus from England. A fortunate combination of circumſtances, by which it became neceſſary for me to go to the large town of Vareſe, in the beginning of autumn, procured me an opportunity of examining a number of cows on their way from Switzerland to the fair of Lugano; and by this means I had a favourable opportunity to make ſuch reſearches as might diſcover in ſome one of them the cow-pox. It was on this occaſion, that, converſing with ſome dealers in cattle, and countrymen who had large dairies in Lower Lombardy, I learnt that the cows among us are ſubject to the cow-pox. In this inquiry I took care to propoſe my queſtions in ſuch a manner as to prevent the riſk of being impoſed upon. A farmer of Cremona, who had bought forty cows in Switzerland, and had driven them from thence as far as Vareſe, aſſured me that almoſt all of them had been ſucceſſively attacked with puſtules on the extremity of their nipples, and ſome of theſe were now converted into incruſtations. I viſited the cows, and had an opportunity of verifying his aſſertions. I picked off ſome of theſe incruſtations with an intention of applying them in fomentation, if, perchance, I could not procure the true pus for inoculation. The ſame farmer promiſed me an opportunity of ſeeing this diſeaſe with my own eyes, and for this purpoſe conducted me to a neighbouring meadow, in which we found a herd of cows belonging to a friend of his. We examined theſe cows, and diſcovered on two of them different red ſpots, which the farmer aſſured me was the firſt ſtage of the diſeaſe: no other ſymptom appeared on the cows, but a ſlight degree of dejection. He aſſured me that this was the very diſeaſe

I was in queſt of, and that, in the courſe of two days, the puſtules would unfold themſelves. At this viſit which I made to the cows, there was preſent a dealer in the Griſons cows, who fully confirmed the truth of theſe aſſertions. He alſo added, that in his country he had ſeen the cows afflicted with a ſimilar eruption on their dugs, and to remove the incruſtations it was common to anoint them with boiled oil uſed for varniſh; and that by this means they fell off in the courſe of two or three days. Early next morning I went again to ſee the cows, examined them anew, and found on one of them four red ſpots already tumid and raiſed into puſtules; three of theſe were ſpread over the nipples, and the fourth lay in the middle of the dugs. The other cow had ſix puſtules; two on the nipples, and the reſt ſcattered above them. Theſe were larger than thoſe of the firſt cow, and around them appeared a ſlight red circle. Apparently theſe puſtules occaſioned much pain to the cows; for, on my approaching to examine them more minutely, they would ſcarcely permit me to touch them for one moment. Although the puſtules were already large and prominent, they did not yet appear to me ſufficiently mature to yield the matter I wanted. As the cows were that day to go forward on their way to Milan, I found myſelf under the neceſſity of following them to their firſt halting-place, in order to examine them again next day. I walked out at an early hour to the meadow where they were at paſture. I examined the puſtules, which appeared to me to be now arrived at maturity. They were lucid, and of a pale red colour, with a brown ſpot in the middle more depreſſed; and I thought this a favourable moment to collect the matter, which, through the aſſiſtance of the herdſmen, I was eaſily enabled to do by repeatedly ſoaking a thread in it. Although I ſaw no reaſon to doubt that this was the true cow-pox, yet, this being the firſt time I had ever ſeen it, I began to ſuſpect that the puſtules might be of that kind which Jenner calls the *ſpurious* cow-pox: I determined, therefore, to decide the matter by experiment. A conſiderable number of experiments, all uniform in their ſymptoms and progreſs, and always conſtant in their reſults, put the matter beyond doubt, and gave me full conviction that this was the true cow-pox. Such and ſo many are the obſtacles to be overcome on the introduction of any innovation, however ſalutary, that I for ſome time deſpaired of being able to induce any one to ſubmit to inoculation with the matter I had collected. In fine, after many fruitleſs perſuaſions, I ſucceeded in my deſign: the

ſucceſs

ſucceſs which attended the firſt inoculations encouraged others to ſubmit to the ſame proceſs."

Dr. Sacco then proceeds to detail three hundred caſes, in which he applied the pus he had obtained in the manner deſcribed above. Theſe caſes were attended with various circumſtances; but the inoculation ſucceeded to produce the cow-pox in all of them; and in a conſiderable number the inoculation for the ſmall-pox was afterwards applied, but without any effect.

ELECTRICITY AND LIGHT.

The following hint for electric experiments is communicated by a perſon who has not a room fitted up properly for making ſuch nice experiments himſelf:

The *electric matter* being ſuppoſed by ſome philoſophers to be the ſame as the *ether* mentioned by Newton, which he imagined conſtantly to reſide near the ſurface of all bodies, I wiſh to have the following experiments made by ſome of your readers, and the reſults communicated to the public by your Magazine.

Let ſmall wire bars be electrified *poſitively* and *negatively*, as it is called, and placed in a convenient ſituation, ſo that rays of light may paſs very near them, and obſerve whether any difference is produced either in the *reflection* or *inflection* of the rays when the bars are electrified; and what that difference, if any, is. Theſe experiments may perhaps give ſome inſight into the two ſtates of electricity, and inform us whether the poſitive is a *condenſed* ſtate, and the negative a *rarefied* ſtate, of the electric matter.

March 15, 1802.

ASTRONOMY.

The aſtrologers alarmed Europe in 1186 by announcing a conjunction of all the planets, which was to occaſion extraordinary ravage. I have ſpoken of this in the preface to my Aſtronomy: but, being deſirous to know whether this rare and ſingular phænomenon really took place that year, C. Flauguergues, aſſociate of the Inſtitute, a zealous aſtronomer, undertook to make the neceſſary calculations, and found, indeed, that on the 15th of September 1186 all the planets were comprehended between 6 ſigns, and 6 ſigns 10 degrees of longitude. This is not exactly a conjunction, but many thouſands of years perhaps muſt elapſe before there will be ſuch an approximation towards a conjunction.

De Lalande.

The ninth planet, diſcovered by Piazzi, affords occupation, and

and with great reason, to astronomers. C. Burckhardt has calculated the derangements which Jupiter may occasion to it, and has thence deduced a new determination of the orbit. The mean distance, 2·7677; eccentricity, 0·0791; inclination, $0^{\circ}\ 37'\ 4''$; epoch 1801, $2^{s}\ 17^{\circ}\ 19'\ 2''$; aphelion, $10^{s}\ 26^{\circ}\ 8'\ 42''$; the node, $2^{s}\ 21^{\circ}\ 5'\ 46''$; revolution, 1681·51 days.

On the 8th Ventose (Feb. 27), at $13^{h}\ 59'\ 15''$ mean time, Lefrançois and Burckhardt determined exactly its right ascension, $186^{\circ}\ 58'\ 44''$, and its declination $15^{\circ}\ 15'\ 55''$.

As we have been nearly a month without being able to observe it, I have sent to all the French astronomers who reside in the south, the positions necessary for finding it. It appears now as a star of the seventh magnitude.

De Lalande.

PROFESSOR VOLTA.

In a letter which we have received from professor Pictet, of Geneva, we are informed that, besides the golden medal voted to professor Volta by the National Institute, the French government has rewarded him for his discovery respecting galvanism with a present of 6000 francs. A few days after his arrival at Lyons, he received the following letter from the minister of the interior:

"The French government, citizen professor, has granted to you a present of 6000 francs (250l. sterling). It has thought this mark of esteem due to the illustrious philosopher who, after enriching science with useful truths for twenty-five years, has come to deposit in the Institute the secret of nature and of the effects of galvanism. I am happy in being the organ of government to a man whom I esteem, and for whom I have long entertained an affection. I beg you to accept the friendship of the consuls. I salute you cordially.

"(Signed) Chaptal."

ACOUSTICS.

A commission appointed by the French minister of the interior, and consisting of C. Lacepede, Prony, Charles, Gossu, and Martini, have given in a report respecting an invention, by C. Montu, of an instrument which the commissioners say may be called a *sono-metre*. Two of these instruments were subjected to their examination, one of them simple, and the other compound, in all its parts. They were both examined with great minuteness, and seemed to be the production of great skill and inventive genius. The name given to this instrument by the commissioners sufficiently indicates the object of it: *sono-metre*, a measurer of sound. The commission, both

both for the benefit of mufic and of the arts in general, have judged it proper that the French government fhould purchafe it at the price of 12,000 francs (500l. fterling).

THE FINE ARTS.

The firft number of Tifchbein's Graphic Illuftrations of Homer, a work long expected, has now appeared. This celebrated engraver, no lefs diftinguifhed by his talents as an artift than by his claffical tafte, having become an early admirer of the poetical beauties of the Iliad and Odyffey, refolved to examine all thofe remains of antiquity which have any relation to the poems of Homer, and, in order to give to his countrymen who had no opportunity of feeing thofe mafter-pieces of art themfelves, at leaft fome idea of them, to make a collection of accurate drawings from the originals, and then to engrave and publifh them. A refidence of ten years in Italy, to which he was invited by his Neapolitan majefty to be director of the academy of painting at Naples, and the acquaintance which he there formed with thofe great patrons and promoters of the arts, Italinfky and Sir William Hamilton, procured him a connection with the firft artifts of Europe; by which means accefs was opened for him to objects in different countries, and he was thus enabled to carry his long-projected plan into execution, notwithftanding the multiplied reprefentations which were made to him refpecting the difficulties to be encountered in fuch an undertaking. Tifchbein began his work, on which he inceffantly laboured for feveral years, with uncommon zeal, patience, and affiduity, and at a very confiderable expenfe. In this manner he formed a valuable feries of drawings, relating to, and illuftrative of, the poems of Homer; and they no doubt would have been long ago publifhed, had he not been interrupted in his peaceful occupation by the tumult of arms, and obliged, in confequence of the taking of Naples by the French, to abandon Italy. By making great facrifices, both in regard to labour and money, he carried off in fafety his valuable treafure, among which was one of the firft mafter-pieces of Raphael, a painting of St. John; together with his drawings for Homer, and fuch of the plates as were already engraved. In the courfe of his paffage by fea, from Naples to Leghorn, a paffage which carried him paft feveral of thofe iflands celebrated in fabulous hiftory, he experienced a violent ftorm in the very place where the drawings of Leonardo da Vinci, and thofe of Michael Angelo which he had executed for Dante, were formerly loft; and the fhip, being unable to weather the ftorm, was driven on the fhores ſhores of Corſica, and wrecked. Tiſchbein's good fortune, however, ſtill attended him, and the drawings were ſaved. Retarded by ſuch impediments thrown in his way by the waves of the ſea, or the arms of the enemy, he was ſeven long months in reaching Caſſel, where he left his valuable collection under the care of his brother, and repaired to Göttingen, to make arrangements for the publication of his work, under the inſpection and with the aſſiſtance of the celebrated profeſſor Heyne, with whom he had already correſponded, and invited to take a ſhare in this important undertaking.

The German text of this work is from the pen of Heyne; and Villers, author of a Commentary on the Philoſophy of Kant, gives a French tranſlation of it. Each number contains ſix plates, and the whole when complete will form two volumes folio; one for the Iliad, and the other for the Odyſſey. The numbers are to be publiſhed in ſuch a manner, that one relating to the Iliad and another to the Odyſſey will appear alternately. The firſt number contains a ſuperb head of Homer, after the Farneſian buſt; Homer inſtructed by the Muſes, after a cameo; the apotheoſis of Homer, from a ſilver cup; the rape of Helen, from a ſarcophagus; the heads of the ſeven principal heroes, from ſeven antique buſts; and the body of Antilochus placed on a car by Neſtor, from a bas-relief in white marble.

MEASURING A DEGREE OF THE MERIDIAN IN LAPLAND.

The following extract of a letter on this ſubject from M. Melanderhielm, perpetual ſecretary of the Academy of Sciences of Stockholm, to C. Delambre, member of the French National Inſtitute, dated October 9th and December 22d, will ſhow what progreſs has been made in this operation.

"The three packages arrived ſafe three days ago. I have received the circle, the double metre with the toiſe, and the books. The whole were in good condition, and had ſuſtained no damage by the way.

"Since theſe articles arrived, Meſſrs. Svanberg, Ofverbom and myſelf have been employed in examining all the parts of the circle, and comparing it with the complete deſcription which you had the kindneſs to communicate to me. We have thus obtained a perfect knowledge of the mechaniſm of the inſtrument, the uſe of all its parts, and the method of making obſervations. As the proſpect from the houſe where I reſide is confined, I ſhall cauſe the circle to be

 tranſported

transported to the observatory of the academy, where I hope we shall be able to make all the observations necessary to prove it, and to acquire the habit of using it before it is conveyed to Tornea.

"The double metre, the toise, and the volumes, which the Institute presented to our academy, were also in good condition, and I beg you will present most respectful thanks to the Institute both on the part of the Academy of Sciences and on my own.

"In regard to the measurement in Lapland, I shall give you an account of every thing hitherto done. Messrs. Svanberg and Ofverbom had no need of assistants during the first journey which they made this year to examine the country; but at present, when the object is to measure the base and the angles, they will have occasion for two able and expert ones.

"In this respect I have found every thing I could desire. The first is M. Holmquist, mathematical assistant at Upsal, where he has laboured for several years in the observatory; the other is M. Paluder, teacher of mathematics in the university of Abo, in Finland. They are both well acquainted with the mathematics and astronomy, and animated with the strongest zeal for the success of the expedition.

"Messrs. Svanberg and Ofverbom returned hither in the beginning of October, their first journey having been attended with complete success. The only thing wanting, in my opinion, is, that, notwithstanding the strictest search, they were not able to find the northern point of the base of 1736. The uncertainty in regard to this point is about two toises. I consider this difference as a great defect, as I wished, above all things, to have exactly the same base, to render our verification more certain and conclusive. They have found all the other stations and points of the measurement of the above period. What they have done besides may be reduced to the following articles: they have erected all the signals necessary for continuing the arc, which will extend to lat. 67° 08′ 36″, almost 30′ beyond that of 1736. They have also given a peculiar construction to the signals, that they may be exactly sure of the point observed. I have sent you the figure and description of these signals. They have caused two new observatories to be built at the extremities of the arc, with huts to serve as lodging-places during the time of observation. But all their researches to find ground proper for a second base have been fruitless. We must be contented with measuring twice the same base as in 1736.

"In

"In regard to the degree of longitude, they have found four proper ſtations on each ſide of the baſe, but it has not been poſſible to extend it further. We ſhall therefore have but a degree at moſt, which in this latitude makes no more than about five or ſix French leagues.

"M. Ofverbom has made fruitleſs attempts to find the intermediate ſtations which divided into two parts ſome triangles a little too oblique, and which are exactly the ſame as thoſe pointed out in your letter. I ſuppoſe that the aſtronomers of 1736 did every thing in their power to have their triangles as convenient as poſſible. Like M. Ofverbom,they muſt have found inſurmountable obſtacles in the mountains and foreſts.

"If no unexpected incident retards their journey, Meſſrs. Svanberg and Ofverbom expect to ſet out for Lapland towards the middle of January next. By theſe means they may employ the months of February, March, April, and even part of May, in meaſuring the baſe on the river Tornea, ſince the ice ſeldom thaws in that country before the end of May."

From the concluſion of this letter it is to be preſumed that the Swediſh aſtronomers are now on their way to Lapland. To form a juſt idea of the zeal and courage which their enterpriſe requires, nothing is neceſſary but to read the work publiſhed in 1738 by Maupertuis under the title of *Figure de la Terre determinée par les Obſervations faites au Circle Polaire*; and particularly page 51, where an account is given of the meaſurement of the baſe, begun on the 21ſt of December 1736, and finiſhed the 27th of the ſame month. It is here ſeen that Meſſrs. Svanberg and Ofverbom propoſe to devote a much more conſiderable time to this operation, in order that no doubt may remain reſpecting the correctneſs of it. The angles which they mean to obſerve at the two extremities of their baſe, between ſignals placed in the very ſame points as thoſe of 1736, will perhaps enable them to find more exactly the northern term of the old baſe. At all events, what ought to leſſen the regret of M. Melanderhielm, as well as ours, is that the triangle on this baſe was ſituated in the manner moſt favourable for making the diſtance between Avaſka and the ſouthern term independent of the ſmall errors unavoidable in the meaſurement of angles. The caſe is nearly the ſame with the ſecond triangle; ſo that the diſtance between Avaſka and Cuituper may, like that of Avaſka from the ſouthern term, give reſults as certain and as concluſive as the direct compariſon of the baſes of 1736 and 1801.

ANTI-

ANTIQUITIES.

The conqueſt of Egypt has enriched this country with a number of antient and rare monuments; ſome of them very entire, and of the higheſt and moſt undoubted antiquity. We ſome time ago announced * that the French, in digging up the earth at Fort Elleve, near the Bogar of Roſetta, had diſcovered a black granite containing inſcriptions in Greek, in the vulgar language of the time in which it was executed, and in hieroglyphical characters; and which inſcriptions, on an examination of the Greek one, appeared all to contain one decree of the Egyptian Prieſts in honour of Ptolemy Epiphanes. We are happy to announce that colonel Turner lately brought this valuable monument ſafe to England in his majeſty's ſhip Egyptienne. It may be conſidered as a treaſure to the learned of Europe; for, though the French had brought home impreſſions taken from it by different proceſſes, it is not poſſible that with theſe ſo much could be effected towards a complete tranſlation as by a view of the ſtone itſelf. The decree being the ſame in all the three characters, it is not an unreaſonable hope that the Greek copy will lead to a thorough underſtanding of the Coptic, and both to ſome knowledge of the hieroglyphical manner of writing. Should this be accompliſhed, what a field for reſearch is opened! Upper Egypt preſents to the aſtoniſhed traveller the ſuperb remains of immenſe palaces and temples filled with theſe characters; even the rocks in ſome places are covered with them, and they are found throughout the country from the mouths of the Nile to the borders of Ethiopia. Let us ſuppoſe for a moment we could decypher and explain them, what an intereſting volume would Egypt unfold to the hiſtorian, the antiquarian, and philoſopher! We ſhould become acquainted with the hiſtory of the firſt ages, now involved in impenetrable darkneſs; we ſhould view the ſacred writings and theſe venerable monuments explaining away the myſtical deſcriptions and expreſſions of each other, and ſee the laboured hypotheſis of many a learned man giving way on all ſides and tumbling into ruin.

CHEMISTRY.

By ſome recent experiments of C. Thenard it appears, that what has been for ſome time conſidered as a peculiar acid, under the name of the *zoonic*, is nothing but a peculiar combination of acetous acid with animal matter.

Some of the foreign chemiſts have announced that the ſuppoſed new metal, made known ſome time ago by Klaproth under the name of *tellurium*, turns out to be only *regulus of antimony*.

* Philoſophical Magazine, vol. viii. p. 94 and vol. ix. p. 141.

XXXVI. *On the Fusion of Malleable Iron with various Kinds of Glass; being a Continuation of the Examination of* C. CLOUET'S *Process for making Cast Steel. By* DAVID MUSHET, *Esq. of the Calder Iron Works*.*

C. CLOUET, in his results of experiments upon iron and steel, states, that a particular modification of iron is formed by fusing malleable iron with glass; and infers, that this change is effected by the combination of a small portion of the glass with the metal.

In my last communication I had occasion to remark, that the mixtures there fused in contact with malleable iron entered into fusion a considerable time before the iron. It appeared, therefore, sufficiently obvious that a perfect glass was formed before any disposition to fusion was indicated on the part of the iron. Reasoning in this manner, I could distinguish no difference of situation betwixt iron exposed in contact with a glass formed by an union of clay and lime, and betwixt the same substance exposed to fusion in contact with window or bottle glass.

If an alteration of quality is effected, and this as being the consequence of an affinity exerted upon the glass by means of the iron, then it appeared probable that the affinity would exert itself in both cases, and be productive of similar effects. In short, since the carbonate of lime had failed in forming steel, it was conceived that the results of the same iron fused in contact with glass, would be analogous to those obtained with a mixture of iron, lime, and clay.

The following experiments are selected from a great variety performed to ascertain this point.

	Grains.
Exp. I. Fragments of the same Swedish iron formerly used - - - -	1312
Bottle glass - 1312 grains.	
From this mixture was obtained a very perfect fusion, accompanied with a dense, smooth-skinned button of iron, which weighed - - -	1292
Lost in fusion	20

equal to $\frac{1}{65\frac{1}{2}}$ part of the original weight of the iron. The fracture of this button possessed a small regular grain of a light blue colour, more resembling the original fracture of

* Communicated by the Author.

the iron than any formerly obtained. One-half of the piece drew eaſily into ſhape, and formed a ſolid handſome bar. This was ſubjected to various teſts, in all of which it exactly reſembled the products obtained with the clay and carbonate formerly deſcribed.

The glaſs in this experiment, when compared with a maſs of the ſame fuſed *per ſe*, was of a darker green colour, poſſeſſed of an additional ſhade of amber.

Exp. II. The ſame experiment, attended with ſimilar reſults, was performed with equal portions of matter, and a loſs of $\frac{1}{85}$th part ſuſtained.

		Grains.
Exp. III. Fragments of the ſame Swediſh iron	-	1103
Bottle glaſs - 551½ grains.		
The reſult in 20 minutes was a finely formed button, weighing	- - - -	1090
Loſs, equal to $\frac{1}{85}$th part,		13

This experiment was performed to aſcertain if the quality of iron was affected by a reduction of the vitrid matter. Nothing materially different as to quality could be inferred. The fracture of the button approached to a regular cryſtallization, in which ſome very perfect cubes of $\frac{1}{10}$th inch upon the ſide were formed.

		Grains.
Exp. IV. Fragments of Swediſh iron	-	875
Bottle glaſs - 1750 grains.		
There reſulted from this mixture a very perfect fuſion, accompanied by a very beautifully cryſtallized button of metal, weighing	- - -	860
Loſt in fuſion		15

equal to $\frac{1}{58\frac{3}{10}}$th part of the original weight of the iron. The fracture of this product preſented groups of large flat granulated cryſtals reſembling in point of colour No. 3. One-half of the button forged with every appearance of ſoftneſs and malleability, and formed a ſolid bar. In ſubſequent teſts, no ſhade of diſtinction could be found betwixt this and the quality of the three former reſults.

It would therefore appear concluſive, that Swediſh iron, by fuſion with bottle glaſs, forms a ſpecies of *caſt malleable iron* exactly ſimilar in its appearance and properties to that formerly obtained when carbonate of lime and argil were uſed. It is further inferable that the reſulting products are not materially affected by various proportions of glaſs, ſeeing that an equal portion, a half, and even a double portion, by weight, to the iron, were productive of no perceptible alteration.

tion. Neither does it appear that the quantity of deficient metal is in the ratio of the quantity of glafs employed.

			Grains.
Equal portions of glafs and iron.	Lofs in metal		65·5
Ditto ditto	Ditto	-	85
Glafs half the weight of iron.	Ditto	-	85
Glafs double the weight of iron.	Ditto	-	58·3

Average lofs equal to $\frac{1}{73\frac{45}{100}}$

	Grains.
Exp. V. Swedifh iron - - -	900
Flint glafs pounded - 900 grains.	
This mixture was expofed to a pretty high heat, which effected a very perfect reduction. The glafs was fmooth and denfe upon the furface. The button of iron -	855
Loft in fufion	45

equal to $\frac{1}{20}$th part of the original weight of the iron. One-half of this button drew pleafantly into fhape, and formed a found bar, which, when fubjected to various trials, indicated none of that want of tenacity defcribed by Clouet, but in every refpect refembled the qualities of metal defcribed in my two laft communications. The glafs obtained in this experiment was of a greenifh blue lead colour, a little tranfparent when broken into thin fragments. Beneath the button of iron I found a fmall mafs of revived lead which weighed 135 grains, equal to $\frac{1}{6\frac{6}{10}}$th part of the weight of the flint glafs.

	Grains.
Exp. VI. Swedifh iron - -	3500
Flint glafs $\frac{1}{2}$, or 1750 grains.	
The mixture was expofed for an hour to a violent heat. A fufion was obtained, and the button of metal weighed - - - -	3380
Loft in fufion	120

equal to $\frac{1}{29\frac{1}{6}}$th part of the original weight of the iron. This product was cut into two pieces, one of which was drawn into a folid bar, under a pretty bright red heat. It afterwards ftood hammering, with caution, at a welding heat, but was fo completely red-fhort that it would not turn at any fhade above a faint red. The fracture of this bar, when cold, was partly granulated and partly filky fibrous, of a very light colour. The glafs approached more to a perfect green than that of No. V; ftill, however, faintly tinged with a fhade of

 lead,

lead. Under the iron was found of revived lead 378 grains, equal to $\frac{1}{4\frac{6}{10}}$th part the weight of the flint glass.

	Grains.
Exp. VII. Swedish iron - -	882
Crown window glass - 882 grains.	
From this mixture resulted a very complete fusion. The metallic button was minutely but regularly crystallized upon its upper surface. I found it to weigh	868
Lost in fusion	14

equal to $\frac{1}{53\frac{4}{10}}$th part the original weight of the iron employed. The fracture of this metal was silvery white, parting in laminæ, destitute both of fibre and grain. This appearance was quite new, and more like zinc than any fracture of iron I had ever before seen. Suffice it to say, that when subjected to trial its quality was precisely the same as found in the former results. The glass was light green, resembling that of No. III.

	Grains.
Exp. VIII. Swedish iron - -	560
Window glass double in weight, or 1120 grains.	
This mixture was reduced to a perfect fusion, and a crystallized button of metal obtained marked with various prismatic shades. It was found to weigh	553
Lost in fusion	7

equal to $\frac{1}{80}$th part the weight of the iron. The fracture of this button was granulated, and in point of appearance and lustre differed considerably from No. VII. It drew, however, easily into shape, and felt soft and very ductile under the hammer. I ventured to double-weld the end of the bar; which it stood pretty well, a few edge cracks excepted. The glass was of a blueish green cast, abounding with waving lines of a faint garnet colour. Thin fragments, however, displayed a grassy green, considerably transparent.

It seems decisive, from these experiments, that the quality of the metal obtained by the fusion of bottle glass, flint glass, and crown glass, with the same quality of iron, was productive of exactly similar results to those described in former papers, where Swedish iron was used with various proportions of argil and carbonate of lime. None of these products exhibited any properties of steel, but all of them considerably altered. The cause of alteration still remains to be developed. If glass has an affinity to iron, this must have been exerted in all these fusions alike, whether the glass was introduced

introduced already formed into the crucible, or afterwards produced by the fufion of one or more earths. In all, fimilar deficiencies of weight have been experienced. This accords not with a combination of glafs, which ought to have added weight to the refults.

Having thus minutely ftated the refults of various experiments here detailed, and claffed their qualities with that of others obtained by different mixtures, and communicated in two former papers, I fhall next exhibit fome experiments with Britifh cold-fhort iron, wherein fome refults approach very nearly to that defcription of *caft malleable iron* defcribed by Clouet as combined with glafs, and incapable of diftending under the hammer.

		Grains.
Exp. IX. Britifh cold-fhort iron	- -	1750
Bottle glafs - 1750 grains.		
There refulted from the fufion of this mixture an elegantly cryftallized button of iron, weighing	-	1663
Loft in fufion, equal to $\frac{1}{19\frac{1}{10}}$th part,		87

This button, without being broken, was put under the hammer, but would not draw into fhape at the loweft red heat. Again this was repeated as follows:

		Grains.
Exp. X. Britifh cold-fhort iron	- -	3500
Bottle glafs - 3500 grains.		
A very perfect fufion alfo refulted from this mixture. The button was lefs perfectly cryftallized, and weighed		3320
Loft in fufion, equal to $\frac{1}{19\frac{4}{9}}$th part,		180

This mafs was alfo found to be utterly incapable of diftenfion under the hammer at any degree of heat.

		Grains.
Exp. XI. Britifh cold-fhort iron	- -	875
Bottle glafs - 875 grains.		
Another elegantly cryftallized button was here obtained, which weighed	- -	831
Loft in fufion, equal to $\frac{1}{19\frac{8}{10}}$th part,		44

This button was as incapable of hammering and diftenfion as the former. The accuracy of thefe refults may be inferred from the refpective deficiencies of the weight. The quality of the whole feems to be nearly that defcribed by Clouet.

		Grains.
Exp. XII. Britifh cold-fhort iron	- -	875
Bottle glafs - 437½ grains.		

 A very

		Grains.
	Brought over	875
A very fine cryſtallized button was obtained from this mixture, which weighed	- - -	808

Loſt in fuſion, equal to $\frac{1}{13}$th part the weight of iron, 67

The fracture of this button was completely deſtitute of grain, laminated, ſmooth, and of a ſilvery white colour. With much caution one-half of it was drawn into a ſmall bar; but in endeavouring to form it into a point, it became looſe, and incapable of further diſtenſion. It felt conſiderably harder under the hammer than the products from Swediſh iron. This reſult, upon the whole, was more ductile than any of the former, and the quantity of glaſs uſed leſs.

		Grains.
Exp. XIII. Britiſh cold-ſhort iron	- -	875
This was expoſed *per ſe* to a very high heat, and a perfect button of metal was obtained covered by a thin film of brown glaſs. The iron weighed	-	805

Loſt in fuſion, equal to $\frac{1}{11\frac{1}{2}}$th part the weight of iron, 70

This button poſſeſſed a brilliant, flat, granulated fracture. One-half of it was drawn into a very ſolid bar poſſeſſing ductility and ſoftneſs in an eminent degree. The quality of this iron was ſo completely cold-ſhort before fuſion, that a ſtout bar of it was eaſily broken by a blow from a hand hammer. It now poſſeſſed the oppoſite extreme of red-ſhort, and was ſo pliant when cold, that a bar nearly [illegible]ths ſquare folded cloſe by the compreſſion of the vice, and afterwards opened fairly, and was bent backwards and forwards ſeven times before it cracked.

		Grains.
Exp. XIV. Britiſh cold-ſhort iron	- -	500
Bottle glaſs - 1000 grains.		
A very beautifully cryſtallized button was obtained by the fuſion of this mixture, which weighed	-	473

Loſt in fuſion, equal to $\frac{1}{18\frac{1}{2}}$th part of the weight of iron, 27

The fracture of this iron diſplayed a double row of very perfect cubes inſerted in each other. The colour was a ſilky light blue, not ſo dazzling as in the former experiment. One-half of this button was ſmooth filed upon the fracture, and forged at a low heat, to endeavour, if poſſible, to obtain a ſolid bar. It however cracked conſiderably, and became looſe and ſhaly. The hardened fracture poſſeſſed a flat, crowded, indiſtinct grain, of a dazzling ſilvery colour; the ſurface turned up a gray ſpotted ſhale, reſembling that of caſt ſteel.

It

It would appear from theſe experiments with bottle glaſs and Britiſh cold-ſhort iron, that a quality of caſt malleable iron is obtained, different, in general, from the products obtained with Swediſh iron; that the leaſt ductile iron is formed when equal portions of glaſs and iron are uſed; and, that the moſt ductile ſtate reſults from fuſing the iron *per ſe*.

It cannot, however, be inferred from this, that the general want of ductility is owing to a combination of glaſs with the iron, ſeeing that when Swediſh iron was employed under ſimilar circumſtances no want of ductility was perceptible; I would rather infer, that the difference betwixt the fuſed reſults was characteriſtic both of the ſpecies of the iron and the nature of the manufacture.

The following experiments were performed with flint and window glaſs, to prove whether any part of the difference, in point of quality, aroſe from the nature of the glaſs uſed.

	Grains.
Exp. XV. Britiſh cold-ſhort iron -	875
Flint glaſs - 875 grains.	
The metallic button reſulting from this fuſion was denſe and very ſmooth ſkinned, entirely free from traces of cryſtallization. It weighed - -	792
Loſt in fuſion, equal to $\frac{1}{10\frac{1}{2}}$th part,	83

The fracture of the button was compoſed of large facets, bright, and inclining to a cubical ſtructure. One-half of the button drew into a looſe ſhaly bar, conſiderably cracked upon the edges. In point of quality it reſembled products XII and XIV. A button of ſoft lead was found beneath the iron weighing 133 grains, equal to $\frac{1}{6\frac{1}{2}}$ part the weight of the glaſs.

	Grains.
Exp. XVI. Britiſh cold-ſhort iron - -	3500
Flint glaſs $\frac{1}{2}$, or 1750 grains.	
A perfect fuſion of this maſs was obtained in an hour. The metallic maſs weighed - -	3317
Loſt in fuſion, equal to $\frac{1}{13\frac{6}{10}}$th part,	183

The fracture of this button was a mixture of granulated cryſtals, and bright laminated facets, ſomewhat inclined on edge. The half of the button drew into ſhape, and formed a bar ſomewhat looſe and cracked. Upon the whole, the difference betwixt XII, XIV, XV, and this, was ſcarcely perceptible. The quantity of revived lead amounted to $\frac{1}{6\frac{1}{10}}$th part the original weight of the flint glafs *.

		Grains.
Exp. XVII. British cold-short iron	- -	875
Window glafs - 875 grains.		
From this mixture, in half an hour, was obtained a fine fusion accompanied by an elegantly crystallized metallic button weighing	- - -	836
Loft in fusion, equal to $\frac{1}{22\frac{4}{?}}$th part,		39

The fracture of this button was composed of irregular cubes inserted in each other. A bar forged from the half of this button was loose, and cracked in the edges. Its quality was much akin to Exp. XVI.

		Grains.
Exp. XVIII. British cold-short iron	- -	500
Window glafs - 1500 grains.		
The button obtained from a fusion of the mixture weighed	- - - - -	453
Loft in fusion, equal to $\frac{1}{10\frac{6}{10}}$th part,		47

The quality of the metal now obtained was equally incapable of being forged as that in Exp. IX and X.

From the nature of the refults of thefe experiments it cannot be fatisfactorily concluded that even the fufion of Britifh cold-fhort iron with glaffes is uniformly productive of the fame quality of metal. But it may be fairly deduced that it invariably is productive of a caft malleable iron, harder and much lefs ductile in every particular than the refults from Swedifh iron fimilarly treated.

The moft probable caufe of this irregularity of quality in the cold-fhort iron I conceive to be cryftallization. In the bar, this quality of iron always prefents cubes more or lefs diftinct. When fufed, and allowed to cool under the circumftances neceffary to produce this effect, a large perfectly cryftallized fracture is the confequence. This does not invariably happen; but in two experiments out of three, I have found it to be the cafe. In a button of 10 to 1200 grains, thefe cubes are found meafuring a full quarter of an inch upon the facet; and fuch fometimes has been the degree of divifion of the fluid, and a confequent perfect cryftalliza-

* In this paper I have repeatedly ftated the fact of oxide of lead being revived when no carbonaceous matter (that in the iron excepted) was prefent. I fhall pafs over any inference to be made from this circumftance, until a fubject more immediately connected with this curious fact is brought forward in its proper place.

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tion, that they appear to have to each other a very flight bond of union. It appears therefore to me obvious, that when a mafs fuch as is now defcribed is put under the hammer, the parts will tend to repel each other, unlefs that precife degree of heat could be afcertained which would coalefce the cryftals without either parting or diffipating them.

In thefe experiments four various fractures of caft malleable iron have been obtained: regularly granulated, more or lefs, on edge: laminated, parting fometimes entire, at other times in large flat facets: cubical, more or lefs perfect: mixed, compofed of the former three.

The firft of thefe, except in one inftance, was peculiar to the Swedifh iron; the fecond were common to both; the third peculiar to the cold-fhort iron, one inftance excepted; the fourth was common to both.

		Grains.
Exp. XIX. Britifh cold-fhort iron - -		875
Kilkenny marble - - -	875 grains.	
Stourbridge clay (old) pot	875 grains.	
A beautiful fmooth-fkinned button was obtained from a fufion of this mixture, which weighed -		813
Loft in fufion, equal to $\frac{1}{14\frac{3}{10}}$th part the weight of iron,		62

This button fplit in circular laminæ, and exhibited, in fome refpects, an entirely new appearance. It forged with more facility than any of the buttons obtained with glafs, and was now uncommonly tough when cold.

		Grains.
Exp. XX. Britifh cold-fhort iron - -		500
Kilkenny marble -	750 grains.	
Stourbridge clay (old) pot	750 grains.	
A very perfect fufion was the refult of this expofure, accompanied by a very fmooth button of metal, on which were impreffed fome faint traces of the ufual cryftallization. It weighed - - -		468
Loft in fufion,		32

equal to $\frac{1}{15\frac{6}{16}}$-th part the original weight of iron. The fracture of this was poffeffed of a regularly granulated form, the cryftals flat, and placed confiderably on edge. It did not in the leaft refemble the former product, but in forging afforded no mark fufficient to diftinguifh any difference betwixt their qualities.

In all thefe experiments we invariably find that the change upon the quality of the iron is great. Fufion invariably communicates a great degree of red-fhortnefs to all qualities; but with

with cold-short iron, the extent of the change is by far the most considerable. Its loss by weight is also much beyond that sustained by Swedish iron.

I meant to have inserted a few experiments with iron uncommonly red-short; but the number already adduced will, I am afraid, have the appearance of prolixity. Suffice it to say, that I have obtained data sufficient to be able to prosecute, with some hope of success, an investigation of the causes of red- and cold-short properties in malleable iron. I shall conclude this subject with the following remarks:—Iron fused with window glass always has its upper surface impressed with a beautiful crystallization, often coloured. This circumstance never takes place when flint glass is used. When bottle glass is used, the crystallization is not so frequent, and seldom so perfect, as with window glass. Crystallization is still less frequent, and less perfect, when a mixture of clay and lime is employed. When carbonate alone is fused with iron, the same want of crystallization takes place as when flint glass is used.

The fusion of malleable iron is productive of the disengagement of an elastic fluid, the escape of which, I am inclined to think, is one of the chief causes of the alteration of its quality. In most cases, when the cover was struck off while the matter was still red-hot, a burst of whitish blue flame issued from the mouth of the crucible. In others, where the fusion had been of short duration, or less perfect, the upper surface of the glass became covered with thin transparent bubbles, each of which appeared filled with a dull lambent blue vapour. These were easily perforated with a pin point, and a slight explosion was heard from each, accompanied by a momentary combustion. In burning, this substance resembled hydrocarbonate *.

Experiment, which was omitted in the former communication, to prove that the carbonic acid was neutral in the fusion of iron, or at least was not productive of steel.

	Grains.
Italian raw marble - -	600
Steel oxide, from the tilt anvil block -	50

This mixture was reduced to a fine powder, and completely triturated. It was then introduced into a Stourbridge clay pot, and exposed to a heat sufficient to fuse it. When cold,

* This fact may lead to a more simple explanation of the formation of inflammable air than that adopted by Dr. Priestley or Mr. Cruikshank. Bar iron with a mixture of glass, without the addition of carbon or its acid in lime, are here productive of an elastic substance possessed of similar properties as that obtained in their experiments.

the

the vitrid mafs was minutely examined, but no metallic globule was vifible. The mixture before fufion was magnetic, owing to the oxide. This property was now entirely loft.

The heat of this experiment was urged moderately, that time might be given for the exertion of any affinity, if fuch exifted, betwixt the iron and the carbonic acid, or betwixt the oxide of iron and the carbonaceous part of the acid. No portion of metal being revived, I conceived this a moft conclufive proof of the nondecompofition of the carbonic acid.

XXXVII. *History of Astronomy for the Year* 1801. *By* JEROME LALANDE.

[Concluded from p. 121.]

M. BODE, of Berlin, has publifhed the laft part of his large Celeftial Atlas in twenty fheets, which contains all the old conftellations, with feveral new ones, and fome thoufands of ftars, with which I furnifhed him; an immenfe labour, of which the aftronomers had need. This beautiful work may be procured at the Collége de France.

On the 27th of September the Helvetic republic adopted the French meafures. This is the firft of the European ftates which has been fenfible of the importance of this univerfal meafure to the general good of civilized nations.

Guglielmini, of Bologna, has made three new experiments on the fall of bodies, to prove the rotation of the earth: he has found the fame deviation from the fouth within a line, though it is not given by theory; but the deviation from the weft he has found as it ought to be. Preparations are making for obfervations of the fame kind at Hamburgh from the tower of St. Michael, at the height of 326 feet.

The obfervatory of Cadiz, during feveral years, has furnifhed us with a feries of important obfervations; but for fome time it has been neglected. General Mazzaredo has caufed a new one to be built in the Ifle-de-Leon, and he has attached to it four aftronomers, officers in the navy—Rodrigo Armefto, Maximo-Lariva Aguero, Julian Canela, and Jofeph Cuefta, who have refided there four years. For ten years paft, a nautical almanac has been publifhed in Spain. I hope navigation and aftronomy will be benefited by it. The telefcope 25 feet in length, made by Dr. Herfchel for Spain, will be fent off in the month of January; and Dupont will go to Spain to mount it.

M. Travaffos,

M. Travassos, secretary of the Academy of Lisbon, has sent me observations by M. Ciera, which have confirmed the longitude of that city; the Nautical Ephemerides published to 1803, and various works of the Portuguese Academy, of which we had no idea, and which the National Institute of France received with much interest. This negotiation was conducted by the chevalier d'Aranjo.

Astronomy was long in a languishing condition in the Batavian republic: M. Fokker has established, at his own expenses, an observatory at Middleburg; he has purchased instruments, and sent us several observations made between 1797 and 1801. M. Fokker, during the revolution of 1795, was member of the committee of public safety, and at that time obtained a tower in the abbey; but the revolution of the 12th of June 1796 interrupted his plans for the improvement of the observatory. He is now engaged in the finance department of Zealand; but his spare time is employed on astronomy, and he has sent me several interesting observations.

In Germany astronomy continues to be cultivated with great assiduity. Baron Von Zach's tour to Bremen and Lilienthal has produced new activity; and the society formed for the purpose of searching the heavens are still occupied with that object. He observes the moon with great diligence; and gives me reason to hope, that I shall see next summer a part of the German astronomers assemble in an astronomical congress at Gotha, as was the case in 1798. Amidst the horrors of war, the French astronomers signalized their zeal for astronomy. General Moreau, being at Cremsmunster, where there is a celebrated observatory, caused a bill to be posted up denouncing the punishment of death against every person who should be guilty there of any depredation; and neither the observatory nor the convent of the Benedictines sustained any injury. It is flattering to the French to have officers who distinguish themselves by a taste for the sciences. It will no longer be said that military men, in consequence of their situation, are ignorant and ferocious.

The Academy of Petersburgh has requested an observer, but Burg and Wurm have been retained by their sovereigns; and this beautiful observatory is still useless, notwithstanding the number of excellent instruments with which it is furnished.

C. Henry has had the satisfaction of erecting the large mural quadrant by Bird, and of making some observations with it.

The irregularity in the degrees of the earth hitherto measured, fured, gave reafon to fufpect fome error in that of Lapland meafured in 1736. M. Melanderhielm, therefore, has obtained permiffion from the king of Sweden to undertake a new meafurement. In the month of April Meffrs. Ofverbom and Swamberg fet out for Tornea, where they erected fignals and built fmall obfervatories. When the ice on the river is thawed, they will meafure a bafe with the rules fent them by the Inftitute. A multiplying circle, made at Paris by Lenoir, will ferve them in the fpring for meafuring the angles; and next fummer we fhall have the folution of this old difficulty.

M. de Mendoza, a Spanifh officer, has publifhed two large collections of tables; one at Madrid, in 1800, entitled *Coleccion de Tablas*; and another at London, in the month of April 1801, which contains tables for the reduction of diftances by the addition of five natural numbers: he has made a new ufe of the verfed fines, which renders numerical operations fhorter and eafier. Thefe tables confift of 407 pages quarto.

M. Garrard has publifhed tables in thirteen pages only; but his method is neither fhorter nor fo accurate.

Mr. Vince, an able Englifh aftronomer, has publifhed the fecond volume of a large treatife on aftronomy.

The ftereotype tables of logarithms, publifhed by Firmin Didot in 1795, have been again corrected. M. Vega, who has caufed to be printed in Germany the largeft collection extant, has verified the French tables, and fent us feveral faults, which are going to be corrected: in all probability they will be the laft, and we may depend in future on correct tables. This is a great benefit for calculators, who have fometimes loft whole days in revifing calculations, which did not agree, in confequence of an erroneous figure.

But as the fmall manual tables are moft frequently employed, I have caufed them to be printed in ftereotype: feveral perfons have corrected them; and in three months I can give all calculators the moft correct, moft convenient, and moft elegant edition that has ever appeared.

C. Verniquet has finifhed the engraving of his large plan of Paris in 72 fheets on a fcale of half a line to the toife, which in correctnefs furpaffes every thing of the kind.

A project was long ago formed and undertaken for making a lunar globe reprefenting all the mountains and craters. Mr. Ruffel has accomplifhed this object in England: his lunar globe, mounted on an ingenious ftand, expreffes all the circumftances of the moon's libration, and fhows that body as fhe ought to appear in the different pofitions of the earth

earth and moon, as well as the variations of the equator and orbit.

M. Philippides, born at Mount Pelion in Theſſaly, who attended the courſe of aſtronomy at the Collége de France in 1794, and who is now at Jaſſi with the hoſpodar of Moldavia, propoſes to publiſh in Greek the Abridgement of my Aſtronomy: he has already publiſhed various works, for the purpoſe of endeavouring to propagate inſtruction in his country.

Three-fourths of the two laſt volumes of Montucla's Hiſtory of the Mathematics are printed. This work will contain the hiſtory of aſtronomy, optics, and navigation; to which I have been obliged to make great additions in conſequence of the too premature death of the learned author.

M. Von Murr, of Nuremberg, who has manuſcripts of Regiomontanus, the firſt reſtorer of aſtronomy before 1500, has cauſed a page to be engraven, an exact fac-ſimile of the character of the manuſcript: he offers to ſell theſe manuſcripts for 2400 francs; they would be a treaſure to a large library.

The aſtronomical poems of Ricard, Lemiere, and Fontanes, had before ſhown how far a view of the heavens is capable of exciting poetical enthuſiaſm. C. Gudin has again proved it by a poem, which contains both the hiſtory of aſtronomy and a deſcription of the heavens, and which diſplays as much correctneſs as elegance.

This year geography alſo has made conſiderable progreſs. Tranchot is conſtructing a map of the four united departments on the ſcale of a line to 100 toiſes: a ſurvey is taking of the country between the dige and the Adda, Piedmont, Swabia, and Swiſſerland; and the miniſter at war cauſed the details to be inſerted in the Moniteur of Auguſt 14.

C. Henry, who has been invited to Munich to conſtruct the map of Bavaria, informs me in a letter that the topographical part is in great forwardneſs; a baſe of 21,649 metres or 11,108 toiſes has been meaſured: it is the longeſt ever meaſured. The large triangles around the capital are already in part cloſed. There are ſome the ſides of which will be from 15 to 20 leagues, and even more. He has already ſwept the horizon ſeveral times with his circle, and with aſtoniſhing preciſion. The laſt ſweep was compoſed of ſix angles; the ſum of which when reduced was not in exceſs, but 8-10ths of a ſecond in 360 degrees; and yet the circle he uſed was not very good. To make up as much as poſſible for what may be wanting in regard to preciſion, he multiplies his obſervations: he never makes leſs than 15 conjugate obſervations,

tions, and he often carries the number to 20. The triangles which Caffini affumed in the neighbourhood of Munich are badly chofen, and the meafurement of them is very incorrect. Without employing his triangles, Henry has already difpofed a feries of 14 triangles, the meafure of which will give us that of an arc of the meridian of fomewhat more than a degree: he hopes that it will ftill be poffible to prolong this arc, which will pafs at a little diftance from Ingoldftadt, and which will afcertain the pofitions of a part of Germany. The travels of baron Von Zach and feveral of his co-operators have alfo fupplied us with new information and new pofitions, which will improve the geography of Germany. Colonel le Cocq continues his map of Weftphalia.

Baron Von Ende, member of the fupreme council of appeal at Cette, has publifhed a volume on the determination of feveral places in Lower Saxony: it is filled with obfervations and calculations.

The geography of diftant countries has affumed alfo a new activity. Captain Baudin, whofe voyage of difcovery I before announced, left the Canaries on the 24th of November, and the Ifle of France on the 22d of March. We have reafon to hope that he has already made interefting difcoveries in New Holland; the only country of the earth which is almoft unknown to us, though it is 2000 leagues in circumference. Bernier, the aftronomer who accompanies him on the expedition, a man of intelligence and courage, leaves nothing to be wifhed for on that head. In the month of June the French government granted paffports to the Englifh veffels, the Inveftigator, captain Flinders, on the point of proceeding on a voyage of difcovery to the South Seas, and to the Lady Nelfon, commanded by lieutenant Grant, who is to accompany the Inveftigator, in exploring the coafts of New Wales.

C. Deguignes jun., arrived from China, where he refided from 1784 to 1797, will, in all probability, when he publifhes the journal of his voyage, give us fome information refpecting that beautiful part of the world.

Baron Humboldt, an enlightened and intrepid philofopher, has gone to South America, where he has travelled 1300 leagues in the deferts, with great labour and amidft terrible dangers, to make us acquainted with the geography and natural hiftory of thofe countries which are ftill new to us.

M. Deferrer has fent me obfervations which give the pofition of Natchetz in Louifiana, and of Guaira in South America; for the former, lat. 31° 33′ 48″, difference of meridian 6h 15′ 21″; and for the latter 10° 36′ 40″ N. and 4h 37′ 11″.

C. Nouet has sent us from Egypt an almanac calculated for that country, and several positions of cities even in Upper Egypt, notwithstanding the climate, the dangers, and inconceivable labour which such observations require. The value of the degree is 56,880 toises; the Egyptian stadium 711 feet; the Egyptian cubit 21·33 inches; the Greek stadium 487·543 feet, and the cubit 19·5017 inches. In a word, he has arrived himself, and brought us the continuation of his labours, accompanied with young Isaac Mechain, the son of one of our most celebrated astronomers, who was his companion and co-operator in Egypt. C. Fourrier has brought us drawings of the zodiacs of Upper Egypt, which attest the high antiquity of astronomy; and he proves that the formation of the constellations goes back 14,000 years, as Dupuis presumed.

C. Marquis, præfect of La Meurthe, has sent to the Board of Longitude observations and manuscripts of P. Barlet, a jesuit of Nancy, which contain interesting things.

I must here say a word of meteorology. C. Lamarc has published a meteorological journal, in which he gives a great many observations, and indicates the variations of the seasons which may be supposed to take place in the course of the year. The minister of the interior has established a meteorological correspondence to multiply observations; and Lamarc, who solicited this establishment, will make it advantageous to the science, which is still in its infancy.

C. Burckhardt, also, has written a long and curious work on meteorology. He has examined 15,000 observations of the barometer, that he may be able to calculate the influence of the winds; and he has found that the south wind gives for mean height 27 inches 11·3 lines, while the east gives 28 inches 1·9 line. He has found also that the height on the borders of the Mediterranean sea is 28 inches 2·2 lines, and on those of the ocean 28 inches 2·8 lines.

Well placed weather-cocks are very rare at Paris. There is none at the observatory, though I requested one on being appointed director; and I have thanked, in name of all observers, C. Bois, tinman, who having built a house on the Quai des Augustins, has erected there a lofty and very moveable weather-cock, with letters indicating the four cardinal points, which will be on a line with a meridian I have traced out on the quay. Astronomers, when they go to the Institute or the Board of Longitude, will have an opportunity of seeing conveniently the direction of the wind; and the same advantage will be enjoyed by the inhabitants of that vast quay, of the Louvre, and the surrounding houses, which

had

had not a single weather-cock in their view, but a great many conductors, which are not very interesting.

On the 3d of November there was in the Baltic a terrible storm, which destroyed some vessels, and was felt even at Brest. On the 7th there was a storm in Provence, which produced 73 lines of water in 2 hours by a wind at S.S.E. It did very great damage at Marseilles; several persons perished in the neighbourhood, and the loss amounts to some millions. C. Thulis has found some memorandums of the storms of July 12, 1748; September 4, 1764; and September 15, 1772: but no person had any remembrance of a storm like that of the present year. The plain of the Po was exposed to an immense inundation.

The Class of the Physical and Mathematical Sciences on the 16th of April chose three astronomers, who were presented to the general assembly for the place of associate, vacant by the death of C. St. Jacques, viz. C. Vidal, Sepmanville, and Bernard.

The first is an uncommon observer, who has alone made more observations of Mercury than all the astronomers of the world since 2000 years. The section of astronomy had presented also C. Pictet of Geneva, Chabrol (de Riom), and Quenot, officers in the navy. I even made out a list of the astronomers known in France, which contained C. Henry, returned from Petersburgh; Nouet and Beauchamp, returned from the Levant; C. Deratte and Poitevin, of Montpellier; Bernier and Bissy, who embarked with captain Baudin; Chevalier, in the department of foreign affairs; Kramp, of Cologne; Duvaucel, at Evreux; Guerin, at Amboise; Mongin, at la Grand-Combe-des-Bois; Maingon and Lancelin, at Brest; Jacotot, at Dijon; Planpain and Degrand, at Marseilles. If we add to these the six astronomers associated at the Institute, it will be seen that this science, the most unprofitable and the most neglected, still furnishes subjects in France. As soon as the happy event of peace exalted the hopes of literary men, I took advantage of it to solicit from all quarters, that astronomy might participate in the benefits of it.

The Academy of Petersburgh has transmitted to me a small present, which it has been accustomed for thirty years to send me for the good of astronomy; and the emperor of Russia has approved the desire of the Academy in that respect.

The king of Etruria has promised to patronize astronomy at Florence. The observatory is already provided with excellent instruments, and Fabroni has assured me that an observer will be placed in it: he has requested one of my pupils;

pils; and this circumstance makes me regret that I have not a greater number.

General Jourdan gives me reason to hope that the observatory of Turin will be put in a proper state; and C. Vassalli, president of the academy, affords me hopes also.

The minister of the marine has given orders that new observations shall be made at Brest on the tides, according to my request, in order to complete my *Trait· du Flux et du Reflux de la Mer*, which I wrote to confirm the excellent theory of Laplace in his *Méchanique Céleste*, and to ascertain what influence the wind has on the tides.

We requested the first consul to procure us from Spain two thousand pounds weight of platina, to construct a telescope of 36 feet; and we have reason to hope for it. Our telescope will, perhaps, surpass that of Herschel.

The observatory of Paris has acquired C. Agoustene. The minister of the interior, C. Chaptal, has agreed that the Board of Longitude may increase its expenses for this new assistant; and I have obtained C. Giroult, whose youth and assiduity give me new aid, and leave me no other regret than that of not being able to procure a greater number.

In my History of Astronomy for 1800, I mentioned the loss which astronomy had sustained on the 5th of November that year by the death of Ramsden: to him we have been indebted, during the course of twenty years, for the best and largest instruments, the most perfect telescopes, and the most ingenious ideas. Troughton, at present, is the most celebrated artist in England, and is preparing to indemnify us for this loss. He has already made excellent instruments; and C. Pictet, of Geneva, lately brought us some of them.

On the 10th of February we lost C. St. Jacques de Sylvabelle, director of the observatory of Marseilles, who distinguished himself by theoretical researches in 1753, as may be seen in the Philosophical Transactions, and then by useful observations: he was 79 years of age, and was still usefully employed. His eulogy will appear in the journal of the Lycæum of his department.

He has been succeeded by Thulis, who has long been assistant director of the observatory. The latter made proselytes and pupils C. Planpain and C. Degrand; but they have both left us, to the great loss of astronomy.

In the month of December 1800, Matteucci died at Bologna: to him we are indebted for the last volumes of the Ephemerides of Bologna, which go as far as 1810. He has been succeeded by C. Ciccolini and Guglielmini, who promise new activity in the observatory, which Manfredi, Zanotti,

notti, and Matteucci, have rendered interesting for a century past.

Chaligni died lately at Madrid; he had long made observations and calculations, by which he has been known with advantage as an astronomer.

M. Chevalier has died at Prague: he made useful observations at Lisbon in 1759, and at Brussels.

On the 8th of October, Gabriel de Bory died at Paris, aged 81: he undertook a journey to Spain in 1751, and in 1758 went to Portugal and Madeira to determine their position. His observations are in the Mémoires of 1768, p. 270, and in those of 1772, part ii. In the Mémoires of 1770 he gave a description of a portable observatory, and in the third volume of the *Savans Etrangers* an observation of the transit of Mercury in 1753. In 1751 he published a description of a marine octant: he diffused a taste for observations through the royal navy: being *chef d'escadre* and governor of the windward islands, he had means of contributing to excite emulation, and he always employed them. In 1765 he was therefore elected a free associate of the Academy of Sciences, and in 1798 member of the Institute.

The Academy and Institute have always been sensible how much need we have of enlightened fellow-labourers to improve our knowledge of navigation, the most difficult of all arts, and the most important of all sciences for the prosperity and greatness of states.

But the greatest loss sustained by astronomy is that of Joseph Beauchamp. He was born at Vezoul on the 29th of June 1752. His observations at Bagdad in Persia, and on the Black sea, were as laborious to him as they were important to us. He set out in 1795 as French consul for Mascate in Arabia; and he wrote to me on his departure as follows: "Remember my attachment to you and to astronomy." He left indeed, with some regret, a country and family who were dear to him: he is certainly one of the martyrs to astronomy. He set out for Constantinople on the 25th of September; we expected him with the greatest impatience, but he had scarcely arrived in Provence when he fell a prey to a malady of which he had not been properly cured. He died at Nice on the 19th of November 1801. Eight days before his death the section of astronomy had presented him to the vacant place in the Institute. I published a notice of his labours in the Moniteur of Dec. 15, 1801.

XXXVIII. *Ana-*

XXXVIII. *Analysis of the Arseniates of Copper and of Iron.* By Richard Chenevix, *Esq. F. R. S. M. R. I. A.*

[Continued from p. 147.]

No. VII. *Yellow hematitic copper ore* *. (See page 4.)

One hundred parts of this ore, boiled with dilute nitric acid, left a yellowish white residuum, which weighed 17. These 17, exposed to a degree of heat sufficient to volatilize the sulphur, left 5, which were silica. The liquor from which this residuum had been separated by filtration, upon being tried for all the different metals, and particularly for arsenic, afforded no traces of any thing but copper and iron. A copious precipitate took place by the affusion of ammonia; the copper was redissolved by adding an excess; and then obtained by volatilizing that alkali, and boiling with potash, after the filter had separated the iron already precipitated. The contents are,

Sulphur - - - -	12
Silica - - - - -	5
Copper, which I believe to be in the metallic state	30
Oxide of iron - - - -	53
	100

In this ore, I believe, for the following reasons, that the metals are in the state I have marked. First, the proportions in the ore announce it; for I always had an excess of weight in the total result, if I did not deduct such a proportion of oxygen as might be contained in 30 parts of copper.

Secondly, there is a considerable disengagement of nitrous gas.

Thirdly, the ore does not attract the loadstone.

And, fourthly, the greater part of the iron (but none of the copper) is dissolved in muriatic acid, forming a green muriate of iron, without disengagement of hydrogen gas.

No. VIII. *Gray vitreous copper ore.* (See page 6.) There are many intermediate states between this ore and the yellow hematitic copper ore; but they are not fair objects of chemical analysis, being merely mixtures of both kinds, in different proportions. The mineralogist, indeed, may dwell upon them, as interesting in studying the products of nature, but they are unsatisfactory subjects for the chemist.

* This and the following (No. VIII.) being the matrices upon which the arseniates of copper and of iron are generally found, I thought it right to give an analysis of them also.

Gray

Gray vitreous copper ore, when obtained in its greateſt purity, is by many degrees the richeſt cupreous pyrites known in nature; and, in the large way, the metal may be extracted by the eaſieſt proceſſes. 100 parts of this ore, in dilute nitric acid, left 12, which were ſulphur. Ammonia, poured in exceſs into the nitric ſolution, rediſſolved, with the exception of 4, the whole of the precipitate which it had formed; the 4 were iron; and, from the ammoniacal liquor, 105 of black oxide of copper, equal to 84 of metallic copper, were obtained by evaporation, and then boiling with potaſh.

Sulphur - - - -	12
Copper - - - -	84
Iron - - - -	4
	100

Although I have mentioned, in the preceding ſtatements, only a ſingle analyſis of each ſpecimen, it is by no means to be imagined, that ſo ſmall a number would be ſufficient to ſatisfy inquiry. None of the above reſults have been taken into account, unleſs confirmed by frequent repetition; and the probationary experiments have been diverſified, as much as lay in my power, by many different teſts, and various chemical reagents.

With regard to the colour of ſome ſpecimens of arſeniate of copper, it is eaſily to be accounted for upon chemical principles. The miſtake under which we have long laboured, that the green is the real oxide of copper, has happily been rectified by M. Prouſt *. He has proved it to be a particular ſubſtance, (to which he has given the very improper name of *hydrate of copper* †,) endowed with peculiar properties, and compoſed of the brown oxide, and of water, in a ſtate of combination. From his experiments, and from what I myſelf have ſeen, I am inclined to draw the concluſion, that we have never yet obtained by art any real ſalt of oxide of copper. In examining, for inſtance, ſulphate of copper, we find it to afford blue cryſtals: and to contain a known quantity of water of cryſtallization, and of what we formerly called the oxide. But that oxide ſtill retains a quantity of water, of which when it is deprived it paſſes to a very dark brown, and changes its chemical nature and properties.

* Annales de Chimie, vol. xxxii. p. 26.

† Copper is not the only metal capable of a ſimilar combination. Cobalt, nickel, and uranium, enjoy the ſame property. This may, in ſome meaſure, explain the change of colour which the liquid muriates of ſome of theſe metals undergo by gentle heat; it may likewiſe throw ſome light upon the ſympathetic ink of cobalt.

If, upon that brown oxide, a sufficient quantity of dilute sulphuric acid is poured, it yields a blue salt, but in a proportion greater by about 24 per cent. than if the green substance had been employed. I imagine, therefore, that the first operation of this brown oxide is, to assume the quantity of water necessary to constitute a hydrate; and, that the combination of sulphuric acid takes place, not between the oxide, but between the hydrate and that acid, to form a salt, which, when crystallized, has taken another portion of water in the act of crystallization. It is a well known fact, that there is a state of concentration, when an aqueous solution of muriate of copper, gently heated, will change from a blueish green to a beautiful brown, which, upon cooling, or by the affusion of water, resumes its former tinge. This brown liquor is probably a solution of muriate of copper; while the blue liquor, like every green or blue solution of a cupreous salt, is a combination of the acid and the hydrate, or (as we should say in this case) a muriate of hydrate of copper. It is true, I have not been able to produce, so often as I could wish, this change of colour. I can, however, adduce the following instance, as being much in favour of my opinion.

It is evident that oxide of copper (for so I shall henceforth call the brown substance) has a very strong affinity for water; because the fixed alkalis, (unless when boiled upon it,) and their carbonates, all of which easily decompose the salts of copper, cannot dispossess the hydrate of copper of its water.

This led me to imagine that I could perhaps, by fire, dissipate not only the water of crystallization, but that contained in the hydrate; and leave the acid, if a potent one, still in the salt. But I found that the affinity of the alkali, acting upon the acid, had, in the humid way, determined an order of combination not to be effected by heat; for even sulphuric acid was expelled before the water of the hydrate could be completely dissipated. Upon reflecting on the fixity of acids, I could find none so proper for this experiment as the phosphoric. I therefore prepared some artificial phosphate of copper, by precipitating the nitrate of that metal by phosphate of soda. When washed and dried, it was in the form of a fine blueish green powder, among which many crystals were discernible almost to the naked eye.

One hundred parts of this, exposed to a gentle red heat, became of a much paler green, but passed entirely to brown when the temperature was sufficiently elevated. I had then a brown phosphate, not of hydrate, but of oxide of copper, and from which no acid had been volatilized. Its loss of weight was wholly from the water which had been expelled, and

and amounted to 15,5. Its other proportions I found, by further analyſis, to be 35 of phoſphoric acid, and 49,5 of oxide of copper. It is not, however, to be concluded from this, that there are really 15,5 of water of cryſtallization, in blueiſh green phoſphate of copper. We muſt recollect that it is a phoſphate of hydrate of copper, and that 49,5 of oxide demand 12 of water to exiſt in that ſtate: 3,5 therefore are the amount of the water of cryſtallization; and its order of union may, with more propriety, be thus ſtated:

Oxide of copper	49,5	forming hydrate of copper -	61,5
Water -	12		
Phoſphoric acid -		- - - -	35
Water of cryſtallization		- - -	3,5
			100,0

And this is the order which ſhould be adopted in the ſtatement of all analyſes of ſalts of copper.

I could eaſily produce, by the ſame method, a pale green, or a brown arſeniate; and, in nature alſo, the colour of the ore accurately correſponds with the proportion of water, as may be ſeen by comparing together any of the foregoing analyſes.

Having thus convinced myſelf, by analyſis, that copper is found in nature united with arſenic acid in different proportions, I next wiſhed to aſcertain whether art could effect ſimilar combinations. For this purpoſe, I poured into arſeniate of ammonia, a ſolution of nitrate of copper. The metallic arſeniate was immediately precipitated in cryſtalline grains, of a blue colour, rather more intenſe than the phoſphate already mentioned; and the liquor, which remained blue, was decanted. The colour which this latter retained, I imagined, was due to the preſence of a greater quantity of nitrate of copper than was neceſſary to precipitate, from its alkaline baſis, the arſenic acid combined with the copper. After a partial evaporation, I poured in alcohol; and found, to my ſurpriſe, that the conſequence was another precipitation, which was much increaſed by allowing the liquor to remain. Cryſtals ſtill more rich in colour than the former, and very evidently rhomboidal, even to the naked eye, were gradually formed. Imagining there muſt be ſome eſſential cauſe of the greater ſolubility of the one than of the other, I reſolved to examine them apart.

One hundred parts of the firſt of thoſe precipitates, expoſed to a low red heat, loſt 22. Boiled with potaſh, there remained undiſſolved a blackiſh brown powder, which, well waſhed and dried, weighed 50. The ſupernatant liquor, ſaturated with nitric acid, and evaporated, was precipitated by nitrate of lead. Upon filtration, 82 were left, which indicate 27 of arſenic acid. Therefore this arſeniate of copper contained,

Copper - - - -	50
Arſenic acid - - -	27
Water - - - -	22
	99

The ſecond artificial compound, which was mentioned above, was evidently more ſoluble than the latter; and analogy might lead us to ſuſpect, in arguing from the generality of ſalts the baſis of which is ſuperſaturated, that it contained an exceſs of acid. It was analyſed in the ſame manner as the laſt, and afforded,

Oxide of copper - - -	35
Arſenic acid - - - -	39,5
Water - - - - -	24
	98,5

Thus then have we two artificial arſeniates of copper, one of which intimately correſponds with one of thoſe which we have recognized among the productions of nature. The other poſſibly will be found, but we are not yet in poſſeſſion of it; for I ſhall preſently mention the reaſon why No. I., although containing 39 of acid, cannot fairly be eſteemed as ſuch. I have not yet been ſo fortunate as to form the other combinations, but do not doubt that art may one day ſucceed in obtaining them.

REMARKS.

Before I conclude this ſection, which hitherto has had for its object a particular account of certain kinds of copper ore, as well as of their matrices, it may not be ſuperfluous to offer a few remarks, not foreign to the preſent ſubject, upon ſome methods generally uſed in the docimaſtic art. To prove the preſence of different ſubſtances in foſſils, is an object of delicate reſearch: but, to determine proportions with accuracy, is the moſt difficult operation of analytic chemiſtry, and often eludes inveſtigation. It is rather a pleaſing reflection to think that we are in the infancy of chemical exactneſs; and that we may ſee the day of improvement, when the errors which we now commit will require all the aid of ſelf-complacency to be in the leaſt excuſed. And it may be of more real utility to ſtate with frankneſs, although we cannot account for them, thoſe anomalous appearances which ſo frequently occur, than to court the phantom of rigid accuracy, the reality

ality of which we can as yet, but in a few inftances, be fure we have attained. For every obfervation, however trivial, of this kind, will haften the arrival of that moment when we fhall be enabled to approach a little nearer to truth.

I have, for many reafons, preferred boiling the nitrate of copper with either of the fixed alkalis, to the method generally recommended, which is, to precipitate all the copper from its folvent, by carbonate of potafh or of foda; then, to rediffolve in muriatic acid; and to precipitate, in the metallic ftate, by a plate of polifhed iron.

Firft, when an alkaline carbonate is ufed, the precipitate is a carbonate of hydrate of copper; and this fubftance is foluble in an excefs of the precipitant. I once evaporated fome very beautiful blue liquor, obtained in an operation of this kind, and found a cryftallized falt, which I became defirous to examine. But, as the folution contained another falt, formed by the acid (which originally held the copper in folution) and the alkali employed, I found it neceffary to form fome hydrate of copper directly for the purpofe.

Some hydrate of copper was therefore prepared, by decompofing the nitrate of that metal by a very dilute folution of potafh, and well wafhing and drying the precipitate: it was that fine powder formerly well known as the oxide of copper. Some of this fubftance was thrown into a folution of carbonate of potafh, through which a current of carbonic acid had been made to pafs for a long time, and they were then flightly heated together. One part of the hydrate became of the fame colour as the real oxide; the other was diffolved, and the liquor was converted into a greenifh blue folution. Thus, one part of the hydrate had yielded its water to the other, in order to favour this quadruple, or rather this double combination, of carbonate of potafh and hydrate of copper: the liquor, when reduced, afforded a mafs, which, repeatedly rediffolved and evaporated, with difficulty affumed any determinate form of cryftallization. This falt is a carbonate of potafh, holding a little copper. It is of a pale blue, and varies in colour according to the quantity of water of cryftallization, and of metal. It is flightly deliquefcent, and foluble in about three parts of water, at 60°, but requires much lefs water when the water is boiling. It cryftallizes by cooling, much like carbonate of potafh. It is foluble in a large quantity of alcohol. It lofes about 43 per cent. of carbonic acid, by folution in a ftronger acid; and, prepared in the manner I have mentioned, contains no more than 5 per cent. of oxide of copper; although carbonate of potafh, when poured into a folution of any falt of copper, appears

to

to retain a much greater quantity. This, therefore, is a sufficient proof of the inaccuracy of attempting to precipitate copper from its solutions, by an alkaline carbonate; for carbonate of soda has, like carbonate of potash, the property of dissolving and crystallizing, as a triple salt, with hydrate of copper.

In addition to this source of error, we may add the very uncertain operation of one metal upon the saline combination of another, whatever may be their affinity for oxygen. Indeed I have so often remarked this very great uncertainty, that I was pleased to find the observation had occurred to Mr. Proust; and I have constantly found that more dependence can be placed upon the method I have adopted, than upon any attempts to precipitate the metals by each other.

With regard to efficacy and promptness, tin or zinc is preferable to iron; but, with any of the three metals, a phænomenon occurs, for which I have not been able to account in any manner the least satisfactory. As the effects are more striking with zinc than with the other metals, I shall enter into particulars respecting the use of that metal only.

When a bit of zinc (or tin, or iron,) is immersed in a solution of muriate of copper, a precipitation begins, and all the copper is finally separated in a few hours*: but, if a little muriatic acid is added to this solution, and the zinc then immersed, a violent effervescence takes place; hydrogen gas is disengaged, and, in less than two minutes, the copper is so completely precipitated in the metallic state, that neither ammonia, nor even sulphurated hydrogen, can discover any vestige of its presence.

It would appear from this, that hydrogen is, in this case, the real reductive and precipitant of the copper. Yet, how can we reconcile the evident contradiction, that, in one case, zinc with muriatic acid will decompose water, that is, that zinc and muriatic acid will attract oxygen more strongly than hydrogen can, yet that, in the other case, hydrogen, whose affinity for oxygen is weaker than that of zinc and muriatic acid, will be more speedy and powerful than zinc and muriatic acid, in attracting that oxygen from copper? Again, how is it possible that zinc and muriatic acid will, in preference to taking oxygen immediately from the oxide of copper, decompose water, the hydrogen of which will unite with the oxygen of the oxide of copper, again to become water, which it originally was? All this appears to me, I confess, as contradictory as to say, one is less than two, two are less

* If any quantity of neutral salt is present in the solution, the precipitation is much retarded, and is seldom complete.

than

than three, yet three are much lefs than one. This opinion, that hydrogen is really the reductive, is the more extraordinary, as it is not founded on the fingle experiment above mentioned.

If a bit of zinc (or tin, or iron,) is thrown into a folution of oxide of arfenic in water, no change of any kind will be effected, even after a confiderable time: but the inftant that muriatic acid is added, effervefcence and precipitation commence; a few minutes fuffice to obtain all the arfenic in its metallic ftate. It may be objected here, that muriatic acid (as muriatic acid) produces fome hitherto unknown modification in the order of eftablifhed affinities. This objection is not plaufible; and I can adduce fo clear an anfwer to it, as to fhow that it is of very little moment. If aqueous arfenic acid is ufed inftead of the above folution, the fame phænomena of effervefcence and precipitation enfue as when muriatic acid had been ufed in the former cafe; and that precife quantity of metallic arfenic is thrown down, which can yield fufficient oxygen to the zinc to combine with the remaining acid undecompofed. The precipitate which is formed, is a mixture of metallic arfenic with arfeniate of zinc; and thefe may be feparated by muriatic acid, which will diffolve the metallic falt, without acting upon the arfenic.

If, inftead of muriatic acid, fulphuric acid is ufed, the fame phænomena take place, in a lefs degree. But, if the experiment is tried with nitric acid, there is no difengagement of hydrogen gas; and the metals effect a precipitation with much lefs rapidity and certainty than in the former cafe.

It is very true, that hydrogen, in its nafcent ftate, may have properties with which we are yet unacquainted; and may determine combinations, which it can in no other ftate produce. But the decompofition of water, in the firft inftance, in order that a new recompofition may, at that moment, be effected by the fame agents, is in itfelf fufficiently paradoxical.

The facts laft mentioned are fomewhat analogous to, and feem even confirmative of, a theory propofed fome time paft by Mrs. Fulhame; but I fhall withhold my full affent, both to her explanation and to any I could give, until thefe evident contradictions can in fome way be further elucidated.

In the analyfis of every ore in which the metal is combined with fulphur, I have found much variation in the quantity of the latter which may be obtained, even in experiments upon the fame fpecimen. If nitric acid is ufed not fufficiently ftrong, part of the ore remains untouched, and will require fubfequent treatment, always difadvantageous in

delicate

delicate operations. If the acid is too strong, a great part of the sulphur is converted into sulphuric acid; so that, in either case, there is room for error. I thought that, to avoid this, (except in cases where any metal which, with sulphuric acid, forms an insoluble salt, was present,) strong nitric acid might be used, and all the sulphur converted into acid. If potash, soda *, or ammonia, are used as precipitants of the different metals, the quantity of sulphur may be easily ascertained. I took a given weight of sulphur, and converted it into sulphuric acid by means of nitric acid. I then neutralized and evaporated it. Nitrate of barytes, poured in, gave a precipitate which, in one experiment, indicated a proportion of sulphur equal to 14,4, and in another to 14,6, contained in one hundred of sulphate of barytes. A difference so trifling need not be regarded. According to M. Lavoisier, sulphuric acid contains 71 of sulphur, and 29 of oxygen; and, according to the synoptic tables of M. Fourcroy, sulphate of barytes contains 33 per cent. of sulphuric acid: therefore, by this calculation, one hundred of sulphate of barytes contain 23,43 of sulphur, instead of 14,4, or 14,6. I do not pretend to account for so great a difference in these results †; but that very difference led me (by exciting me to doubt those which I had obtained, and inducing me to frequent repetition,) to a more positive conviction of the proportions I have mentioned. M. Lavoisier obtained his proportions by combustion; and, admitting even that nothing was lost, it must have been rather difficult to obtain the sulphuric acid in a state proper to value the quantity. Indeed I do not know of any direct experiments which prove, in a satisfactory manner, that we have ever obtained that acid perfectly free from water; unless when combined with an earth, or an alkali, in some salt, and that salt calcined in a very strong red heat.

To ascertain the quantity of metallic arsenic in mispickel, arsenical pyrites, &c. the most advantageous method is, to acidify it by nitric acid, and then to combine it with oxide of lead. This arseniate of lead (containing, as was before said, 33,2 per cent. of acid,) may be estimated to contain 22 per cent. of metallic arsenic. If both sulphur and arsenic are present, lead may be equally serviceable, after both have been acidified; for sulphate of lead is not materially soluble in any acid; whereas, on the contrary, arseniate of lead is very much so.

* See note in page 144.

† I was particularly cautious in ascertaining that, during that experiment, no sulphureous acid had been produced, the formation of which would have easily accounted for any difference.

When

When copper and iron are to be feparated, one fingle affufion of ammonia will not always fuffice. That two may be fometimes neceffary, is an objection to the method I propofe for the fubfequent ebullition with potafh. But, when I ufe that of precipitating the copper by iron, it requires no previous precipitation by any alkali. It is fufficient to add muriatic acid to the original folution of the ore in nitric acid, and evaporate to drynefs. The nitric acid is diflodged from the oxide of copper, and the muriatic takes its place. If a fingle evaporation is not fufficient, a fecond (for the operation is very fhort, and caufes no lofs upon filters, &c.) may be attempted; and when the iron ufed for the purpofe of revivifying the copper is put in, the liquor may be made to boil; by which means the procefs is rendered much more certain and expeditious. Some iron will neceffarily be diffolved, and the quantity muft be noted. The liquor, which contained muriate of copper and of iron, now contains only the latter. Boiled with a little nitric acid, it will become red; and then ammonia, or potafh, will give a red precipitate, which, well wafhed and dried, will reprefent 61 per cent. of metallic iron. All thefe metals having thus been precipitated, no conftituent part of the ore, except the fulphur, which, in the firft treatment, had been converted into fulphuric acid, is contained in the liquor; and from it, when neutralized, this latter may be precipitated by nitrate of barytes, which will reprefent 14,6 per cent. of fulphur. The abfolute neceffity of conftantly ufing pure alkalis, in this method of analyfis, is too evident to be infifted upon.

GENERAL VIEW OF THE FOREGOING ANALYSES.

In taking a retrofpective furvey of the experiments above related, upon the various natural arfeniates of copper which we have examined, we fhall find,

Firft, That natural arfeniate of copper exifts in three different ftates of combination; the firft containing 14 per cent., the fecond 21 per cent., and the third about 29 per cent. of acid.

Secondly, That each of thefe may contain different proportions of water, either as conftituting a hydrate, or as water of cryftallization.

Thirdly, That, upon lofing its water, arfeniate of copper will pafs from blue to pale green, and finally to brown, as in No. I.

Fourthly, That No. I. is the only real arfeniate of copper, all the others being arfeniates of hydrate of copper.

Fifthly, That No. I. is not to be admitted as an arfeniate of of copper containing 39,7 per cent. of acid. For, if we put it on the ſame footing with the others, in admitting a due proportion of water into its compoſition, we ſhall, by calculation, reduce it to that claſs containing 29 per cent.

Sixthly, That in beginning with that kind which contains the leaſt quantity of acid, and riſing progreſſively to that which contains the greateſt, we ſhall find the order to be thus:

No. VI. contains	-	14 per cent.
No. V. - -	-	21 per cent.
Nos. I. III. and IV.	-	29 per cent.

No. II. ſeems to be a particular ſpecies. It conſiſts of a much greater proportion of oxide, with a leſs quantity of water, (and this its external colour announces,) combined with nearly the ſame proportion of arſenic acid. Indeed, if certain characters did not ſpeak ſo ſtrongly in favour of this diviſion, I ſhould not have heſitated to claſs it with the laſt-mentioned kinds. But it is found in many ſtates; which ſeems to indicate, that the water is by no means in the ſame degree of intimate combination that it is in the others; and this alone may ſerve to diſtinguiſh it to the eye of the mineralogiſt.

If, to the above natural arſeniates, is added the ſecond artificial arſeniate, we ſhall have another proportion of acid, at the rate of 40 per cent. Here then we have two ſimple ſubſtances combined in four different proportions, and producing ſeven diſtinct combinations.

But, what is not the leaſt to be admired, is the wonderful accordance in the order which two ſciences, operating with very different inſtruments, have allotted to the ſame ſubſtances. By that, not only the ſagacity of Nature becomes very ſtriking; but, from the acknowledged accuracy of one method of inveſtigation, the reliance to be placed upon the other is rendered more conſpicuous; and each receives additional ſtrength and confirmation. Chemiſtry has long been in the habit of aiding the ſcience of mineralogy, of which it laid the foundation; but it was not till lately that cryſtallography could form a judgment of its own, much leſs confirm the truth of the ſource from which it ſprung.

Section II.

Arſeniates of Iron.

The arſeniates of iron remain now to be examined. Included, formerly, among arſeniates of copper, they have been ſeparated from them upon the authority of chemical analyſis. For, although to recogniſe, by external character and form in all their modifications, ſubſtances already known, is particularly

ticularly the province of cryſtallography; yet he who would expect that it ſhould declare the nature of thoſe ſubſtances which it beholds for the firſt time, would exact more than it ever has promiſed, or ever could perform. Among foſſils, it may claſs, and find new ſpecies; but chemical analyſis is the baſis of all arrangement among metallic ores. In them, to ſeparate, is the taſk of the one; to aſſign a place, is the buſineſs of the other.

Cupreous Arſeniate of Iron *.

One hundred parts of this arſeniate, expoſed to a low red heat, loſt 12, which were pure water. Nitric acid was poured upon the reſiduum; and, finding that it was diſſolved with difficulty, the ebullition was continued during ſeveral hours. The liquor was then filtered. Sixty parts, which ſhall preſently be examined, remained undiſſolved. Into this filtered liquor, nitrate of lead was poured, which occaſioned a precipitate, as uſual; but the operation was diſcontinued until I ſhould obtain all the arſeniate of copper which I imagined to be contained in the ore. For this purpoſe, I had recourſe to the 60 parts mentioned above. They were in the form of a greeniſh gray powder, very hard and gritty, which had every appearance of ſilica, contaminated by a ſmall portion of copper interpoſed between the molecules of that earth. I reſolved to treat it in the ſame manner as all ſiliceous ſtones, and proceeded to boil it with potaſh.

In leſs than three minutes it became of a very red brown, from the greeniſh gray which it originally was; and ſeemed conſiderably attenuated in its particles. The liquor was decanted, and examined. It was found to contain arſenic acid; and the precipitate, which had reſiſted the action of the potaſh, was proved to be a mixture of iron and copper.

Theſe preliminary experiments were ſufficient to indicate a ready method of analyſis. 100 parts, boiled with potaſh, immediately became of a deep reddiſh brown. The liquor was ſeparated from the reſiduum by filtration; and, after the uſual neutralization, evaporation, and affuſion of nitrate of lead, (all of which operations were detailed in the firſt part of this paper,) gave a precipitate correſponding to 35,5 of

* This ſpecies had been mentioned by Mr. Prouſt, but in a manner which, as it was a new ſubſtance, and demanded particular attention, does not give all the ſatisfaction which that chemiſt generally affords. No doubt the ſcarcity of the ore prevented his making every neceſſary reſearch; and I may deem myſelf fortunate in having been ſo near the ſpot in which it is found. My friend Mr. Hatchett very obligingly gave me a ſpecimen of this ore, which he had received from Dr. Pallas, who had brought it with him from Siberia, where it had been found.

arſenic

arſenic acid. The firſt reſiduum weighed 53. Diſſolved, as far as they could be, in muriatic acid, there remained 3, which, upon examination, were found to be really ſilica. Ammonia, poured in exceſs into the muriatic ſolution, rediſſolved 22,5, which were copper; and 27,5 of iron remained behind. The proportions were,

Silica - - -	3
Arſenic acid - -	33,5
Oxide of iron - -	27,5
Oxide of copper - -	22,5
Water - - -	12
	98,5

None of theſe experiments were ſufficient to determine, whether this ore is in the ſtate of a triple ſalt, or merely a mixture of two arſeniates. As, in a ternary combination, the proportion of acid might vary, it cannot be juſtly called in to aid us in our inquiry. The ſolubility of one part of the ore being much greater than that of the other, and in different quantities of each ſalt, incline more to the opinion that it is but a mixture.

Simple Arſeniate of Iron.

This arſeniate, expoſed to any degree of heat, gave but an unſatisfactory reſult, with regard to the quantity of water. The arſenic acid is volatilized from this ore with peculiar facility, for which I ſhall attempt hereafter to account.

Some ſubſequent experiments, however, have induced me to fix the quantity of water at about 10,5.

One hundred parts, boiled with potaſh, left 58,5. The liquor, treated, as uſual, by nitrate of lead, gave 31 of arſenic acid. The 58,5 left four, which muriatic acid could not diſſolve, and which were ſilica. Ammonia diſſolved 9; and there remained 45,5 of iron. This analyſis preſents the following reſult:

Silica - - - -	4
Arſenic acid - - -	31
Oxide of iron - -	45,5
Oxide of copper - -	9
Which will leave for water	10,5
	100,0

This ore appears to be a pure arſeniate, mixed accidentally with a little copper; as ſome of the copper arſeniates caſually give traces of iron. This is the kind mentioned by Mr. Klaproth as an arſeniate of copper, and the firſt known under

under that denomination. Heating it on charcoal, before the blowpipe, he perceived a ſmell of arſenic, and, at length, obtained a metallic button, which was found to be copper. That there is copper in this ore, is evident from analyſis. But the mere arſenical ſmell was not a ſufficient ground to aſſert that it contained arſenic acid; for this metal might, with as much probability, have been in any other ſtate. If, indeed, that very accurate and able analyſt had, upon trying the ore with the blowpipe in a platina ſpoon, perceived no fumes, he might then have concluded that the arſenic muſt be in the ſtate of acid, and that charcoal was neceſſary to operate a partial reduction, to which the arſenic owed its volatility and its ſmell. But no ſuch experiment is reported.

It is alſo rather extraordinary that Mr. Gmelin ſhould have taken this ore out of the claſs of arſenical ores, and left it as an unknown ſpecies of copper; when, in fact, it is an arſenical ore, but not an arſenical ore of copper.

I examined ſome cryſtals which are commonly attached to the ſpecimens of this ore. They were thoſe which, according to M. de Bournon, are in a ſtate of decompoſition. By this ſpontaneous decay, they become of a deep browniſh red, not unlike the ſubſtance called colcothar; but they ſtill retain their cubic form. They contain a little acid and water, owing, perhaps, to their having eſcaped from total decompoſition. The ſame theory that accounts for the difficulty of aſcertaining the quantity of water will account for the red colour they thus aſſume.

When green ſulphate of iron is expoſed in a crucible to a red heat, it is well known that ſulphureous acid is diſengaged in great quantities; and that, if the operation is continued long enough, there remains a red powder. In this caſe, the green oxide of iron has taken up oxygen from the acid; and this latter has been partly decompoſed, and almoſt totally volatilized. Now, in the ſpecies here ſpoken of, the iron, as in the green ſulphate, is in the ſtate of green oxide; therefore capable of receiving an additional portion of oxygen. But arſenic acid will, at a high temperature, loſe a part of its oxygen, and, retrograding to the ſtate of white oxide, will be volatilized; and ſtill more eaſily will thoſe changes take place, when oxide of iron, ready to receive, and arſenic acid, ready to yield oxygen, are in contact. A leſs degree of heat, therefore, will ſuffice to drive off this acid, from green arſeniate of iron, than from arſeniate of copper. But we muſt not from this conclude, that the affinity of the latter metal for arſenic acid is ſuperior to that of the former; for, the attraction of green oxide of iron for oxygen, and of caloric for

white

white oxide of arsenic, determines a new order of divelling affinities.

But most of the mineral acids that have been tried have been found capable of uniting with iron in two states; in the state of green oxide, and in that of red oxide, the residuary powder above mentioned. I was desirous to know whether I could, in any manner, imitate, by art, the last natural products I have examined, as I had already imitated, in some degree, an arseniate of copper. For this purpose, I decomposed green sulphate and red sulphate of iron, by arseniate of ammonia; and, having well washed and dried the precipitates, proceeded to examine them.

The green arseniate was acted upon by heat, in the same manner as the natural one, and exhibited the same appearances. By the usual methods, I found its proportions to be,

Oxide of iron - -	43
Arsenic acid - -	38
Water - -	19
	100

This is not the same proportion of acid that is contained in the natural arseniate; however, I state them both as I found them. The other artificial arseniate, which is of a pale greenish red, afforded,

Red oxide of iron -	36,5
Arsenic acid - -	41,5
Water - - -	20,0
	98,0

These salts agree with the generality of the known salts of iron; all of which contain a greater quantity of oxide, as the oxide itself contains less oxygen.

By boiling with nitric acid, it was easy to convert the green arseniate of iron into the red; and such is the case with all the salts of green oxide of iron. As, during the course of these experiments, I had occasion to make some remarks upon the divers habitudes of this metal, which, as far as I know, have not all been observed, I shall terminate the whole of these analyses (as I have already done that part of them which treats of arseniate of copper in all its known varieties) by simply stating what has occurred to me.

I happened to boil some muriatic acid upon a greater quantity of iron than the acid could dissolve. I found a perfectly limpid and colourless liquor remain, which, nevertheless, was a solution of muriate of iron. This colourless liquor being decomposed by arseniate of ammonia, the precipitate was

was of a greenifh white, and foluble in a great quantity of water; but, paffing to a much deeper green, quickly fell to the bottom. A pruffiate or a gallate poured into the faid folution, occafioned no change till it had ftood a confiderable time expofed to the contact of the air. By potafh, and by foda, a white precipitate was thrown down, which quickly affumed a green tinge; and that tinge increafed fo much as to become a very deep grafs green in a few minutes. Ammonia occafioned a white precipitate, which was rediffolved by an excefs of the precipitant. The ammoniacal folution affumed the fame greenifh tinge, and fpeedily depofited an oxide of iron, which was firft of a deep green, but inftantaneoufly became black, with fome yellowifh ochrey particles on the furface. If, however, thefe precipitations were effected in a bottle well ftopped, and defended from the contact of the atmofphere, no change of colour took place; and that portion which was diffolved by an excefs of ammonia remained in the folution. In endeavouring to diftil, or to evaporate, the water of this colourlefs folution, in order to obtain cryftals, it became of a light green, the intenfity of which augmented, as the diftillation was continued. I could not, therefore, hope to procure this falt in a cryftalline form. Hence, it is evident that we have a white muriate of iron, which, as well as the oxide it contains, is very fufceptible of affuming an addition of oxygen; for to that alone I attribute the precipitation caufed in a folution of that falt, into which the different reagents above mentioned had been poured; a precipitation which did not take place till after it had been expofed in a fituation where it could abforb the quantity of oxygen neceffary to produce a change in its principles.

This folution of white muriate of iron, when expofed to the air, becomes green, and is then in the ftate of green muriate, well known. At a certain degree of oxidation, I have obferved the precipitate formed, to be foluble in the carbonates of potafh and of foda, and ftill more fo in that of ammonia; but, upon abforption of oxygen, to be quickly abandoned by them, and then to fall to the bottom, in a blackifh powder mixed with yellow. If into a folution of green muriate of iron nitric acid is poured, both liquors being cool, and not too concentrate, the muriate will become of a blackifh brown, not unlike malate of iron. Precipitated by the alkalis, it yields a black powder, no longer foluble in them, but which refembles, in every refpect, common black oxide of iron.

If this green muriate of iron is further expofed to the air,

 the

the liquor becomes red, but ſtill gives a blackiſh green precipitate; but, if it is boiled with nitric acid, it then is converted into a red liquor, which yields a red precipitate, by all the alkalis and earths capable of cauſing a precipitation. From theſe obſervations, upon the different combinations of iron with oxygen, and of oxide of iron with muriatic acid, ſome concluſions may be drawn intereſting to mineralogy as well as to chemiſtry. The variety of colour in many ſtones in which iron has been found, is a fact which, although we cannot deny our aſſent to direct experiments, has never been accounted for in a ſatisfactory manner. In white, green, yellow, black, red; in a word, in foſſils of every colour, iron, with ſometimes the help of mangaueſe, and lately of chromium, has been regarded as the colouring matter of every ſhade; but it ſeems almoſt paradoxical, that the ſame ſubſtance ſhould aſſume and communicate ſo many tints. In mica, kaolin, amianthus, aſbeſtos, rock cryſtal, and all white ſtones, I believe it to exiſt as the white oxide; and that ſtate is its firſt degree of combination with oxygen. In them, this metal is not very abundant; for, if it were, I have ſome reaſon (as ſhall be proved immediately) to think that they would obey the loadſtone ſtrongly. In lapis ollaris, ſerpentines, and many green ſtones, we have the green oxide; and moſt of theſe are magnetic; nay, as Mr. Humboldt has obſerved, ſerpentines enjoy the property of polarity. And thence I conclude, that the rarity of this metal, in the firſt claſs, prevents them from participating that quality. This I believe to be its ſecond ſtage of combination with oxygen. In the ſtate of black oxide it is frequently found, and is too well known to need further comment. I believe this to be its third ſtage of oxidation. But there is a red liquid muriate, which gives a very dark greeniſh precipitate, approaching to dark brown. What the ſtate of that precipitate is, I have not yet determined; but I imagine it to be a mixture of black and red. Brown and yellow oxides, I am confident, are mixtures of ſimple oxides, and neither of them is an oxide *ſui generis*. The red oxide is the extreme of oxidation, and affords many beautiful colours in nature and in art.

It is much to be deſired, for the advancement of analytic chemiſtry, that experiments upon the proportions of oxygen with which metals are capable of uniting, under different circumſtances, and upon the combination of thoſe oxides with all the known acids, together with many others of their properties, would attract the notice, and engage the labours, of accurate manipulators. Experiments of this kind have been deſpiſed, from an idea of their reſembling a mere mechanical employment;

employment; but, so far is that from the truth, they may justly be considered among the most difficult problems of chemistry; and it is only from the rigid and constantly similar results of such experiments, that we can hope to attain an intimate knowledge of the principles with which nature has originally operated.

[To be continued.]

XXXIX. *Comparison of the Definitive Metre with a Standard of the English Measures, carried from London to Paris by* M. A. PICTET, *Professor of Philosophy at Geneva* *.

AS the measurement of the earth, and researches respecting the figure of it, had, at various times, and in different countries, during the course of the 18th century, been an object of the labours of the most distinguished philosophers †, when an idea was lately conceived in France of deducing from the dimensions of our globe a standard of unity, to which every thing susceptible of measurement or weight could be referred, it was necessary to make an effort proportioned to the importance of an enterprise become a national concern. In the midst, therefore, of a long and destructive war, and amidst a thousand difficulties of every kind, a series of triangles was established between Dunkirk and Barcelona, comprehending the tenth part of an arc of the meridian extending from the equator to the pole, that is to say, the fourth part of the whole circumference of the globe; and the ten millionth part of this arc so determined was adopted as representing the unity of the whole metric system. This plan was executed with standards composed of substances capable of resisting the influence of the weather; and by then establishing, as was carefully done, the exact ratio of the length of the metre to that of a common pendulum which swings seconds, on the borders of the sea, in a given latitude, the determination of this unity was rendered independent of any catastrophe that might alter or destroy its types, in the construction of which all those resources presented by philosophy and the arts, now brought to a very high degree of perfection, were employed.

On the other hand, geodesian operations, designed to fur-

* From *Bibliothèque Britannique*, by professor Pictet, No 148

† Swedish astronomers are now employed in again measuring a degree of the meridian, which was measured in Lapland under the polar circle by the French academicians above sixty years ago.

nish materials for a new map of Great Britain, were undertaken, about 25 years ago, in England, and have been carried on since that time. These labours, begun by the late general Roy, have been conducted with a great deal of sagacity and precision, and the results of them may afford interesting data in regard to the figure of the earth. A distinguished member of the Royal Society of London, Sir George Schuckburgh, has employed himself, with success, in researches calculated to establish the precise length of the standards, which have served as a basis to all the geodesian measurements made in England.

It was therefore to be regretted that labours so analogous in their nature, carried on in two neighbouring countries, and susceptible of acquiring, by comparison, a new degree of interest, should remain unconnected for want of a real standard of the measures of one of these countries, viz. England. We experienced this regret in a very lively manner at different periods; and we can with truth assert, that if the hope of procuring this mean of comparison was not the only motive of the journey undertaken by one of us to England, it was a strong inducement towards it.

Our colleague, professor Pictet, when he arrived at Paris, took some steps to obtain an authentic metre, in order that he might submit it to examination before the Royal Society of London, of which he is a member; but, in consequence of the shortness of his stay in Paris, these steps were not attended with the desired effect. He, however, took advantage of a longer residence in England to procure from Troughton, an artist celebrated for his accuracy in constructing and dividing geometrical and astronomical instruments, a standard exactly similar to one he had constructed for Sir George Schuckburgh, and to which that philosopher referred the principal English measures in the memoir above mentioned. Our colleague also caused to be constructed by the same artist the comparative apparatus of Sir George Shuckburgh, consisting of two excellent microscopes, one of which is furnished with a micrometer that divides the English inch into ten thousand equal parts. On his return to Paris he took the earliest opportunity of laying these articles before the Minister of the Interior and the National Institute. This learned body named three of its members to proceed to a regular comparison of the definitive metre with the standard just mentioned. This labour, of a simpler nature than it might at first appear, employed the commissioners during five sittings of four hours each, and was conducted with all the care and attention that the nature of the object required.

C. Prony,

C. Prony, to whom these researches were particularly interesting, as it is to him we are indebted for a French translation of general Roy's memoir on the first trigonometrical measurements made in England, noted down the results; and it was in the cabinet of that learned man, and by the assistance of a comparative apparatus belonging to him, that the principal operations were made. He was so kind as to transmit to us an authentic copy of the report given in to the Institute; a production thought to be so generally interesting, as to be read in the public sitting of the last quarter. He adds, "that this report will be soon followed by a memoir, in which he will give more circumstantial details respecting the observations he made; together with a description and drawing of his comparative apparatus." In the mean time we shall lay before our readers the report, assuring them that we have taken every possible care, in revising the proofs, to avoid typographical errors.

NATIONAL INSTITUTE.

Extract from the Registers of the Class; Sitting of the 6th Nivose, Year 10 (Dec. 27, 1801.)

A member, in name of a commission, read the following report on a comparison of the standard metre of the Institute with the English foot:

"C. Pictet, professor of philosophy at Geneva, submitted to the class, in the month of Vendemiaire last, an interesting collection of objects relating to the sciences and the arts, which he procured during his travels in England.

"Among these objects was a standard of the English measures on a brass ruler, strong and well polished, about 49 English inches in length, 36 of which make the English yard, and divided along its whole length, by lines exceedingly fine, into tenths of an inch.

"This standard was constructed, at the request of C. Pictet, by Troughton, an artist of London, who has the well-earned reputation of dividing instruments with great accuracy: it was compared at London with another standard made by the same artist for Sir George Schuckburgh, and it was found that the difference between these two standards was no more than the difference between each of the divisions, that is to say, quantities absolutely insensible.

"The standard of C. Pictet, then, ought to be considered as identical with that of Sir George Schuckburgh: a detail of the experiments which ascertain the exactness of the latter, may be seen in the Philosophical Transactions for 1798.

"C. Pictet had laid before the Institute, together with

the

the above rule, divided into tenths of an inch, a comparer or inſtrument proper for eſtimating the ſmall differences between the meaſures, conſtructed by the ſame artiſt, and of which the following is a ſhort deſcription:

"The comparer is compoſed of two microſcopes with threads, which are placed in a vertical ſituation. The faces of the rule to be examined being horizontal, and at a convenient diſtance from each other, by making them move along a metal rod with diviſions, one of the microſcopes remains fixed near one of the extreme lengths of the point to be compared, and ſerves to aſſure the immobility of that point; the ſecond microſcope is towards the other extremity of the ſame length, and fixed alſo; but the frame which bears theſe threads may be moved by means of a micrometer ſcrew the threads of which are equal to [illegible]th of an inch Engliſh; and the plate paſſed over by the index being divided into another 100 parts, gives [illegible]th of an inch Engliſh for every diviſion of the plate. By means of this arrangement, if the microſcopes are firſt placed in ſuch a manner that the interſections of their threads correſpond to the extremities of any length intended to be compared with another which differs from it by a quantity leſs than 1 10th of an inch, the difference of the two lengths, by employing the micrometer, may be eſtimated in 10.000th parts of an inch.

"The threads placed in the focus are diſpoſed obliquely in regard to the lines of diviſion, ſo that a correſpondence is known to have taken place when a line of diviſion divides into two equal parts the acute angle formed by the two threads.

"We ſhall here obſerve, that, in the year 1785, Ramſden employed, in the like manner, two microſcopes adapted to an inſtrument he had invented to meaſure the dilatation of metals. General Roy gave a deſcription of this inſtrument in the 75th volume of the Philoſophical Tranſactions.

"C. Pictet offered to entruſt the claſs with the Engliſh ſtandard and the comparer above deſcribed, to enable them to determine the ratio of the metre to the Engliſh foot: the claſs accepted this offer with gratitude, and charged C. Legendre, Mechain, and myſelf, (Prony,) to join with C. Pictet in order to determine the ratio between the ſtandard of the platina metre and the Engliſh foot.

"The following is a ſhort account of the reſult of the labour undertaken on this ſubject at the houſe of Lenoir for a firſt compariſon:

"The 49 inches of the Engliſh rule terminate at diviſions traced out on one of the faces of that rule; and the ſtandards

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of platina and iron of the Institute have no division, the length of the metre being given by the distance between their extremities. This circumstance prevented, on the one hand, the length of a metre from being taken immediately on the standards with the microscopes; and, on the other, did not permit the immediate estimation of the metre of the Institute in English inches, by the process employed for fixing the standard of the new measures; a process which consists in resting the extremity of the metal rule subjected to examination against the knee of a fixed gauge, and applying to the other extremity a sliding knee, constructed for estimating the difference between the measures, the ratio of which is to be determined, or the identity of which is to be ascertained.

"The artist Lenoir tried to surmount these obstacles by employing a copper rule, a metre in length, cut at its extremities into a very sharp bevel, in such a manner, that this rule could be compared with the standard of the Institute according to the usual method of extreme contact, and which being placed above the English rule, the edges of the bevel produced on the surface of that rule the effect of divisions parallel to those delineated upon it. By these means, the microscopes could be used for estimating in inches, and ten thousandth parts of an inch, the distance between these edges.

"By this method, the standard metre of platina, and another metre of iron, both belonging to the Institute, were compared with the English foot; the two former measures being constructed in such a manner, that at the temperature of melting ice they are equal to each other and to the ten millionth part of a quarter of the meridian. It was found that at the temperature of 15·3 degrees of the centigrade thermometer the platina metre was equal to 39·3775, and the iron metre to 39·3788 English inches, measured on the rule of C. Pictet.

"But these first observations made known to the commissioners that the process they employed might leave some uncertainty, in consequence of the great difficulty of placing the intersection of the threads at the exact extremity of the bevel of the rule, which served as a term of comparison. A reflection or irradiation of light which took place at that extremity, prevented our clearly distinguishing whether the optic axis of the microscope was exactly tangent to the small surface which terminated that bevel.

"To remedy this inconvenience, one of the commissioners*

* This method, as simple as ingenious, was proposed by C. Prony; and C. Paul of Geneva, who was accidentally present, carried it into execution; it was attended with complete success.

proposed

proposed the following method, which was adopted:—It consisted in tracing out, on a small slip or rule of metal, of the same thickness as the English rule, a very delicate line perpendicular to the length of the rule. This piece of metal was made to rest against a fixed knee, and the microscope with fixed threads was brought to the line above mentioned: the piece of metal containing that line was then removed, and the metre to be compared was put in its room, with one of its extremities resting against the fixed knee, and the piece of metal placed at the other. It is here evident that the line traced out on the piece of metal was in this new position at the exact distance of a metre from its former position when resting against the fixed knee; and, by making the threads of the second microscope correspond with this line, the distance between the points of intersection of the two threads was exactly a metre. To estimate the metre in English inches, nothing then was necessary but to put into its room the divided rule, to place a line of one of its divisions under that of the intersection of the threads which was fixed, and to estimate at the other extremity, by means of the micrometer, the fraction of division, which with the whole number of these divisions gave the length of the metre.

" The comparison was repeated on the 4th of Brumaire at the house of one of the commissioners by the method here described; and after several observations, which corresponded in a very satisfactory manner, it was found that, at the temperature of 12·75 degrees of the centigrade thermometer, the platina standard of the metre was 39·3781, and the iron standard 39·3795 English inches.

" The two metres having been constructed to be equal at the temperature of freezing, the operation from which the preceding results were obtained may be verified by finding what the ratios would be at that temperature. For this determination we have the correct experiments of Borda, and the commission of weights and measures on the dilatation of platina, copper, and iron, from which it results, that for one degree of the centigrade thermometer, platina dilates 0·00000856, iron 0·00001156, and copper 0·00001783; and from these data it is found that, at the temperature of freezing, the platina metre of the Institute is equal to 39·38280, and the iron metre to 39·38265 English inches, measured on the standard of C. Pictet.

" The difference 0·00015 between these two lengths, less than $\frac{1}{300}$th of a line, or $\frac{1}{270000}$th of a metre, may be absolutely neglected. The result of our labour therefore is, that, supposing the platina and iron standards of the metre belong-

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ing to the Inſtitute, and the Engliſh rule of C. Pictet, at the temperature of freezing, the metres, which at that temperature are equal to each other and to the ten millionth part of the quarter of the meridian, are equal to 39·38272 inches meaſured on the Engliſh rule of C. Pictet.

"Done at the Mathematical and Phyſical Claſs of the National Inſtitute, Nivoſe 6, (year 10).

"(Signed) LEGENDRE, MECHAIN; and PRONY, Reporter.

"The Claſs approves the report, and adopts the concluſions.

"Certified agreeably to the original.

"Paris, Nivoſe 26, year 10."

XL. *Letter from Count* MOROZZO *to* C. LACEPEDE, *reſpecting a Parrot hatched at Rome; together with ſome Obſervations on the duration of the Life of theſe Birds* *.

NO one can know better than you do, that it is impoſſible to have the natural hiſtory of birds ſo complete as that of quadrupeds. To render it more perfect, we muſt collect, as you have done, all the new facts and correct obſervations made ſince the immortal Buffon publiſhed his work, in order that we may form a new one ſtill more complete. That I may contribute towards this branch of natural hiſtory, permit me to ſend you an account of an intereſting fact, viz. the hiſtory of two parrots of the family of the Amazons, which laid eggs ſeveral times, and which have reared a young one, brought forth at Rome in the courſe of the preſent year 1801.

1ſt, I ſhall firſt give you a hiſtory of theſe parrots, with an account of their laying and incubation.

2d, Of their habits and attachment, and in what manner they reared the young one.

3d, I ſhall examine the ſpecies to which they belong: I ſhall give a deſcription of the male, the female, and the young parrot, and compare them with thoſe given by ornithologiſts.

4th, I ſhall relate ſome very rare caſes of parrots hatched in our climates, and the difficulties that oppoſe this circumſtance.

5th, In the laſt place, I ſhall offer ſome conjectures on the duration of the life of parrots compared with that of other birds, which will give me an opportunity of obſerving that

* From the *Journal de Phyſique*, Ventoſe, an. 10.

there

there is a conſtant relation between the time employed in incubation and the duration of their life.

I. *Hiſtory of the Parrots, their Laying and Incubation.*

The parrots of which I am going to give the hiſtory, belong to M. Paſſeri, a Roman.

In the firſt place, I muſt expreſs my gratitude to the owner for having permitted me to examine and obſerve them very cloſely, and for having communicated to me the obſervations which either he or his mother had an opportunity of making, for ſixteen years, on the manners and habits of theſe birds, which enables me to give a correct hiſtory of them.

M. Paſſeri obtained at Marſeilles, in 1786, a female parrot of the family of the Amazons. Some months after, he got, at Avignon, a male under the name of the Amazon parrot of Brazil. He put them together, being both tame, and ſuffered them to go about the houſe at their pleaſure. They never had a ſmall chain on their legs, nor any other badge of ſlavery. They often remained on the ſtick placed for them, and ſometimes they retired in the night to a large iron cage which was not ſhut; and in every other place where they have ſince been, they have always enjoyed the fulleſt liberty. From the moment they ſaw each other, they conceived a ſtrong mutual attachment, the cauſe of which, no doubt, was their difference of ſex; and their friendſhip ſtill continues to ſuch a degree, that if ſeparated for a few moments they are in the greateſt agitation, emit loud cries, and their uneaſineſs continues till they are again brought together. When they came into the poſſeſſion of M. Paſſeri they had attained to their full growth, but he had no knowledge of their age.

The male pronounced diſtinctly ſeveral French words, as it does ſtill; but the female emits only loud cries, and chatters very much, without articulating a ſingle word. They have travelled a great deal with their maſter, being firſt carried to Rome, thence to Forti, Valentano, and Nagni, and again back to Rome: during theſe journeys they were ſhut up ſeparately in ſmall wooden boxes called *ſabots* *.

The female laid eggs ſeveral times. The firſt time this circumſtance was obſerved was at Forti, about five years ago. She produced two eggs, which ſhe depoſited in a hole near the kitchen chimney; but the inceſſant noiſe of the people going backwards and forwards, prevented her from continuing to hatch them. Beſides, unforeſeen circumſtances obliged the owner to change his reſidence.

* This method of carrying birds is very convenient; for, as they cannot turn themſelves, they do not ſpoil their plumage.

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The ſecond time was at Valentano, where ſhe produced two eggs, which ſhe depoſited in a corner of the room, without preparing any neſt. Having ſat on them ſome days, it was then concluded that it would be better to place them under a pigeon; but, though the pigeon ſat on them for a conſiderable time, they were not hatched.

The female laid for the third time laſt year (1800). She produced two eggs about the middle of May, and depoſited them on the ground; but ſome days after they were found broken. Whether it was done by the male that he might enjoy the female again, or whether it happened by ſome other cauſe, is not known. In the beginning of June, however, ſhe again laid two eggs *; but ſhe depoſited them this time in an earthen veſſel which, the handle of it being broken, was employed as a chaffing-diſh: it was half filled with aſhes, and ſtood behind a door which concealed the female while ſitting.

The female ſat for forty days, and on the 25th of July one of the eggs was hatched; but the young one died next morning.

As M. Paſſeri was deſirous to confirm the fact of a parrot being hatched at Rome, he carried it to the hoſpital of San-Spirito; but putrefaction being too far advanced, it was thrown away. It was ſeen, however, by ſeveral of the young ſurgeons at that inſtitution.

The fourth, or rather the fifth time, that the female laid, (for ſhe had laid once beſides the three times above mentioned,) was in the preſent year 1801. She depoſited three eggs in the ſame veſſel filled with aſhes, which ſtood in the ſame place as before. The incubation laſted forty days, and on the 24th of June a young one was hatched: ſome days after, the other eggs were thrown away as unfruitful. This young parrot is at preſent in good health, and as large as a pigeon.

It remained almoſt naked for the firſt fortnight, being covered only with ſoft down: the ſmall gray tubes of the quills then began to appear, and from theſe iſſued, like a ſmall bruſh, the green feathers, which grew progreſſively to ſuch a degree, that on the 20th of Auguſt, that is to ſay, at about the end of two months, it was completely clothed.

On the 12th of July, that is, on the 18th day, the young parrot began to emit cries for the firſt time.

On the 14th, that is, on the 20th day, it began to open its eyes.

* It is very common for birds to lay a ſecond time, when their eggs have been broken or carried away.

On

On the 20th of Auguft, when it was well covered with feathers, the mother, which had conftantly flept in the neft with its young, abandoned it, and flept by the male as ufual.

On the 25th of Auguft, the young parrot flept out of the neft.

II. *Habits of thefe Birds—their Attachment—Method of rearing their Young.*

It has not been obferved that thefe parrots fpeak more in fummer than in winter. They did not feem to fuffer any inconvenience from the heat: yet they wafh themfelves in fummer, and, when the fun is ardent, place themfelves in the fhade: on the other hand, they feek for the fun in winter. They feem much affected by a change of weather; and during ftorms they are under great agitation, and fend forth loud cries.

No particular time of their moulting has been remarked: it takes place gradually in the courfe of the year.

Thefe birds copulate only once a year, in the fpring: it was always in the latter end of March and beginning of April that they fhowed figns of it. At this time, the male redoubles his careffes to the female, which returns them by marks of the greateft fenfibility. They kifs with their bills, fcratch each other, and ftroke, one by one, the fmall feathers of the head. Sometimes the male, preffing his throat, by a fort of rumination throws up fome morfels he has fwallowed, and prefents them with his bill to his companion, which eats them with great avidity.

After thefe preludes, they copulate like pigeons. They copulate feveral times a day: they have been feen to copulate even three times in a morning, and it is to be prefumed that they did fo feveral times more in the courfe of the day; which proves that they are exceedingly warm.

According to our obfervations they conftruct no neft. They collected no feathers, bits of ftraw, or any other light bodies within their reach. The female depofited her eggs in the fame veffel, without carrying any thing to it.

The eggs are as large as thofe of a pigeon, and are entirely white. As fome authors have obferved that the eggs of the Amazon parrot are fpotted with gray, while thofe of the parrot in queftion were white, there is reafon to conclude that the ftate of domefticity has an influence on the colour of the eggs, as it has fometimes on that of the plumage. This obfervation did not efcape Ariftotle *.

The number of eggs was always two, except once, when

* Hift. Animal. lib. vi. c. 2.

there

there were three. Though theſe eggs were often handled, the female never made the leaſt heſitation to ſit upon them *.

Of the eggs laid this and the preceding year, only one was hatched.

The female alone ſits on the eggs. The period of incubation is forty days †. The female ſcarcely ever leaves them, except to drink, or for any other neceſſary purpoſe; and then ſhe immediately returns. During that time, the male ſtands ſentinel at a little diſtance: he is very attentive to the leaſt noiſe, and never ſtirs from the ſpot but to carry food to the female, which he always breaks with his bill during the incubation.

When the young parrot is hatched, both the old ones carry it food. The young one did not begin to eat alone till the 70th day. The old ones continued to cut the food with their beaks for more than ten days.

They gave to the young one the ſame food as they uſed themſelves. Being accuſtomed to every thing, they ate rice, meat, paſte, Turkey corn, hemp-ſeed, and fruits; but they were fondeſt of the ſeeds of the turnſole, carthamus, melon, and cucumber.

The following fact is highly worthy of attention:—The owner ſeeing that his young parrot increaſed in ſize, and fearing that the veſſel would be too ſmall to contain the mother and her young one, he put into a baſket ſome linen rags and feathers, depoſited it in the ſame place where the veſſel ſtood, and conveyed the young parrot into it: the mother immediately repaired to the baſket, and ſeemed highly ſatisfied with her new habitation; but ſome hours after, ſhe began to cut with her beak one edge of the baſket: in three days ſhe finiſhed her work, having made an aperture in it four or five inches in width and ſix or ſeven in height. The twigs were cut as neatly as if by the ſharpeſt inſtrument.—It appears to me that this fact can be explained only in two ways. The mother, ſeeing her young one grow big, cut the edge of the baſket, that when it was in a ſtate to go abroad it might have no occaſion to climb over the edge, which was about ſix inches in height.

III. *Deſcription of the Male, the Female, and young Parrot, in order to aſcertain to what Species they belong.*

Had ornithologiſts left us exact deſcriptions, it would be eaſy to compare different ſpecies: theſe deſcriptions are very often deficient and imperfect, and very often the male alone,

* Buffon made the ſame obſervation, vol. ix. 8vo. p. 287.

† Buffon ſays that the male and female Amazon parrots ſit alternately.

or the female, has been delineated. It is well known that there is a difference among birds according to their fex; and this has produced great ambiguity, of which many inftances might be produced: the cafe has been the fame, in my opinion, with the male in queftion.

The male is as large as a pullet of four months old: its length from the root of the bill to the extremity of the tail is 14 inches. The top of its head is of a beautiful yellow colour, which extends to the throat and the lower part of the neck. On the head, the feathers appear to have an orange tint, becaufe there is a double row, the longeft of which are yellow, and the fhorteft of a rofe colour. The reft of the neck, the upper part of the back, and the cover of the wings, are of a brilliant green. The breaft and belly are of a yellowifh green, and the tip of the wings of a beautiful yellow intermixed with orange. The plumes of the wings are variegated with blue, violet, and very bright red. The two exterior feathers on each fide of the tail have their internal barbs red at the origin of the quill, and of a dark green at the extremity, which is yellowifh green: the other feathers are of a dark green, terminating with a yellowifh green. On the thighs it has very thick yellow feathers, like breeches *à la Suiffe:* the bill is of an afh-gray colour, black at the tip: the iris of the eye is orange: the feet are gray, and the claws black.

According to this defcription it appears that, by the head and tips of the wings, it may belong to the family of the criks; but its fize, and the plumage of the back and belly, feem to give it a refemblance to the firft fpecies of the yellow-headed Amazon of Buffon. The large yellow breeches *à la Suiffe*, as far as I know, have been defcribed by no ornithologift; and this peculiarity is fufficiently remarkable to make a feparate clafs: befides, the iris of the eyes of the parrots in queftion is orange inftead of yellow, as in the criks; and the bill is afh-coloured; while that of the Amazons is red at the bottom, and that of the criks white.

The female, which is fmaller than the male, is as large as a pigeon: her length from the root of the bill is only 11½ inches. The top of the head is yellow, and the tips of the wings of a bright red. The plumage has a perfect refemblance to that of the yellow-headed Amazon of Buffon: it is certainly the *Pfittacus major, viridis alarum, cofta fuperne rubente*, the Amazon parrot of Barrere *, and not the *Pfittacus macrourus viridis, genis nudis, humeris coccineis, Pfittacus*

* France Equinoxiale, p. 144.

tacus

tacus nobilis of Linnæus *. In my opinion Linnæus alluded to ſome ſpecies of Ara, as the individuals in queſtion have not their cheeks bare. There can be no doubt that the female is the real Amazon, but it is difficult to determine to what ſpecies the male belongs. It is poſſible that the latter may be the male of this ſpecies, and that ornithologiſts may have examined and deſcribed only the female, and may have ſuppoſed both to be perfectly ſimilar; which, however, is rarely the caſe. It cannot be claſſed either with the ſpecies approaching the yellow-headed Amazon, and known under the name of the baſtard Amazon, or demi-Amazon; for none of theſe have yellow breeches on the thighs: to obtain, therefore, a complete hiſtory of theſe birds, it would be neceſſary to deſcribe always both the male and the female.

The young parrot, when examined at the third month, had the head and lower part of the neck yellow, like the father: its length from the root of the bill was about 10 inches, and, at four months, 11 inches. The body is exactly like that of the father; but its plumage is of a much livelier and more ſplendid green. The tips of the wings are orange, the ſmall red feathers being interſperſed with yellow: it has the yellow breeches, but leſs tufted than thoſe of the father; the ſame legs, gray feet, and black claws: the bill is gray, and blackiſh at the point. The eye till the third month was gray, and the iris of the ſame colour; but it gradually changed, and at the fourth month it was yellow. I have no doubt that it will become orange, like that of the father. Its tongue is very black, and the noſtrils well marked: the progreſs it has made in ſize during the courſe of four months, induces me to think that it will ſtill grow, and become larger than the father.

This living individual may afford an opportunity of making many important obſervations, which it would be intereſting to follow.

1ſt, To know at what age it will attain to its full growth.
2d, Whether any change will take place in its plumage.
3d, At what age it will be in a ſtate to procreate.
4th, How many years it will live.

I have requeſted the owner of this parrot to attend to the three firſt articles; for, in regard to the duration of life, as I believe it to be very long, it can be known only to the ſecond or third generation after us. I have begged him alſo to note down to what age the mother will lay. At preſent ſhe muſt be about twenty years of age, for it is about fifteen

* Linn. Hiſt. Nat. ed. 10. p. 97., ed. 13. p. 140.

fince fhe came into his poffeffion; and, as fhe was then of the full fize, fhe muft at leaft have been five years of age. I begged him likewife to be attentive during the next year, and to obferve whether one egg alone will produce, and whether the reft will be equally fruitful.

IV. *Inftances of fome Parrots which have laid in Europe.*

The parrots both of the old and new continent never pafs the tropics, and feem confined to a zone of 23 degrees on each fide of the equator: in their wild ftate they never pafs thefe limits, which nature feems to have prefcribed to them. When tranfported beyond thefe latitudes, they live, and are fenfible to love, notwithftanding the difference of climate. We have fome inftances therefore, but very rare, of parrots producing young in our temperate countries; though it often happens that they lay eggs without any germ. Buffon afferts, on the teftimony of the *Gazette de Litterature* of the 17th of November 1774, that M. de la Pigionière had a male and a female parrot in the town of Marmande, in Angumois, which for five or fix years never failed to lay eggs that produced young, which were reared by the father and the mother. Thefe parrots were of the fpecies called the *jaco*, or afh-coloured parrot of Guinea, *Pfittacus cinereus feu fubcæruleus* of Aldrovandi. He quotes father Labat, who fpeaks of two parrots which feveral times produced young at Paris; and the abbefs of Beaumont-les-Tours, who had two young parrots hatched in the month of January; but the cold foon killed them.

Thefe are fingle facts which give us no information refpecting the habits of thefe birds either during the time of their laying or incubation. We, however, no where find that the Amazon parrots, or any others of America, ever produced in Europe.

Though the climate of Rome is not fo cold as that of Paris, as the mean temperature at the end of March and the commencement of April is 17 degrees of Reaumur *, I am not of opinion that this was the principal caufe of thefe parrots producing; as the fame thing muft have often taken place in Sicily, Malta, and other countries lying more to the fouth: but of this we have no inftance. I am therefore induced to think that the parrots in queftion produced at Rome, becaufe they had been long together, were exceedingly fond of each other, when they became acquainted, and always enjoyed the completeft liberty; becaufe they were probably of the fame age,

* Tab. Meteorolog. de Rome, par l'abbé Scarpellini.

and

and becaufe the owner, finding that the eggs were not clear like thofe of the preceding year, but contained a germ, thought proper to leave them in a detached chamber expofed to the fun, gave the parrots more liberty and abundance of food. I am fully perfuaded that thefe caufes contributed much more than the climate to their producing.

If inftead of one young one, which I believe to be a male, there had been a female alfo, it is very probable that a breed might have been eftablifhed at Rome. We may hope for it the next year.

Conjectures refpecting the Duration of the Life of Parrots, deduced from the Time of their Incubation, and compared with that of other Birds.

Salerne afferts that he faw a parrot 60 years of age ftill alive and active; M. de la Borde fays that he faw one which was more than 46 years, and I myfelf have feen one which had been in the fame family 75 years.

Though Ollina, an excellent obferver, was of opinion that the life of parrots, taken at a medium, did not exceed twenty years, I am of opinion that he was deceived, having, perhaps, had an opportunity of obferving only a fmall number of individuals.

All naturalifts agree in affigning a long life to parrots, to which Linnæus gives the epithet of *longævi*. This fact, then, is inconteftable. The parrots in queftion furnifh a new proof of it. According to the calculation already made, the age of the female cannot be lefs than 20 years; for, as it is five fince fhe firft laid, it thence refults that fhe was then 15; but if in the order of nature thefe birds begin to lay at that age, this would be another proof of their longevity, fince we know that the large birds, with which we are beft acquainted, are capable of laying before they are a year old, and that they live at leaft nine or ten times as long: we may thence conclude that thefe parrots muft live 130 or 150 years. It may be objected, indeed, that it fometimes happens that the females do not become mothers till an age greater than that eftablifhed by nature: but this is rarely the cafe among quadrupeds, and ftill lefs fo among birds. The parrots in queftion lived together ten years without laying, though they enjoyed the fame liberty and the fame facility of copulating as they afterwards had; and therefore we may fuppofe that nature was not difpofed for it.

Buffon thinks that the life of birds is longer in proportion than that of quadrupeds, according to the time employed in their growth. As we can obtain certain data refpecting the time employed for the whole growth only in regard to a ſmall number of individuals, let us try whether we cannot, by induction, diſcover that the longevity of birds is proportioned to the duration of their incubation.

I ſubjoin the following table in order that the reader may ſee, at one view, the period of the incubation of different birds, and the duration of their life in the next column. I muſt however obſerve that the time of incubation is not altogether the ſame in all climates: in warm climates it is accelerated, and in cold climates retarded. But the difference is very ſmall, and can occaſion no ſenſible difference in the table.

Table of the Period of Incubation, and of the Duration of Life of ſeveral Birds.

Names of the Birds.	Incubation. Days.	Duration of Life. Years.	Authors.
The ſwan	42	200 nearly	Aldrovandi.
Parrot	40	100 nearly	Wolmaer.
Gooſe	30	80 and more	Willughby.
Eagle	30	Never obſerved by any naturaliſt.	
Buſtard	30		
Duck	30		
Turkey	30		
Peacock	26 to 27	25 to 28	Ariſtotle. Pliny.
Pheaſant	20 to 25	18 to 20	Treatiſe on Pheaſants.
Crow	20	100 and more	Heſiod.
Nightingale	19 to 20	17 to 18	Buffon.
Pullet	18 to 19	16 to 18	Buffon.
Pigeon	17 to 18	16 to 17	Several obſervat.
Linnet	14	13 to 14	Willughby.
Canary bird	13 to 14	13 to 14	Treatiſe on theſe Birds.
Goldfinch	13 to 14	18 to 20	Buffon.

We ſhall here obſerve, that it appears by this table that the ſwan and parrot, which employ the longeſt time in incubation, are not thoſe which live longeſt: the longevity of the former of theſe birds has been carried to 200 years, and that of the latter to 100.

In regard to the oſtrich, the life of which the antients carried beyond 100 years, we can find no relation between this period and that of the incubation, as the female ſeldom ſits on the eggs, which are hatched by the heat of the ſun. The caſe is the ſame with the crane and the heron, to which the

the antients aſſigned a life equally long; but we have no obſervations in regard to their incubation *.

The gooſe, which employs thirty days in incubation, lives beyond the age of 80. The eagle, buſtard, turkey, and duck, are the next in the table in regard to the period of incubation; but it has never yet been compared by any one with the duration of their life.

All the other birds mentioned in the table, live nearly in proportion to the time of their incubation; but, as theſe obſervations were made chiefly on domeſtic or captive birds, it is probable that there may be ſome difference between theſe and thoſe which are in the wild ſtate.

"We know beſides," ſays M. de Buffon, "that we never give time to our domeſtic birds to go through the complete period of life aſſigned to them by nature."

In regard to birds in their wild ſtate, it is almoſt impoſſible to obtain any correct information. Very few ornithologiſts, therefore, have ſpoken of the duration of their life; and when they are caught in ſnares, they endeavour to judge of their age by marks which are very ambiguous. (See *Ariſtotle.*)

The conſtant ratio which I have obſerved between the time of incubation and the duration of the life of birds, ſuggeſted to me the idea of examining what relation there is between the duration of the life of quadrupeds and the time employed in geſtation; and I have made out the following table:

Table of the Period of Geſtation and of the Duration of Life of ſeveral Quadrupeds.

Names of the Animals.	Geſtation. Months.	Durat. of Life. Years.	Authors.
Elephant - -	24 -	150 to 200 -	Ariſtotle.
Camel - - -	12 -	40 to 50 -	The ſame.
Buffalo - -	12 -	20 to 25 -	Cajetano.
Aſs - - - -	12 -	25 to 30 -	Buffon.
Mare - - -	$11\frac{1}{2}$ -	25 to 30 -	Ariſtotle.
Cow - - -	9 -	14 to 15 -	The ſame.
Stag - - -	$8\frac{1}{2}$ -	35 to 40 -	New Treatiſe on Hunting.
Roebuck - -	$5\frac{1}{2}$ -	12 to 15 -	Buffon.
Sheep - - -	5 -	12 to 15 -	The ſame.
Goat - - -	5 -	11 to 13 -	The ſame.
Dog - - -	2 -	11 to 15 -	The ſame.

By comparing theſe two tables a very great analogy will be found between the time of the geſtation of quadrupeds

* See Paulus Jovius.

and that of the incubation of birds, in regard to the duration of life. Thus we obſerve that, among quadrupeds, the elephant, which lives from 150 to 200 years, goes with young 24 months; and that the camel, which goes with young 12 months, lives 40 or 50 years. Among birds, the ſwan, which ſits 42 days, and the parrot, which ſits 40 days, are thoſe which live longeſt, their life exceeding 100 years. In looking over this table, the ſame relation appears to be preſerved. The horſe and aſs, which have the ſame period of geſtation, live nearly the ſame time; that is to ſay, 26 or 30 years. The domeſtic fowl and pigeon, which have nearly the ſame period of incubation, attain to the ſame age; and the caſe is the ſame with the goat and the ſheep, as well as with the canary-bird and the linnet.

I muſt, however, confeſs, that in theſe tables there are two very ſtriking exceptions. The ſtag, among quadrupeds, goes with young a month leſs than the mare, and yet lives about ten years longer: and, among the birds, I obſerve that the crow, which ſits only twenty days, lives far beyond 100 years, according to Heſiod and ſeveral other obſervers.

Theſe exceptions ſtill further prove that our knowledge reſpecting this part of natural hiſtory is very limited, and prove the neceſſity of making more obſervations. Beſides, the difficulty of aſcertaining the exact duration of the life of quadrupeds is equally great as that experienced in aſcertaining the duration of the life of birds, ſince theſe obſervations can be made only on a ſmall number of individuals, taken, for the moſt part, in a ſtate of domeſticity; and M. de Buffon ſagely remarks, that man, wiſhing for continual enjoyment, ſhortens the period.

If, notwithſtanding theſe exceptions, ſome relation has been ſuppoſed to be found between the time employed for perfect growth, and the duration of life, it appears, from a view of this table, that there is a more direct connection between it and the time of geſtation: nature alſo ſeems to have preſerved a kind of connection between the time employed in geſtation and that of incubation in regard to the duration of the life of birds and quadrupeds.

The period of the incubation of the parrot, which no naturaliſt had before obſerved, has led me into conſiderations ſomewhat prolix; but ideas ſuggeſted merely as hints, when aided by experience and more correct obſervation, often conduct to important truths.

XLI. *De-*

XLI. *Description and Natural History of the Wild Goat of the Alps.* *By* M. BERTHOUT VAN BERCHEM *jun.*

[Concluded from p. 121.]

BUT having faid enough in regard to this animal in its confined ftate, let us now haften to reftore it to liberty: it is on the fummits of the fteepeft rocks, fufpended, as we may fay, between the heavens and the earth, that it ought to be confidered, as it is there that it difplays all its ftrength and agility. It is particularly interefting to fee the eafe and facility with which it traverfes the moft awful rocks, the bare fight of which infpires terror, and which are inacceffible to man. When it leaps, it does not feem to take a fpring; it directs its fight with the greateft precifion to the place it wifhes to reach, and never miffes its aim. When tranquil, it generally carries its head low; but when running, it holds it elevated: it even throws it fomewhat back, which heightens the agreeablenefs of its figure. When it wifhes to raife itfelf to the top of a perpendicular rock fifteen feet in height, it does it at three leaps, or rather three fucceffive jumps, of five feet each: one might fay that it fcarcely found any point of fupport on the rock; and it appears to have touched it only that it might be thrown higher, with the fame force that an elaftic ball would be if impinged againft a hard body. It never makes more than three leaps in this manner. If it be between two rocks ftanding near each other, and wifhes to get to the top, it accomplifhes its object by jumping from the one to the other. It traverfes the glaciers alfo with great rapidity, but only when chaced: when its courfe is voluntary it avoids them, and does not feem to walk on them with fuch confidence as among the rocks.

I fhall now give an account of the manner in which the wild goat lives when peaceably fuffered to follow its natural habits, when not molefted in its retreats; and fhall then defcribe the manner in which it is hunted. We fhall fee in what manner man, who of all beings is the moft deftructive, penetrates into the almoft inacceffible places which nature has affigned to this animal, and where, as appears, it ought to be fheltered from his rapacity. We fhall fee how, at the rifk of his life, and braving the moft imminent dangers, he purfues it, and has diminifhed the number in fuch a manner, that, though it was formerly difperfed over all the high mountains of Swifferland, Savoy, and the Valais, it is now found only in fome places, and even in fmall numbers.

 Thefe

Thefe goats feed during the night in the higheft woods, and never further than a quarter of a league from the fummit; but, when the fun begins to illuminate the tops of the mountains, they leave the woody regions in flocks, afcend the mountains as they feed, and proceed fometimes to very confiderable heights. They feek the declivities turned towards the eaft and fouth, and lie down in the higheft and moft elevated places. But when the fun has got through three-fourths of his courfe they defcend to the woods, feeding as they proceed, in order to pafs the night. When it threatens to fnow, they defcend alfo into the woods for the fake of fhelter.

Thefe animals affemble in flocks of 10, 12, or 15; formerly they were more numerous, but at prefent they are often in fmall numbers. All the males fix years of age, and thofe above it, always remain in higher places than the females or the males that are younger. The older they are, they are the lefs fond of living in fociety. They become hardened againft the cold, froft, and all the feverity of the feafons. Very often thefe old goats have the tips of their ears withered, and, as it were, dead. Severe cold muft neceffarily produce this effect on extremities where it is well known the circulation of the blood is eafily checked. They lead a folitary life on the fleep fummits, and never defcend from them. To conclude what we have to fay in regard to their habits, we fhall here obferve, that in winter they are accuftomed to place themfelves on eminences oppofite to that part of the horizon from which the ftorm proceeds, and to remain there like ftatues: they only go from time to time to feed in the neighbourhood, and then return to their ftation. The females and young have not this habit; they remain at all feafons in places lefs elevated.

As the fore-legs of thefe animals are fhorter than thofe behind, it is natural to fuppofe that they can afcend with more facility than they defcend. Nothing but the feverity of the weather can induce them to remain in the low diftricts; and, if a few fine days take place in winter, they immediately quit the woods and afcend the mountains.

Winter is their feafon of love, and the month of January that of their greateft heat. All the goats, of whatever age, then mix together; the males combat, and the females belong to the victors; after which they return to their ufual order. The females go with young five months, and bring forth in the laft week of June or firft of July. They produce only one at a time, of the fize of a cat, and which, like the young chamois, walks as foon as dropped. An hour after its

its birth it can conceal itſelf among the rocks *. When the female is delivered ſhe licks its eyes, head, and neck; teaches it, at an early period, to leap; and beſtows on it the care of an attentive and vigilant mother. While ſuckling, ſhe remains in the holes of the rocks. She calls her young by bleating; but the hunters think they have remarked that it is by geſtures that ſhe uſually expreſſes her deſires.

As it often happens that flocks of wild goats, chamois, domeſtic goats, and ſheep, paſture at a ſmall diſtance from each other, I endeavoured to find whether theſe animals ſometimes intermixed and produced in that ſtate of liberty; but I could obtain no certain information reſpecting this fact, which would be of great importance to natural hiſtory. It only appears that the wild goat and the chamois never intermix in their natural ſtate.

The beſt time for hunting the wild goats is towards the end of ſummer, and in autumn, in the month of Auguſt and September. It is then that they are in beſt condition. This ſpecies of hunting is exceedingly dangerous and fatiguing, and therefore can be undertaken only by the inhabitants of the mountains. The hunters, beſides reſolution to look down without fear from the greateſt heights, muſt have ſufficient addreſs and ſteadineſs of foot to get out of difficult paſſes, and to take accurate aim. They muſt alſo be endowed with great ſtrength and vigour to endure hunger, cold, and the greateſt fatigue. The moſt determined hunters reſide in the mountains of the Lower Valais: almoſt all the peaſants of Servan † follow

* A hunter one day ſurpriſed a female chamois while bringing forth her young one on a rock: a ſentiment placed by nature in the breaſt of all mankind retained his arm, and prevented him from killing her: he viewed this intereſting ſpectacle for ſome moments, and ſaw the mother lick her kid. He then prepared to fire, but the chamois and her young one both inſtantly diſappeared, and concealed themſelves among the rocks.

† The village of Servan is ſituated in the mountains of the Lower Valais, and at the diſtance of two or three leagues from Martigny: its ſituation is the wildeſt and moſt pictureſque I ever ſaw. It ſtands on the declivity of a chain of mountains which borders on the weſt that part of the valley of Trient, which is only a continuation of that of Orſiere, in Upper Faucigny. At the bottom of this valley runs the water of Berard with a roaring noiſe, and, uniting itſelf to the Trient, aſſumes its name. Torrents, which daſh down on every ſide from the ſummits of the mountains in ſuperb caſcades, contribute to enlarge its ſtream. That ſilence of nature, that calmneſs, at the ſame time awful and delightful, which is experienced in the middle of a foreſt during a fine ſummer's night, never prevails in this abode. The air is continually agitated by the noiſe of the torrent which flows through the middle of the valley, and by that of the numerous caſcades which join it. But this continual and hollow roaring of the water is often interrupted by the tumbling down of large maſſes, and the fall of

follow this occupation; and as the animal is not found in their mountains, they go to hunt in the East Valley, having first obtained permission from the inhabitants for that purpose. A hunter never goes out alone; he generally associates with himself one or two others, furnished with rifle guns* and a bag of provisions.

They associate for the purpose of gain, but dangers and perils unite them; and indeed there are many reasons why two hunters should be united. Being obliged to spend the night among the rocks, and at very great heights, they construct a wretched hut with pieces of turf, where they sleep without fire or bed-clothes, and on their awaking often find the entrance of it blocked up by two or three feet of snow. When surprised by night while in the pursuit of a wild goat, it frequently happens that, not being able to disengage themselves from the rocks, they are obliged to pass the night standing in

of the rocks, which, attacked by the slow action of the moist elements, crumble to pieces through age, and choke up the bottom of the valley. These torrents, by tearing open the sides of the mountains, unveil to us their interior structure. In this country, every thing presents the image of destruction: it is in the middle of these degraded forests of firs and pines, of those accumulated rocks which pass through all the states of decomposition to sand, and from the state of sand to that of mould, that the geologue discovers the composition of mountains, and collects facts for the theory of the earth. But in the midst of this disorder one may discover the hand of man and the labour of industry: excited by his wants; sheltered from all quarters by the rocks, and in a good exposure, the inhabitants have collected a little earth, which they cultivate and sow with corn to supply them with food.

Of all the cascades with which this valley is decorated, the most interesting for the naturalist and the painter is that, no doubt, of Maupas, between the village of Finio and that of Servan. The torrent of the Trieve, the white foaming waters of which dashing against the rocks, which they break and carry with them to their bed, form several cascades; between very high rocks, a plank laid across serves as a passage, and from this tremendous bridge the torrent is seen precipitating itself from one fall to another, and flying to a distance below the feet: before is a rock cut into the form of a peak, and which seems to be an insurmountable barrier; but a small narrow path, cut out in a zig-zag form and furnished with a parapet, enables the traveller to pass over it, and to consider this sublime and magnificent view, which the horrid noise of the torrent renders still more striking. The more you advance towards Servan, the less steep the declivity of the mountain becomes, while the valley becomes broader and better cultivated. But in traversing this wild country one is not astonished that the inhabitants should seek for and be fond of those dangers to which they expose themselves in hunting the chamois and wild goats: they are bred up to this occupation; the rocks and precipices are, as we may say, their element, and when they quit their habitations they find no perils to which they are not accustomed.

* These pieces can fire twice without being reloaded; but they have only one barrel, with two locks. See a description of them in *De Luc's Recherches sur l'Atmosphere.*

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each other's arms, that they may remain awake: theſe are ſituations which none can have an idea of but thoſe who have traverſed the rocks. I cannot here help making one reflection: It is among the inhabitants of theſe wild and horrid mountains that I found the greateſt honeſty and friendſhip; in a word, all thoſe virtues which render men worthy of eſteem.

As theſe animals aſcend to the higheſt regions early in the morning, the hunters muſt get to thoſe places before them. The goats are then ſeen coming to paſture, and, though their ſmell is exceedingly acute, they do not ſmell the hunters, who, concealing themſelves behind the rocks, ſuffer them to approach within 30 or 40 paces before they fire.

The following appear to be the moſt natural cauſes why theſe animals do not ſmell the hunters:—In the morning the ſun heats and rarefies the upper regions of the air, while the lower ones are ſtill cold and condenſed. From this want of equilibrium there muſt neceſſarily reſult a current which proceeds upwards, and which conſequently carries in that direction the emanations proceeding from the hunter, who is above the goats. This effect, indeed, muſt take place during the greater part of the day; for the ſun, by warming the declivities of the mountains, muſt produce along them an aſcending current. Beſides, as theſe animals feed as they go along, their noſtrils become filled with dew, which muſt have a very great influence on their ſmell. However, it is eſſentially neceſſary to get to the heights before the goats reach them; if they anticipate the hunter, they ſmell him, and make their eſcape: it would in that caſe be of no uſe to follow them; for, when once put to flight, they never ſtop till they get to a very great diſtance, and when they think they are beyond all danger *. It is ſingular that the animal never flies till it ſmells the hunter; for if it only ſees him it

* The reader will not be diſpleaſed to ſee here an account of the manner in which the inhabitants of the iſland of Crete hunt the wild goat, as deſcribed by Belon:—Some of the peaſants who live on the ſummits of the high mountains are ſuch dexterous ſhooters with the bow, and principally around the mountain of Spachia and Madara, that they pierce them with their arrows at the diſtance of 25 paces; and, to be more ſure of them, carry females, which they have bred up and tamed from their youth, and tie them up in ſome paſſage of the mountains where the males are accuſtomed to paſs. The hunter conceals himſelf behind the buſhes oppoſite to the wind, knowing that the male goats have ſo acute ſmell, that they could ſmell them at the diſtance of 100 paces. The male finding the female in his way ſtops, and the peaſant then diſcharges his arrow.—*Obſerv. fol.* 14. *verſ.*

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is ſatisfied with whiſtling and looking at him, but as ſoon as it ſmells him it takes to flight.

The female never abandons her young, unleſs ſhe is hunted: if it cannot follow her ſhe leaves it, and the latter conceals itſelf in the burrows of the marmot, or other holes of the rocks; ſometimes at the depth of ſix feet.

When the danger is over ſhe returns to call it, and ſearches till ſhe finds it: but if ſhe remains too long, the young goes to look for the mother; it iſſues from its hole, calls its mother, and enters ſome other hole with fear and emotion, and with ſigns of the greateſt uneaſineſs. If it ſees the mother it runs up to her; but if ſhe is wounded, and lying, as ſoon as it ſmells the blood it betakes itſelf to flight; then returns, approaches with the ſame eagerneſs, and again retires for the ſame reaſon. It can hardly be conſoled for the loſs of its mother; ſearches for her a long time, and never quits the place where it was brought forth and where it loſt her.

When a wild goat is wounded, all the reſt make their eſcape and abandon it: they are ſeized with fear and terror, and fly with the utmoſt rapidity. The hunter can diſtinguiſh the wild goats that have been wounded, by their walk, which is ſlow. They ſuffer their heads to fall ſometimes to the one ſide and ſometimes to the other, and they ſoon after lie down, even when the wound is ſlight.

Man, though the greateſt enemy and principal deſtroyer of theſe wild goats, is, however, not the only one whom they have to dread. The wolves and eagles purſue their young*. The mothers, on theſe occaſions, eſcape with them to caverns or hollow places in the rocks, make their young enter firſt, and then, having entered themſelves, preſent their head to the aperture in order to face the enemy.

There are few animals the natural hiſtory of which is diſfigured with more fables by ſuperſtition, and more errors by ignorance, than that of the wild goat. The naturaliſt, as already obſerved, is obliged to truſt to the hunters, and theſe indemnify themſelves for the labour and fatigue they have experienced by the falſehoods which they propagate: beſides, an animal the hunting of which is ſo painful, ought, in the eyes of the vulgar, to have ſomething of the marvellous connected with it. Hence the property aſcribed to the horns of the wild goat of deſtroying the force of poiſon; and hence the opinion of the hunters, that if their pieces are rubbed with certain herbs they cannot hit the mark. We ſhall

* See an account of the combats of the lammer-geyer and the chamois in *Cox's Letters on Swiſſerland*, vol. i. p. 267.

not

not enlarge on theſe popular tales, but we think it neceſſary to refute two opinions which are very much believed among naturaliſts: one is, that when the wild goat or the chamois is purſued by the hunter among the rocks, they return, attack him, and puſh him down the precipices: this is a miſtake: it ſometimes happens that, if one of theſe animals is wounded, it endeavours to eſcape when the hunter approaches it, and, as it always ruſhes towards the precipices, it overturns the hunter if he happens to be in its way; but it never ſearches for him, and, on the contrary, avoids him. It is ſaid alſo that the wild goat throws itſelf down precipices, falls on its horns, and rarely ſuſtains any injury. But I can aſſert that it always leaps from one rock to another; and that it is ſo expert, and ſo ſure of its aim, that it has never any occaſion to throw itſelf on its horns: that if this ever happens, it is by accident; and beſides, it is ſufficient to look at the animal to be convinced that it could not take ſuch leaps with impunity. It has a long body, its legs are ſhort, its limbs are thick, and it weighs nearly 200 pounds: how, then, is it poſſible that it ſhould throw itſelf from the rocks without being torn by their ſharp points, or daſhed to pieces by its fall? I have been more particular in regard to this fact, becauſe the ſame error is not only related by moſt naturaliſts, but is received alſo among the Tartars *, who are hunters of wild goats, like the people of the Valais of Swiſſerland; and becauſe this conformity of opinion among people ſo diſtant might induce ſome to believe in the reality of the fact. But is it not well known that the ſame prejudices are found among different nations? Ignorance is every where the ſame, and every where the ſame objects have given riſe to the like errors. Thoſe naturaliſts who are ſo fond of final cauſes, wiſhing to give utility to the large horns of the wild goat, have adopted this opinion; and it is thus that the mania of wiſhing to explain every thing injures the cauſe of truth, and often perpetuates popular tales. M. Pallas, deceived by the accounts of the Tartars, mentions, as a proof of this aſſertion, a wild goat which he ſaw having one of its horns broken †: but this, in all probability, was owing to another cauſe. Their large, thick, and almoſt uſeleſs horns are often broken by the tumbling down of the rocks; and on theſe occaſions they are often killed themſelves. For this reaſon, the inhabitants of Cormajor traverſe the bottoms of the mountains to collect the horns carried down by the ſnow. M. Pallas ſays, alſo, that the wild goat is of ſo ſavage a na-

* Pallas *Spicil. Zool.* faſc. xi. p. 50.

† Spicil. Zool.

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ture that it never can be tamed. If he alludes to the old wild goats, he is right; but the inftances above mentioned prove that, if the wild goat be caught young, it is fufceptible of education: and this opinion is confirmed by that of Belon, who fays, that in the ifland of Crete this animal is tamed when young.

When one of thefe animals has been killed, it is left on the fpot till it becomes cold. The entrails are then taken out, and the blood is put into the ftomach. In confequence of this method it never acquires a bad fmell, which takes place when this precaution is neglected. Large wild goats, without the entrails, weigh 180 or 200 pounds, of 18 ounces. The female from 70 to 80 and 90 pounds. The flefh of this animal is excellent; it has the tafte of mutton, but is more fucculent.

The fkin can be dreffed exceedingly well: it becomes very pliable, and refembles that of the goat. It is employed for manufacturing fhamoy leather. Various fmall articles are made of the horns, fuch as cups and goblets. The blood is employed in falfe pleurifies; but, as its properties are owing to the plants on which the animal feeds, that of the common goat and ram, when fed in the fame manner, has the fame efficacy *.

The fale of thefe goats is liable to many variations: it depends on the weight of the beaft, and the eagernefs of the purchafer. Formerly they were cheaper, becaufe more common. At prefent they are fold fometimes for four louis-d'ors. The chamois alfo is fold for one louis-d'or. The wild goat is fond of falt: it licks the rocks which contain Epfom or Glauber's falt, and the waters which hold it in folution. It feeds chiefly on the wild plants that grow on the higher Alps, fuch as filky wormwood, *artemifia glacialis*; but in winter it eats the lichens and young fhoots of the trees. Like that of Siberia †, it prefers thofe places which abound with the dwarf birch, the Alpine willow, the *thalictrum*, faffafras, rofe-wort, *rhodiola*, the willow-herb, *epilobium*, &c.

Thefe animals are much fcarcer than they were formerly; they are found at prefent only in fome mountains of Savoy and the Valais, whereas formerly they were difperfed throughout all the higher parts of Swifferland ‡. But this is not aftonifhing;

* Hift. Nat. par Buffon, vol. xii. p. 164.

† Spicil. Zool. fasc. xi. p. 49.

‡ My friend M. Wittenbach, firft paftor of the church of St. Efprit at Berne, who has travelled over and defcribed the mountains of Swifferland with the eyes of an enlightened naturalift and the fpirit of a good obferver,

aſtoniſhing; nature, man, the beaſts and birds of prey, are all enemies with which they are ſurrounded, which perſecute and deſtroy them, and againſt which they have no other reſource than ſpeedy flight: but this rarely preſerves them. This animal, therefore, is now confined to a ſmall number of the ſteepeſt ſummits.

It is found in the valley of Cormajor, to the ſouth of Mont-Blanc, on the ſouthern declivity of that chain of mountains, and in the diſtrict between Mont-Blanc and the frontiers of the Valais; but it is never ſeen in the chain oppoſite to that of Mont-Blanc, and which forms the other ſide of the valley of Cormajor. It exiſts alſo in the mountains which form the Val-Savaranche. But it is among the mountains of the valley of Cogne, which is the frontier of the valley of Pont, in Piedmont, that it is now moſt common, and always on the declivities expoſed to the ſouth.

It is no longer found in the Valteline, the frontier of the Valais; but it muſt ſtill exiſt in the mountains between the valley of Seſia and that of Vieche. It was hunted, about 30 or 40 years ago, in the mountains of Upper Faucigny: but the breed is now entirely deſtroyed in that quarter; and it is probable, if the hunters continue to purſue it with the ſame ardour, that the race will become totally extinct.

In reflecting on the fury with which theſe animals are purſued and deſtroyed, the cauſe of it appears to me to have been the great abundance which there was of them formerly; and, though a ſingle goat did not produce much, as a great many were killed during one hunting excurſion, the gain was conſiderable. At preſent, ſince the number has greatly decreaſed, and ſince the hunters often go a great way without finding any, their ardour ought to be relaxed in conſequence of the dangers to which they are expoſed: and beſides, the inhumanity of perſecuting and tormenting a mild and timid animal, which has no other reſources to defend itſelf but flight and

ſerver, has furniſhed me with the following notice:—"There were formerly wild goats in our Alps, but none of them are now to be found. I have ſeen at Unterſeven two ſuperb horns of that animal, which is ſaid to have been killed formerly in the Alps of Lauterbrunn. Theſe animals were deſtroyed by the hunters, as well as the caſtors, which formerly frequented the borders of Lake Brientz, and along the Arr towards Thun. I am certain M. Bourrit is miſtaken when he ſays that he ſaw one traverſe the chain of mountains that ſeparates Lauterbrunn and Grindelvald, ſince none of the hunters of that country ever fell in with any during their excurſions. It is aſſerted that wild goats are ſtill found, but rarely, in the wildeſt mountains of the country of the Griſons: for my part, I never ſaw any in my travels, and none of the hunters were able to give me any certain information reſpecting them."—*Letter of M. Wittenbach to M. Van Berchem jun.* Berne, May 27, 1785.

4 addreſs,

addrefs, a motive, indeed, of little importance to a being who thinks himfelf the fovereign of animals, but who is only their tyrant, and knows no other laws than thofe of the ftrongeft, the hunters ought to confider, before they leave their wives and families, that they not only expofe themfelves to be killed, or maimed during the remainder of their lives *, but neglect their property and affairs, and, if they perifh, their families are reduced to beggary. If thefe confiderations are not fufficiently ftrong to deter them from this kind of hunting, let them reflect, that they can entertain no hope of making their fortunes; fince no one acquires riches by this occupation, but, on the contrary, they are often lamed, and drag out their exiftence in mifery. But fuch is the force of habit and the power of illufion, that the accidents which happen to the hunters do not deter them; for they are fcarcely cured when they return to the fame occupation †.

The wild goat of Siberia, as may be feen by the defcription of Pallas ‡, feems to differ from that of the Alps only by a few

* The elegant tranflator of Coxe expreffes himfelf in the following manner in regard to hunting the chamois:—" A great number of men lofe their lives by this kind of hunting: the thick fogs of the Alps, which fuddenly envelop the country with a dark veil, make the hunters wander into the glaciers, where they perifh with cold and hunger. Sudden ftorms moiften the rocks, and render them fo flippery, that the hunter, however furnifhed with cramps his fhoes may be, cannot attach himfelf to them; and fometimes the heat has fo dried their fcorching faces, and covered them with duft of fo moveable a nature, that the wretched hunter who clambers up them is obliged to moiften them with his blood, by making large wounds in the foles of his feet and his legs." *Coxe's Letters on Swifferland*, p. 273. M. Ramond, the tranflator, adds, that the abbé d'Engelberg congratulated himfelf that he had loft only five men in one year by hunting the chamois; and his diftrict is, at moft, but a hundredth part of the Swifs Alps.

† One of my friends traverfing the mountains one day with a guide, the latter fhowed him a fpot where the year before he had remained more than fix hours with his thigh broken, and without any hopes of relief. A young fhepherd, by the greateft accident, heard the reports of his fufee, which he fired as a fignal of diftrefs, and at laft went to call affiftance. This accident happened to him while hunting the chamois, and yet it did not difguft him with the occupation.

‡ The wild goat of Siberia is no longer found in the Uralian Alps and the civilized countries of Siberia: it inhabits the fteepeft mountains of that chain, which unites with the Taurus between Eaftern Tartary and Siberia. It is found alfo in Eaftern Siberia beyond the Lena, and in the countries of Kamfchatka. It is believed likewife that it frequents the defert forefts bordering on the middle country of the Tongufians, to the eaft of the Jeniffee and beyond the Lena; but thefe animals are every where few in number, and frequent the moft inacceffible places. *Spicil. Zool.* fasc. xi. p. 32. 52. The old goats in that country are called *taeghé* or *teke*; the females *kinna*, and the kids *bitfchiga*. But M. Pallas thinks that thefe

a few ſlight characters, which, however, are not ſufficient to make them two diſtinct ſpecies. The former has always a very large beard; while that of the latter is very ſhort, and ſometimes it has none. The hair of the Siberian wild goat ſeems, in general, to be longer and thicker than that of the Alpine wild goat; but the moſt ſtriking difference, though not very ſpecific, is that found in the horns. The anterior face of the horns of the Siberian wild goat is not ſo flat, it is more convex; and is not comprehended between two longitudinal ridges, as in the wild goat in queſtion: the tranſverſal knotes are not terminated by a ſalient tubercle on the internal border of the face. In other reſpects, theſe horns have a perfect reſemblance.

The Cretan wild goat, according to Belon, is certainly of the ſame ſpecies as the Alpine wild goat: but it appears that there are two kinds of it, one of them perhaps the *ægagrus* *.

The chamois is much more common than the wild goat, though it is hunted ſtill more; but it is more widely diſperſed, as it inhabits the leſs elevated mountains; beſides, being more reſtleſs and ſhier than the wild goat, it is more difficult to catch it. It is found in all the high mountains of Swiſſerland, the Valais, and Savoy.

M. Perroud has given a very good account of the manners of this animal, which has been inſerted in Buffon's Natural Hiſtory †: we ſhall add to it a few obſervations. We are aſſured that in the mountains there are two ſpecies of the chamois: one, which is much ſmaller than the other, keeps in the higher diſtricts, and aſcends to the ſame elevations as the wild goat. The large ſpecies, on the other hand, frequent the leſs elevated regions, and live chiefly in the woods. Theſe two ſpecies, however, are only two varieties, or rather two conſtant races, which can mix and produce. The large chamois have two holes behind their horns, which are not found in the ſmaller kind. Theſe holes penetrate to the organs of the head. The males of this large ſpecies weigh from 80 to 100 pounds, and the females from 60 to 70; whereas the males of the leſſer ſpecies weigh only 60 or 70 pounds,

theſe names are corrupted from thoſe which the Moguls and the Calmucks give equally to the wild goat and the domeſtic goat. The latter call the male *takja*, the female *jama*, and the kid *eſſiga* or *idſhija*. The Chineſe alſo call the male wild goats *takia*; and they employ their horns, as the Turks do, for their war-bows. The ſhavings of theſe horns are uſed in medicine. *Id.* p. 50.

* We found two different kinds, as we made appear by the diverſity of their horns, brought from Cyprus and Crete, which we preſented to M. John Choul, bailiff of the mountains of Lyons, &c.—*Belon*, fol. 14.

† Vol. xii. p. 160.

and the females no more than 40 or 50. The large kind are fond of being alone, and in small flocks. Their limbs and body are shorter, and consequently thicker in proportion than the small kind, which are longer and slenderer.

The males can produce at the age of two years. The rutting time is in November and December, and they bring forth their young in June. The large chamois bring forth sometimes in May. They generally produce one at a time, and rarely two. These animals are hunted in the same manner as the wild goats, except that dogs are sometimes employed. They are always in fear; have their eyes and ears on the watch. They never feed in tranquillity. They crop a little grass, then raise their heads; and while they eat, look on all sides around them: if one of them perceives any thing, the whole flock take to flight; whereas a flock of wild goats all whistle before they set off. In regard to the sentinel which, it is said, they post to give them notice of danger, it appears to be a mere fable, founded on the timidity of these animals. Both races of the chamois exist in the mountains of the country of Hasli, of Grindewald and Lauterbrun, as well as in those of Savoy and the Valais. It might here be proper to examine, with Messrs. Buffon and Pallas, to what wild animal the goats are indebted for their origin; whether to the wild goat of the Alps, as Buffon thinks, or to the *capra ægagrus*, according to Pallas; and whether the chamois and Alpine wild goat are the same, or two distinct species. These interesting questions would require to be examined at full length *, but in the mean time I shall mention my own opinion. I have endeavoured to examine those of Buffon and Pallas with that impartiality which every one who sincerely searches for truth ought to employ, and especially when he discusses the opinions of men so justly celebrated. I agree therefore with Buffon, that the Alpine wild goat ought to be considered as the origin of all the goats. The conformity of their manners and figure, and the circumstance of these breeds intermixing and producing, are sufficient proofs of it. In my opinion, the *capra ægagrus* and the capricorn are varieties in the species of the wild goat, and have concurred with it to form the different races of our goats. But, I think, with M. Pallas, that the chamois cannot be of the same species as the wild goat, since they do not mix in a state of nature, and I consider it as an intermediate species between the goats and the antelopes.

* I discussed this question in the natural history of the jackall, which appeared in the *Journal de Physique* for November 1786.

XLII. On

XLII. *On the Spiral Vibration of a Stick or Rod. By* G. F. F. Chladni *.

THE vibrations hitherto known of a stick or rod, that is to say, of an elastic body extended in a certain direction, are either transversal vibrations, where the rod or the parts into which it divides itself vibrate sidewards, in such a manner as to form a great number of curved lines; or longitudinal vibrations, where the rod or each of its parts contracts and extends itself according to the direction of its length, so that it sometimes rests against one and sometimes against the other of the vibration nodes. The former kind of vibrations were first determined by Daniel Bernoulli, and afterwards, in a more complete manner, by Euler, in the Transactions of the Imperial Academy of Sciences for 1769: but the latter were first made known by myself in a paper on the longitudinal vibrations of rods and strings, published at Erfurt in 1796, and which may be found also in the Transactions of the Electoral Academy of Sciences of that city. For some time I have observed, that, besides these two directions of vibrating motion, there is a third, where the rod, or each part into which it divides itself, turns alternately to the right and left in a spiral form, while the vibration nodes or the boundaries of the vibrating parts remain motionless, as in the other kinds of vibration. Such spiral vibrations may be produced with most ease on a sufficiently long cylindric rod, the surface of which has been made as smooth as possible, if the rod be held gently between two fingers at the place where there is a vibration node, and if it be rubbed in a spiral direction in a vibrating part with a piece of woollen rag held between the fingers of the other hand. When the rod is of wood or metal, the rag must be previously strewed over with resin; but when a glass rod, or, what is the same thing, a thermometer or barometer tube is employed, it must be moistened with water, or be strewed over with very fine sand. The ways in which a rod, when it is either entirely free, or fastened at one end and free at the other, or fastened at both ends, can divide itself into vibrating parts, and the position of the vibration nodes, are entirely the same as in the case of longitudinal vibrations. The sharpness and flatness of the tone depend also on the same laws; and therefore, to avoid all unnecessary prolixity, I shall refer to the above-mentioned essay

* From *Der Gesellschaft Naturforschender Freunde zu Berlin Neue Schriften*, vol. ii. 1799.

on

on longitudinal vibrations. There is this difference, however, that, as far as I have obferved, the tone, when the vibrations take place in a fpiral direction, is a fifth lower than when the rod vibrates in a longitudinal direction under the fame circumftances.

By the fpiral vibration one phænomenon, which I have mentioned in my difcoveries refpecting the theory of found, but refpecting which I formed an erroneous opinion, can be explained. On a prifmatic rod, one end of which was faftened in a vice, when I rubbed one of its edges, in a diagonal direction, with a violin bow, and ftrewed fand over one of its horizontal fides, there appeared on this fide a line proceeding along its length, where the fand, which was thrown from the other parts by the vibration, remained at reft; and this appearance took place on each fide of the rod when held horizontally. The reafon of this is, becaufe at the edges which are further diftant from the axis the range during the vibrations is greater than in the middle of each fide; and on this account the fand which is thrown from the nearer places to thofe on the edges muft accumulate themfelves longitudinally in the middle of each fide neareft the axis where the vibrations are weakeft.

XLIII. *Obfervations on Maddering; together with a fimple and certain Procefs for obtaining, with great Beauty and Fixity, that Colour known under the Name of the Turkey or Adrianople Red.* By J. M. Haussmann.

[Concluded from p. 175.]

The Procefs.

After making a cauftic ley of one part of good common potafh diffolved in four parts of boiling water, and half a part of quicklime, which I afterwards flaked in it, I diffolved one part of powdered alum in two parts of boiling water; and while this folution of fulphate of alumine was ftill warm, to avoid re-cryftallization, I fpeedily poured into it fucceffively, always ftirring it without interruption, the above-mentioned cauftic ley, till the alumine it had at firft precipitated after faturation to excefs with fulphuric acid had been rediffolved. I left at reft this folution of alumine, which exhaled ammonia, and which, on cooling, formed a precipitate of fulphate of potafh in very fmall cryftals. I then mixed a thirty-third part of linfeedlinſeed-oil, with which the alkaline ſolution of alumine formed a kind of milky liquid *. As the oil gradually ſeparates itſelf from this mixture under the appearance of cream, it muſt not be employed till it is again ſhaken. The ſkains of cotton or linen ought to be ſucceſſively immerſed in it, and equally preſſed, that they may be then expoſed to dry on a pole in the order in which they have been taken from the mixture. They muſt be dried under ſhelter from rain in ſummer, and in a warm place in winter, and be left in that ſtate for twenty-four hours: they muſt then be waſhed in very pure running water, and be again dried; after which they are to be immerſed in an alkaline ley, preſſed and dried a ſecond time in the ſame manner as the firſt, taking care, however, to recommence the immerſion in the ley with thoſe ſkains which have been laſt in the oily mixture, becauſe the firſt never fail to carry away a larger portion than the laſt: it will be proper alſo to conſume the mixture each time, that it may not have leiſure to attract the carbonic acid with which the lower region of the atmoſphere is always charged, eſpecially in manufactories; for the alkali, by paſſing to the ſtate of carbonate, ſuffers the alumine to be precipitated, and loſes the property of mixing with the oil.

Two immerſions in the alkaline ſolution of alumine mixed with linſeed-oil will be ſufficient to obtain a beautiful red; but, by continuing to impregnate the ſkains a third and even a fourth time with the ſame circumſtances as the firſt, colours exceedingly brilliant will be produced.

The intenſity of the red propoſed to be obtained will be in proportion to the quantity of the madder employed. By taking a quantity of madder equal in weight to that of the ſkains the reſult will be a red, which, by clearing, will be changed to a roſy ſhade: on the other hand, ſhades of crimſon, more or leſs bright, will be obtained by employing two, three, and even four times the weight of madder, without ever forgetting the addition of chalk, if the water employed does not contain ſome of it. Four parts of this colouring ſubſtance will produce a red too intenſe and beautiful to be employed in commerce, as it would be too dear to find purchaſers.

By making an oily alkaline ſolution of alumine with two or three parts of water, and impregnating the ſkains twice, and even four times, in the manner above mentioned, bright ſhades will be produced without the uſe of much madder; but they will not have the ſame intenſity as thoſe procured

* In fact, a ſaponaceous liquor is formed containing alumine.—Edit.

with even as little madder by means of the ſame ſolution concentrated.

The beſt method of obtaining ſhades lively as well as bright, is to expoſe the dark reds for a conſiderable time, when they have been cleared, to the action of a ley of oxygenated muriate of potaſh, or of ſoda with exceſs of alkaline carbonate, in order to have ſuch a degree of ſhade as may be required: but it may readily be conceived that this method would be expenſive.

To have the oily alkaline ſolution of alum nearly in the ſame ſtate of concentration, it will be neceſſary to employ an areometer to determine the degree of ſtrength of the cauſtic ley before it is employed for the ſolution of the alumine. This cauſtic ley muſt be made with the beſt common potaſh that can be procured, and the degree it gives by the areometer muſt be noted, in order that, if potaſh of an inferior quality be afterwards employed, the ley obtained may be carried to the fixed degree by evaporation.

Cauſtic ley made with four parts of good common potaſh * cannot contain a large quantity of foreign ſalts. By making it on a large ſcale, when the limpid part has been decanted, it will be neceſſary to ſhake the depoſit, for ſome time, twice every day, that the reſt of the alkaline liquor may be decanted; and that none of what ſtill remains in the depoſit may be left, it ought to be diluted with more water, which may be afterwards employed to lixiviate the cotton, which muſt be well purified and cleaned before it is dyed; which may be done by lixiviating and ſoaping, or merely boiling it in water and then rinſing and drying it. As wringing with the hands may derange the filaments of the ſkains of cotton and linen, and conſequently weaken the thread, it will be proper, in operating on a large ſcale, to ſqueeze them by means of a preſs.

In regard to thread or linen to be dyed of a beautiful dark and fixed red, it muſt be well bleached, and impregnated at leaſt four times ſucceſſively with the oily alkaline ſolution; becauſe, not only alumine and metallic oxides adhere with more difficulty to linen than to cotton, but becauſe theſe mineral ſubſtances, when coloured, abandon linen much eaſier than cotton when clearing. It ſtill remains to examine whether, between each impregnation with the oily alkaline ſolution of alumine, cotton or linen thread requires to be left at reſt for a greater or ſhorter time before it is rinſed and dried.

* I have no doubt that, where potaſh cannot be procured, ſoda might be employed.—THE AUTHOR.

All

All fat oils may be employed in the mixture with proper precautions; but linfeed-oil mixes better, and remains longer fufpended in the alkaline folution of alumine: I never tried fifh-oil, which, perhaps, would be preferable. It is probable alfo, that in operating on a large fcale, it would be beft to diminifh the quantity of linfeed-oil in the mixtures with the alkaline folution of alumine; for I have had reafon often to obferve that too much oil hurts the attraction of the colouring parts of the madder: a thirty-third part of linfeed-oil always produced the beft effect in my trials on a fmall fcale.

In regard to the procefs of dyeing cotton and linen thread, fufficiently charged with alumine, by the oily alkaline folution of that earth, the fkains muft firft be difengaged from every faline fubftance, as well as from the fuperfluous oil, by rinfing them a long time in very pure running water; after which they muft be arranged, without drying them, on an apparatus, which the operator may conftruct according to the form of the boiler,—in which it is to be placed in fuch a manner, that during the procefs of dyeing the fkains may be continually fhaken and turned, in order to catch every where, and in an uniform manner, the colouring particles. The bath muft be compofed of madder, mixed with a fixth of pounded chalk, and diluted with about 30 or 40 parts of water. The heat muft be carried only to fuch a degree that the hand can be held in the bath for an hour without being fcalded; and it is to be maintained at this degree for two hours, either by diminifhing or increafing the fuel. Three hours dyeing will be fufficient to exhauft the madder: the fkains when taken from the bath muft be wafhed in a large quantity of water to cleanfe them; they are then to be cleared by boiling them a pretty long time in water containing bran inclofed in a bag, adding foap and alkaline carbonate to give the red a rofy or carmine fhade.

As I never had occafion to dye cotton or linen thread on a large fcale, I employed a fmall boiler, which ferved me at the fame time for the procefs of clearing: in the latter operation I confined myfelf to boiling the fkains, properly arranged, in water containing a bag filled with bran, for eight hours fucceffively; and, that I might not interrupt the ebullition, I replaced the evaporated part by the addition of more boiling water. In this clearing I employed neither foap nor alkali; yet I obtained a red fuperior in beauty and fixity to that of the Levant, and which, in every refpect, will bear a comparifon with the beft colours dyed in France.

For dyeing my red, I employed three parts of the beft madder for one part in weight of dry cotton thread.

With the precaution I took to obtain an uniform fhade I could have dyed at one time, but I fhould always recommend performing this operation at two different times, taking each time half a portion of madder and of chalk, if the fkains cannot be continually turned in the boiler: it may ferve alfo for clearing, by adapting to it a cover fo as to fuffer very little of the vapours to efcape, becaufe it would be too expenfive to replace the part evaporated by more boiling water. By operating on a large fcale, and concentrating the heat in the boilers, keeping them almoft clofe, there, perhaps, would be no need of employing eight hours ebullition to clear and fix the colours. I have every reafon to believe that this clearing of the Turkey red gave rife to the idea of bleaching with fteam: it muft have been feen that colours by being cleared lofe confiderably in regard to their intenfity; and perhaps it has been obferved at the fame time that the packthreads employed for arranging the fkains were bleached during the clearing, efpecially when alkalies were added.

A great variety of colours and of different fhades may be obtained by following the procefs here defcribed for obtaining beautiful and durable reds. In this cafe, the oily alkaline folution of alumine muft not be employed till the required fhade of oxide of iron or indigo blue has been given; but whatever may be the colour or fhade which you wifh to give, before you fix the alumine on the fkains of cotton or linen, thefe fkains muft always be firft well boiled, by which means the adhefion of the indigo fecula as well as that of the oxide of iron will be increafed in the fame manner as that of alumine coloured by the colouring parts of madder when fubjected to the action of the heat of boiling water before they are impregnated with the oily alkaline folution of alumine. As the method of dyeing indigo blue in all its fhades is well known, it is needlefs to detail it; and as to giving a rufty yellow colour, which may be done at little expenfe, nothing is neceffary but to moiften the fkains well with a folution of the fulphate of iron, to prefs them equally, and then to immerfe them in a cauftic ley of potafh, which will precipitate and fix the oxide of iron of a difagreeable colour, but which will not fail to affume a rufty yellow fhade by attracting and becoming faturated with the oxygen of the atmofphere: thus yellow will be more or lefs dark according to the quantity of the fulphate of iron in folution. More intenfity and even more equality may be given to the rufty yellow by moiftening the fkains a fecond ſecond time in the ferruginous ſolution, and immerſing them in the cauſtic ley. Care, however, muſt be taken not to uſe ſoda for this operation, becauſe it generally contains ſulphur, which blackens oxide of iron by mineralizing it.

The ſkains coloured blue and ruſty yellow, treated with oily alkaline ſolution of alumine, will produce, by maddering, dark purple and chamois colours, violet, lilac, puce, mordoré, &c. It may be eaſily conceived, that if, inſtead of maddering, the ſame ſkains prepared for maddering be dyed with kermes, cochineal, and Brazil wood, logwood, wood of St. Martha, woad, yellow wood, quercitron, yellow berries, &c. a great variety of colours will be obtained: the ſhades may even be varied *ad infinitum* by mixing the colouring ingredients with each other in different proportions. The affinity of adheſion of the colouring parts of all theſe ingredients varies alſo to ſuch a degree, that the ſhades ariſing from a yellow or olive-green will be changed or totally metamorphoſed by a ſecond dyeing with madder, kermes, cochineal, or Brazil wood; and will furniſh orange ſhades, capucine, carmelite, burnt bread, bronze, &c. As the preliminary preparation of the ſkains by the oily alkaline ſolution of alumine might be too expenſive for ſome of theſe colours, the proceſs I deſcribed in the *Annales de Chimie* for the year 1792, p. 250, may be ſubſtituted in its ſtead. This proceſs conſiſts in treating the ſkains alternately with ſoap and ſulphate of alumine, the exceſs of the acid of which has been ſaturated with one of the alkaline carbonates or with lime: this method is very expeditious. In the courſe of a day, eſpecially in ſummer, the ſkains may be prepared and dyed red as well as other colours; which, for the moſt part, may be ſubjected to ebullition, and will bear clearing with bran for a quarter or half hour, and even ſome of them for a whole hour. It is alſo to be obſerved that there are none but madder colours, the alumine and oxide of iron baſes of which have been fixed on the ſtuffs by means of the oily alkaline ſolution, that can acquire perfect fixity by the action of heat of boiling water; and that the fixity is very inferior in all madder colours the earthy and ferruginous baſes of which have been applied to ſtuffs by means of acid ſolvents.

Alumine, fixed in abundance on cotton or linen ſtuff by means of a highly concentrated alkaline ſolution, attracts very eaſily the colouring parts in the proceſs of maddering. The caſe is not the ſame when the ſame earth is applied by the moſt highly concentrated acetic ſolution of alumine; and it is abſolutely impoſſible to finiſh maddering at one time, even when a profuſion of madder is employed, and the opera-

tion

tion is repeated three and even four times *. This circumstance will give rise to new and interesting experiments; but my observations prove in the mean time that maddering, in general, requires to be managed with the nicest attention.

XLIV. *A new Method of separating Silver alloyed with Copper in Bullion. By* C. Napione †.

METALLURGISTS well know that argentiferous copper, which does not contain half its weight in silver at least, cannot be subjected to cupellation. In that case, recourse is had to an operation, called *liquation*, with lead; but, unhappily, it is attended with inconveniences which in many cases render it impracticable.

To extract silver from copper completely, in one liquation, practice has shown in the founderies where this operation is performed on a large scale on black copper, that for a quintal of the latter there must be at least 70 or 80 pounds of copper, and more than 8 or 9 ounces of silver; and in this case the proportion of lead to the copper ought to be 11 parts of the former and 3 of the latter.

But as in *billon* (base) money the silver generally forms from 1-12th to 1-4th of alloy, and sometimes more, it is evident that several liquations must be repeated to impoverish the copper; for though in the first the lead carries with it a great deal of silver, it is not always in the same proportion, and the latter portions are more difficult to be extracted.

If we suppose that four successive liquations are sufficient to extract the silver of our *billon* money, which is of the quality of 3 deniers 10 grains, estimating it at the lowest, we should have 55 livres for the whole expense of the separation for each quintal of that money.

If the process of the celebrated Cramer, of which M. Jars has given a short account in the third volume of his *Voyages Metallurgiques*, can be attended with any advantage, it would, however, be of very little use to us.

C. Napioné, knowing that sulphur has a greater affinity for copper than for silver, conceived the idea of reducing into scoriæ a large quantity of rich copper by combining it directly with sulphur, in the hope of obtaining, by this kind of dry *depart*, the silver concentrated in a portion of the copper

* Concentrated acetic solution of oxide of iron is attended with nearly the same difficulties.

† From the *Annales de Chimie*, No. 122.

ſo as to be able to refine it immediately by cupellation, and to obtain at the ſame time ſcoriæ, leſs rich, for amalgamation.

The experiments which he relates prove that he was not deceived in his expectation. He took a pound and ſix ounces of argentiferous copper of the quality of 3½ deniers the marc, and, having fuſed it in a crucible, mixed with it two ounces of ſulphur. Having ſeparated the *matte* or ſcoriæ, he repeated the ſame operation twice on the *cupreous regulus*; and after the three operations he obtained 1 pound 5 ounces and 22 deniers of *matte*, and 4 ounces 16 deniers and 12 grains of metallic regulus, which, though pretty ductile, ſplit at the edges when flattened with the hammer.

The trial made on *mattes* or ſcoriæ of three meltings united, ſhowed that they contained only 11 pounds 3 ounces of ſilver in the docimaſtic quintal, and the quality of the regulus was 7 deniers 16 grains per marc; a quality at which it may be ſubjected to cupellation without any other proceſs.

After having well pulverized the *mattes*, and mixed with them muriate of ſoda in the proportion of 12 to 100, and the ſame quantity of quicklime, and after calcining it for four hours under a muffle, he amalgamated the mixture. The reſult of this amalgamation and of a ſubſequent one procured him only a very ſmall quantity of ſilver. He again calcined it, for three hours and a half, with 1-10th muriate of ſoda; and after three calcinations of the ſame kind under the ſame circumſtances, and three alternate amalgamations, his *mattes* contained no more than 1 ounce and 18 deniers of ſilver per quintal.

C. Napioné obſerves, that if his firſt calcination had been carried further, he could have extracted the whole of the ſilver in three amalgamations. Having repeated, indeed, the ſame operation at the mint, employing the ſame proportion of ſulphur, he was able, after three ſucceſſive calcinations and three amalgamations, to obtain a reſiduum, which contained only an ounce and a half of ſilver per quintal. The ſilver extracted on this occaſion was of the quality of 11 deniers and 22 grains. It was of importance, after this laſt trial, to undertake the experiments on a great ſcale.

On a hearth prepared with light *braſque* *, as for the refining of common copper, and which C. Napioné had placed in a ſituation where there was a current of air, a baſon was dug capable of containing about a quintal and a half of melted matter: the chimney was diſpoſed in ſuch a manner,

* A lining or coating of a mixture of clay and pounded charcoal.—Edit.

that

that a proper degree of heat could be given to the bason and the fused metal. The *brasque* being very dry, a quintal of our *billon* money was fused in the bason through charcoal; and the charcoal being then removed from the top, a workman, furnished with a long iron spoon, threw sulphur on the melted mass, while another workman stirred the matter with a clay rod. In proportion as sulphuret of copper was formed, the surface of the mass was sprinkled with a brush dipped in water, and another workman, with an iron fork, removed in plates the *mattes* as they became fixed. There was obtained in this manner, after the operation, a cupreous button, and *mattes* nearly in the same proportion, and containing the same quantity of silver, as in the trials on a small scale. A quintal and a half of matter was thus fused at once, and it is probable that a greater quantity might be fused in the same manner.

The *mattes*, well pulverized, were subjected to calcination with muriate of soda and lime in a cupelling furnace arranged for that purpose.

After a calcination of ten hours, the matter was amalgamated in one of those mills commonly used in the mint for extracting silver and gold from the refuse of different operations. After the first amalgamation, which lasted only 14 hours, the third part of the silver contained in the oxide was extracted; and, as it is in the second amalgamation that the greater part of the silver is attacked by the mercury, we may conclude, without any danger of being deceived, that three amalgamations will be more than sufficient to extract, on a large scale, the whole silver of the *mattes*.

These mills, which contain from 26 to 30 pounds of matter, may be put in action, to the number of forty, by a single water-wheel: besides this advantage, their capacity may be augmented.

By the process of C. Napioné, not only silver but the gold united with it may be extracted from argentiferous copper; and this may be done by one liquation.

As the water of the washing of the first amalgamation contains a great deal of sulphate of copper, the metal may be extracted from it by cementation with iron, or by precipitating it by carbonate of potash, which will produce cupreous carbonate of a bright green colour. The same water afterwards evaporated gives sulphate of soda in abundance.

The water of the second washing and the third amalgamation is colourless; and there may be extracted from it, with advantage, by evaporation, muriate and carbonate of soda.

The oxide of copper freed from the silver may be sent to the

the fmelting-houfes at the copper-mines to be reduced along with the ores, and refined by the common method.

C. Napioné, having calculated the expenfe neceffary for his procefs when executed on a large fcale, concludes that, in the prefent circumftances, it will never amount to a fifth part of that which refining by common liquation requires.

XLV. *Defcription of the Duke of Bridgewater's Drain Plough* *.

SIR,

I AM favoured with yours of the 15th inftant. The model of the guttering-plough fent you, was a prefent to the Society from his Grace the Duke of Bridgewater, made by Robert Tomlinfon, one of his Grace's conftant workmen.

I procured the following account this morning from the duke's farmer:—In clay or ftiff land that lies flat, the plough cannot go too deep; but if it lies on a declivity, about five inches deep is fufficient. In foft light foil, the plough fhould go as deep as it can in all fituations, becaufe the fides moulder into the gutters. The beft time of draining is about Michaelmas, or as foon as the grafs is eaten; and the whole fhould be accomplifhed betwixt that time and Chriftmas.

In clay ground that has never been drained, fix good horfes will be requifite to draw the plough. In every following year the plough fhould be run through the fame gutters, and four horfes will then be fufficient. I am, Sir,

Your moft obedient fervant,

THOMAS BURY.

P. S. The fhare of the plough muft be well fteeled, and fhould be ground fharp.

Worfley,
September 18, 1800.

Mr. Charles Taylor.

*** At Broughton, in the neighbourhood of Manchefter, confiderable quantities of ftiff clay, pafture, and meadow land, have been much improved, under the infpection of the Secretary of the Society, by the ufe of this plough.

After the cattle were houfed for the winter, three horfes

* From the *Tranfactions of the Society for the Encouragement of Arts, &c.* 1801.—The thanks of the Society were this feffion voted to his Grace the Duke of Bridgewater for a model of this drain plough, prefented by him to them, and fent with the above communication from his Grace's agent, Mr. Thomas Bury, of Worfley Mills, near Manchefter. The model, which is made upon a fcale of one inch to a foot, is placed in the Society's repofitory for public infpection.

were

were employed to form drains with the plough at proper intervals: the ſmall drains were made at the diſtance of about nine yards from each other, in old furrows of the ground, and about five inches deep: the ſod, when cut out by the plough, was of a wedge-like form, and turned out by it upon the ridges of the land, entirely ſeparated from the drain or gutter. Theſe ſods were afterwards divided acroſs, by a ſpade, into lengths of about two feet each, then toſſed by a pitchfork into a cart, and placed in a heap in the field, along with ſtrata of quicklime in a powdery ſtate: the whole maſs was reduced to a compoſt by the froſt during the winter, and in the following ſpring was laid upon the ſurface of the land, and formed an excellent top-dreſſing.

The water from the ſmall drains was directed into larger drains, made by lowering the ſhare of the plough to the depth of nine or more inches. Little or no loſs of land aroſe from the ſmall drains, as natural graſſes were produced therein early in the ſpring. It will be highly advantageous to repeat the operation every winter: it is eaſily and expeditiouſly performed; and no perſon, without an actual experiment of the fact, can form a ſufficient judgment of the great benefit ariſing to vegetation by the removal of cold ſtagnant water, during the winter, from land of every deſcription.

Deſcription of the Duke of Bridgewater's Drain Plough, (Plate VII. fig. 1.)

AB, the beam of the plough.

CD, the handles,

E, the ſhare or ſock.

F, the coulter, or firſt cutter of the ſod; which coulter is fixed to the ſhare.

G, the other coulter, or ſecond cutter, which ſeparates the ſod from the land, and directs it through the open ſpace betwixt F and G. This coulter is connected with the ſhare and the beam.

H I, the ſheath of the plough.

K, the bridle or muzzle, to which the ſwingle-tree is to be fixed.

LM, two wheels of caſt iron, which may be raiſed or lowered by ſcrews at N preſſing upon the flat irons OO, to which the axis of each wheel is fixed. Theſe wheels regulate the depth which the ſhare is to penetrate into the earth.

P, a chain with an iron pin, to move the ſcrews at O.

XLVI. *De-*

XLVI. *Description of Mr.* Knight's *Drill Machine for sowing Turnip-seed**.

SIR,

I HAVE sent you a small instrument for sowing turnips, which I have tried on several different soils, and think I can venture to assert, that it will sow the seed and cover it perfectly well in any soil that is nearly in a proper state to receive it. It is necessary either to harrow the ground *across*, or to roll it, previously to the instrument being used, that the labourer may see the rows he has made: but I have always found the crop to succeed better after the roller than after the harrow, though the ground has been very strong.

The instrument is so extremely simple in its construction, that it is almost unnecessary to give a description of its mode of action; but as parts of it may probably be broken in carriage, I have added the following sketch: See Plate VII. fig. 2. —A, the iron wheel, which, running on its edge, formed by two concave sides, makes the groove into which the seeds fall. I have sometimes used a wheel with straight sides; but I think that concave sides, when well executed, are to be preferred in strong soils, and indeed in any soil. B is a wheel moving on the same axis with A, and turning the wheel C (which gives out the seed) by means of a strap. I have several sizes of the wheel B, in order to increase or diminish the rapidity of C; and consequently to sow more or less seed. D, the tube through which the seed passes, and falls into the channel made by the iron wheel. E, the feet of the instrument. F, six lengths of jack-chain; which I find cover the seed remarkably well. The chain is perhaps preferable to any kind of harrow, because it can never become encumbered by loose straw, which is almost always found on or close to the surface, when the ground has been manured; and the iron cutting-wheel has a similar advantage over any kind of share. G I, the seed-box. H H, the handles of the machine.

The labour of using the instrument is very small. My workman usually accomplishes four statute acres, or something more, in a day; and last night, with the one I send, he sowed an acre and a half after six o'clock in the evening. There are two holes before the axis of the great wheel, to receive two pieces of cane, which point out the proper width

* From the *Transactions of the Society for the Encouragement of Arts, &c.* 1801.—The silver medal was this session voted to Thomas Andrew Knight, Esq. of Elton, near Ludlow, for this invention. A complete machine was presented by him, and is placed in the Society's repository.

of the intervals between the rows. I ufually place my rows at eighteen or twenty inches' diftance; and I wifh my plants to ftand not more than fix inches apart in the row; for I find that three fmall turnips weigh about as much as one large turnip, are more folid, and I think more nutritious, and certainly are much lefs apt to fuffer by unfavourable weather. The ground between the rows is, of courfe, worked with the hoe. Should the Society, at their next meeting, approve of the inftrument, and will afford it a place in their repofitory, I will beg them to accept it; if not, I will requeft you to return it at your leifure. I am, Sir,

Your moft obedient fervant,

THOMAS ANDREW KNIGHT.

Elton, near Ludlow,
June 22, 1800.

Mr. Charles Taylor.

N. B. The angle which forms the edge of the wheel A, muft be made more or lefs acute, and the inftrument more or lefs heavy, proportional to the ftrength of the foil. I have fometimes added weights of lead over the axis of the wheel, but it will rarely be found neceffary. I have tried the inftrument on different foils, and I think it will anfwer on any. A great advantage may be derived by fowing turnips with it, at a time when horfes, now commonly ufed for the fame purpofe, are engaged in other employments. A few days are frequently of importance in fowing turnips, which by fortunate rains have got a wonderful ftart of thofe which have been fown a day or two later.

Fig. 3. is a fection, on a larger fcale, of the feed-box G, in fig. 2. The wheel marked C, is alfo the fame as in that figure: it is fixed upon the axis of the cylinder I, which is pierced upon the furface with holes at K, for the feed. This cylinder turns round within a groove at the bottom of the box, and is fo well fitted therein, that no feed falls from the box but what is delivered by the holes K. A fmall brufh, marked L, rubs againft the cylinder, to clear out any feeds which may remain in the holes.

The feeds fall into the tube underneath the cylinder, and from thence into the channel made by the indenting rim of the iron wheel.

The loofe chains which follow, cover the feeds with earth, as before mentioned.

Fig. 4. a front view of the wheel, exhibiting its edge.

XLVII. *Notices*

XLVII. *Notices respecting New Books.*

Manchester Transactions.

THE Second Part of Vol. V. has just made its appearance. The following are the contents:

On Tragedy, and the Interest in Tragical Representations: an Essay. By the Rev. George Walker, F. R. S. and Professor of Theology in the New College, Manchester.—Experiments and Observations to determine whether the Quantity of Rain and Dew is equal to the Quantity of Water carried off by the Rivers and raised by Evaporation; with an Inquiry into the Origin of Springs. By Mr. John Dalton.—Experiments and Observations on the Power of Fluids to conduct Heat; with Reference to Count Rumford's Seventh Essay on the same Subject. By Mr. John Dalton.—Experiments on the Velocity of Air issuing out of a Vessel in different Circumstances; with the Description of an Instrument to measure the Force of the Blast in Bellows, &c. By Mr. Banks, Lecturer in Natural Philosophy.—Essay on the Beautiful in the Human Form; and Inquiry whether the Grecian Statues present the most perfect Beauty of Form that we at present have any Acquaintance with. Communicated to the Society from a Correspondent through the Rev. George Walker.—A Defence of Learning and the Arts, against some Charges of Rousseau. In two Essays. By the Rev. George Walker, F. R. S.—Observations on the Nervous Systems of different Animals; on Original Defects in the Nervous System of the Human Species, and their Influence on Sensation and Voluntary Motion. By John Hull, M. D.—Experiments and Observations on the Heat and Cold produced by the Mechanical Condensation and Rarefaction of Air. By Mr. John Dalton.—Account of some Antiques lately found in the River Ribble. By Mr. Thomas Barritt.—Experimental Essays on the Constitution of mixed Gases; on the Force of Steam or Vapour from Water and other Liquids in different Temperatures, both in a Torricellian Vacuum and in Air; on Evaporation; and on the Expansion of Gases by Heat. By Mr. John Dalton.—A Review of some Experiments which have been supposed to disprove the Materiality of Heat. By Mr. William Henry.—An Investigation of the Method whereby Men judge, by the Ear, of the Position of Sonorous Bodies relative to their own Persons. By Mr. John Gough.—On the Theory of Compound Sounds. By Mr. John Gough.—Meteorological Observations made at Manchester.

chester. By Mr. John Dalton.—Appendix. I. Explanation of a Roman Inscription found in Castle field, Manchester. By Mr. Thomas Barritt: with a Note on the same Subject by Dr. Holme.—II. Note to Mr. W. Henry's Paper on Heat.

Ausführliche Geschichte der Theoretisch Praktischen Uhrmacherkunst, &c. A History of Clock- and Watch-making, both Theoretical and Practical, since the earliest Method of dividing the Day to the End of the 18th Century. By I. H. MORIZ POPEE, 1801. 8vo. 564. p. 8.

[Continued from p. 179.]

V. *The invention of clocks moved by wheels and weights, and their progressive improvement till the middle of the seventeenth century.*—The invention of clocks moved by wheels and weights, as well as the period of the invention, is totally unknown. The author, however, has collected the researches of learned men on this subject, and refutes the opinion of those who make the invention to be not older than the 14th century. The invention, however, was not very great, as the more perfect water-clocks were before that period furnished with wheels, so that the only improvement was the substitution of a solid body to act as a moving weight instead of water. The principal point was to produce uniformity in the action of these weights; and in this respect the first clocks moved by weights were deficient till the time of Huyghens. It is not therefore to be wondered at, that the application of weights to clocks as a moving power should excite so little attention as to be scarcely thought worth notice by cotemporary writers: water and sand were indeed more convenient than a solid weight, which requires too much room for its motion. The author is of opinion that the origin of clocks with weights is as old as the 11th century, and adduces very probable grounds in support of it; but it is not properly ascertained, whether the inventor was an European or a Saracen. The oldest complete clock moved by weights, of which there is any certain testimony, is that sent by the sultan of Egypt in the year 1232 to the emperor Frederic II, the value of which was at that time estimated at 5000 ducats. In the 13th century many of the church-steeples in Italy were furnished with clocks moved by weights that struck the hours. About the same time the well-known clock-house at Westminster-hall was furnished with a clock that struck the hours; the expense of which was defrayed by a fine imposed on one of the judges. The clock moved by weights of the abbot Richard of Wallingford, which by many has been considered as the oldest, was constructed in the 14th century. In the

6 year

year 1394 Padua obtained the first clock, supposed to have been constructed by the celebrated philosopher, physician, astronomer and mechanist James Dondi. That such clocks might be introduced into England, Richard III. gave a patent to three Netherlanders, in the year 1368. Courtrai in France had a clock that struck the hours so early as the year 1332: it was carried away in that year by Philip the Bold of Burgundy, and conveyed to Dijon, where it is still to be seen. The first large clock at Paris was erected in 1364 by a German artist, Henry von Wick, who received daily six French sous, together with free lodging in the tower of the Palais, to which the clock was removed in 1370. Bologna obtained the first public clock in 1356; Pavia, in 1402; Breslaw, by Schwelbelin, in 1368; Strasburg, in 1370; Augsburg, in 1398; Nuremberg, in 1402; and Venice, in 1497. The first clocks were expensive, and many cities, desirous of having such machines, were not able to raise money to purchase them. In the year 1523, the fines levied from the students at Oxford were employed to defray the expense of the clock erected on the church of St. Mary. Private individuals, however, about this time began to obtain clocks. The first instance of a clock with weights being employed for astronomical purposes occurs in 1484, when Walther, as he says, with a well regulated clock, made an observation of Mercury. Tycho had three of these clocks, which showed minutes and seconds: he found, however, that they were exposed to variations from the influence of the atmosphere and wind; on which account he caused to be constructed a quicksilver clock, in which distilled quicksilver, instead of sand and water, showed minutes and seconds. To produce an uniform fall, so much quicksilver dropped from another vessel into the hour-vessel as was sufficient to keep it always at the same height. As the study of astronomy required more accurate measurers of time, we are indebted to that science for the improvement of these instruments, the construction of which does so much honour to the human genius.

VI. *Invention of watches and [illegible].*—Hele of Nurnberg is generally considered as the inventor of watches or spring-clocks, as they ought properly to be called; the first of which he is said to have constructed in the year 1500. According to others, [illegible] of [illegible] was the inventor: but his first clock was constructed in 15[illegible], and consequently twenty years later than Hele; who, it is certain, made small spring-clocks or watches so early as 1500. Nurnberg and Augsburg were the first cities of Germany in which watches were made. Hele's watches not only indicated the hours, but struck them also. Andrew Heinlein, who trod in Hele's footsteps, made small clocks in the smelling-balls which were used in his time. In the architectural office at Augsburg there is a watch, above 200 years old, which strikes the hours: it is contained in a crystal case, and was made by Buschman. There were striking watches in France in the time of Louis XI. The oldest watch in England, that goes still pretty well, is of the year 1540, and is preserved in the palace of Hampton Court.

VII. *Invention of the fusee, pendulum, and spiral spring.*—The fusee was in all probability invented in England about the end of the 16th century, by whom is not known, and was thence introduced into Germany. It was examined geometrically by Varignon and De la Hire, in order to determine the most advantageous form; which, however, does not always avail, on account of the inequality of the spring, and therefore advantage is taken of other mechanical means: of this kind is the balance, by means of which the power of the spring can be adjusted, and which undoubtedly was invented in Swisserland about the beginning of the 18th century. In the first watches with fusees, the diameter was small, and the box large and broad. The catgut, by which it was wound up, passed eight or nine times around the fusee. This made the watch clumsy and ill-shaped. The catgut, however, was soon exchanged for a chain, which consists of small plates of steel united together with great labour. In the middle of the 17th century, Huyghens invented a better method of regulating the movement of clocks. In the year 1657 he applied the pendulum as a regulator to large clocks moved by weights; and, some years after, recommended the balance spring for watches. Before that period some had used, instead of the spoon-formed balance, one in the form of a ring, or employed a small fly wheel; and Hautefeuille used at first a swine's bristle, and then a weak, straight, steel spring, for regulating the balance. In the year 1674 Huyghens caused a watch with a spiral balance spring to be constructed by Turet at Paris. Dr. Hook entered into a dispute with Huyghens in regard to this invention, and proved that he had invented such a watch for Charles II., which had the inscription *Robert Hook invenit* 1658, *Tompion fecit* 1675; and that he had solicited a patent for such watches in 1660, but did not obtain it till 1675.

[To be continued.]

XLVIII. *Pro-*

XLVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

On the 1st of April a short notice was communicated to the Society of some observation, made by M. Bode of Berlin, relative to the Ceres Ferdinandea. He makes the opposition of that planet to have been on the 17th of March last at $4^h\ 36'$ mean time.

The rest of the evening was taken up in reading the remainder of Count Bournon's paper on corundum, noticed in our last. The reading was continued on the 8th, but without getting through the paper, of which, from its nature, being chiefly a description of the forms of crystals, we can give no account in a short notice.

On the 15th and 22d there was no meeting, owing to their falling in Passion week and the Easter holidays.

FRENCH NATIONAL INSTITUTE.

Account of the labours of the Mathematical and Physical Sciences during the second quarter of the year 10.

Mathematical Part, read by C. Delambre, secretary.

Astronomy—Piazzi's Planet—New Lunar Equations.

About the beginning of December Baron Von Zach observed Piazzi's star in a group of others; but, having nothing to distinguish it from them, he was not able to find it again, and to assure himself that he had seen it, till towards the end of that month. Dr. Olbers had observed it also at Bremen about the same time. The difficulty which astronomers experienced in finding it, must be ascribed to its smallness and the unfavourableness of the weather; for, according to the elements which M. Gauss deduced from the observations of Piazzi, an instrument could be easily directed to the altitude of this star, and in two minutes it would have been in the middle of the telescope. Before we received this intelligence from Germany, we endeavoured to take advantage of every moment when the sky was favourable. C. Mechain, whose researches were less interrupted, and who had observed all the small stars among which the planet could reappear, perceived it, for a moment, on the 23d of January; but day having surprised him too soon, he could only examine in haste the configuration of the stars which occupied the field of the telescope. On the 24th, however, he examined it with more accuracy. On the 25th he communicated to us his observation. There remained then no more difficulty. In the evening, when the planet was difengaged from the vapours of the horizon, I obferved it with a parallactic inftrument, and followed it for fix hours without interruption to affure myfelf of its motion. By a great deal of attention I was able to fee it on the meridian, where I miffed it the preceding evening in confequence of foreign circumftances. After that period we followed it clofely, and always obferved it when the weather would admit; that is to fay, about five times in thirteen days; for from the 25th of January to the 31ft of March, I could fee it only twenty-fix times on the meridian. It now paffes a few minutes before the Georgium Sidus; and this proximity facilitates a comparifon of the two planets with which aftronomy has been enriched in our time. That of Herfchel appears as a beautiful ftar of the fifth or fixth magnitude, and may be eafily diftinguifhed by the naked eye. That of Piazzi efcapes the beft eye, even when it attempts to find it in the place indicated by the telefcope, in which it has been obferved. It is indeed only of the feventh magnitude: owing, however, to an union of circumftances in this feafon, it appears in all its fplendour; for it is at its neareft diftance to the earth, and at a fmall diftance from its perihelion. It was much lefs brilliant when found: it can never now be loft.

C. Flaguergues, affociate, has fent us obfervations he made at Viviers. Among thefe is a new determination of the latitude of that place, which he has found to be 18″ further north than marked in the *Connoiffance des Tems*; alfo, obfervations of feveral fpots, and particularly two which have reappeared at the fame place after one or two revolutions of the fun around his axis.

C. Laplace has communicated to the clafs his labours for improving the theory of the moon. He has announced that, inftead of one equation of a long period, of which there was fome reafon to fuppofe the exiftence, he has difcovered two. This complication rendered the labour more difficult. Befides, thefe analytical calculations are fo embarraffing, and depend on fubftitutions fo delicate, that it is proper to call in the aid of obfervation to fix the precife value. But as the period of thefe new equations is more than 180 years, we cannot flatter ourfelves with the hope of knowing both with complete accuracy until one or more periods have been obferved.

Experiments which prove that all bodies, of whatever nature, are fubject to the magnetic influence, by C. Coulomb. The new experiments undertaken by Coulomb, and which he repeated before the Inftitute, induce us to believe that the action of magnetifm extends throughout all nature, fince of all the bodies hitherto tried none of them has efcaped the in-

fluence

fluence of the artificial magnet. But this action, though real, has not the fame force in all bodies; and in the greater part it is neceffarily very fmall, fince it has hitherto eluded the notice of philofophers.

C. Coulomb gave to each of the bodies which he tried the form of a cylinder or fmall bar: in this ftate he fufpended them by a thread of filk, fuch as it is when taken from the cod, and placed them between the oppofite poles of two bars of fteel. The filk thread can fuftain no greater weight than eight or ten grammes without breaking; it was therefore neceffary to reduce to very fmall dimenfions the needles formed of the different bodies fubjected to experiment. C. Coulomb made them of from feven to eight millimetres in thicknefs: thofe of metal he made only one-third of that fize.

For thefe experiments he placed the bars of fteel in the fame ftraight line, their oppofite poles being diftant from each other five or fix millimetres more than the length of the needle which was to ofcillate between them. The refult of thefe experiments has fhown, that of whatever matter the needles confifted, they always arranged themfelves exactly in the direction of the two bars; and that, if removed from this direction, they always returned to it by ofcillations, the number of which was often thirty in a minute. The weight and figure of the needles being given, it was therefore eafy to determine the force which produced thefe ofcillations.

Thefe experiments were made in fucceffion with fmall plates of gold, filver, copper, lead, and tin; fmall cylinders of glafs, a bit of chalk, a fragment of bone, and different kinds of wood.

Phyfical Part, read by Lacepede, fecretary.

Chemiftry and Mineralogy.—C. Monge has given an account of feveral important obfervations in regard to the theory of the earth, and which he made during his travels in Italy and Egypt.

C. Ramon has given obfervations on the ftructure of the mean and inferior mountains of the valley of Adour, one of the moft interefting in the Pyrenees.

C. Lamarck has publifhed an interefting work entitled *Hydrogeology, or Refearches on the Influence which the Waters have on the Surface of the Earth, on the Caufes of the Exiftence of the Bafon of the Seas, its Difplacement, &c.*

C. Seguin, affociate, read two memoirs refpecting cinnabar; in which he endeavours to prove that ethiops and cinnabar are only compounds of fulphur and mercury, without oxygen and hydrogen: that thefe two fubftances differ from

each

each other only in the proportion of their principles, and particularly in the degree of the union of their moleculæ; that this proportion and this degree of union are invariable in the cinnabar, and, on the other hand, very variable in the ethiops: and, in the last place, that cinnabar is composed of thirteen parts and one-third of sulphur, and eighty-six parts two-thirds of mercury.

C. Seguin has written a third memoir on colophonium. After having shown that good colophonium is nothing but resin completely purified from essential oil, and from which a portion of its hydrogen has been taken; and after having proved that the colophonium used in commerce is more or less deficient, he indicates a new process for obtaining it of a good quality.

Mineralogists had hitherto considered the *oisanite* or *anatase* of Haüy as a peculiar kind of stone. But Vauquelin, by subjecting this mineral to analysis, has found that it is crystallized oxide of titanium. The *anatase*, therefore, must hereafter be classed among metals, and in the *titanium* genus. However, as the form of this mineral is not the same as that of native oxide of titanium, Vauquelin thinks there is reason to presume that the anatase holds in combination some substance which has disturbed the common order of crystallization; and this he proposes to verify when he can procure a sufficient quantity of this matter.

C. Sage has described in a memoir processes by which he was enabled to separate, in the dry way, silver from cobalt, and to purify the latter substance, as well as nickel, in such a manner, that these two semi-metals, when fused into thin plates, could be easily magnetized according to Coulomb's method; and that, when suspended by a silk thread, they indicated the poles, and exhibited the magnetic phænomena observed by Klaproth, Haüy, and some other philosophers.

The same chemist read another memoir on the alteration produced by light on sulphurated red arsenic, known under the name *realgar*. He has shown that this realgar and *orpiment*, or yellow ore of arsenic, are only the same substance differently coloured; that light changes realgar into orpiment; and that the latter mineral, which passes to the state of realgar by the action of fire alone, returns to that of orpiment merely by the contact of light.

Botany.—C. Ventenat has presented the sixth and seventh number of his *Description des Plantes rares du Jardin de Cels*, &c.; which C. Redouté jun. has enriched with his drawings.

In the sitting of the National Institute held on the 5th of Germinal (26th March), C. Lalande, having requested leave

leave to ſpeak, addreſſed the Inſtitute as follows:—"Citizens colleagues, the Royal Society of London decrees every year a prize, founded by Copley, to the perſon who has written the moſt important work in regard to the ſciences.

"I requeſt permiſſion from the Inſtitute to place in the Mont de Pieté 10,000 francs, the intereſt of which ſhall be employed for giving a gold medal, or the value of it, every year to the perſon who ſhall have made the moſt curious obſervation or written the moſt uſeful memoir relating to aſtronomy in France or elſewhere, whether belonging to the Inſtitute or not, according to a report of commiſſioners whom the Inſtitute ſhall chooſe from the ſection of aſtronomy or the analogous ſections.

"Failing an obſervation or memoir ſufficiently remarkable, the commiſſion ſhall have the right of decreeing the medal, as an encouragement, to any pupil who ſhall have given proofs of his zeal for aſtronomy. They may alſo reſerve it to be doubled the next year.

"If the Inſtitute, before it accepts this foundation, thinks it has need of the permiſſion of government, I beg it may requeſt it. I ſhall be indebted to it for being able to reſtore to aſtronomy a part of what I have received, and of what I have been hitherto able to make."

The Inſtitute received this propoſal with great ſatisfaction, and decreed that commiſſioners ſhould be nominated from the three claſſes to ſee it carried into execution.

We may here add, that there is at the ſecretary's office of the Inſtitute a will, dated February 3, 1768, by which C. Lalande bequeathed to the Academy of Sciences the whole of his property for the purpoſe of giving annual rewards for the improvement of aſtronomy; but at that time there was no aſtronomer in his family.

In the public ſitting of Germinal 15, (April 5th,) C. Lalande read a memoir on the ninth planet.

C. Toulongeon, a memoir on the eſtabliſhment and foundation of new colonies.

C. Champagne, an account of the life and writings of C. Creuzé Latouche.

C. Mongez, a memoir on the agricultural implements of the antients, and particularly their ploughs.

C. Cuvier, an account of the life and works of C. Darcet.

The following prize ſubjects have been propoſed by the Inſtitute:

1ſt, *Ancient Languages.*—A critical examination of the Greek and Latin authors who have written on Egypt ſince the earlieſt ages to the time of the cruſades.

The

The prize will be a gold medal of the weight of five hectogrammes, and will be adjudged in the public sitting of Nivose 15, year 12 of the republic. Papers will be received till the 15th of Vendemiaire, year 12.

History.—What has been the influence of the reformation of Luther on the political situation of the different states of Europe, and the progress of knowledge?

The prize will be a medal of the same value, and will be adjudged in the public sitting of Messidor 15, year 11. Papers will be received till the 15th of Germinal, year 11.

Chemistry.—The Class of the Mathematical and Physical Sciences had proposed, in the year 8, the following question:

"What are the characters which in vegetable and animal matters distinguish those which act as a ferment and those in which they produce fermentation?"

But as the memoirs received did not fulfil the conditions, the Class again proposed it for the year 12.

The prize will be a gold medal of the value of a kilogramme, and will be adjudged in the public sitting of Germinal 15, year 12.

Painting.—What is the influence of painting on the arts of commercial industry? To make known the advantages which the state derives from this influence, and that which it has reason to expect?

The prize will be a gold medal of the weight of five hectogrammes, and will be adjudged in the public sitting of Nivose 15, year 12.

MUSÆUM OF NATURAL HISTORY, PARIS.

A letter from C. Martin, dated Cayenne, 28th December, contains very satisfactory details respecting the cultivation of the spiceries: he only waits for the favourable season to undertake some trials in regard to that of the pepper plant. Victor Hugues, agent of government, has assigned to him a piece of ground for that purpose. The object is to ascertain what are the trees fittest for serving as props to the pepper plants, giving preference to those which, being susceptible of multiplication by slips, have a thick spongy bark, and which rising to a small height have, however, a long duration. But this is not the only result which it is wished to obtain by these trials. The island of Cayenne is the district which was first cultivated in that colony; the soil in some parts is exhausted, and it is necessary to leave it at rest before any other plants can be committed to it with hopes of success: beside this inconvenience, it is exposed to a scourge still more prejudicial to cultivation,—it is ravaged by ants, which devour

devour every thing. There are some places where it is impossible to prevent the devastation occasioned by these insects, and many districts are abandoned to them. But since the introduction of the pepper plant it has been observed that they do not seem to touch the leaves of that shrub. Some plants scattered throughout the different estates have been spared by them. If this fact is confirmed in regard to large plantations, the soil of the island will be gradually renewed, and its produce will increase in the same proportion.

C. Martin tried to propagate the female nutmeg-tree by layers. If the operation succeeds, it will, no doubt, be the speediest and surest means of propagating this spicery; for among the nutmegs which are sown, and which germinate exceedingly well, there is a much greater number of male than of female individuals. He proposes also to graff the latter on the males, and to try for this purpose the different processes likely to produce a satisfactory result.

In regard to the bread-fruit trees, they thrive in an admirable manner. C. Martin announces that he will soon have twelve new layers to separate from their parent stock. Some of them have already suckers, and the rest will have them soon. He has observed, that layers in general, if the branches are somewhat strong when they are buried, will produce fruits the same year.

A letter has been received from C. Reidlé, the gardener who accompanies captain Baudin, dated Isle de France, April 20, 1801. The following is an extract from it:

"We arrived here on the 14th of February, five months after our departure from Paris. Our passage from the island of Teneriffe took up more than four. You have no doubt learned the accident which I met with two days before I quitted the island. I fell from a rock about 45 feet in height, in consequence of which I was confined to my bed during three months. I still suffered from my wounds on my arrival at the Isle of France; and I entertained great fear that this misfortune would have prevented me from labouring during the remainder of the voyage. But, thank God, I recovered, and now traverse the mountains as before. I have already collected 255 species of plants, without reckoning double specimens for the herbal of the musæum.

"I have made a catalogue of the colonial garden. I have marked those trees which are found in the Musæum of Paris, and those which are wanting there: the latter are in number sixty. C. Ceré has promised to give me plants on our return from the South Sea. I have given him in exchange the following trees: two free olive, two pears, two apples, a cherry, an apricot, a peach, an almond, a cheſtnut, two wild cheſtnuts, eight ſmall walnuts; making part of the collection of trees entruſted to my care when I left France.

"He made me taſte ſome of the fruit of his bread-fruit trees, which I found delicious. He diſtributed ſome to the principal inhabitants of the colony, to induce them to cultivate them. That which we ate weighed eight pounds. Eight ſtill remain on the trees which have produced. Theſe trees are 18 inches in circumference and 15 or 16 feet in height. They would have been higher had they not been broken at the top by the wind.

"I cannot ſufficiently expreſs the civilities I have experienced from the inhabitants of this iſland. I have viſited their gardens, and every where left ſeeds of the pulſe and flowers of Europe. During our paſſage I planted walnuts, and raiſed a multitude of young plants. I have diſtributed about thirty in the colony, and have ſent ſeveral to the Iſle de la Reunion.

"I cannot inform you whither we ſhall go when we leave the Iſle de France. This day we have received orders to go on board, and we are ready to ſail. But every thing in our expedition has been greatly changed: our touching here has been hurtful to it in more than one reſpect. The ſailors have deſerted to go on board privateers. Some of them have been caught. The captain has landed two ſick officers. Several of the naturaliſts ſeem determined to go no further: as for my part, I ſhall proceed with the veſſel wherever it goes."

By a ſecond letter, dated the next day, C. Reidlé announces that the departure of the veſſel was fixed for the 23d of April; he thinks that they were bound to New Holland.

BRITISH MINERALOGICAL SOCITY.

In the beginning of the year 1799 a few gentlemen attached to the ſtudies of mineralogy and chemiſtry, with the view of illuſtrating an intereſting department in the natural hiſtory of their native country, which had hitherto been paſſed over with ſlight regard, agreed to form an aſſociation for this purpoſe under the name of the Britiſh Mineralogical Society. They ſaw, with regret, while inſtitutions of this kind were multiplying in the ſtates of Germany and other parts of the continent, that the Britiſh iſlands, among their general and provincial ſocieties, poſſeſſed none whoſe attention was ſpecially directed to this important object. Without being jealous of foreign interference, they were ſorry that Engliſhmen ſhould be almoſt wholly indebted to ſtrangers for an acquaintance with their own mineral treaſures, and that the names

of

of Ferber, Klaproth, Rafpe, Jars, and Faujas de St. Fond, fhould ftand the foremoft among thofe who have illuftrated the mineralogical geography of Britain. Induced by thefe fentiments, and confcious that every acceffion of facts, however fmall, in fo extenfive and fo little explored a department, was not only of intrinfic value, but might ferve as the foundation and commencement of wider inveftigations, the original members, though few in number, hefitated not to commence a fociety, whofe objects, in their full magnitude, can only be adequately effected by length of time and liberal affiftance. They have quietly employed themfelves for more than three years in the gratuitous analyfis of fpecimens, in the gradual acceffion of members both capable and well difpofed to exert themfelves, to the beft of their abilities, in furthering the intentions of the Society; in arranging correfpondencies, and forming various internal regulations, which, uninterefting as they may be to the public, are effential to the profperity of the Society. Having overcome moft of the difficulties which at firft prefented, and having received from various quarters unequivocal affurances of cordial cooperation, the Society feels itfelf called upon to make a public official ftatement of its objects, and to communicate from time to time fuch abftracts of its proceedings as may contribute in any degree to the advancement of fcience or the arts, and convey to the correfponding members a brief but faithful record of the labours of their affociates.

One of the primary objects is to obtain as much information as poffible concerning the local or geographical mineralogy of Britain. On this account the Society is ftrenuoufly exerting itfelf to obtain lifts of mines, quarries, and mineral fprings, from the different counties, together with fpecimens and fuch particulars as the proprietors may be willing to communicate. From many of the correfponding and ordinary members, the Society expects alfo, with confidence, to receive accounts, as accurate as circumftances will allow, of the prevalent rocks and ftrata in various mineral diftricts, their extent, direction, and other particulars which may hereafter ferve as materials upon which to found a general hiftory of the mineral topography of the ifland. Among other valuable communications already received for this purpofe, a manufcript map of Cornwall deferves efpecial notice.

A very ferious obftacle to mineralogical inquiries is the want of an uniform nomenclature among thofe who are actively engaged in the working of mines: the fame fubftance is not unfrequently called by different names, or the fame name is applied to different fubftances even within the fame mineral diftrict; the fcientific and technical terms are perpetually at variance, and give rife to innumerable errors. A reform in the language of the miners, how defirable foever, is yet wholly impracticable; but the Society expects to render no fmall fervice to future mineralogifts, by collecting from Cornwall, from Wales, from Derbyfhire, and other diftricts, full and accurate lifts of the miner's terms, accompanied by illuftrative fpecimens, the former of which, in due time, will be laid before the public, and the latter will be preferved in the Society's cabinet as authorities, to which a liberal accefs will at all times be allowed.

When it is recollected how often valuable ores, by appearing under an unufual form, or by prefenting difficulties in their analyfis that are not to be overcome by the common methods, are neglected and thrown afide; it will not be deemed a trifling object in the Mineralogical Society to undertake the analyfis of fpecimens of this defcription. All fuch will be gratuitoufly analyfed, provided the conditions already fpecified in the circular letters of the Society * are ftrictly complied with.

The above are the leading views of the Britifh Mineralogical Society, and will always occupy the principal part of its attention. It is by no means, however, intended to neglect other objects: the examination of minerals which have been either not at all, or but imperfectly inveftigated, will occafionally form a part of its employment, and every occurrence that throws light upon the difficult fubject of chemical analyfis will be carefully regiftered in the books of the Society.

LIST OF MEMBERS.

A. Aikin, prefident.
W. H. Pepys, fecretary.
C. R. Aikin, fecretary for correfpondence.

Members in Ordinary.

W. Allen.	R. A. Cox.
A. Tilloch.	P. Sandman.
R. Knight.	R. Stocker.
Th. Cox.	C. Lynam.
Dr. Babington.	R. Bingley.
R. Phillips.	

Correfponding Members.

R. Kirwan, Efq. P.R.I.A. Dublin.
W. Henry, Efq. Manchefter.
D. Mufhet, Calder Iron-Works, N.B.
L. Dillwyn, Efq. Walthamftow.

* See Philofophical Magazine, vol. vi. p. 369, and vol. ix. p. 282.

Rev.

Rev. W. Turner, Newcaſtle on Tyne.
J. Williams jun. Eſq.
G. Overton, Eſq.
H. Campbell, M. D.
M. Tupper, Eſq.
G. Yeates, M. D. Bedford.
J. Taylor, Eſq. Taviſtock.

XLIX. *Intelligence and Miſcellaneous Articles.*

ASTRONOMY.

Another new Planet diſcovered.

"M. OLBERS, a celebrated aſtronomer of Bremen, writes to C. Burckhardt that he diſcovered on the 28th of March a new comet or planet which reſembles a ſtar of the 7th magnitude. It had 184° 56′ of right aſcenſion, and 11° 53′ of north declination, at 9h 25′. He obſerved it ſeveral days ſucceſſively, and the 1ſt of April, at 8h 1′, it had 184° 15′ of right aſcenſion, and 12° 54′ of declination.

"I have learned that prince Henry of Wurtemberg, who reſides at Hamburgh, has purchaſed the beautiful aſtronomical inſtruments made at Paris by Megnié, one of our ableſt artiſts.

"This prince is brother-in-law to the empreſs dowager of Ruſſia; and there is reaſon to hope that the emperor Alexander, whoſe munificence in every thing that relates to the ſciences is well known, will favour the eſtabliſhment of the obſervatory projected by the prince of Wurtemberg, and which is wanting to the large city of Hamburgh.

"LALANDE."

In the above notice, which we have tranſlated from the French official journal (the *Moniteur*), our readers will obſerve announced a diſcovery of Dr. Olbers. On the ſame ſubject we have received the following notice from an able aſtronomer, to whom we have been indebted for ſeveral valuable communications:

"The following are the obſerved places of a new celeſtial body diſcovered by Dr. Olbers, at Bremen, and conjectured to be a new planet:

1802.	R. A.		Declin. N.
March 28,	184° 56′ 49″	-	11° 33′ 30″
29,	184 46 36	-	11 52 59
30,	184 35 32	-	12 15 8."

Dr.

Dr. Olbers's planet has, we underſtand, been ſeen in England by Mr. Stephen Lee, of Hackney, and Mr. Aubert, of Highbury, who both agree that it has a greater reſemblance to the old planets than Ceres Ferdinandea.

ANTIQUITIES.

A ſuperb ſtatue of Achilles, nine Roman palms and a half in height, and in perfect preſervation, has been lately dug up in the environs of Oſtea. The hero holds a metal lance in his right hand and the *parazonium* (ſword) in the left: the helmet, which covers his head, has a large plume ſimilar to that of the celebrated ſtatue of the Villa Borgheſe, which is not of ſo excellent workmanſhip. This production, the work of one of the beſt ſculptors Greece ever produced, is not inſcribed with the name of the artiſt. The inſcription *Votiva Marti*, engraved on one of the legs, proves that it was conſecrated to the god of war.

UNCOMMON DISEASE.

The thigh of a woman, who lately died in Geneva, has been ſent to the Medical Society of that city, as exhibiting a very extraordinary phœnomenon. It is ſtuck quite full of ſmall thorns, which the woman, it appears, had been in the habit of ſwallowing, and which gradually making their way through the inteſtinal canal and the blood-veſſels, ultimately lodged in the femoral muſcles. C. Albert is appointed to draw up a report upon the circumſtances of this ſingular caſe.

A caſe ſomewhat ſimilar occurred ſome years ago in the infirmary at Nottingham. A woman was admitted as a patient, from one of whoſe breaſts a number of pins were diſcharged with excruciating pain; and the diſeaſe was by her ignorant neighbours aſcribed to the effects of witchcraft. The breaſt was obliged to be cut off, and the woman recovered perfectly; but, ſtrange to tell, inſtead of being cured of a habit of putting pins in her mouth when undreſſing, and with which ſhe often went to ſleep, in ſome time after, ſhe returned to the infirmary, and was obliged to loſe the other breaſt. We do not recollect whether ſhe recovered after this ſecond operation.

VOLCANIC ERUPTION.

By letters from Banda, one of the Spice iſlands, intelligence has been received at the Hague of a dreadful exploſion of the burning mountain in that iſland. For ſome time before, the air had been humid, accompanied by ſubterranean noiſes; but ſo violent an exploſion has not been known for many years. All the plantations were entirely devaſtated; a great many inhabitants loſt their lives, and the country, to the extent of ſeveral miles, was as inundated by the lava.

LI. *Biographical Account of the late Dr.* PULTENEY. *By* J. AIKIN, *M.D.*

RICHARD PULTENEY, M.D. F.R.S. L. & E. was born in the year 1730 at Loughborough, in Leicefterfhire. His parents had thirteen children, of whom he alone arrived at the age of maturity. From early youth he was of a delicate habit, and fuppofed to be inclined to a confumption; and it was by means of rigid temperance, which he obferved during his whole life, that he maintained himfelf in a tolerable ftate of health. He has recorded (in Mr. Nichols's Hiftory of Leicefterfhire) his obligations to his uncle, Mr. George Tomlinfon, of Hathern, who poffeffed fome property in that village, and adorned an obfcure ftation with virtue and fcience. "Thofe (fays Dr. Pulteney) who remember and intimately knw the fubject of this memoir, will not, it is believed, judge it otherwife than impartial, though, confeffedly, a tribute from his neareft relative, one who reveres his memory with the trueft affection, who, through the early ftage of life, received from him, as from a father, the genuine dictates of wifdom, virtue, and religion; all of which were truly exemplified in his own conduct throughout the whole of his life." From this relation he imbibed his tafte for botanical ftudies; and it was probably through his inftigation that he was deftined to the medical profeffion.

The youth's firft fituation in a profeffional capacity was that of apprentice to an apothecary in Loughborough; an humble fchool, which, however, his induftry and talent for obfervation were able to render inftructive. He paffed through the ufual courfe of a country education, and then complied with an invitation to fettle at Leicefter. That town, like moft provincial capitals, was divided into two political and religious parties; and it was that of the diffenters (to which his parents belonged) whence Mr. Pulteney received his fupport. His fphere was ftill further narrowed, by the limitation of practifing only as an apothecary; for it was thought due to the confequence of the party, to poffefs a furgeon of their own as a feparate profeffional character, which office was filled by Mr. Cogan, a man of merit and agreeable manners.

Few remarks can be neceffary on the hardfhip of placing perfons of abilities and liberal fentiments in fituations fo unfavourable to the acquirement of that reputation and thofe emoluments

emoluments which are juftly due to profeffional fuperiority; and in which they muft be reduced to an unworthy and degrading dependence upon a few party-leaders!

Mr. Pulteney was of a timid and cautious difpofition; and, though his mind was by no means formed for fhackles, his temper was not firm enough to enable him effectually to affert his freedom. It would be an unpleafant tafk to dwell upon the fhare he had in thofe "fcorns which patient merit of the unworthy takes;" or of the ftruggle he maintained with narrow circumftances, which obliged him to contract habits of rigid œconomy, rendered more neceffary by the paffion for buying books, to which he was content to facrifice every other inclination. Science was, indeed, his great refource under the difcouragements of his fituation, and it eventually proved the means of raifing him from obfcurity. To his private friends he was known as one who had inquired largely and thought freely on a variety of topics. To the public he firft appeared as a votary of the pleafing ftudy of botany. He became a correfpondent of the Gentleman's Magazine at an early period; and communicated to it, anonymoufly, a feries of valuable letters concerning the poifonous plants of this country, and a differtation on Fungi, contained in the xxvth volume of that mifcellany. To the fame publication he fent, in 1757, a tranflation of a curious paper in the Upfal *Amœnitates Academicæ* on "the Sleep of Plants." This fubject he purfued more at large in a paper inferted in the lth volume of the Philofophical Tranfactions, for 1758, entitled "Obfervations on the Sleep of Plants, with an Enumeration of feveral Plants which are fubject to that Law." He had before appeared among the contributors to the Philofophical Tranfactions by a "Catalogue of the rare Plants of Leicefterfhire, with Botanical and Medical Obfervations;" vol. xlix. for 1756. This paper he gave to Mr. Nichols, in an improved ftate, in 1795, who has inferted it in the firft volume of his hiftory of that county. In 1758 he printed, in the Gentleman's Magazine, a tranflation from the fame Amœnitates, of the inftructive paper entitled "Pan Suecus," giving a catalogue of plants which, from experiment, were found to be either chofen or rejected as food by the different fpecies of domeftic quadrupeds. This he adapted more particularly to Englifh readers by referring to Englifh authors; and he fubjoined to it fome notes and obfervations. Its utility caufed him afterwards to annex it, in a more enlarged form, to his "View of the Writings of Linnæus."

He diftinguifhed himfelf in a manner more purely profeffional by a paper publifhed in the Philofophical Tranfactions, vol. lii.

vol. lii. for 1761, giving an account of a ſingular medical caſe attended with palpitation of the heart and other uncommon ſymptoms, and which, upon diſſection, exhibited a preternatural enlargement of that organ. In 1762 he received the honour of being elected a fellow of the Royal Society. His name was now aſſociated to thoſe of men of ſcience in various departments; and his perſonal merits were becoming known to a wider circle of acquaintance, to whom he was endeared by his modeſt worth, and the good ſenſe and diſcretion which peculiarly characterized him. Nor can it be doubted, that, even with his original diſadvantages of ſituation, he would have attained a reſpectable ſhare of buſineſs at Leiceſter, though ſtill in that inferior branch of the profeſſion on which he had at firſt entered, to which, however, he had added the practice of midwifery. But it was his lot to poſſeſs a friend whoſe ardent and enterpriſing ſpirit was an admirable corrective of his own diffidence, and who eſteemed him too much to acquieſce in his continuing in a rank and employment beneath his merits. This was Mr. Maxwell Garthſhore, then eminent in medical practice at Uppingham, in Rutlandſhire. By means of a common friend, much revered by both, they were made acquainted in the year 1758, and this acquaintance ſoon ripened into a warmth of friendſhip which death alone could extinguiſh. As it was Mr. Garthſhore's own plan, after a reſidence for ſome years at Uppingham, to take the degree of doctor at Edinburgh, where he had received his medical education, he ſtrongly urged Mr. Pulteney to accompany him thither, and offer himſelf to the examinations of the univerſity, though he had never enjoyed the advantage of academical inſtruction there or elſewhere. His reluctance was at length overcome; and the two friends ſet out upon their expedition in the ſpring of 1764. Mr. Pulteney was already known by reputation at Edinburgh, particularly to Dr. Hope, the profeſſor of botany; and he had the benefit of his companion's extenſive connections in the place. He paſſed through all the neceſſary preliminaries with credit, and in May received the honours of graduation. The ſubject of his inaugural diſſertation was "De Cinchona," or, On the Peruvian Bark; of the natural and medical hiſtory of which important article he gave a very ſatisfactory and inſtructive account. The botanical deſcription is particularly accurate, and is illuſtrated by a plate; and his Theſis has been thought worthy of reprinting in a collection of the moſt valuable compoſitions of the kind which the medical ſchool of Edinburgh has produced.

A circumſtance relative to his graduation, honourable to

himſelf,

himſelf, but affording matter of reflection relative to the conduct of public bodies, ought not to be paſſed over in ſilence. The univerſity of Edinburgh had now for a conſiderable time been riſing in reputation as a ſchool of medicine, and its degrees in that faculty became of courſe more and more reſpectable. It is well known that the univerſities of Scotland, modelled upon thoſe of the continent, have adopted the practice of conferring degrees upon examination, without requiring in the candidates a previous reſidence in their own ſeminary, or, indeed, in any other. In ſome of them the examination itſelf has been diſpenſed with, and the requeſted diſtinction has been beſtowed upon perſons at a diſtance, in conſequence of mere recommendation. It is no wonder that ſuch a laxity ſhould have thrown occaſional diſcredit upon academical honours; nor that the public ſhould have been prone to confound the degrees conferred at univerſities ſimilarly conſtituted, in one general note of diſeſteem. The Edinburgh medical ſtudents juſtly conſidered themſelves entitled to be regarded among thoſe of the profeſſion who had received the greateſt advantages of education, and were the moſt deſerving of thoſe teſtimonials of competency which titular diſtinctions imply. They had therefore begun to remonſtrate againſt a mode of conferring degrees which might confound them with perſons altogether unworthy of the honour; and their diſcontent had been aggravated by ſome late inſtances of notorious incapacity in Edinburgh doctors by favour. Thinking their complaints not ſufficiently attended to, ſome of the ſtudents of the longeſt ſtanding had entered into a mutual engagement publicly to oppoſe every future attempt at decorating with the degree of doctor of phyſic at Edinburgh any perſon who ſhould not have ſtudied there, and to take their own degrees elſewhere in caſe their oppoſition ſhould prove unſucceſsful.

It happened that Mr. Pulteney was the firſt candidate under theſe circumſtances, after this reſolution was adopted. The ſubſcribers handſomely expreſſed to him their concern that a perſon of his acknowledged merit ſhould be the object of their oppoſition; but they adhered to their determination. His reputation and intereſt carried him through the conteſt; but he was (I believe) the laſt in favour of whom the condition of ſtudying at that individual ſeminary has been violated. And ſo ſenſible have the Edinburgh profeſſors ſince become, that augmenting the credit of their univerſity's degrees, and the difficulty of obtaining them, was conducive to their own perſonal emolument, that they have extended the period of requiſite ſtudy there from two to three years, and made it comprehend every fet of lectures which can poffibly be conftrued as belonging to a complete medical courfe!

As Dr. Pulteney had now affumed a new rank in the profeffion, it was advifable that he fhould look out for a new fituation. The firft plan which fuggefted itfelf to his London friends, was to procure him an introduction to the celebrated earl of Bath, then in a very declining ftate of health. This was effected; and the earl, upon infpection of his pedigree, recognized his defcent from the antient family of which his own was a branch. He alfo, upon converfing with him, was fo favourably impreffed with his profeffional and literary merits, that he refolved to attach him to himfelf in the character of domeftic phyfician. He propofed to fettle upon him an appointment of 400*l.* per annum; and the connection would probably have been attended with mutual fatisfaction and advantage, had not the death of the earl followed fo fpeedily, that Dr. Pulteney received only one quarterly advance of his intended falary.

Not long after this event, a medical vacancy happening at Blandford, in Dorfetfhire, he was urged by Dr. Watfon, Dr. Baker, and others of his friends, to go down and occupy it. Provided with their warm recommendations, but an utter ftranger to all the inhabitants of the town and its vicinity, he fixed his abode in that fpot which was to be his refidence during the whole remainder of his life. A fmall country town, in the midft of a neighbourhood compofed of the ufual ingredients of provincial fociety, was not, perhaps, exactly the fituation moft defirable to a man whofe mind was enlarged by free fpeculation and fcientific purfuit: but it was now Dr. Pulteney's bufinefs to eftablifh himfelf in his profeffion; and to that object, prudence required that facrifices fhould be made. This is, indeed, the condition of all who have their way to make in the world; and perhaps a juft fenfe of true dignity of character, as well as regard to pecuniary advantage, fhould lead a man to place before him, as his *primary object*, the attainment of fuccefs in the profeffion which he has chofen; and to confider as fecondary and fubordinate all reputation or gratification derived from other fources. Dr. Pulteney, therefore, feems to have fat down with the refolution, not only of fulfilling his medical duties with the utmoft punctuality, but of avoiding every thing which might in the leaft degree involve him in differences with thofe on whofe good opinion he was to depend. He was fenfible that by his removal he had entirely *changed his latitude*; and though he was not a man to fhift his fentiments and language according to his company, yet he was conftitutionally cautious, and could, without much effort, practife the allowable policy of filence. "Commune with thy heart and be ftill," was the maxim of 36 years of his life. That it exerted its natural influence upon his character, will not be denied; but it did not prevent him from being a very amiable, ufeful, and refpectable member of fociety.

The fituation of Blandford had not hitherto afforded any great fcope for medical practice; but Dr. Pulteney foon extended its limits. His reputation fpread through the circumjacent country, and he received profeffional calls from the market and trading towns in a compafs of twenty or thirty miles round his centre, as well as from many of the country families of principal diftinction in that part of the kingdom. As his induftry was great, and his expenfes were moderate, he began to accumulate property. He continued to live in a ftate of celibacy till October 1779, when he married mifs Elizabeth Galton, of Blandford. He could not have chofen more fortunately for domeftic happinefs; and the addition this connection made to his comforts was proportionable to the want he had previoufly felt of that fociety which alone can intereft the heart. No children were the fruit of this union; but in the additional fociety of an amiable young relative of Mrs. Pulteney he enjoyed the pleafure of an adoptive parent.

He continued to employ his leifure in occafional writings on topics of medicine and natural hiftory. In 1772 he addreffed a letter to his friend Dr. Watfon (publifhed in the Philofophical Tranfactions, vol. lxii.) concerning the medicinal effects of the *Œnanthe crocata*, an umbelliferous plant of a poifonous nature, the juice of which was exhibited, by miftake, inftead of that of the water-parfnep. In the lxviiith volume of the fame collection, for 1778, he gave an accurate account of the bills of mortality for the parifh of Blandford during forty years paft, with obfervations. To the London Medical Journal, vol. v. he communicated an account of the poifonous effects of the Hemlock Dropwort, (the *Œnanthe crocata* above mentioned.)

He had hitherto appeared as an author only in detached memoirs inferted in periodical publications. But in 1781 he ventured to offer to the public a feparate volume, on a fubject, indeed, with which no man could claim a more intimate acquaintance. This was "A General View of the Writings of Linnæus," 8vo. The purpofe of this work was to afford an exact fynopfis of all the labours of the great Swedifh naturalift, who appears to have been the object of

his

his warmeſt admiration. Along with the account of his works, memoirs of his life are interwoven, chiefly extracted from the different writings of Linnæus. In the prefatory advertiſement Dr. Pulteney ſpeaks with great modeſty of his performance, which, however, was very well received by the friends of natural hiſtory, and obtained for him the preſent of a medal from Stockholm, as an acknowledgment of the juſtice he had done to the fame of the illuſtrious Swede. Many judicious obſervations and valuable points of information are interſperſed in the work. It concludes with a ſynoptical account of all the papers contained in the firſt ſeven volumes of the *Amœnitates Academicæ*.

Some years afterwards a more extenſive and original work proceeded from Dr. Pulteney's pen, and which muſt have coſt him much varied reſearch in its compoſition. This was his "Hiſtorical and Biographical Sketches of the Progreſs of Botany in England, from its Origin to the Introduction of the Linnæan Syſtem;" 2 vols. 8vo. 1790. He paid a juſt tribute to ſcientific merit in dedicating the firſt volume of this performance to Sir Joſeph Banks; and a grateful return to long friendſhip, in inſcribing the ſecond to Sir George Baker and Dr. Garthſhore. The work itſelf is highly valuable, as an example of that union of the hiſtory of men with that of an object of their common purſuit, which is ſo peculiarly intereſting and inſtructive. It has likewiſe made an addition to national biography, which will be duly prized by thoſe who are attached to their country's reputation. It is marked throughout with that candour and diſpoſition to commend which always characterized the amiable author.

Whilſt he was thus tracing the progreſs of his favourite ſcience in books, he was by no means inattentive to the volume of nature as it lay diſplayed before him. The county in which he reſided is conſiderably furniſhed with objects worthy the notice of the naturaliſt, eſpecially in the foſſil kingdom. How well he had made himſelf acquainted with theſe treaſures, the preſent writer obtained a proof, which laid him under a particular obligation. This was a brief but maſterly account of the products of Dorſetſhire, communicated to him for the uſe of his little work entitled "England Delineated." He afterwards enriched the ſecond edition of Mr. Hutchins's "Hiſtory of Dorſetſhire" with a catalogue of the birds, ſhells, and plants obſerved in that county; and during his laſt illneſs he had under reviſal a plate of Dorſetſhire foſſils communicated by himſelf. The formation of a muſæum was the amuſement of many years of his life. By gradual additions, he accumulated a ſtore of natural productions in various

 claſſes,

classes, which was to him a perpetual source of pleasing contemplation, and will, doubtless, become to many students of nature a means of instruction, in the possession of the Linnæan Society, to which it was bequeathed.

Dr. Pulteney, in his latter years, frequently expressed a wish to retire frrom business, and take up his residence in the metropolis, for the sake of the scientific advantages with which it is so amply furnished; but his habits of life were become too strong to permit him to resolve upon so great a change. He continued, though with diminished ardour, to follow his professional avocations, till he was attacked with a pleuritic complaint, which, after great sufferings, put a period to his existence on October 13, 1801, at the age of 71.

By his last will he gave a signal proof of the deep impression which his early friendships had made upon his mind, and which no subsequent connections of common acquaintance could obliterate or equal. After a handsome provision for those who on every account were entitled to the first place in his remembrance, the remaining objects of his liberality were some of the friends of his early days, and even the sons of those friends. He likewise paid a due attention to the claims of charity by bequests to the Salisbury, Leicester, and Edinburgh infirmaries, and to the poor of the parish of Blandford; and he displayed his regard to science by similar bounties to the Royal Societies of London and Edinburgh, and to the Linnæan Society.

Such are the brief memoirs which I have been able to collect concerning Dr. Pulteney; whose life affords, indeed, but little biographical variety, but presents an encouraging picture of modest merit gradually making its way to success, and science, even of the most retired kind, becoming the passport to public esteem and reputation.

I am happy in being enabled to enrich this article with some interesting anecdotes relative to the early life and studies of its subject, obligingly communicated by Dr. Arnold, of Leicester, in whose words they will be the most satisfactorily perused.

To Dr. Aikin. Belle Grove, Leicester, April 19, 1802.

DEAR SIR,

IT is with pleasure I sit down to make you acquainted with some particulars relative to the friend and tutor of my youth the late Dr. Pulteney, with whom I had the happiness and the benefit of enjoying the most familiar intercourse for the space of more than two years and a half, from the beginning of March 1760, to the middle of October 1762; that

that is, from about the 30th to the 32d year of his age; during which time I was a boarder in his houſe, for the ſake of his inſtruction in pharmacy and botany, as a preparation for my medical ſtudies at the univerſity.

He was born at or in the neighbourhood of Loughborough, was much at Hathern, with his uncle Mr. George Tomlinſon of that place; and ſerved an apprenticeſhip at Loughborough to an apothecary, whom I well knew in my youth, of the name of Harris; a man of ſome humour, but of ſmall abilities in his profeſſion, from whom he derived little information but in the common proceſſes of pharmacy: for every thing more than this, he was indebted to his own genius and induſtry.

A taſte for botany he imbibed almoſt in his infancy, from his uncle Tomlinſon, who was a botaniſt of the old ſchool.

His firſt attention to this purſuit was merely the effect of imitation. Seeing his uncle ſearching for plants, as they walked together in the fields; ſeeing him take them home, examine them by the deſcriptions of Gerard and Ray, at that time the ſole, or at leaſt the beſt, and certainly the only good guides in Engliſh botany, and compare them with the rude wooden cuts of the former; diſplay ſome of them upon paper, preſs and dry them, and fit them for a place in his *Hortus Siccus*; he began to do the ſame, to aſk his uncle the names of plants, to make imitative collections, to draw up infantile catalogues and deſcriptions of the plants which he found within the circle of his botanical excurſions and reſearches; and ſometimes he added figures of ſuch as he admired for their beauty, or eſteemed for their rarity.

How early he was a botaniſt appears from what he ſays under the article *Campanula patula* in his "Catalogue of ſome of the more rare Plants found in the Neighbourhood of Leiceſter, Loughborough, and in Charley Foreſt," inſerted in the introductory part of the firſt volume of Mr. Nichols's Hiſtory of Leiceſterſhire, at p. clxxvii., where he has this note:—"In the drier parts of Buddon Wood, and the hedges and lanes adjoining. Firſt diſcovered in England by Mr. Brewer, in 1726, near Worceſter, as recorded by Dr. Dillenius. Next in this place (Buddon Wood) by the writer of this catalogue, in 1742, who a few years afterwards communicated the ſeeds to the gardens of Chelſea and the Britiſh Muſæum *."

Of

* Let me here obſerve, that I have found the *Campanula patula* between the fourth and fifth mileſtones, under the hedge, on the left ſide of the old carrier's road from Birmingham to Coventry; and have obſerved it there,

Of his youthful catalogues above mentioned I have ſeen and examined ſeveral, with deſcriptions and coloured drawings of the moſt remarkable plants, in very ſmall books. They were drawn up at different times, and, like the different editions of a book, were progreſſively improved and enlarged as his experience in botany and his knowledge of the plants in the neighbourhood were improved and enlarged. Some of them were, I believe, made as early as when he was only eight or nine years of age; and I particularly remarked that of the *Campanula patula*, which, as we have ſeen, had not been many years known to be an indigenous plant of this iſland, and which he himſelf had diſcovered in and near Buddon Wood in the year 1742, that is, when he was only twelve years old: there were repeated coloured drawings in the moſt recent of them; done, indeed, in ſuch a ſtyle of mediocrity as might be expected from a boy of that age, but affording a very good likeneſs of the plant which they were intended to repreſent.

This taſte, once implanted in his mind, grew and flouriſhed, and was ultimately productive of extraordinary improvement in the ſame way. Imitation gave a turn to his purſuits; and excellent abilities, united with indefatigable diligence, led him on to that perfection in both the ſcientifical and practical knowledge of botany, a ſtudy at that time but rarely cultivated, to which he afterwards attained, and which at length raiſed him high in the eſtimation, and drew to him the frequent correſpondence, of the moſt eminent botaniſts of the time.

He was early in the habit of examining plants in their ſeveral native places of growth, of being moſt exact in the inveſtigation of them, and in noting their minuteſt differences

there, invariably, for about twenty years; where ten or a dozen plants are annually to be found, in the month of July, exactly about the ſame ſpot; from whence, ſome years ago, I collected ſeeds, which I ſowed in my garden, of which this beautiful plant forms a conſiderable ornament every year from the beginning of July to the latter end of October. I have alſo found it in the road from Birmingham to Sutton Coldfield; and about a mile from Stourbridge, in Worceſterſhire, where, beſides the common purple ſort, I ſaw one or two plants of a beautiful ſnowy white. This plant is not only rarely to be met with; but I never found it growing plentifully but in Buddon Wood. In general, it is ſeen very thinly ſcattered within a ſmall circumſcribed ſpot.

It is to be obſerved that Buddon Wood is not a great way from Hathern, where he firſt imbibed a taſte for botany; or from Loughborough, where he ſerved his apprenticeſhip; and that none of theſe places are far remote from Charley Foreſt, where many of the rare plants were collected; and that at Leiceſter he was for ſeveral years ſettled as an apothecary, where he remained till his thirty-ſecond year; all which places were the chief ſcenes of his early or more mature herborizations, and furniſhed plants for his catalogue.

ences and varieties; in drawing up lifts of thofe which he had inveftigated; in making memorandums of the places in which he had found thofe which were lefs common, and of the natural habitations of all; and drew up, occafionally, botanical tables, as well illuftrative of the Linnæan fyftem, as of other matters interefting to the accurate botanical ftudent.

But while he attended fo diligently to the improvement of his knowledge in botany, he was not lefs diligent in the ftudy of the theory and practice of medicine, of chemiftry, and of every branch of fcience which could contribute to the advancement of his knowledge of that profeffion to which he had dedicated the moft ferious bufinefs of his life, and in which his fuccefs was no lefs remarkable than in his botanical purfuits.

His moft favourite medical writer was the celebrated Frederic Hoffman, whofe voluminous works he read over and over with indefatigable attention and perfeverance. Indeed, in whatever ftudy he was engaged, his diligence was intenfe. I have often known him to fit, when the engagements of his profeffion did not call him from his ftudies, and have often fat with him, for whole days together, without interruption, but by the neceffary interpofition of meals, to which, on fuch occafions, we ufually turned with reluctance. Sometimes, when he was very intent upon a fubject, he had a habit, not to be commended, of overfpreading his pillows with books, of reading, and referring from one to another; in fhort, of ftudying in bed till very late, fometimes till long after midnight.

Thefe habits of clofe application, both by day and night, ferved to render more delicate an originally weak conftitution; and might have been ferioufly pernicious in their confequences had they not been frequently interrupted, and fometimes for long intervals, by the neceffary bufinefs of his profeffion, which, though not great, was not inconfiderable, and by long botanical excurfions, which we ufed to make on foot, once or twice a week in the fummer months, and not rarely in thofe of fpring and autumn.

He took great delight in the hiftory of fcience and literature, and of fcientific and literary men; in which he was a moft diligent inquirer, and had collected a ftock of information, both publifhed and original, of which few men are poffeffed. But nothing feemed to give him fuch exquifite pleafure, or fo to excite in him the ardour of enthufiafm, as the lives and travels of naturalifts; and efpecially the herborizing adventures, the great fatigues, the hair-breadth efcapes, the

difcouraging

difcouraging difficulties, and the happy fuccefs, fo common and fo interefting in the narratives and hiftories of the botanift who engages in travels of difcovery.

He read the modern Latin books of medicine and natural hiftory with great facility; and was particularly converfant with the works of Hoffman, Van Swieten, Ray, Rumphius, Van Royen, Linnæus, and of fome other eminent medical and botanical writers: but had little ftudied, and read with difficulty, the claffical Roman writers of antiquity; and thofe of Greece not at all. In the French language he was completely verfed, and read a good deal.

The acquifition of a new book of merit gave him the moft fenfible pleafure. The joy which he expreffed on the firft arrival of the fecond volume of the fo greatly enlarged tenth edition of Linnæus's *Syftema Naturæ*, which was publifhed at Stockholm in 1759, of which I was a witnefs, I fhall never forget. He feized upon it with eagernefs, ran over it with avidity, and could fcarcely quit it till he had drawn up an analyfis of the whole, which, I think, he fent to the Gentleman's Magazine, as he had before fent an analyfis of the firft volume to the fame valuable repofitory.

His conftitution was from his birth fo delicate that his parents found difficulty in rearing him; and experienced great anxiety about him, as he was an only child. At the time when I was in his family he had frequent illneffes, very commonly from fatigue, efpecially from riding on horfeback, which always, even after fhort journeys of perhaps ten or twelve miles, very much difcompofed him, and after long rides brought on a confiderable degree of fever. He had frequent ulcerous fore throats of a very uncommon kind; being accompanied with great debility, but very little fever; fcarcely any fwelling, and no great inflammation of the throat, tonfils, or uvula; but large floughs, commencing and running from thefe parts, which were very rapid in their progrefs, but foon yielded to the ufe of the Peruvian bark and proper gargles, without producing any alarming fymptoms or great fuffering. I was then very young in the knowledge of phyfic; but thefe ulcerous, fpreading floughs appeared to me, as they did to him, very fingular; and I have never met with any thing like them fince.

Great as was his attention to medicine and natural hiftory, his reading was not confined to books in thefe branches of knowledge only. He was well read in hiftory, morals, and the philofophy of the human mind; to the latter of which he paid particular application; and, while he ftudied with diligence the appearances of external nature, and the ftructure and

and offices of the human body, did not overlook the operations and mechanifm (if I may ufe fo bold a metaphor) of the human mind; on which I have often heard him difcourfe, not only with energy and eloquence, but with great profundity of argument and accuracy of difcrimination.

Among his early correfpondents were the celebrated Dr. Hill, Mr. Hudfon, the author of the *Flora Anglica*, and Dr. Watfon, F. R. S., befides others both regular and occafional. And among the early refpectable friends whom his medical merit had acquired, were Sir George Baker and Dr. Garthfhore, who then refided in this neighbourhood; the latter at Uppingham, as you well know, and the former at Stamford. This acquaintance and correfpondence with Dr. Watfon began before or about the time of his firft entrance upon bufinefs. Having long been in the habit of reading the papers of that eminent botanift in the Philofophical Tranfactions, in the xliid volume of which, No. 471, for November and December 1743, his firft botanical paper was publifhed, being an "Account of Dr. Haller's *Enumeratio Stirpium Helvetiæ*, extracted and tranflated from the Latin," &c., and to which he had continued to contribute frequent botanical as well as electrical and medical papers; and having, therefore, long admired him as an experienced and firft-rate botanift, he conceived a ftrong defire of cultivating his acquaintance, took opportunities of fending him botanical communications by letter; and at length, having occafion to make a journey to London, introduced himfelf to him perfonally; and, being kindly received, from that time a regular correfpondence and friendfhip commenced between them, which did not ceafe till the death of Dr. Watfon. This intimacy, I have good reafon to believe, commenced foon after he had finifhed his apprenticefhip; and not only opened to him the moft unreferved intercourfe with Dr. Watfon whenever he had occafion to vifit London, but was the means of his introduction to the acquaintance of many other men of eminence, as philofophers and naturalifts.

At the time when I was his pupil, his *Hortus Siccus* of Britifh plants was very large; and was particularly copious in the order of graffes, in the collection and inveftigation of which he had been fingularly indefatigable.

He not only correfponded with, but was vifited by, the moft eminent philofophers and naturalifts. Among thofe who vifited him while I was an inmate in his houfe was the earl of Macclesfield, then, I think, prefident of the Royal Society, who, I remember, was uncommonly pleafed with his large and beautiful collection of Englifh graffes.

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I have now mentioned the moft material circumftances which have occurred to me concerning our late refpected friend. There are feveral of them which I have related rather becaufe they were facts, (and every fact relative to a departed friend feems interefting to his furvivors,) than becaufe I thought them of fufficient importance to be laid before the public. I am, dear Sir,

With great efteem,

Yours very fincerely,

THOMAS ARNOLD.

LII. *Analyfis of the Arfeniates of Copper and of Iron. By* RICHARD CHENEVIX, *Efq. F. R. S. M. R. I. A.*

[Concluded from p. 229.]

SECTION III.

Analyfis of the Red Octaedral Copper Ore, in which the Metal exifts in a State hitherto unknown in Nature.

IN the courfe of the experiments which have been ftated in the preceding fections, I have had occafion to examine a great number of copper ores, and particularly of copper ores from Cornwall; but the only one which has afforded any interefting refults, is the well known fpecies called red copper ore, cryftallized in regular and brilliant octaedrons. It has been fo long known, and fo often mentioned by mineralogifts, that it may excite our wonder when we reflect, that its chemical nature has never been afcertained. For it would be an injuftice to that very accurate and fcrupulous analyft M. Vauquelin, to fuppofe that he meant to pronounce decidedly upon that point by the fingle experiment which he had made*, and which is mentioned by the abbé Hauy in a fhort extract of his cryftallographical arrangement of mineral fubftances, publifhed in the *Journal des Mines.*

Romé de Lifle, the baron de Born, Lametherie, the abbé Hauy, and indeed every other mineralogift, concur in calling this fubftance red calx of copper; but fome of them affert that it contains a portion of carbonic acid. Among the many analyfes which have been made of this ore by Fontana, Monnet, De Born, Renovantz, and others, I could not find one, that in the proportions, or even in the ingredients, refembled what I had found to be its contents. The

* He merely poured muriatic acid upon the ore; and, as it was entirely diffolved, without effervefcence, concluded it to be an oxide, and not a carbonate, of copper.

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higheſt amount of copper (that given by Fontana) does not exceed 66 per cent., and is far ſhort of the real quantity. The remainder, as he ſtates, conſiſts of water and of pure and fixed airs. The difference in the reſults I had obtained, together with ſome new facts, which I had occaſion to obſerve during my experiments, induces me to treat the ſubject at ſome length; referring for its external characters to thoſe mineralogiſts above mentioned, who have amply deſcribed the ore, and confining myſelf entirely to its chemical analyſis, and ſome analogous experiments.

One hundred parts of very pure and regularly cryſtallized red copper ore were reduced to a fine powder, and diſſolved, without the aſſiſtance of heat, in nitric acid.

During the operation, a very violent effervefcence, accompanied by a diſengagement of nitrous gas, unuſually copious and rapid, took place. When theſe phænomena had ſubſided, the ſolution was blue, like every other nitrate of copper; and the ore had entirely diſappeared. The liquor, perfectly limpid, was evaporated to dryneſs; muriatic acid was poured in, and the nitric acid was expelled by a ſecond evaporation. Into the muriate of copper, which remained behind, a plate of poliſhed iron was immerſed, which, after the uſual phænomena, gave a precipitate that was found, upon examination, to be copper, and amounted to 88,5. In order to complete the hundred parts, it would be neceſſary to add 11,5. But fire expelled from the ore neither water nor any other volatile ſubſtance; nor did the weight of a given quantity appear either to diminiſh or to increaſe by long expoſure to a moderately elevated temperature. The only oxide of copper with which I was acquainted, as exiſting in nature, contains 20 per cent. of oxygen. I had therefore 8,5 of copper exceeding the quantity I ſhould have obtained had the ore been wholly compoſed of black oxide of copper. And, on the other hand, as I had convinced myſelf that no loſs of weight had been occaſioned by any part of the metal remaining unprecipitated by the iron from its ſolution, I could not conclude the ore to be in the ſtate of native copper. I was led, therefore, to imagine, that it might be a mixture of thoſe two ſubſtances; and that muriatic acid, by diſſolving the one, and leaving the other untouched, would be the moſt effectual means of producing the ſeparation I deſired, and of determining the proportion of each.

Upon 100 parts of the ore a ſufficient quantity of ſtrong muriatic acid was poured. A total ſolution was effected, accompanied with diſengagement of caloric. The liquor was, at firſt, of a very deep brown, approaching ſomewhat to the tinge which water will receive when ſtrongly impregnated with the colouring matter of dried vegetable ſubſtances; but, upon being expoſed to the air, and boiled ſome time, it became like every other muriate of copper; and a plate of poliſhed iron precipitated 88 of metallic copper. From this laſt experiment it was evident that no metallic copper was contained in the ore. But ſtill the deficit to be ſupplied by oxygen amounted to no more than 12; while the copious diſengagement of nitrous gas, in the firſt experiment, indicated that the metal was not at its maximum of oxidation; and the rapidity with which it ſeized upon an addition of oxygen, ſufficiently ſhowed how ſtrong was the affinity of that principle for copper, in that particular ſtate in which it exiſts in the ore.

I imagined it would be expedient to attempt ſome precipitations by other reagents, and make ſome further experiments. For this purpoſe, I diſſolved ſome more of the ore in ſtrong muriatic acid; and, when I thought that the acid had taken up as much as it could contain, and that the colour had arrived at its deepeſt tinge, I gently drew off the clear liquor, uſing all the precaution which the nature of the experiment allowed, to preſerve it from the contact of the atmoſphere, and proceeded to examine it. Knowing this ſolution of muriate of copper to be very concentrate, I attempted to dilute it; but what was my ſurpriſe, when, upon the firſt affuſion of water, I ſaw the liquor become turbid and milky, and a very abundant heavy precipitate, of a white colour, fall to the bottom!

Struck with the novelty of this appearance, I proceeded to collect as much of the ſubſtance as I could, in order to give it a thorough examination. For this purpoſe, I decanted the ſupernatant liquor, and continued to waſh the precipitate. Upon every ſubſequent addition of water, I perceived that the precipitate loſt a little of its whiteneſs, and drew towards an orange colour, not unlike the precipitates of platina. I ſoon found, therefore, that by this method I had no chance of obtaining, in a permanent and conſtant ſtate, this muriate of copper, fit to be ſubjected to experiments proper to determine its internal nature and proportions. I then attempted to make uſe of alcohol, as precipitant, inſtead of water; but I found the ſalt to be ſoluble in it, when the exceſs of acid neceſſary for its ſolution in water was preſent. Nor was I more ſucceſsful, when, after having precipitated by water, I waſhed with alcohol; for the colour of the ſalt paſſed gradually from very white to a ſhade of orange; leſs rapidly, it is true, in this caſe, but ſtill ſo as to convince me that I could

could not even thus procure, in a ſtate conſtantly ſimilar, the ſalt I wiſhed to examine. The only concluſion which all theſe experiments entitled me to draw, was that, in the firſt inſtance, water precipitated the muriate of this particular oxide of copper from its ſolution, but in a manner very different from that in which muriate of antimony, of biſmuth, and ſome other metallic ſalts are acted upon. When into either of theſe muriates water is poured, a precipitate enſues, but it retains a very ſmall portion of acid, if any; whereas, in the caſe before us, it is a ſalt, and not an oxide of copper, that is thrown down. In order to effect in this ſalt a decompoſition ſimilar to that which takes place in muriate of biſmuth, or of antimony, it is neceſſary to draw off the firſt liquor, and then proceed to waſh copiouſly. The precipitate will by degrees aſſume an orange colour, which, as we ſhall preſently ſee, is the real appropriate colour of this oxide of copper, prepared in the humid way.

It is evident alſo, from this precipitation, that this oxide of copper combines with muriatic acid by a very ſlender affinity.

As it did not appear to me, that I ſhould obtain any thing very ſatisfactory from this combination with muriatic acid, I reſolved to try ſome other acids. Sulphuric, phoſphoric, oxalic, citric, acetic, tartareous, and acetous acids were each poured upon known quantities of the ore, and kept in bottles completely filled and well ſtopped, in order to prevent any abſorption of atmoſpheric oxygen. The liquors generally became blue; and, upon trial, were found to contain the common and well known ſalts of copper, compoſed of the reſpective acid, and the oxide of copper containing 20 per cent. of oxygen; while a large portion of the ore appeared to remain in its original ſtate. But, as I was certain that there could be no decompoſition in moſt of theſe acids, under the above circumſtances, and moreover, that no oxygen could be taken in from the atmoſphere, it became a matter of no ſmall intereſt to examine from what ſource the metal diſſolved had acquired the neceſſary quantity of oxygen to favour its ſolution, and afford the uſual ſalt of copper, in which it is oxidated in the proportion of 20 per cent.

I repeated, with all the above acids, the experiments tending to ſatisfy that inquiry; but, as the reſults from all were nearly ſimilar, I ſhall mention that only which proved to be the moſt ample and the moſt concluſive.

One hundred parts of the pulverized ore were introduced into a ſmall phial, and dilute phoſphoric acid was poured in, ſo as to fill it. A ground-ſtopper cloſed it completely; and

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in that ſtate it was ſuffered to remain three days, during which time the bottle was frequently ſhaken. The acid became at firſt of a light blue, and increaſed in colour by remaining upon the ore. At the expiration of the above term, the liquor was decanted; the reſiduum was well waſhed and dried, and weighed 42. The blue liquor contained merely common phoſphate of copper, held in ſolution by an exceſs of acid. Upon the 42 parts of reſiduum, ſtrong muriatic acid was poured, which did not appear to produce the ſmalleſt change or effect. It was evident, therefore, that ſome previous alteration had been produced; for, if it had remained in its original ſtate, muriatic acid would have acted upon it, as in the caſe already mentioned. To operate more effectually, nitric acid was added, and the whole gently heated. A complete ſolution followed, during which much nitrous gas was diſengaged. The remainder of the nitric acid was expelled by evaporation; and a plate of poliſhed iron, immerſed in this muriate of copper, afforded a precipitate of metallic copper, weighing within one part as much as the weight of the firſt reſiduum. It was evident, therefore, that a partial reduction of the ore had taken place; and, what is ſtill more ſtrange, had taken place by means of the preſence of an acid.

In many obſervations which have preſented themſelves in the courſe of various analytic experiments, ſomething ſimilar had before occurred to me. I have known metallic oxides yield a part of their oxygen, one to the other, in favour of ſome particular ſolvent. When the metallic oxide *A*, for inſtance, containing 25 per cent. of oxygen, is in contact with the metallic oxide *B*, containing 10 per cent. they will each remain quieſcent in their reſpective ſtates. But, if the ſolvent *C* comes to be added, and if the ſubſtance *B*, at 10 per cent. of oxygen, has no affinity for *C*, but at 15 or 20 per cent. has a very powerful affinity for it, then may the oxide *A* lend a part of its oxygen, in order to favour the combination of *B*, at 15 or 20 per cent. with the ſolvent *C*. Indeed, as ſoon as I ſaw the phoſphoric acid aſſume gradually a blue tinge, and the undiſſolved powder begin to wear a more brilliant appearance, I imagined I ſhould not fail to recognize the ſame fact in this caſe. When phoſphoric acid has remained long enough upon the pulverized ore to diſſolve all it can, the oxygen is concentrated, as it were, to the amount of 20 per cent. in the part which is diſſolved; and all that which could not be diſſolved has (through the twofold affinity of copper for oxygen, to the amount of 20 per cent. and of phoſphoric acid for that oxide of copper, at that

degree

degree of oxidation,) yielded up its entire ſhare of oxygen, to favour the combinations which take place in a new order, the only one which can exiſt among the ſubſtances now preſent. It is, therefore, to the diſpoſing affinity *, cauſed by the preſence of the phoſphoric acid, which ſeeks to combine with black oxide of copper, that the reduction of 42 per cent. of this ore is entirely to be attributed. All the acids above mentioned are capable of producing the ſame change, but in a manner, perhaps, not quite ſo diſtinct or ſatisfactory.

From the foregoing experiments it appears, that copper exiſts in this ore in a ſtate hitherto unknown in nature; and that it contains much leſs oxygen than has ever been ſuſpected in any oxide of copper; for, from the quantity which was precipitated in the metallic ſtate by iron, it appears to be combined in the proportion of about 11,5 per cent. To confirm this idea, and aſcertain, as nearly as I could, the preciſe quantity, I diſſolved 100 parts in nitric acid; then boiled with potaſh, and filtered. One hundred and eleven remained upon the filter, which, as they had combined with a new portion of oxygen from the nitric acid, were in the ſtate of black oxide, and correſpond exactly to 88,75; ſo that I believe I ſhall be within one per cent. of the truth, in aſſerting the proportions to be,

Copper - -	88,5
Oxygen - -	11,5
	100,0

When, into a ſolution of muriate of ſuboxide of copper, liquid potaſh or ſoda is poured, a bright yellow precipitate, not unlike the precipitate of platina, takes place. This precipitate differs only in colour from the original ore; for it is ſoluble in muriatic acid, and affords the ſame ſolution and precipitation by water, and the ſame appearance with alcohol. It is likewiſe ſoluble in nitric acid, but with diſengagement of nitrous gas, and gives the ſame appearances with the other acids above enumerated. The difference of colour ſeems to ariſe merely from the tenuity of its molecules, compared to the mechanical pulverization of the natural oxide. When alone and dry, it is much more permanent in its nature than when combined with muriatic acid; but any part

* As the term *prediſpoſing affinity* has been objected to, I have uſed the term *diſpoſing*, which, I truſt, will not be thought improper. When in two bodies which, while together, remain in their original ſtate, the equilibrium of their principles comes to be broken by the preſence of a third, we cannot but allow that it is this third which has diſpoſed them to the rupture of that equilibrium; and, moſt certainly, be the fact explained as it may, whatever diſpoſes may be called diſpoſing.

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of it that happens to be in contact with a filter, becomes green, and then blackish, leaving a mark of the same shade upon the paper. Were it not for this property of changing, it might be of use in the art of painting; for the colour is extremely beautiful, and would be highly valuable if durable.

The precipitate caused in the muriate of copper by the carbonates of potash and soda, is of a brighter yellow, and is a real carbonate of suboxide of copper. But, if ammonia is poured, at first in a small quantity, into the above solution, the precipitate is blue; and, upon adding an excess of the precipitant, the whole is redissolved, and the liquor is like any other ammoniacal solution of copper.

In order, however, to determine in what state the copper was dissolved by that alkali, I poured some ammonia upon 100 parts of this suboxide in a well-closed phial. The liquor became blue; and I expected to find that part of the ore had been reduced, as with phosphoric acid; but the residuum was entirely soluble in muriatic acid, with the usual phænomena.

A spirituous tincture of galls, poured into muriate of suboxide of copper, afforded no precipitate, owing, I suppose, to the excess of acid; but sulphurated hydrogen gas threw down a black, and prussiate of ammonia a lightish brown, precipitate.

I endeavoured to obtain muriate of suboxide of copper by evaporation, and by distillation in a retort; but, as I could perceive the liquor constantly assume a blueish tinge, I could not reckon upon the purity of the salt, sufficiently to submit it to analysis.

Such were the principal experiments, which the fleeting and precarious existence of the salt allowed me to make upon it. But, from some properties which I had remarked, I could perceive that this ore was a natural oxide of copper, nearly in the same state as that artificial oxide which Mr. Proust had found in the white muriate of copper, obtained by pouring a recent solution of muriate of tin into a solution of muriate of copper.

If, however, by the very nature of the substance, (which, as I saw it ever changing, I thought it would be loss of time to examine further,) I have been turned aside from more certain results, I have been more successful in imitating by art the state of this natural product.

By exposing oxide, hydrate, or carbonate of copper, without addition, to a violent heat, in an open crucible, I frequently obtained the suboxide, which then presented all the properties already recognized in the above species of copper ore. In one instance, I so far succeeded, that, upon the very first inspection,

inspection, the well-experienced eye of the Count de Bournon recognized a lump of it to be a mass of semi-fused, artificial, red copper ore.

But I have found a method of producing at pleasure, in the humid way, all the new salts, and the oxide above described. As I had found about 11,5 per cent. of oxygen to be the quantity contained in the ore, I took that quantity of black oxide of copper which corresponded to 1,5 of oxygen; (57,5 of black oxide was the proportion thus indicated;) on the other hand, I took 50 parts of metallic copper, which had been precipitated by iron from muriate of copper, and which was in a state of tenuity not inferior to the finest powder. These were well mixed, by trituration in a mortar, and put, with muriatic acid, into a well stopped phial. A violent disengagement of caloric took place; the liquor became of the same colour as with the ore, and contained a salt in every respect similar to that afforded by the ore; while a portion of metallic copper remained, with all its lustre, at the bottom of the phial. The solution was decanted, and the residuum of metallic copper weighed 7,5. Consequently, 42,5 had been dissolved, which, with 57,5 of black oxide, complete the hundred parts.

No experiment could prove, in a manner more satisfactory, the quantity of oxygen contained in 100 parts of this suboxide; nor could any afford results more important, or more conclusive. The passage of a portion of oxygen from one part of the metal to another, to favour its solution, as already stated, is proved beyond the possibility of doubt; and is doubly interesting, as it is the inverse of what happens to the ore when treated by phosphoric acid.

In the experiments of Mr. Proust, he has estimated the quantity of oxygen, contained in 100 parts of this oxide, to be 17. This proportion was calculated upon the deficit of a single analytic experiment, made upon the salt of muriate of suboxide of copper, after having determined the quantity of acid, of water, and of metallic copper. But, first, the salt cannot easily be obtained (as I have before observed) in a state sufficiently certain to be relied on, in an experiment of this nature; and, in the next place, it is probable, as happens in almost every analysis, that the deficit was greater than the real quantity of oxygen. For, the agreement between the analytic and synthetic experiments I have just stated, seems to confirm 11,5 to be more exactly the proportion.

When, in the dry way, the above proportions of metallic copper and of black oxide of copper, or, when metallic copper with a corresponding proportion of hydrate or of carbon-

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ate of copper, were intimately mixed, and heated at a low red heat, the oxygen ſeemed to be equally diſtributed through all the maſs; and every particle ſeemed perfectly homogeneous.

In the ore here ſpoken of, it is by no means rare to find large pieces of real native copper; and, whether we conſider it mineralogically or chemically, it certainly is an intereſting ſubſtance. But, how much more will it be eſteemed, if it is regarded with a view to public utility!

The Baron de Born has mentioned a gray cupreous pyrites, which, he ſays, contains 90 per cent. of copper. I have analyſed a ſimilar one from Cornwall, (gray vitreous copper ore, p. 212,) which I found to contain 86 of the ſame metal. But if we reflect, not ſo much on the quantity as upon the extreme purity of this copper, and the wonderful facility with which this uſeful metal may be extracted, it will be found much ſuperior to every copper ore hitherto diſcovered. It would be well worth the attention of miners, to keep a conſtant look-out for this ſubſtance, which, I am informed, is not rare in Cornwall. It contains no iron, and no ſulphur; the abſence of which latter is a peculiar advantage. It is a fact not generally known, I believe, that there is hardly ſuch a thing in commerce, as copper which does not contain a little ſulphur; at leaſt, I have rarely met with any ſuch; and it requires but a very minute portion of ſulphur to increaſe the fuſibility of copper. The advantage of obtaining copper free from ſulphur, is too obvious to require to be pointed out; and that advantage does this ore poſſeſs.

To work it ſeparately, if ever it ſhould be found in ſufficient quantity, would well repay the labour it would coſt; and a very ſmall mixture of any diſoxidating ſubſtance would, in a ſhort time, reduce immenſe quantities.

From the foregoing experiments we may perceive into how many errors we may be drawn, if, in arguing from the reſults which we obtain, we pronounce too haſtily upon the ſtate in which a ſubſtance exiſts, in the ſubject of any analyſis. After what has been ſhown, with regard to the action of muriatic acid upon a mixture of metallic copper and black oxide of copper, both reduced to powder, and of the action of phoſphoric acid upon the ore itſelf, it may be ſtill a doubt whether this ore is really a ſuboxide, or a mixture of metallic copper and oxide of copper, at 20 per cent. of oxygen. But, as ſimilar proportions of both, after having been made red-hot, preſented all the properties and appearances of the ore much more ſtrongly than when ſimply mixed, it is fair to conclude that it is a real ſuboxide. Had not muriatic acid

been

been ufed, the natural conclufion would have been, that the ore was a mixture, or at moft a combination, of thefe two fubftances; for fuch did it appear to be by the teftimony of the other acids. The truth is, we are but little acquainted with the exact ftate in which fubftances exift in many natural combinations. However, in the mineral kingdom, fuch fallacious conclufions are lefs frequently to be dreaded than in the vegetable and animal kingdoms. But, in every refearch, it is important to leave as little room for them as poffible; and he who would indicate a fure and conftant method of afcertaining whether, in many cafes, what we deem a component part, is not, in fact, a product of the operation, would render to fcience a fervice, the real value of which is, perhaps, not now entirely forefeen.

LIII. *Communications on the Subject of Navigation, from* JOHN COOKE, *Efq. M.R.I.A.*

SIR, Dublin, April 25, 1802.

I REQUEST permiffion to communicate to the public, through your Magazine, the inclofed attempts to advance the art of navigation; they confift of methods of meafuring diftance at fea, and of difcovering currents, with the plan of a new fea chart. My object in publifhing them is to offer them to the confideration of others who are more capable of detecting their imperfections and of fuggefting remedies than myfelf, for which I fhall be thankful; but, as I intend to make experiments, and to follow up thefe inventions, I do not wifh to have it underftood that I relinquifh my exclufive right to them by this ftep, or that any perfon may make ufe of them without my confent. I am, Sir,

Your moft obedient fervant,

To Mr. Tilloch. JOHN COOKE.

THE diftance through which a fhip fails is the product of the time and velocity of its progrefs: the time is eafily afcertained by well-known methods, and the log-line exhibits the degree of motion during the experiment with tolerable certainty: but, fince the motion is perpetually varying, this method would require an uninterrupted fucceffion of experiments to afford an accurate meafure; therefore it is a defideratum in navigation to obtain fome eafy method of regiftering the feveral changes of velocity which take place in failing, with the intervals of time between each; which objects,

 with

with a practical contrivance for finding the diſtance at any time by theſe data, are aimed at in the following ſcheme.

Let a ſmall globe of metal be attached to the end of a line which is to be paſſed over pulleys fixed in the ſtern-poſt of a ſhip, in ſuch a manner that this globe may be drawn through the water after the ſhip at ſo low a level as not to be affected by the ſhip's wake or the motion of the rudder, and ſo that the other end of the line may be conducted into the veſſel, where it is to be joined to the end of a ſtrong ſpiral ſpring of ſufficient ſize and elaſticity to expreſs, by diſtinct expanſions and contractions, every variation of reſiſtance which may affect the immerſed globe. If a ſhip, furniſhed with ſuch an inſtrument, move at ſea with an aſcertained degree of velocity, and if the expanſion of the ſpring, at the ſame time, be marked on a ſcale annexed, this degree of expanſion will be peculiar to that degree of velocity; and if the ſcale be filled with all the uſeful graduations taken from actual experiment, the ſhip's rate of motion may be known thereby at any time, whatever the law of hydrodynamic reſiſtance may be.

Then ſuppoſe a cup, with a ſmall perforation in the bottom, containing that ſort of metallic ſand which is uſed for hour-glaſſes, to be joined to the extremity of the ſpring, the ſtream of ſand which iſſues from it will have a common motion with the ſpring, ſo that the point of the ſcale on which this ſtream falls will mark the rate of progreſs, and (ſince ſand* flows equably) the quantity which falls upon that point will mark the time, the product of which quantities is the diſtance paſſed over during that time. Therefore, if there be a receiving veſſel for the ſand, divided tranſverſely into ſeveral compartments, ſo that the partitions between them may be at the ſame diſtances aſunder as the graduations of the ſcale, the ſum of the products ariſing from the multiplication of the quantities of ſand received in all of them, by the velocities denoted by the compartments which hold them reſpectively, will be the diſtance ſailed at any time.

However, though this method furniſhes quantities from which the diſtance may be computed, conſiderable delay muſt ariſe from the meaſurement of each ſeparate portion of ſand, and the multiplication of each of theſe by its reſpective degree in the ſcale; which difficulty may be removed in the following manner:

It is evident, from the nature of the ſcale, that the orifices of the compartments of the receiver muſt be unequal; but if they

* It has been proved, by experiment, that the diſcharge of ſand in this caſe will be the ſame in equal times, whether the ſuperincumbent weight be great or ſmall,—contrary to the eſtabliſhed laws of *fluids*.

they be made to diverge as they defcend, fo that they may be equal at the bottom, the unequal fcale will be changed into a fcale of equal parts, the fand received into it will be fo diftributed thereby, when lodged at the bottom, that the diftance of each portion of it from the end of the receiver will be as the fhip's velocity when it fell; and, confequently, the fum of the products arifing from the multiplication of the quantity in each compartment, by its diftance from the end of the receiver, will be as the diftance failed. But the fum of thefe products is equal to the fingle product of the diftance of the common centre of gravity of the fallen fand from the extremity of the receiver, by the weight of all the fallen fand; and, if the receiver be furnifhed with a falling handle, this centre of gravity may be found at any time by finding tentatively that point of the handle by which it may be fufpended, fo that the receiver containing the difcharged fand may not incline to either fide: and if this experiment be made with the hook of an ounfel containing a fcale graduated for the purpofe, the weight or quantity of fand difcharged may be known; alfo, if the falling handle be graduated into equal parts, correfponding with the divifions of the compartments at the bottom of the receiver, and marked with numbers reprefenting the rate of failing which thefe divifions denote, the number on this handle at the centre of gravity, multiplied by the number on the fcale of the ounfel, will fhow the diftance failed at any time.

In order to adapt this inftrument to the feaman's ufe, it is neceffary to confider the irregularities of motion occafioned by the waves, and to provide againft their effects. Thefe are of three forts. Firft *rolling*, or a motion about the longitudinal axis of the fhip, which does not feem capable of difturbing the operation of the inftrument. Secondly, *lee-way*, which takes place when a fhip moves in any direction except that of her keel: in this cafe, the ftring, being drawn tranfverfely, will not have its full effect on the pulley at the bottom of the ftern-poft, but will act as an oblique force, and confequently the diftance will be reprefented too little; to remedy which, the lower pulley muft be formed fo as to have a motion like that of a caftor on the foot of a table, whereby it may be turned by the line into its own direction, and then the action of the ball will produce the required effect. Thirdly, *pitching*, or a vibrating motion about the tranfverfe axis, which, if the ftring to which the ball is faftened be fhort, in an high fea, would give a ferpentine motion to the ball, that would reprefent the diftance greater than it fhould be; to

prevent

prevent which, the ſtring is to be of ſuch a length as to render the angle through which it vibrates very minute, whereby its tendency to deflect the ball from a rectilinear path will be diminiſhed; and if (as Mr. Boyle and others aſſert) the greateſt height of a natural wave be no more than ſix feet, it will not require a very long radius to render the angle ſubtended by a fine of ſix feet ſo ſmall, that the divarication occaſioned by it may become inconſiderable.

This may be ſubjected to experiment in an eaſy manner: let a boat, with the inſtrument annexed, be drawn on a canal through ſuch a ſerpentine line in an horizontal plane as it would deſcribe in a vertical plane in paſſing over waves, and the effect on the ball will be the ſame in both caſes; by which means it will be poſſible to find the error occaſioned by pitching, and the length of ſtring neceſſary to reduce it.

It may occur as an objection, that the immerſed ball cannot be kept at the ſame depth at all times, and that it may receive different degrees of reſiſtance from the ſame velocity at different depths, which muſt occaſion error: but the experiments of C. Coulomb, publiſhed in the Phyſical Memoirs of the French National Inſtitute during the third quarter of the year 8, tome iii. p. 288, prove the reverſe; his words are—" L'on peut conclure de cette expérience que lorſqu'un corps ſubmergé ſe meut dans un fluide, la preſſion ou la hauteur du fluide au deſſus du corps n'augmente pas ſenſiblement ſa reſiſtance."

But the greateſt difficulty attending this ſcheme is to form the ſcale of the ſpring that is to aſcertain the ſeveral degrees of preſſure which a globe of certain dimenſions meets from the ſeveral requiſite degrees of velocity. For this purpoſe, let a man be furniſhed with a ſlender chain one yard in length, ſo faſtened between his ſhoes as to limit the length of his ſteps; let him keep his eye fixed on the vibrations of a pendulum, and, by ſtepping to the full length of his chain in time with the oſcillations, his motion will be equable, and the rate of it will be exactly known. If a man ſo prepared ſhould draw a boat along a ſtagnant canal, the rate of the boat's motion will be equable alſo, and aſcertained; and this rate may be varied to any required rate within certain limits by altering the length of the pendulum: then if the inſtrument be attached to the boat, the expanſion of the ſpring at each degree of celerity with which the man proceeds may be marked on the ſcale: for the higher degrees of velocity the man's motion muſt be increaſed by ſome mechanic power, as the axle in the wheel, whereby additional force may be con-

verted

verted into velocity, and the degree of velocity afcertained by the power of the machine. This experiment fhould be made in a wet dock filled with fea water.

It fhould alfo be obferved that thefe experiments will afford a fcale which will anfwer only for balls of the fame dimenfion as that with which the original experiment was made, and therefore the mould of the original ball fhould be preferved; but this is not the cafe of the fpring, for the fcale of any fpring may be eafily found when the fcale of one is graduated in this manner:

Let the extremities of the new fpring and of the original fpring be joined, fo that they may act againft each other; then pull them afunder until the index of the original fpring fhall touch fome graduation, and then the fcale of the new fpring may be marked where its index then ftands, with the degree of velocity at which the index of the original fpring ftands; and in this manner all the neceffary graduations may be transferred from one fcale to another, though the fprings fhould be of very different powers; becaufe, fince the ball is to be the fame, the force exerted by each degree of velocity remains unaltered.

However, in a very high fea this method may be attended with fome inconvenience: the ftring may be frequently broken, and a certain degree of error will attend a very ftrong pitching motion; in which cafe the following method may be ufed to afcertain diftance by means of thefe waves, which interrupt the operation of the inftrument:

Let any fmall floating fubftance be thrown out a-head of a fhip at fea, and let the number of times be noticed when it is feen diftinctly on the top of a wave; alfo let the interval of time between the firft and laft of thefe elevations be obferved by a ftop-watch; which time being divided by the number of elevations will be the time in which a wave paffes through its own breadth, (becaufe the floating body is not protruded by the action of the waves.) But a wave paffes through its breadth in the fame time in which a pendulum whofe length is equal to that breadth performs one vibration *: and tables are or may be conftructed to fhow by the time of vibration the length of the pendulum; which tables will of courfe fhow, by the time of undulation, the fpace through which any wave moves in that time, and alfo the breadth of the wave †. Therefore, if a fhip failing *right before* the wind were to accompany the fame wave through the

* Quære, what effect has a current in difturbing this law of the waves?

† Or the breadth and velocity of the waves may be found by an experiment with the log-line.

fea,

sea, her rate of sailing would be known by the time of the wave's undulation: but, since the ship receives the impulse of the wind more powerfully than the wave, it moves faster and makes more way than a wave by the breadth of all the waves which it passes through; and since a ship pitches on passing over each wave, and since the number of pitches may be registered, without trouble, by an oscillating instrument, (like the watch which shows the number of steps which the wearer makes,) the number of waves whose breadth is to be added to the progress deduced from the tabular rate is known; whereby the distance made by a ship *sailing before the wind* may be found, provided the time of undulation be ascertained whenever there is reason to suspect a change in the motion of the surface of the sea. But when a ship sails *upon a wind*, the distance deduced from these data requires correction. Suppose two ships placed on the same wave, and that one runs directly before the wind and keeps time with this wave, and that the other sails in a course which forms an angle with the direction of the wind, retaining, however, her place on the same wave; it is evident that then the space described by the former will be to the space described by the latter, as the cosine of this angle to radius: also, if these ships, pursuing these courses, pass over the same waves, or the same number of similar waves, the direct path of the former in passing these waves is to the oblique path of the latter as the cosine of the same angle to radius also: therefore, in sailing *upon a wind*, the result which this method of calculation gives, is to the true distance as the cosine of the angle which the rhomb forms with the direction of the wind is to radius, and may be corrected accordingly: or, if this angle be assumed as a course, and the result obtained by this method be taken as difference of latitude, the *corrected* distance will be found in the distance column of the common nautical tables.

But both these methods must prove erroneous where the ship is borne away by a current; and the common method of estimating the effect of a current by the apparent motions of a sunken body is fallacious; because the boat may be in quiescent water, and the current may exist only below; or the upper and lower strata of water may be parts of the same current flowing in the same direction with different degrees of velocity: therefore any theory which points to a true measure of currents should be announced, though it may require long experience to perfect it.

Let two ships *lie-to* at a considerable distance asunder (suppose twelve miles): if a gun be fired from each from a situation

tion which may be four yards above the water, it may be feen from the other at an elevation of fifteen yards above the water; and confequently, the interval of time between the obfervation of the flafh and report of a gun from either veffel may be obferved at the other at this diftance, which time, according to the laws of found, will be about $55\frac{1}{2}$ feconds: and, if there be no current, each will obferve the fame interval of time between the flafh and the report of the fhot from the other; but if they float on a current which runs at the rate of feven knots an hour, for inftance, one veffel advances 570 feet towards the point whence the found iffued during the paffage of the found, and will confequently hear it half a fecond fooner than if fhe remained at reft; whilft the other, by receding from the former veffel, lengthens the fpace through which the found is to pafs by 570 feet, and therefore will hear it half a fecond later: therefore there will be an entire fecond of time difference between the obfervations of the two fhips: and in fuch cafe it may be inferred that the fhip which obferved the fhorter interval floats on a current which runs *towards* the other fhip, and *vice verfa:* fo that two fhips, making this experiment in feveral points, will detect the exiftence of a current, and the true direction of it; and, if it be not too much to expect from the accuracy of future obfervers, that the fubdivifions of a fecond fhould be diftinguifhed, the *rate* of the current may alfo be found from the diftance between the fhips and the time of the found's paffage from one to the other, provided this portion of time can be meafured with fufficient accuracy.

[To be continued.]

LIV. *On the Nature of Heat. By a Correfpondent.*

IT has long been a queftion whether the fenfation we experience, called *heat*, arifes from a fubftance of a diftinct nature *per fe*, or whether it is generated from an intenfe vibratory motion of the infenfible particles of bodies. In reafoning on this fubject, our arguments can only be founded on the effects that are produced by this unknown caufe: for it feems more than probable that the degree of accuracy of experiment neceffary to render our conclufions certain with regard to its intrinfic nature, is incompatible with the prefent means of our knowledge. The following obfervations are intended therefore, by directing the attention to a confideration of fome of the chief phænomena of heat, to fhow that we can feek

feek for an explanation of them in no way fo fatisfactorily as by fuppofing them to originate from the univerfal prefence of a very fubtile fluid called by the French writers *calorique*, which penetrates, without exception, all bodies, and is equally diffufed through all fpace.

That heat is only a property of common matter, a fpecific motion of the particles of bodies, is an hypothefis perfectly inadequate to the folution of the phænomena alluded to. Lord Bacon is the firft modern philofopher who attempted to elucidate at length any theory on this fubject; and we find him, in a treatife written exprefsly for this purpofe, and entitled *De Forma Calidi*, deducing, from an enumeration of the feveral phænomena and effects of heat, its general properties, and hence defining it to be *an expanfive undulatory motion in the minute particles of the body, by which they tend with fome rapidity towards the circumference, and at the fame time incline a little upwards*.

This fyftem, which evidently confiders heat not as an original inherent property of any particular fort of body, but as mechanically producible in it, was adopted by moft of the mechanical philofophers of that age, by Boyle, Newton, and Defcartes; and has been fuppofed to have received no inconfiderable fhare of weight from the later experiments of Count Rumford and Profeffor Pictet. Before, however, we acknowledge the truth of this hypothefis, it is neceffary we fhould firft afcertain if it is capable, in its application, of accounting for the various phænomena of nature; for, unlefs thefe are found to admit by it of a fimple and obvious interpretation, we are juftified in concluding that it is unequal to the province affigned it. That the theory under confideration will not ftand the teft that is here fubmitted as the criterion of its truth, cannot be well doubted after an attention to the following facts:—If a thermometer be placed under the receiver of an air pump, and the air be fuddenly exhaufted from it, the thermometer will fink feveral degrees, and very quickly after will rife again to its ufual height. Now if heat confifts in vibrations, according to the tenets of this hypothefis, it may be afked, How comes it to pafs that the fmall quantity of matter that remains within the receiver is firft infufficient, and afterwards fufficient, to maintain the temperature originally indicated by the thermometer? This is a fact which no reafoning can fatisfactorily account for fimply on mechanical principles, and can only be reconciled on the fuppofition that there is fuch a fluid as caloric poffeffed of a nature peculiar to itfelf.

The univerfal power of expanfion that heat poffeffes (for we we are entitled to presume that, where a contrary effect is witnessed, it proceeds from the expulsion of some fluid or matter foreign to the substance on which it acts,) is another fact wholly inconsistent with the theory in review. Thus, when we perceive that heat, applied to any substance, invariably occasions its dilatation, we are justified in concluding that it is enabled to produce this effect only by virtue of its own proper extension; and if we allow that it is capable of extension, we cannot refuse our assent to its being a bodily substance. If we suppose this effect to be produced in any other way, as by the impression of any mechanical motion on the parts of the substance, it is difficult to conceive how this could produce a lasting expansion of the particles; it would excite, indeed, an undulatory or vibratory effect, occasioning a change in the situation of the particles for the time, but there would still be a tendency in them to return again to their former situation, and an oscillatory effect only would be produced, perfectly distinct from extension. It is, however, impossible to reconcile this fact with the supposition that heat is a quality, or an adventitious and accessory property resulting from the intestine motion of the particles of bodies, and not a substance of itself. "Whenever," says Mr. Locke, "we perceive a number of qualities always existing together, we are warranted in the conclusion that there is some substance which produces those qualities." If heat depended on motion, it is natural to expect that it would display a greater facility in its passage through elastic bodies than those that were more dense; yet we find that no rule of this sort obtains, but that it passes through the former equally slow with the latter. Again, if heat be generated by motion, it is reasonable to expect that its laws of propagation would be always analogous to the latter; yet it is hardly necessary to observe how few things we are more ignorant of than the rule which takes place in the progression and communication of heat in bodies of unequal temperatures.

Those who have adopted the theory of vibrations have assumed a position which there is no satisfactory evidence to justify their doing; for it does not appear in any one instance that we are able to demonstrate the existence of vibrations in heated bodies, while certain sounds will cause the most solid substances to vibrate perceptibly without any heat being apparently produced. The ingenious experiments of Count Rumford, it will not be disputed, have at least proved that fluids are very imperfect conductors of heat. That they are, as affirmed by the Count, perfect non-conductors, is an inference that cannot legitimately be drawn even from his own

5 reasonings;

reafonings; and, from the later experiments of Dr. Thompfon, and Mr. Murray of Edinburgh, is perfectly inadmiffible. If, however, the experiments of Count Rumford to prove that heat is conveyed with much greater difficulty downwards than upwards, be allowed to be fufficiently conclufive, how is this phænomenon to be reconciled by the theory of thofe who make all heat to be generated by motion? The effect is not at all proportionate to the caufe; for no good reafon can be fhown, according to this hypothefis, why heat fhould with more difficulty be made to defcend in bodies than to afcend in them; for the fame motion will be communicated to the furrounding medium in all directions. Is not this fact irreconcileable with the doctrine alluded to? If, however, we affume the theory that heat is a peculiar ethereal fluid, all the difficulties that have before been enumerated will admit of an eafy folution, and the phænomena of heat will be reconciled in a fimple and beautiful manner: indeed, on any other principle it would be extremely difficult to account for moft of the operations in chemiftry, and it would introduce a degree of uncertainty into the theory of that fcience that would go very nearly to deftroy it altogether.

Thus, if we do not confider caloric as a fubftance, we are no longer at liberty to conceive that it is capable of attraction. Oxygen gas, we are told, is oxygen combined with heat and light; and thus we find that, when oxygen enters into combination with any body, the two latter are always evolved. If oxygen gas and phofphorus be prefented to each other, the oxygen combines with the phofphorus, and the caloric flies off. This is readily accounted for on the fuppofition that caloric is a fluid *fui generis*, and therefore capable of attraction; and confequently we find this opinion very generally adopted by chemifts, as alone able to afford an adequate folution of the phænomena in queftion. The names of Boerhaave, Lavoifier, Black, Crawford, and Fourcroy, are among the moft celebrated partifans of this doctrine, which, for its beautiful fimplicity, its perfpicuity, and the eafe with which it is applicable to all the phænomena of heat, is much fuperior to that which ftill divides in opinion many of the philofophers of Europe. The ftrongeft, and indeed the only argument of weight that has been adduced againft this theory, is that of Count Rumford, arifing from fome experiments made by him concerning the fource of the heat excited by friction. From the accuracy with which thefe experiments were conducted, and the nature of the refult, it certainly is not very eafy to draw any inference that is perfectly fatisfactory. It is evident that a change of capacity in the body, as

has

has been ſuppoſed by ſome, could not have effected the phænomena deſcribed: for no ſuch change could be diſcovered to have taken place; and if it had, it ought to have been ſufficiently great to have accounted for all the heat that was produced. In reflecting on this experiment, we cannot but admit, that with the preſent means of our knowledge we are incapable of explaining the effect that is produced; yet it would not be very philoſophical to conclude, that any ſyſtem was abſolutely falſe, which, though it readily ſolved the chief phænomena required, might, in ſome inſulated inſtance, be found apparently incompetent to the taſk*. Should we not, if our ſyſtem be previouſly founded on a broad and ſolid induction, and eſtabliſhed on the firm baſis of experiment, be led to attribute rather any deficiency of explanation in any particular inſtance, to a want of knowledge of ſome minute links in the chain, than as involving any inſuperable obſtacle to the doctrine itſelf? And ſhould it not lead us, inſtead of abandoning, to review our theory with more circumſpection and a greater caution?

No one will be inclined, perhaps, to diſpute the Newtonian theory of gravitation, yet there are phænomena which it is difficult to conceive how it accounts for. Thus it would ſeem a contradiction to the eſtabliſhed principles of our great philoſopher, to ſuppoſe that a body divided into parts, however minute, can poſſibly aſcend in a fluid ſpecifically lighter than itſelf. A little acquaintance with the operations of chemiſtry will ſhow that this is a fact that occurs in the ſolution of various ſolids, and that without any motion being communicated to the veſſel containing them; the ſolid becoming diffuſed throughout the ſubſtance of the diſſolving fluid, and appearing to overcome the natural tendency of bodies towards the centre of the earth, and to have ſome new power of aſcent impreſſed on its particles.

To conclude, then, it appears that we are entitled to conſider caloric, in the preſent ſtate of our knowledge, as a ſubſtance *ſui generis*, not becauſe we can ſenſibly demonſtrate the truth of our opinion, but becauſe it is the leaſt exceptionable theory of the two, and becauſe the phænomenon of heat admits from it an interpretation more ſimple and obvious, and more agreeable to the analogy of nature. P.

* If our correſpondent had attended to this fact, that in the experiments alluded to, the Count, with all the care and precaution he employed, had not ſucceeded in inſulating from the contact of caloric the bodies ſubjected to friction, he would not have found them ſtand at all in the way of his argument. Was not the apparatus wholly immerſed in a bath of caloric—the atmoſphere?—Edit.

LV. *On the different Proportions of Carbon which constitute the various Qualities of Crude Iron and Steel.* By DAVID MUSHET, *Esq. of the Calder Iron Works* *.

IT is of considerable importance to the manufacturer to ascertain the absolute portion of charcoal which in his process becomes united with the metal to form cast iron. Having once admitted the fact, that it is in the ratio of the carbon presented to the metallic particles that he obtains a determinate quality of crude iron, experiment will enable him to deduce, that in manufacturing the richest natures of iron his produce from the ore will be more, by the extra quantity of carbon necessary to constitute this quality, than when the inferior numbers of iron are produced. In all cases, therefore, in making cast iron, considerable quantities of the coal become united with the iron, forming, by weight, a portion comparatively great.

From the usual processes hitherto employed in manufacturing blistered steel, the positive quantity of carbon which became united with the iron never became an object worthy of the attention of the manufacturer. It was sufficient to him that his bars possessed blisters sufficiently large and prominent to assure him that his steel was sufficiently converted. The unerring test of practice through a long series of operations confirmed the correctness of this deduction; and it must have appeared a matter of little importance to the formation of steel, that its direct operation of principle should be developed, or the laws which regulate its affinity in cementation.

No additional fact was necessary to the production of cast steel. The simple fusion of bar steel, regulated by such circumstances as practice, and the various uses to which this steel is applied, was all that was necessary to be known; the affinity of iron for carbon, and the various proportions in which it exists with the metal, forming the different qualities of steel, here met with no elucidation.

On the contrary, we still find that the union of iron and charcoal to form steel, is a matter of doubtful opinion among manufacturers, and the weight gained by its cementation generally denied.

It is only lately that a process has been brought into use for making cast steel, which has for its basis or principle the direct proportions of carbon necessary to form steel, illustrative at the same time of that beautiful phænomenon of affi-

* Communicated by the Author.

nity

nity betwixt iron and carbon which conſtitutes the endleſs varieties of this metal.

To inveſtigate this ſubject with accuracy, it appeared to me that a ſeries of experiments, made by the fuſion of bar iron with well prepared charcoal of wood in veſſels completely air-tight, would be productive of the greateſt nicety of reſult. With this view, the proportions of iron and charcoal recorded in the following experiments were each of them introduced into crucibles of Stourbridge clay while the clay was yet moiſt. The top was accurately cloſed upon the contents, and the crucibles ſet aſide to dry for eight or ten days. Previous to the introduction into the melting furnace they were baked in a common annealing fire to bring them to an ordinary red heat: from this furnace, while hot, they were ſucceſſively placed in the aſſay-furnace for reduction.

The following are the details of theſe experiments: Grains.

Exp. I. Pieces of Swediſh iron - - 1173

Charcoal $\frac{1}{2}$, or - grs. $581\frac{1}{2}$

The mixture was expoſed to a moderate degree of heat for 70 minutes, when the crucible was withdrawn. When cold it was carefully examined, and found free from cracks.

Charcoal untaken up, of a deep black colour, $310\frac{1}{2}$

Charcoal diſappeared in the fuſion - 271

equal to 48 per cent. of the original quantity.

The button obtained was ſupercarbonated crude iron, and weighed - - - - 1233

Gained in weight, by the combination of carbon, 60

equal to $\frac{1}{19}$th part fully of the firſt weight of iron.

The fracture of this button was exactly that of No. I. pig iron, towards the upper ſurface largely granulated; and below, a ſubſtratum of ſmall grained metal reſembling No. III. pig iron. Grains.

Total weight of the mixtures - $1754\frac{1}{2}$

Charcoal remaining $310\frac{1}{2}$ grains.

Iron obtained - 1233 grains. 1543

Total loſs in fuſion 211

The quantity of charcoal which diſappeared in this operation was equal to $\frac{1}{4\frac{1}{10}}$th part the weight of the iron.

Grains.

Exp. II. Swediſh bar iron - - 1093

Charcoal $\frac{1}{4}$, or - grs. $273\frac{1}{4}$

 The

	Grs.	Grs.
Brought over	$273\frac{1}{4}$	1093
The fusion of this mixture was effected in fifty minutes. When cold, the crucible was found entire, and containing of charcoal not taken up	$90\frac{1}{4}$	
Lost in the fusion	183	

equal to 67 per cent. of the original weight of charcoal. A fine button of supercarbonated crude iron was found, which weighed - - - 1143

Gained in weight by combination of charcoal 50

equal nearly to $\frac{1}{23}$d part the weight of the iron employed. The fracture of this button was inferior in point of lustre and prominency of crystal. It was, however, similar to the usual run of the No. I. iron of the manufacturer.

The quantity of charcoal which disappeared in this experiment was equal to $\frac{1}{6}$th part the weight of the iron.

		Grains.
The aggregate of the mixture weighed		$1366\frac{1}{4}$
Charcoal remaining	$90\frac{1}{4}$ grains.	
Iron obtained -	1143 grains.	$1233\frac{1}{4}$
Lost in weight upon the whole		133

Exp. III. Swedish iron in pieces - -		1143
Charcoal $\frac{1}{6}$th, or - grs.	190	
This mixture was fused in thirty minutes, and was allowed to cool to a low red heat before it was removed from the furnace. When cold, the crucible was found without cracks, and the charcoal not taken up weighed - -	55	
Charcoal disappeared, equal to 71 per cent.	135	

The metallic button now obtained was superbly carbonated so as to resemble a mass of carburet of iron. It weighed - - - - 1193

Original weight of the iron - - 1143

Gained by combination of charcoal 50

equal nearly to $\frac{1}{23}$d part the weight of the iron. All the surface presented by this button was uniformly covered with a rich and lustrous coating of carburet, indicating a superior quality of crude iron. The quantity of charcoal which disappeared on this fusion was equal to $\frac{1}{8\frac{1}{2}}$ part the weight of the iron employed.

The

		Grains.
The aggregate of mixture here weighed	-	1333
Charcoal not taken up	55 grains.	
Iron obtained -	1193 grains.	1248
	Total lofs in the melting	85

It is worthy of remark, that the lofs of charcoal in thefe experiments, together with the general lofs in fufion, diminifh in a feries proportionate to the quantity of charcoal introduced, bearing no perceptible relation to the quantity of iron. The weight gained by the metallic buttons refpectively, indicate nearly the fame degree of faturation of the carbonic principle, whatever difference of appearance exifted as to external appearance in No. III. It therefore appeared a matter of fome curiofity, worthy of inveftigation, what became of the extra lofs of carbon in No. I. and II. beyond that fuftained in No. I. when the richeft iron was produced.

Charcoal difappeared	No I.	II.	III.	
Equal to - -	$\frac{1}{4\frac{3}{10}}$	$\frac{1}{6}$	$\frac{1}{8\frac{1}{2}}$	
Gained by iron -	$\frac{1}{19}$	$\frac{1}{22}$	$\frac{1}{23}$	Average $\frac{1}{21\frac{1}{3}}$d part.
General lofs in fufion	211	133	85	

		Grains.
Exp. IV. Swedifh iron	- - -	1060
Charcoal $\frac{1}{10}$th, or	106 grains.	

From this mixture a very perfect fufion was obtained, in which all the charcoal difappeared except about $\frac{1}{2}$ grain, compofed of fmall granules poffeffing a rich deep black colour. The metallic button weighed 1093

Gained by the combination of charcoal 33

equal to $\frac{1}{32}$d part the weight of the iron employed. In addition to the weight of the button, the fides and top of the crucible contained a confiderable number of minute fpheres of iron poffeffing prifmatic colours. The furface of the prefent product was cryftallized in radii. The fracture was that of highly blown crude iron of a pale filvery white colour, marked with an imperfect cryftallization. The quantity of charcoal which difappeared in this operation was equal to $\frac{1}{10}$th part the quantity of iron at firft introduced.

	Grains.
The aggregate weight was - -	1166
Charcoal remaining $\frac{1}{2}$ grain, and iron obtained 1060	$1060\frac{1}{2}$
Total lofs in this fufion	$105\frac{1}{2}$

Exp. V. Swedifh iron	- - -	1000
Charcoal $\frac{1}{15}$th, or	- -	66

This fufion was found complete after half an hour's expofure.

 The

The charcoal had totally disappeared. The metallic button was regularly crystallized, and weighed 1000 grains, being exactly the same weight introduced. Besides the button, some thousands of metallic globules entirely covered the sides and concave top of the crucible. It was found impracticable to collect them all. By estimation they appeared to be from 15 to 20 grains. The fracture of this button was whitish blue, clear, and possessed a lustre similar to the fracture of zinc. An approximation to grain was visible, and, from minute comparison, a very early stage of steel was indicated.

	Grains.
The aggregate of the mixture weighed -	1066
Button of iron 1000, globules taken at 20, -	1020
Total loss in the fusion	46

Exp. VI. Swedish iron - - - 1035
Charcoal $\frac{1}{20}$th, or - 51½ grains.

The mixture yielded, by fusion, a metallic button partially crystallized, weighing - - 1032

Lost in fusion, equal to $\frac{1}{345}$th part, 3

The charcoal had entirely disappeared. The fracture of this button displayed a very regular grain, similar to that of steel. In subjecting it to forging and other tests, it proved to be steel of a soft quality.

	Grains.
Original weight of mixture -	1086½
Iron obtained - - -	1032
Total loss in fusion	54½

Exp. VII. Swedish iron - - - 1055
Charcoal $\frac{1}{30}$th part, or 35 grains.

A very perfect fusion was obtained from this exposure. The metallic button was found beneath a covering of glass possessed of a white streaky surface, the mass of which was of the transparency and colour of a smoky topaz: it was found to weigh - 1052

Lost in fusion, equal to $\frac{1}{351}$ part the original iron 3

In this experiment, also, the charcoal had completely disappeared. The present product was minutely examined, but gave no indications of steel. After being deeply cut with a chisel, it was broken with very great difficulty across the anvil. It was then forged, and plunged hot into water; but did not harden. It generally resembled those qualities of iron obtained by fusion with earths and glasses. This experiment was repeated four times, and always attended with a similar result;

refult; fo that it feemed deducible from it, that $\frac{1}{3}$th part of carbon in addition to any quantity of iron was infufficient to form fteel; and, referring to the refult of No. VI., it appeared that even $\frac{1}{20}$th part formed a fteel much too foft for the general purpofes of manufacture. This conclufion, however, being at variance with facts I had already eftablifhed upon the formation of caft fteel in common crucibles, was here inadmiffible. It was therefore neceffary to feek for an explanation of the phænomenon of the charcoal difappearing in clofe veffels, formerly alluded to, before any certain knowledge of the exact quantities of charcoal could be afcertained, which were neceffary to form either caft iron or fteel in veffels made impervious to the air *. I uniformly remarked in the prefent experiments, that when the quantity of charcoal introduced was from $\frac{1}{30}$th to $\frac{1}{200}$th the weight of the iron, a portion of glafs was conftantly formed upon the furface of the metallic button. The quantity generally increafed as the proportion of charcoal decreafed; fo that, in fome experiments, 300, 350, and 400 grains of amber-colour glafs was obtained. The upper furface of this glafs was frequently of a pure pearly white colour. In one experiment, where $\frac{1}{70}$th of charcoal was ufed, I obtained a large quantity of glafs cellular throughout. Each cell was furmounted upon the top with concentric circles of pearly lines, forming a curious and pleafing effect.

Having fully fatisfied myfelf that operations performed in clofe veffels thus prepared were fubject to uncertainty, arifing as well from the formation of glafs as from fome unknown affinity exerted upon the charcoal; and having performed feveral experiments with well filled open crucibles, with charcoal alone, wherein I found little comparative lofs, I performed a very accurate feries of experiments, which fhall be forwarded for the next number of the Magazine.

LVI. *On the Fate and Character of the Monk* ROGER BACON. *By Governor* POWNALL. *Read before the Literary and Philofophical Society, Bath* †.

THERE has been much buftle about learning in the world: every age has had its learning; the produce and growth of a defultory activity of the imagination, according to the fafhion of thinking prevalent at the time: whilft the

* This inveftigation will form a feparate pap.r for July.

† Communicated by the Author.

 flow

flow silent progress of the reasoning power is hardly ever noticed in its operations, and seldom received in its effects when they come forward, because not understood at the time.

It would be a curious detail were we to enumerate the various fancies and follies which have been called learning, which have been studied and applauded as such in every age and every country; but it is at the same time a humiliating truth, that knowledge, which it should seem is congenial to the right operation of the human intellect in the investigation of truth, has come forward to men but slowly and sparingly, and at epochas of very distant periods, and at the intervals of very distant centuries: and what is more humiliating in the history of man is, that, when at any time it appears, it comes into the world as an *enfant trouvé*, and is received as such, rather than nurtured by those whose relation to it should be in practice, as it is in nature, congenial to it. True knowledge is therefore generally either despised by the conceit of the learning in fashion, or more generally suspected as dangerous; which, indeed, it always is to the false and artificial authority which assumes and takes the lead in the world.

It is with pain that I have to notice several eminent examples of this melancholy truth in the course of my stating the character and fate of the monk Roger Bacon, who is the subject of this paper.

When Socrates came forward to the world, the powers of the human mind, absorbed in the false learning of Greece, had been employed to propagate the sophistry and support the sophists of the time, whose system and business were to keep mankind from true knowledge by an avowed principle of such ignorance as was incapable of attaining truth; the purport of which system was to hold men in a servile obedience to authority, as those who had the command of that authority should impose it. Socrates in his theology taught only those truths which had been known to the antient philosophers of Greece, and which they had brought from the east. What they affected to conceal he taught openly, so far as went to establish the knowledge of *a moral system* connecting the divine and human nature. *Here he was original:* this was a new truth brought forward to the world. In this, as it is truly said of him, he brought down truth from heaven, and established it on a moral foundation on earth.

As to his manner of teaching, he with great address, in accommodation to the learning of the time, professed to commence in ignorance. But whilst the sophists led, in synthetic reasoning, from ignorance to a total incapacity in man for truth; he, commencing his theorem from stated

9 ignorance,

ignorance, proceeded by *analytic induction* to the eliciting of truth: as the algebraists have since done, from a theorem stated on an unknown quantity, bringing out the true one. He thus not only brought truth to light by reason, but by the very mode of his reasoning taught the use, the extent, and the right of human reason. This was sufficient to decide the fate which both the teacher and his doctrines experienced. His doctrines were despised, and attempts were made by the wits of the time to ridicule his knowledge: it was represented as evolved from the clouds*. But, as truth will stand the test of ridicule, nothing remained but *to decide by authority* that the truths which he taught were inadmissible; and they were therefore *interdicted by power:* and Socrates himself was prosecuted and condemned to death as a false teacher, as a corrupter of men, an heretic, unbeliever, and despiser of the gods! What is still more humiliating to the character of man, the doctrines which he taught, and the truths which he published, were perverted where received, and even by his disciples. The homely simplicity of his doctrines was sophisticated and corrupted, in a line of learning, to uses perversive of their nature.

I will not here glance, although it is in my mind's eye, at the similar fate of a Divine teacher who came forward in the world to enlighten man, and to instruct him in his most essential duty and interest.

Pursuing therefore the fate of Socrates, which is not irrelative to the matter of this paper, I shall proceed to state that Plato, his favourite disciple, who was a really great mathematician, and particularly conversant in the *analytic process* of that science, and who followed the steps of induction marked out by Socrates, mixed so many mystic conceits and theories, that he sublimed and subtilized truth to a mere learned vision, such as the latter Platonists afterward taught.

Aristotle, on the other hand, a distinguished disciple of Plato, converted the knowledge of physics, both material and intellectual, into *an artificial system* of axioms, data, and maxims, and formed thereon *a logical frame* of synthesis; which gave occasion to his immediate followers and to the Peripatetic school to found their learning on the authority of teaching, instead of being founded on *truth* led by experience to knowledge, to the disgrace of human intellect, and the perversion of true philosophy.

It is lamentable to reflect how many theories and mysteries, divine as well as natural, sprang from this foul ground;

* Vide the Nubes Aristophanis.

which,

which, like meteors, blazed in the regions of learning for a time, but, like ſuch meteors, not deriving from true and permanent light, from proper ſources, were ſoon extinguiſhed.

Truth, however, was purſued with great energy, *occulto velut arbor in ævo*, in the ſtudies and labours of thoſe few votaries of it who were called mathematicians; and many detached unconnected diſcoveries were made, which hardly ventured to ſhow themſelves, or, where they did, were imputed to magic.

At length, after many ages of darkneſs, Roger Bacon, that great luminary, was born in 1214. He, like another Socrates, in the ſpirit of truth, in the analytic line of true knowledge, endeavoured to ſtate the *Impedimenta ſcientiæ*, the *Cauſas ignorantiæ*, and to remove all the obſtructions which ſhut up the road to truth: he then, explaining by fact and example the *Utilitas ſcientiarum*, advanced, by an experimental inveſtigation of nature, and by an analyſis of truth, to his *Reſtauratio humani intellectûs*, and his *Inſtauratio ſcientiæ.*

Reaſoning, however, above the ſcale of the learning of the age in which he lived, his doctrines were neither underſtood nor received; and, where they could not be repreſſed by the learning of the time, *they were interdicted by the ruling authority* of it. Although the truths which he diſcovered, and endeavoured to diſcloſe to the world, were liſtened to by ſeveral men of knowledge, and ſome of them in power *, and though they were received even by the *ſupreme* † authority, held infallible; yet, by *a ſin againſt the holy ſpirit of truth*, they were unacknowledged, and ſuffered to languiſh in obſcurity. They, with the teacher, were delivered over to perſecution; truth was extinguiſhed, and the diſcoverer and teacher impriſoned for the crimes of magic and hereſy.

Here again, on the ſtage of this world, was truth devoted to ſilence and darkneſs, and the diſcoverer and teacher of it thrown back into obſcurity, and his doctrines loſt in oblivion, except what ignorance rakes up to memory in diſgraceful tales of tradition. The ſtream, therefore, of this original ſpring, whence the knowledge of latter times derives its ſource, ran, like the Rhone, concealed and unknown for three or four centuries; whilſt in the mean time various empirics in philoſophy, various baſeleſs theories, (amidſt which the ingenious fables of Deſcartes more eſpecially,) took the lead and aſcendancy in the learning of the world. This ſtream, however, like the Rhone, emerged again to light on the face

* Biſhop Groſthead. † By the Pope.

of

of the earth, when its courſe became directed with happier ſucceſs, and under more fortunate auſpices, by the energy and ſpirit of *another Bacon*, the great lord St. Albans, who, although perhaps an original alſo, was a ſecond and more fortunate one; yet even not generally known and acknowledged as ſuch till near half a century was elapſed from the time in which he wrote.

Although Sir Iſaac Newton had made the chief part of his diſcoveries on light and colours, and had written the firſt parts of his Treatiſe on Optics, and, "at the deſire of ſeveral gentlemen of the Royal Society, ſent them to the ſecretary of that ſociety, where they were read in 1675; and although the other parts were added about 12 years after; yet to avoid, as he ſays, being engaged in diſputes about theſe matters, he delayed the printing and publiſhing of them to 1704; and would ſtill have delayed, had not the importunity of ſome friends overcome the repreſſion which he experienced from the jealouſy of ſome authoritative leaders." Such is the invariable ſpirit and temper of man even in an enlightened age, and ſuch is the unavoidable fate of thoſe who would enlighten and aſſiſt the knowledge of the world. Such, at leaſt, were the feelings of this great and humble ſpirit; ſuch was his conviction, that the age in which he firſt made his diſcoveries was neither willing to receive nor capable of poſſeſſing them, to the purpoſe of truth, that the world might have been deprived of them for ever, as in fact it is at this moment of ſome diſcoveries which he had in view and in purſuit, but which he ceaſed to follow up, and has ſuggeſted only in queries. What a loſs to mankind that theſe ſuggeſtions, except in ſome particular inſtances, have not been purſued! For, whatever and howſoever great the diſcoveries which have been brought forward by the new philoſophic chemiſts may be, yet theſe important ſuggeſtions would, as they have done in ſome inſtances where they have been taken up, have led to great and intereſting philoſophic facts hardly at preſent conceivable.

It was from the ſame opinion of the temper and want of qualification in that age, perhaps alſo from an oppreſſion of ſpirit which he felt, that on another occaſion, when conſulted by Dr. Bentley, at the time of his being appointed to preach the *Boyle's* lectures, Sir Iſaac ſays in his letter, "There is yet another argument in proof of the Deity, which I take to be a very ſtrong one; but until the principles on which it is grounded are better received, I think it more adviſeable to let it ſleep." Such has ever been the fate of knowledge, both to the diſcoverers and to truth itſelf, on its firſt appearances

appearances amongſt thoſe who hold the lead of learning. Shall we then wonder at the fate which this true and real philoſopher, the monk Roger Bacon, experienced in an ignorant, ſuperſtitious, and ſervilely debaſed age?

To point out the real merit and character of this wonderful man, which is the purport of this paper, I ſhall now proceed to a review and general analyſis of his doctrines as ſtated in his *Opus Majus*.

I truſt it will be found to be an amazing extenſive plan, founded in experience of nature and on the analyſis of truth; conducted by the moſt penetrating inventive faculties of induction, and the moſt exact preciſion of combination; that it is, as I ſaid before, a *Reſtauratio humani intellectûs*, and an *Inſtauratio ſcientiæ*.

I. It firſt ſtates the cauſes of error, and then clears the ground of them; diſcards all pretenſions to knowledge which obtrude themſelves under the form of learning.

Sufficit nobis in inquiſitione proprii intellectûs ut, quantum poſſimus, cauſas et occaſiones erroris extraneas longius a debilitate ſenſûs noſtri religamus. Quatuor verò ſunt maxima comprehendendæ veritatis offendicula, quæ omnem quemcunque ſapientem impediunt; et vix aliquem permittunt advorſùm titulum ſapientiæ pervenire.

1mo. Fragilis et indignæ authoritatis exemplum.

Mark here the ground on which, in thoſe days of eccleſiaſtical and civil tyranny, the crime of hereſy againſt the church, and of treaſon againſt the ſtate, might be imputed, and which was the real ground of his perſecution and impriſonment.

2do. Conſuetudinis diuturnitas.

3tio. Vulgi ſenſus imperiti.

4to. Propriæ ignorantiæ occultatio cum oſtentatione ſapientiæ.

His omnis homo involvitur, omnis ſtatus occupatur.

II. Theſe four points he examines, explains, and diſcuſſes ſpecially and at full length under their ſeveral heads. The ground thus cleared, and the foundations thus laid, he proceeds in the *ſecond part* in analyſing grammar to its elements; wherein he not only goes to the art of language, but to the powers and operations of the mind, by an inquiſition into the human underſtanding, as Mr. Locke, and Mr. Harris in his Hermes did afterward.

Then, paſſing by the commonly received mode of reaſoning, as it moves in the trammels of logic, which this great original ſtates to be not only perverſive, but at beſt a mere left-handed hold by which we take truths already diſcovered;

by

by which we lock them up in an artificial ſyſtem, and which is of no uſe or aid in the purſuit and the diſcovery and invention of truth; he then treats of the virtue, power, and uſe of mathematics, properly ſo called, both analytic in the inveſtigation of truth, and ſynthetic in conducting it to practical ſcience.

4. By this conductor he goes firſt through the inveſtigation, and next through the didactive explanation and proof of the powers and operations of nature, by the principles of the laws of motion, attraction, gravity. Guided by theſe principles he proceeds to the laws of viſion, perſpective, and optics in general; and to the laws by which light is acted upon. And here he goes to the mechanical and inſtrumental, ſo as to explain the practical part, bringing forward many improved uſes and applications leading to new diſcoveries.

5. He proceeds hence to aſtronomy, in an inveſtigation of the phænomena of the heavenly bodies. In this part ſome remains of aſtrological prejudices not yet eradicated obſtruct his inſtitute.

6. The heads above ſtated take up five parts out of ſix into which his *Opus Majus* is divided. He then, in the *ſixth part*, comes to the main ground and true baſis on which the *Inſtauratio ſcientiæ* is founded and built up, which is *experimental induction*. This he explains in detail, and illuſtrates by example; recurring again and again to the phænomena of nature terreſtrial and celeſtial:—* Duo ſunt modi cognoſcendi, ſcilicet, per argumentum et experimentum. Argumentum concludit, et facit concludere quæſtionem; ſed non certificat, neque removet dubitationem, ut quieſcat animus in intuitu veritatis, niſi eam inveniat viâ experientiæ.

Et quia hæc ſcientia experimentalis *à vulgo ſtudientium eſt penitùs ignorata*, ideo non poſſum perſuadere de ejus utilitate, niſi ſimul ejus virtus et proprietas oſtendantur.

Whoever, diveſted of preoccupation and prejudice as they ſpring from the *Conſuetudinis* † *diuturnitas*, and the faſcination of received faſhion in thinking, reads this great work, with comparative reference to the *Inſtauratio ſcientiarum* of the ſecond Bacon, the lord St. Albans, will find both works ſimilarly grounded, and ſimilarly conducted, as to the method and form; both alike built up on an inſtitute of induction by *experimental ſcience*; both in the execution purſuing the ſame line to the ſame point; and muſt conclude either, according to the old proverb, "*that great wits jump*;" or that the latter Bacon, although perhaps equally an original as the

* Cap. 1 and 2, partis 6tæ, de Scientiâ experimentali.

† Explained in the ſecond head of part i.

firſt,

firſt, had read and received a reflected ray of illumination from the works of the firſt Bacon, our philoſopher.

Theſe works of the monk had been buried, amidſt the manuſcripts of ſuch learning as is never read, for three or four hundred years before the time in which the lord St. Albans wrote; and continued in the ſame ſtate for near a century after, to 1733, when they were publiſhed by the learned editor Dr. Jebb; yet they might not have eſcaped the ſcrutinizing, induſtrious, inquiſitive genius and informed ſtudies of the ſecond great philoſopher. But let me here repeat, that whatever cauſe or chance firſt wrought this ſecond great genius into operation, he was equally an original as the firſt. For, whoever conſiders the proceſs of the operations of intellect, and the origin and progreſs of knowledge, will find it difficult, if not impoſſible, to mark what that ſpring, internal or external, is which firſt becomes a cauſe from a coincidence of ideas that gives courſe to it.

As it hath become an almoſt generally received opinion, that the inſtauration of true philoſophy in the preſent age derives its ſource primarily and ſolely from Bacon the lord St. Albans, I cannot but think it juſt in this paper to endeavour to reſtore to the *firſt original* that portion of merit which is due to him; and fairly to appreciate the former, without derogating from the latter: and to recommend to this ſociety to trace back the origin of their philoſophy to a more remote ſource, ariſing within the bounds of the county in which this ſociety is eſtabliſhed *; from a ſpring which, however *illaudatus*, unhonoured, and almoſt loſt in antiquity, will always be a peculiar honour to this county, as generally ſo, in every extent and degree, to the Britiſh nation at large.

Almoſt all ſocieties, originally from ſuperſtition, but ſince from the habit of cuſtom, have adopted ſome ſuppoſed patron ſaint. Inſtead of adopting a patron, let this ſociety avow as their pattern the philoſopher monk. Though no ſaint, it would be an honour to any philoſophic ſociety to adopt his *principles*, and to follow his example.

1ſt, To adopt his principles by an independent ſpirit of philoſophizing, which will not be repreſſed or kept down by the authority of any name howſoever great; will not be reſtrained within the bounds of any ſociety or ſect howſoever reſpectable; will not ſubmit to any dominant lead, but, without oppoſition or faction againſt others, will unabated follow the ſteps of truth with undeviating inveſtigation; by an ar-

* Roger Bacon was born near Ilcheſter.

dour

dour which knows no repulfe, yet with an humility of diffidence which knows no prefumption.

2dly, By a temper which repels every tincture of fafhion howfoever honoured by the times or confecrated by antiquity, *confuetudinis diuturnitate.*

3dly, By a judgment which, whilft it refpects the experience of mankind, defpifes the prejudices of the vulgar, great or fmall.

4thly, By a true pride, which, confcious of what it is capable of, knows how to commence its firft fteps to knowledge, by feeling and acknowledging its ignorance; and not by a falfe fhame attempting to hide it, and with a ftill more ridiculous vanity labouring to clothe it in the femblance and under the garb of learning. Of this tribe of learned gentlemen our monk fays, "*Apparentia folùm tenet eos, non curant quid fciunt*; fed quid videantur fcire coram multitudine infenfatâ."

Having thus adopted his principles, let us *follow his example alfo* in the mode of philofophizing; 1ft, By a patient induction and cautious combination of particulars, without being hafty to form them into fyftems on the firft flattering femblance of difcovery: 2d, That we do not fuborn affumed experiments to bear falfe witnefs to fuppofed facts; but to take more merit from the truth difcovered than from the difcovery; and to eradicate, if it be poffible, all vanity from the heart, *et veritatem fine dote quærere.*

Laftly, It will not be beyond the fcope of this paper, and, I would hope, not offenfive to this learned fociety, to fuggeft, whether amongft their other learned ftudies and philofophical refearches there may not be many a commenced, though interrupted, inftitute of experiments; many a glimpfe of difcovery not yet brought out to open day in the works of this great original philofopher, well worth their attention for examination at leaft: yet

Non jurare in verba magiftri.

I fhall not here take notice of the difcovery of lenses, nor of their firft ufes in magnifying objects, and particularly as reading-glaffes; becaufe the refraction and reflection of light, and of the rays of the fun, by lenses and mirrors, were known to the antients *, though I do not meet with any mention of their ufe to aid in *reading*. But I believe the antients wrote in fuch uncials as fuited the powers of the eye without this aid. Another reafon why I do not take notice of this as a difcovery to be imputed to Bacon is,

* Vide Scholiaft to the Nubes of Ariftophanes.

that

that he mentions the power and operations of glaſſes on light, (if I * remember and underſtand him aright,) rather explaining theſe operations already in uſe, than as a diſcovery made by himſelf. I think, however, that it is clearly a fact, that, like Sir Iſaac Newton, and like our Herſchel, the honour of the preſent age, he made thoſe arrangements which produced the teleſcope; and that, like them, he applied his improvements to aſtronomical obſervations. He particularly mentions a previous trial which he made, as our common mechanic opticians do, of reading ſmall writing at a very great diſtance.

I do not mention the diſcovery of *gunpowder* as imputed to our monk, becauſe, although he † gives the mode of making this powder; and deſcribes its exploſive and coruſcant power; and clearly points out that he had in his mind a deſtructive uſe to which it might be applied in *comburation*, made to burn at a great diſtance: yet he withholds himſelf from explaining *this uſe*; and does not in any part mention its exploſion, as applied by the *inſtrumentality of tubes* to a projectile force that was to throw the miſſile weapons of cogles or balls. Which application of it renders it ſpecifically *gunpowder.* His words are: "In omnem diſtantiam, quam volumus, poſſumus artificialiter componere *ignem comburentem* ex ſale-petra et aliis." What theſe other articles are he mentions in another part of his works, namely, *ſulphur* and *charcoal.*

I cannot reſtrain myſelf from mentioning the idle traditional ſtory of friar Bacon's *brazen head*, which was to ſpeak and announce time, becauſe I think it founded in a real though miſtaken fact. From conſidering what one may elicit out of the nonſenſe of it, I have perſuaded myſelf that it was an experiment in horology, and the model of a clock meaſuring time by the oſcillation of a pendulum, and by various movements in *the face* of this head, (as is actually the exiſting fact in the clock at Baſle,) marking the diviſions of time, together with a mechaniſm that ſhould announce by ſound thoſe diviſions, which, in alluſion to the *face* and *head*, was called ſpeaking; ſimilar to the crowing of the cock on the clock at Straſbourg, or to the cuckoo on the wooden clocks which the Germans ſell about our ſtreets.

There remains ſtill to be mentioned, inconteſtably to his unrivalled honour, that he was the firſt diſcoverer of the error of meaſuring time by a falſe period of the year; and the firſt projector of the reform of the calendar and other

* Removed from my books, I quote from memory.

† In his tract De Secretis Operibus Artis et Naturæ.

tables

tables of time, founded on his own aſtronomical obſervations; which reform he propoſed to pope Clement V. It was the fate of this propoſal to be neglected; whilſt the imperfect Gregorian reckoning, which it hath been found neceſſary ſince to correct, was adopted. I will cloſe this paper with a tranſlation of the words of Dr. Jebb, the learned editor of the *Opus Majus*, as they ſtand in his preface:—"The propoſal made by Bacon to the pope Clement was one of the nobleſt efforts of human induſtry: conſidered in all the circumſtances under which it was framed and compoſed, it is one of the moſt ſtupendous inſtances of the force of human genius which hath been recorded; and will do honour to our learned countryman as long as the ſun and moon endure."

LVII. *Obſervations which ſeem to prove the Neceſſity of obſerving and meditating a long Time before any deciſive Opinion is formed in Philoſophy in general, and particularly in regard to the Cauſe of Earthquakes. By* COUREJOLLES*.

HOWEVER painful may be the reſearches neceſſary for diſcovering the cauſes which Nature employs in her operations, we muſt obſerve with great attention and indefatigable aſſiduity in order to collect thoſe facts which are propereſt for conducting us to the attainment of this object. It does not belong to men to invent cauſes; they exiſt in nature, and it is their buſineſs to diſcover them.

If in the exact ſciences the preciſion of a reſult eſcapes us, and if we cannot extend our conception but to that term where incommenſurability of dimenſions ſtops calculation, how can the creators of ſyſtems pretend to the privilege of penetrating to the bottom of the abyſſes where Nature has concealed her primitive cauſes? Nothing then but experience, obſervation, and compariſon, guided by geometrical method, can conduct us with wiſdom in our reſearches.

Senſible of this truth, I have thought it my duty to follow this progreſs in my obſervations on every thing that may relate to the cauſe of earthquakes; for it would be imprudent to aſſign one before a variety of facts are eſtabliſhed. This method is the more certain, as Nature often ſpontaneouſly unveils herſelf: but a long time is neceſſary to collect good obſervations: and we ought never to be precipitate in making them appear, becauſe in the ſtudy of the ſciences there is an age at which the paſſions blind us by a too ardent deſire

* From the *Journal de Phyſique*, Pluvioſe, an. 10.

of acquiring an early reputation; and, when we have obtained it in that manner, too great a degree of self-love obliges us to maintain our errors, and to cover them with darkness, that we may preserve them from the attacks of truth. From this weakness of the human mind, we may judge how dangerous we become to those desirous of being instructed, when, under the shield of a reputation, we employ every means in our power to perpetuate an erroneous opinion. Men would be much happier than they are, if those whose documents they follow had proceeded only step by step in their researches, and had been able to free themselves from those transports of pride which they improperly confound with that kind of self-love which conducts to good. They ought to have the courage to retract their errors; but unfortunately we have no school to teach us to employ a few words so difficult to be pronounced—" I am in the wrong."

The habit of reflecting on, and comparing for a long time, a great number of facts respecting earthquakes, might embolden me to pronounce in regard to the manner in which subterranean matters inflame, either by the decomposition of water and pyrites, or by other causes, neither the nature nor movement of which is perceived. I, however, acknowledge my insufficiency to be such, that I dare not venture to decide on the real cause of earthquakes. Had I listened only to the voice of self-love, I should soon have fabricated a system; but I cannot do better than submit my remarks to the examination of the most acute philosophers, who, perhaps, will succeed much better than I could have done. I shall therefore give an extract from a very extensive work which I composed on earthquakes, but which having been lost by the burning of Cape François, I can only mention the principal observations, which may serve as data to those employed in similar researches.

1. Earthquakes are more strongly felt in all parts which front the west, than in those which have a different situation.

2. The earthquakes felt in districts of great extent which face the north have never occasioned shocks sufficiently strong to throw down houses.

3. Districts which face the south have frequently experienced those violent shocks which occasion great damage; but this situation is less dangerous than that of districts which front the west.

4. Districts which look towards the east are sometimes subject to slight earthquakes.

5. Large islands, such as Madagascar, Borneo, Ceylon, Java, Celebes, the Moluccas, the Philippines, the Japan

isles, the Great Antilles, and all the other extensive islands of the globe, experience earthquakes in the same manner as continents; that is to say, with more violence in the districts which front the west and the south, than in those which look towards any other quarter.

6. Small islands, in consequence of their small extent, feel the effects of earthquakes almost in an equal manner throughout all their parts: it is observed, in particular, that the most violent shocks are always towards the west and south.

7. Parts of the earth confined between two seas, as the isthmuses of Porto Bello, Suez, and several others, experience the effects of earthquakes on the two opposite sides.

8. In a great extent of coast which looks towards the north, when changes of direction occur so as to look towards the west, as that of the northern part of Africa towards Oran, or as that of the gulph of Marecaybo, earthquakes are accompanied with peculiarities analogous to their position; that is to say, those parts of the coasts which look towards the west are subject to very violent earthquakes.

9. Small islands lying to the west or south of continents, or of other islands much larger, experience very violent shocks. Several islands which were in this situation have disappeared in consequence of violent earthquakes, by which they were swallowed up in the sea.

10. Among the coasts which face the west or the south, there are places where earthquakes are felt with more violence than in others: it is almost always in these places that the most violent shocks are repeated.

11. Great earthquakes are almost always preceded and followed, some time before and after, by small shocks.

12. Two earthquakes have often taken place on the same day at two places at a considerable distance, and separated from each other by sea or by intermediate lands which have not experienced the least shock.

13. Great earthquakes almost always take place after long rains.

14. Earthquakes have often been followed by malignant fevers and epidemical diseases.

15. Earthquakes are sometimes preceded or followed by hurricanes.

16. We had a very violent hurricane at Cape François about an hour before the famous earthquake on the 3d of June 1770. This hurricane came from the mountain called La Charboniere, more than 40 leagues distant from the Cape. The earthquake overturned this mountain, so that many of the inhabitants, negroes, and cattle, were swallowed up.

 17. The

17. The water barometer eſtabliſhed in my great meteorograph indicated on that day a depreſſion of two inches eight lines: in general, it fell only two inches during the greateſt variations.

18. The ſtorm experienced throughout the whole iſland of St. Domingo in 1788, proceeded alſo from the mountain De la Charboniere. This I aſcertained in the following manner: A few days after the ſtorm, having made a tour through the iſland on a particular miſſion, I ſaw in every place where I paſſed that the trees overturned by the hurricane were lying in the direction of radii proceeding from the ſummit of the mountain to the different places which I viſited, though almoſt all diſtant from its baſe 15, 20, and 30 leagues.

As certain winds occaſion rain, while others occaſion drought, it will be neceſſary that I ſhould mention their meteorological effects, to be able to judge of the influence which they have on earthquakes and volcanoes.

19. It is well known that when two contiguous parts of the atmoſphere are of different denſities, the denſeſt part, which conſequently has the greateſt elaſticity, moves towards that which has the leſs, forming a current of air, which extends towards the ſide where there is the leaſt reſiſtance.

20. The current of the trade winds at ſea follows the courſe of the ſun; but it changes its direction near iſlands and continents.

21. The eaſtern part of the iſland of St. Domingo receives the trade wind without changing its direction.

22. The whole north coaſt of the ſame iſland changes the direction of the trade wind; and, inſtead of ſuffering it to continue its natural courſe to the weſt, attracts it, and makes it incline towards the land, to form a compound current proceeding from north-eaſt to ſouth-weſt.

23. The weſtern part, from Port à Piment to Port au Prince, attracts the air of the weſt towards the eaſt, in a direction altogether contrary to the trade wind, and to that of the eaſtern part, or of Samana.

24. The ſouthern part of the ſame iſland attracts the trade wind, and makes it change its natural direction from eaſt to weſt, to form a current of air proceeding from ſouth-eaſt to north-weſt.

25. All changes which take place in regard to the current of the trade winds ariſe, in all probability, from the ſun darting his rays in the ſolid parts of the iſland, which imbibe them in the ratio of their denſities, and even of their colours.

26. All

26. All thefe warm bodies dilate the ambient air of the atmofphere by which they are covered.

27. The vapours of the fea, being attracted around all the coafts towards the centre of the ifland, according to the 20th, 21ft, 22d, 23d, and 24th obfervations, produce an afcent of the clouds much more confiderable on land than at fea. This elevation of the vapours appears in a very remarkable manner in all the iflands of the torrid zone, when one is at the diftance of fix or eight leagues from them, and even more. Experienced mariners employ this indication fometimes to determine the proximity of land.

28. All the different breezes here mentioned change their direction in the night-time: they decreafe in velocity from fun-fet to 9, 10, or 11 at night. After which a calm takes place for fome moments; the land wind then prevails during the whole night, in confequence of the condenfation of the vapours of the atmofphere, which being cooled, and acquiring gravity by the abfence of the fun, in their defcent expel the ambient air, which they prefs on all fides from the centre towards the circumference, that is to fay, from the interior of the ifland towards the fea: this is what is called the land breeze.

29. Of all the breezes here mentioned, the eaft and north winds are more frequent in the countries of the torrid zone than thofe of the weft and fouth, except in the diftricts mentioned in the 23d and 24th obfervations.

30. The mountains form fo many dikes that oppofe the courfe of the clouds which are there accumulated, and, being condenfed, fall down in rain on the fides and at the bottom of the mountains.

31. The plains on the fide where the clouds are accumulated are fertile, and thofe on the oppofite fide are dry. Thefe mountains therefore divide thefe countries into two different climates.

32. Earthquakes are much more frequent in arid countries than in diftricts watered by rain; becaufe, in all probability, the vapours of the interior part of the country are there collected, and proceed thither more than to thofe where the rains render the earth compact.

33. The faces of mountains which look towards the north and the eaft, are more cooled by the rains than thofe which look towards the fouth and weft.

34. In the Spanifh plains, fituated in the centre of Saint Domingo, there are feveral infulated mountains, which, notwithftanding their fmall extent, exhibit the different temperatures indicated around their fides by fhades of verdure ana-

logous to the coolnefs or drynefs occafioned by thefe different pofitions.

35. The mountains of countries fubject to great rains abound, in general, with very hard rocks which rife abruptly; the places which are lefs fteep are covered with compact, red, black, and fat earth, which produces vigorous trees of a dark green foliage, loaded with creeping plants, and furrounded by herbaceous fhrubs, and heaps of manure arifing from the rotten leaves and trunks.

36. The mountains of arid countries, being compofed of a great deal of light earth, are rounded at the fummits; they have in general a more gentle declivity than thofe of rainy countries, and, when too rapid, fragments of them crumble down: the trees which grow on thefe mountains produce very hard timber, and fcarcely any creeping plants are found near them.

37. The earth of mountains watered by rains being compact, as obferved in the 35th obfervation, does not permit the rain water to penetrate them; it trickles down the furface, and flows into the ravines, at the fame time that it falls from the heavens.

38. The mountains in arid countries are in general covered with calcareous earth or chalk, and the fragments of other tender ftones mixed with the powder of dried vegetables, which altogether form a fpongy light earth. This earth is eafily penetrated by rain water, and it retains it in the bofom of the mountains or of the eminences by which they are furrounded. This water efcapes merely by a flow filtration, which produces continual fprings that maintain the ftreams in the ravines; and thefe, in their turn, fupply the rivers that fall into the fea. It is for this reafon that thofe parts of St. Domingo towards the weft have more ravines and running ftreams than the northern and eaftern parts of the ifland. The cafe is the fame in the Great Antilles.

39. The calcareous ftones found on the mountains of the weft part of St. Domingo exhibit the empty nefts of the polypes by which they were formed. Thefe foffils are found in all the mountains of St. Marc and the Gonaives in fuch abundance, that when broken with a large hammer it is more difficult to find any without the impreffion of a madrepore than thofe from which it is effaced *.

40. In

* To afcertain with certainty whether thefe foffils have been left in confequence of the furface of the fea becoming lower, I caufed a well to be dug at the bottom of a rock on a plantation which I had oppofite to the Cape Town, in order to erect there a machine I had conftructed at a great expenfe,

40. In the north-eaſt part of St. Domingo it is obſerved that the calcareous ſtones of the mountains are very hard, and exhibit no traces of their origin: they muſt be poliſhed before it is poſſible to diſtinguiſh in them, as in other marbles of the ſame kind, corals, the polypiers of madrepores, and the other characteriſtic marks of calcareous ſtones.

41. The diſtricts which front the weſt are thoſe where volcanoes manifeſt themſelves in preference to others.

42. Next to the diſtricts which front the weſt, thoſe which look towards the ſouth are thoſe where ſubterranean fires ofteneſt produce volcanoes.

43. Extenſive diſtricts which look directly to the eaſt have no volcanoes, unleſs a continent or large iſland be to the eaſt of the place where they might manifeſt themſelves.

44. The volcanoes of Sicily, according to ſeveral obſervations made on different eruptions, ſeem to be only ramifications of Veſuvius ſituated to the north-eaſt of Ætna, Lipari, and the other ſpiracles in the weſtern part of the kingdom of Naples.

45. There are only two or three volcanoes in all thoſe parts of the globe which are known, that incline towards the ſouth-eaſt: they are thoſe of the iſland of Bourbon and of Kamtſchatka.

46. In no part of the known world is there a volcano in a ſituation that looks towards the north.

47. Of the great number of volcanoes found in ſituations looking to the weſt, there are 42 in the courſe of 400 leagues

expenſe, that poſterity might know it in a poſitive manner; but, being in haſte to ſet out for France, in conſequence of my being appointed a deputy of the colony, and as my conſtituents pretended that the aſſembly of the ſtates-general would be ended before my arrival if I did not ſpeedily depart, I was obliged to leave this machine in May 1789, without being able to finiſh it. It conſiſted chiefly of various movements which turned four large copper cylinders, each ſix feet in height, with their floats and other apparatus, according to the form of my meteorographs, in order to mark the height of the tides each day, every month, and every lunar year for a century.

The riſing and falling of the ſea was to make the water enter and flow out by a canal a foot in breadth: a ſmall aperture of ſix lines only in diameter placed at the bottom of the well was to receive the water of this canal ſo as to ſuffer it to riſe and fall only in a very gentle manner. A large float of copper hermetically ſealed was to ſerve as a moving power to wind up this clock each tide by means of a kind of ſcapement.

But unfortunately at the time the Cape was burnt my houſe was plundered, and all the copper, iron, and ſteel work of this machine were carried away; ſo that I have been told no veſtiges of it are left. A large cheſt full of manuſcripts, the fruit of thirty years labour in the arts and ſciences, uſeful to the colony, ſhared the ſame fate.

in the Cordilleras, which extend along the South fea from Chili to Panama.

48. There are reckoned to be 30 or 35 volcanoes on the coaft of New Spain which looks towards the fouth-weft on the fide of the Pacific ocean.

49. The volcanoes fituated on lakes or deep gulphs, and which have a narrow mouth, manifeft themfelves fometimes differently. That of the lake of Nicaragua is fituated on an ifland to the eaft of the ifthmus which feparates that lake from the South fea, or to the north-weft of the other parts of the continent.

50. Volcanoes are more violent after long and heavy rains, than after and during the time of drought.

51. When rain falls in an equal quantity on all the parts of a mountain, that which falls on the fummit muft naturally run down and penetrate very little into the earth; while the fides receive not only that of the heavens, but that alfo which comes from the fummit.

52. The fides of a mountain become plane in proportion as they become lower, and to their diftance from the fummit.

53. The loweft parts of the fides of a mountain muft neceffarily imbibe more water than the higher parts. The quantity imbibed muft be greater as the inclination of the fides decreafes in approaching the bafe.

54. In the interior part of fteep mountains there muft be places to which the water cannot penetrate. Thefe places, in all probability, are thofe which approach neareft to the vertical line, proceeding from the fummit and terminating perpendicularly towards the middle of the bafe.

55. If the filtration of rain water through the earth on the fides of mountains ftops up the pores, and if the fubterranean vapours do not find room to efcape through thefe fides, they muft proceed towards thofe parts which approach the vertical line drawn from the fummit, which is the place where they can find a paffage by foaking through the pores of the dry earth which muft be there found.

56. If gas and fubterranean fires are thrown towards the axis of the mountain by the intermiffion of waters which ftop up their other paffages, the elafticity of thefe fluids accumulated at the centre muft neceffarily increafe. It appears that, from this obfervation, we may deduce the reafon why volcanoes often break out after long continued rains.

57. Whether it be the decompofition of water, or any other caufe, that makes the fire of volcanoes be difengaged, this fire muft neceffarily, as already faid, purfue that route where it

it finds the leaſt reſiſtance to its free paſſage. It appears, then, that this route muſt be that where the waters have not ſtopped up the pores of the earth. It is for this reaſon, probably, that volcanoes manifeſt themſelves at the ſummits of mountains.

58. The crevices of volcanic mountains incline oftener from their centre towards the weſt or the ſouth than towards the other ſides. It is always towards thoſe parts that volcanoes throw out their lava. The reaſon of this ſeems to be explained by the 31ſt, 32d, 33d, 34th, 35th, and 36th obſervations, and in particular by the 37th and 38th.

59. The moſt violent volcanoes almoſt always manifeſt themſelves in the higheſt mountains.

60. It would appear that the ice on the ſummits of theſe mountains ought to ſecure the earth found there from being penetrated by the ſnow water which the temperature of the air may ſometimes diſſolve; and, as this temperature acts only on the ſurface of the ſnow, the ice below ought to throw off that which is melted towards the ſides where the declivity is, and by theſe means prevent the water from penetrating the earth of the ſummit to moiſten that at the centre of the mountain.

61. Volcanic mountains which are not very high, or which are not ſituated agreeably to the general poſition to which ſubterranean fires ſeem to proceed, have in general only weak volcanoes, which often in the end become extinct.

62. Many of the extinguiſhed craters have become lakes, or reſervoirs of rain water, at the ſummit of mountains; ſprings commonly manifeſt themſelves around their ſides, and ſerve to fertilize the lands at their bottom.

63. Hiſtory makes mention of many lakes which have been formed by mountains being ſwallowed up: all countries ſubject to earthquakes have a great many of them.

64. The earth has been ſeen to ſwell up ſo as almoſt to form a hemiſpherical mountain, and to burſt ſuddenly, with a horrid exploſion, and leave nothing but a lake in the place.

65. Many iſlands have emerged from the ſea: ſome of theſe have exiſted for a long time, others have gradually diſappeared; and ſome, in conſequence of a continual undulation, occaſioned by ſubterranean fire, have diſappeared after being ſlowly conſumed.

It is here proper to obſerve, that all theſe events take place in continents or iſlands the poſitions of which are analogous to thoſe mentioned in the 1ſt, 3d, and 9th obſervations.

Though this memoir is only a very ſhort extract from a manuſcript loſt during the troubles of the Cape, I think it my my duty to publiſh the moſt ſtriking parts of that work which my memory has enabled me to preſerve. It is poſſible that among theſe obſervations there are ſome particulars which require to be rectified: but I ſhall leave to thoſe who may be deſirous of employing their time on ſimilar reſearches to examine them with attention, and to increaſe the number ſo as to form a complete work founded upon facts.

LVIII. *Obſervations on an Optical Phænomenon produced by Moon-light in the Vapours of the Atmoſphere. By Profeſſor* WREDE*.

ABOUT fifteen minutes paſt ten at night on the 14th of December 1800, I obſerved above the horizon of Joachimsthal, in the Ukermark, a very ſingular kind of reflection of the rays of the moon's light, and ſuch as I had never before obſerved. The temperature of the air, in conſequence of a thaw, was extremely moderate, and towards the ſouth and weſt the ſky was perfectly ſerene, ſo that ſtars from the firſt to the third magnitude, notwithſtanding the great ſplendour of the moon's light, could be clearly diſtinguiſhed on the blue ground of the atmoſphere. Towards the eaſt and north, however, it had a milk white colour, and there appeared near the horizon in the north-eaſt quarter a few ſtreaked clouds, which the common people generally conſider as an indication of wind. But theſe as well as the other cloudy parts were ſo tranſparent that ſeveral of the circumpolar ſtars, ſuch as the Great and Leſs Bear, Cepheus, and Caſſiopeia, could be obſerved. The lower parts of the atmoſphere, as well as thoſe ſtrata in which the above clouds floated, appeared on the firſt view to be entirely at reſt. The moon ſeemed to be nearly in the weſt-ſouth-weſt quarter, and was ſo bright that the ſpots of her illuminated ſide could be eaſily diſtinguiſhed. In regard to her aſtronomical poſition ſhe was near the ſtar π in the eaſtern band of the Fiſh, a little to the weſt of Mars, and much further from Jupiter. In this poſition ſhe was ſurrounded by two very eccentric rings, the ſmaller of which, being concentric, had the form of an ellipſe; but the larger was circular, as repreſented in the annexed engraving, (Plate VIII.)—The principal axis of the ellipſis lay ſouth and north, and the conjugate axis eaſt and weſt. It was interſected in the former direction by the eccentric circle,

* From *Der Geſellſchaft Naturforſchender Freunde zu Berlin Neue Schriften*, vol. iii. 1801.

which

which at the ſame time paſſed apparently through the middle of the moon, and its centre lay to the eaſtward in the zenith of the obſerver near Capella in Auriga. The extent of this large circular ring, the apparent thickneſs of which was ſcarcely equal to the half of the moon's diameter, covered about the following places of the heavens: The band of the Fiſh, the head of Andromeda, a part of Frederick's Glory and the Dragon; the feet of the Leſſer Bear, Alioth in the tail of the Great Bear, the Small Lion, and the neck of the Large Lion, ſo that Regulus ſtood about two or three of the moon's diameters without the circle. It then proceeded between the Crab and the Water Serpent, above the Unicorn, under Orion's belt, above the Brandenburgh-Sceptre, Eridanus, George's Harp, and the Whale's Back, to the band of the Fiſh. The ellipſe extended, according to its greater diameter, from the ſtar Θ to the northern arm of Andromeda, to the ſtar τ in the Whale, and then ran eaſt above the tail of Aries, ſo that Mars was included by it near δ of that conſtellation: on the weſt ſide it extended nearly to the equinoctial colure; on the ſouthern and northern edges the ellipſis appeared to be double, and ſometimes triple. There were ſeen here pale ſhining rings in pairs, one pair of which had a greater tranſverſe axis, and the other a ſmaller, than the principal ellipſis. The latter was the caſe, in particular, towards the end of the phænomenon. It ſometimes appeared as if the two exterior rings formed a particular ellipſis, which was concentric with the principal one, but which had a ſmaller conjugate axis, and interſected the bright, luminous, elliptical ring in four places. Where the latter was touched by the circular ring there appeared two ſtriking paraſelena, the northern one of which, about eleven o'clock, exhibited a few faint priſmatic colours; but the ſouthern one did not diſperſe the light in the ſame manner, only that at its circumference it had a ſomewhat yellowiſh white appearance. The points where the falſe ellipſis interſected the large circular ring, did not exhibit the ſmalleſt appearance of priſmatic refraction. The proper elliptical ring from the moon towards the north-weſt was very ill defined, and ſcarcely uninterrupted; and this was the caſe alſo with the large ring towards the neighbourhood of the Unicorn, where there were fewer vapours, and where the ſky towards the horizon was perfectly ſerene. In other reſpects, both rings appeared very diſtinct, and of a white colour; but this character was loſt by the large one within the ellipſis, and the nearer it approached the moon, which overpowered and obſcured it by her ſplendour. At this place, alſo, its thickneſs appeared to be as great as in the neighbourhood

hood of the Unicorn: in other places it was quite uniform. The elliptical ring towards the eaſt exhibited a very white and thicker part, which became deranged, and no longer coincided with the ellipſe the nearer the phænomenon approached to its end. This happened a few minutes paſt eleven o'clock, after both the rings had loſt their colour, and when the moon began to be ſurrounded by a yellow nimbus. At twenty minutes paſt eleven both the circle and ellipſe had diſappeared; but the nimbus around the moon had ſo increaſed, that, according to the eye, it was equal to ſix diameters of the moon, or about three degrees. The vapours in the atmoſphere were at this time no longer diſtributed in an uniform manner, but thrown together in maſſes of different denſities, which formed regular clouds, and in which ſome motion was obſerved. Neither ſnow nor rain, however, followed, but the weather as well as the temperature of the atmoſphere continued the ſame. About eleven o'clock the following evening a little cold rain fell; but the drops were exceedingly fine, and it did not long continue.

Having finiſhed the deſcription of this phænomenon, it might naturally be expected that I ſhould give ſome explanation of the cauſes which produced it; but I have not ſufficient confidence in my own abilities to undertake ſo difficult a taſk. As far as can be deduced from the principles of optics and the doctrine of light, it may be aſcribed to ſome inſulated clouds of different thickneſs and denſity, or to ſeveral ſtrata of clouds lying near and above each other. The phænomenon of the paraſelena, where both rings apparently interſected each other, gives us reaſon to conjecture that they actually cut each other; and that therefore we may admit as the cauſe the firſt caſe, that is to ſay, one ſingle ſtratum of clouds of unequal thickneſs and denſity. It is not improbable that, in regard to the elliptical ring, the caſe was the ſame as with the lenticular elevations in common window glaſs, which diſtort objects, extend them in length, and give to round things an elliptical form. But, in my opinion, it is more difficult to explain why the larger ring was perfectly circular, and appeared ſo eccentric; why the image of the moon lay at the end of the radius in the periphery. It is very probable, as has been already remarked by various obſervers, that during this phænomenon there were in the thick vapours of the atmoſphere ſeveral hollow cones or funnels which, bordering on the baſis of the image, formed ſeveral rings; for the latter occaſioned, in the places of interſection, an appearance of the priſmatic colours. Should this conjecture be agreeable to truth, the higher geometry, and par-

 ticularly

ticularly the doctrine of conic sections, might be employed for explaining similar phænomena. But respecting this point time must determine.

But that the above observed phænomenon may in some measure be useful to meteorologists, I shall here add a few remarks. If we estimate the transverse diameter of the ellipse at 60 , and the diameter of the circular ring at more than 20°, it must be admitted that the whole phænomenon was at no great height in the atmosphere, else it must have comprehended fewer of the constellations: the vapours in which it appeared floated therefore in a very low situation, and near the observer's horizon. It thence follows, that this phænomenon could be visible to no other person, unless he had the same stratum of clouds over him, and beheld the moon through it; and it is probable that the phænomenon might have remained unnoticed had the observer been a mile towards one side from the position in which he then stood. We may admit, not without reason, that all such circles with which the sun and moon are sometimes surrounded are not at a very considerable distance from the surface of the earth, and therefore are not visible in a greater extent than 40 or 50 miles. If the highest clouds over the sea can be seen only at the distance of about 20 geographical miles above the horizon, how much more must a phænomenon which takes place in far lower districts of the atmosphere escape the notice of different observers whose positions are at a great distance from each other, and particularly when its visibility depends on the angle under which an object must be viewed to observe that peculiar image which is produced! We never see the rainbow in a cloud, or rather in the falling drops of rain, when the sun is either too high or too low. In the high Alps of Swisserland it sometimes happens, that instead of a bow there is seen a complete circle of the prismatic colours in the drops precipitated from the atmosphere. In the same manner, the visibility of various other phænomena in the atmosphere depends on the point of sight which is favourable for the observation. We therefore ought not to assert that such or such phænomena seldom occur; we ought rather to say that they have been seen by individual observers, and in particular geographical points of sight.

I shall embrace this opportunity of making some observations on an assertion which appeared in one of the public journals of last year. Speaking of a rainbow produced by moonlight, which was observed on the 5th of September 1800 from the observatory of Göttingen, the account concluded with

with afferting that this very rare phænomenon had been obferved fince the period of Ariftotle only eleven times, and in Germany only once, by Weidler, in 1719. I can, however, produce two inftances of a lunar rainbow feen in Germany. In the year 1799 or 1798 I obferved in Pomerania, at a village in the neighbourhood of Wollin, a rainbow of this kind, even though the moon was not full. If my memory does not fail me, it was then a mild harveft evening, and at moft only eight o'clock. The fky was pretty clear, and the moon was about 30 or 40 degrees above the horizon towards the eaft. Towards the weft I obferved a cloud at about the fame height, and in this was feen the rainbow, the colours of which were exceedingly lively. Its ends only were pale, and very faint. It was larger than a quadrant, but did not form a complete femi-circle. The livelinefs of the colours did not long continue after it was firft obferved; for a weft wind drove the cloud before it, and a fmall fhower fell. In the month of October 1798, about eleven at night, I faw a fecond lunar rainbow, during a fhower, in a field of Joachimfthal. The moon was in the eaftern quarter, and at a confiderable elevation. The extent of this rainbow was about 180 degrees, and it ftood with both its extremities on the ground. Its colours, however, were exceedingly faint; but the refraction was fo apparent, that a fecond pale rainbow was obferved near to it. I have mentioned thefe two obfervations to fhow that it is improper to fay that thefe phænomena feldom occur; we ought only to fay that they have been feldom delineated and made known. If the friends of natural philofophy could make obfervations in every place where fuch phænomena occur, or were thofe feen by accidental obfervers, who often, through indifference, confider fuch things as hardly worth notice, communicated to them, there is no doubt that my affertion would be confirmed.

LIX. *Note upon a peculiar Vegetable Principle contained in Coffee.* By RICHARD CHENEVIX, *Efq. F. R. S. M. R. I. A.**

IN a veffel calculated to confine the vapour of water, I heated a confiderable portion of that liquid upon about a pound of raw coffee imported directly from Martinico, and of the qualities of which I was well affured. I then filtered the liquor, and reduced it nearly to drynefs, in a glafs eva-

* Communicated by the Author.

porating

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

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

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

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

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

Nitric acid produced a ſimilar effect.

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

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

With muriatic, phoſphoric, and the vegetable acids, there was no change, even of colour.

With muriate of gold, of platina, of copper, there was not any change, but what would naturally reſult from a mixture of the colours of both liquors.

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

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

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

A ſolution of animal gelatine did not give any precipitate with this vegetable principle. The effect of tannin upon gelatine is well known.

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

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

I have not had an opportunity to look for this new principle in other vegetables.

LX. *Bio-*

LX. *Biographical Account of* JOSEPH DE BEAUCHAMP, *the Aſtronomer. By* DE LALANDE.

JOSEPH DE BEAUCHAMP was born at Vezoul on the 29th of June 1752. In 1767 he entered into the order of the Bernardins, of which his uncle Mirandeau had a regular abbey. The latter having been appointed biſhop of Babylon in 1774, deſtined his nephew to participate with him in his functions, and ſent for him to Paris that he might fit himſelf for that purpoſe by ſtudying the Arabic, theology and medals, of which the biſhop was exceedingly fond. Young Beauchamp, who had a decided taſte for the ſciences, attended the College de France, and made a rapid progreſs. I ſeconded his turn for aſtronomy by ſhowing him how uſeful he could be to us in Aſia. In 1780 he made it his chief ſtudy, and was of great ſervice to us. Soon after he ſet out for the Eaſt, and on the 15th of September 1781 arrived at Aleppo with his uncle, who was not able to continue his journey. Young Beauchamp, therefore, proceeded alone to Bagdad to diſcharge the epiſcopal functions.

In 1781 I obtained from the miniſter of the marine inſtruments which I ſent to him, and which he employed with great advantage. An account of his journey from Aleppo to Bagdad was publiſhed in the *Journal des Savans* for 1784: his obſervations made at Bagdad, and ſome notices reſpecting the Turks and the Arabs, may be found in the ſame work *. In the month of January 1784 he ſet out for Baſſora: a chart of the courſe of the Tigris and the Euphrates from Diarbeker to the Perſian gulph, that is to ſay, for an extent of 300 leagues, which he conſtructed on that occaſion, is in my poſſeſſion; and I publiſhed a ſhort account of this journey in the *Journal des Savans* for 1785 †. The different volumes of this journal, as well as the Memoirs of the Academy, contain a great many obſervations made by Beauchamp; ſuch as the paſſage of Mercury over the ſun's diſk, May the 4th, 1786 ‡.

I have alſo a map of the country round Babylon which he conſtructed. He brought to the abbé Barthelemy drawings of monuments, inſcriptions, and medals, found at the antient Babylon, as well as Arabic manuſcripts. In his firſt journey, having embarked on board a ſmall Arabic veſſel, it was

* *Journal des Savans* 1784, p. 332 and 470.

† For the month of May, p. 246 and 285.

‡ *Journal des Savans* 1787, p. 361. For an account of the eſtabliſhment of his obſervatory ſee p. 301 and 498.

overtaken by a calm at ſea, and, being ſhort of water, he was expoſed to the ſcorching heat of the tropical ſun for forty-eight hours without a drop of water to cool his mouth; in conſequence of which he was ſeized with a violent fit of illneſs on his return to Baſſora.

In 1787 I induced him to undertake a journey to the Caſpian ſea, to ſettle the queſtion in regard to its ſituation, and to determine the longitudes in that part of Perſia, in regard to which there were from five to ſix degrees of uncertainty. The reſult of his obſervations I publiſhed in the Memoirs of the Academy for 1787. During this journey he was maltreated and plundered, and he was thrown into a fever which laſted eighteen months. He however conſtructed a chart of his route, which Baron von Zach has publiſhed. He obſerved an eclipſe of the moon at Caſbine on the 30th of June 1787: it was one of the moſt important ever obſerved. He returned from his Perſian voyage to Bagdad on the 14th of January 1787 *.

Obſervations of Mercury, which are ſo rare in France, were among thoſe things which I had particularly recommended to him; and he did more in this reſpect than all the European aſtronomers together, and than was ever done ſince the origin of aſtronomy: he ſaw Mercury nearer the ſun than was ever before obſerved. I publiſhed ſeveral of his obſervations in the Memoirs of the Academy; and to him I was indebted for the principal aſſiſtance I received in conſtructing the tables which I publiſhed of that planet. He obſerved alſo ſome ſtars which could not be ſeen at Paris; and his manuſcripts which he left to me will furniſh a ſupplement to the immenſe collection of ſtars I have publiſhed in the *Hiſtoire Celeſte.*

Baron von Zach, in his excellent journal, has made frequent uſe of Beauchamp's obſervations; and he cauſed to be engraved for it his map of Perſia. Beauchamp had undertaken a general review of the heavens to rectify the poſitions of the ſtars, and he had aſcertained that of ſeveral thouſands, when he learned that the department of the marine had withdrawn the annual gratification of 2000 livres which he enjoyed, and without which he could not ſubſiſt at Bagdad. He therefore left Bagdad on the 1ſt of December 1789, and arrived at Paris on the 3d of September 1790.

In 1791, and the following years, I did not ceaſe to ſolicit the miniſters of the National Aſſembly that he might be ſent

* An account of this journey to Perſia may be found in the *Journal des Sçavans* for January 1790, p. 726. For his notes on the Babylonian antiquities ſee the Journal for 1790, p. 796.

5 back

back to Bagdad to resume his observations. In the month of February 1795 I obtained from the Convention, through the means of Gregoire, an indemnification for him; and on that occasion Beauchamp said, "I see that you draw upon me a bill of exchange which I can pay only in Turkey. I shall therefore set out; but if any misfortune befalls me you must remember my devotion to you and to astronomy." He indeed quitted, with some degree of regret, a country and family which he loved, and by which he was beloved. But it was not till the 3d of March 1795 that I obtained, by the aid of C. Volney, his nomination to the consulship of Mascate, in Arabia. He came to Paris on the 27th of March, but was obliged to proceed to Italy to procure a ship bound to Constantinople: he encountered various impediments, and in the month of April 1796 was still in Italy. He at length set out, went to make some observations in the Archipelago, and did not arrive at Constantinople till the 22d of November 1796. Towards the end of May 1797 he undertook a journey from Constantinople to Trebisonde, which was of great importance to geography, as he rectified the charts of the Black sea, which were exceedingly erroneous. On the 9th of December the same year he returned from the Black sea, and made preparations for proceeding to Mascate; but the war rendered this voyage dangerous and useless.

In the month of March 1798 he was sent to Egypt till he could find an opportunity of proceeding to Mascate, and, towards the conclusion of that year, offered to Bonaparte to go as a negotiator for peace to Constantinople; but he was arrested as well as all the other French, and remained three years in confinement.

Being at length set at liberty, in consequence of the peace, he quitted Constantinople on the 23d of September 1801, though still indisposed, and had scarcely arrived at Nice when he expired on the 19th of November. A few days before, he had been appointed by the first consul commissary-general at Lisbon; and he lived long enough to learn that this distinguished mark of favour had been conferred upon him.

Few men have employed the short course of human life to so much purpose. Beauchamp possessed knowledge and merit of various kinds: the duties of religion were not neglected by this philosopher, and the congregation of the Propaganda at Rome testified its satisfaction with the zeal he had displayed in his apostolic functions. I requested his uncle to endeavour to procure for him the reversion of his bishopric, which he promised to do; but he died in 1798 in the hospital of incurables, after having interfered, to no purpose, in the episcopal revolution.

Never was astronomer exposed to so many hardships in making observations. By the excessive heat of Bagdad he was reduced sometimes to a state of the greatest debility, and his laborious and dangerous journeys always brought on severe illness. He died a victim to his zeal; and it increases our regret, that the number of the martyrs to astronomy is already so great. [The annexed engraving represents Beauchamp in his Arabian dress.]

LXI. *Notices respecting New Books.*

Ausführliche Geschichte der Theoretisch Praktischen Uhrmacherkunst, &c. A History of Clock- and Watch-making, both Theoretical and Practical, since the earliest Method of dividing the Day to the End of the 18th Century. By I. H. Moriz Popee, 1801. 8vo. 564 pages.

[Concluded from p. 276.]

VIII. *Further progress of the theoretico-practical part of clock- and watch-making till the end of the 18th century.*—In order to render the oscillations of the large clocks isochronous, Huyghens invented the cycloidal plate; and De la Hire assures us, that this cycloidal pendulum clock, though often compared with the motion of the fixed stars, did not differ in eight days but a few seconds from the mean motion of the sun. But Huyghens himself soon found that the plate could not be made in a cycloidal form, and that the silken threads by which the pendulum was suspended did not answer the purpose, but that a pliable spring could be used in their stead. He then invented, for the purpose of more perfect regulation, that singular pendulum called *pirouette*, which has a cruciform motion. This, however, was also rejected. About the end of the 17th century, Derham and Hook conceived the idea of making the pendulum swing in small arches, and applying heavy lenticular weights. This method was adopted by Le Bon and De Rivaz at Paris, and by Clement in London, who also invented the so called English pallet. The author here gives an account of the discovery, that the pendulum does not every where vibrate with the same velocity, its vibrations being quicker near the pole and slower towards the equator; on which account, clocks that go well in one place alter their rate of going when brought near to the pole

pole or the equator. He also mentions the disadvantages attending the stopping of the balance-wheel in watches; and relates what Sully, Hook, Hautefeuille, and Du Tertre, individually did to obviate this stopping; and also the experiments of Le Roy, Tompion, Flameville, and Facio, in this respect. Berthoud taught the method of cutting the teeth in the balance-wheel mathematically. John Prier of London, and Samuel Anguilins, a Swede, made researches also on the same subject. The author mentions also Schott's proposal in regard to moderating pendulum clocks, as also those of Du Tertre and Le Roy. The recoil scapement, invented by Clement in 1680, was changed by Graham for a dead scapement, according to which the scapement-wheel at each fall of the pallet remains at rest. Graham also made the scapement in watches to rest by means of the cylinder and pallet-wheel; by which the friction was lessened, and the machinery rendered fitter for greater and easier vibrations. This scapement, however, requires oil, which is often hurtful. In order to lessen friction still more with the dead scapement, Berthoud improved an invention of Mudge, which consists in this, that the balance-wheel is not checked by the balance itself, but by a particular catch which the balance lets loose, by which means the balance continues its oscillations while the wheel is retained by the catch. The balance makes two vibrations, one forwards and another backwards, so that the tooth brought to rest is set free at the second vibration. This kind of scapement, which is a striking proof of human ingenuity, is called the *free*. The author here describes, in an interesting manner, the different kinds of free scapements invented by Magellans, Platier, Rendal, Robins, Grant, and Breguet. From the end of the 17th century, celebrated mathematicians employed themselves in endeavouring to discover a general theory for all the parts of watches, of which the author gives a particular account. Another difficulty to be overcome in regard to clocks and watches, was the influence which heat and cold has upon the expansion and contraction of metals, and which alters the motion of the whole. Graham is considered as the first who made experiments with a view of freeing the pendulum from such changes. He first conceived the idea of making pendulums of ebony, fir-wood, walnut-tree, &c.; but found that the moisture of the air produced other inconveniences. Experiments of the same kind were afterwards made by Magellan, Fontana, Ludlam, Schröter, Crostwaite, and Köhler. Graham next constructed a pendulum of an iron tube, which was filled to a certain height with quicksilver. Troughton, instead

instead of an iron tube filled with quickfilver, employed one of glafs, having at the end a bulb like the tube of a thermometer. But ftill happier was Graham's idea, to conftruct a pendulum of feveral rods of different metals, fo combined, that the expanfion of the one fhould be fully compenfated by the contraction of the other; and this gave rife to the gridiron pendulum. Before Graham, however, Short, Caffini, and Ellicot had entertained a fimilar idea of a compound pendulum; and the plan of a gridiron pendulum was firft carried into execution by Harrifon in the year 1726. The author here gives a defcription of the pendulums propofed by Graham and Harrifon; and mentions the experiments of Berthoud, Grenier, and Syffert, for improving them. He fpeaks alfo of Ellicot's lever pendulum, Grenier's lever pendulum, and the pendulum with the fmall gridiron; as alfo Rivaz's tubular pendulum; and the fimpleft compenfation pendulum of all, invented by Faggot, a Swede, about the year 1740, and which Schmidt the watchmaker, of Stettin, has lately employed with much ingenuity. The higheft degree of perfection, however, to which watch-making has attained, is in the conftruction of the nautical time-keepers, for the invention of which large premiums were offered in England, France, Holland, and Spain. Huyghens and Sulley made attempts for this purpofe, but they were not attended with fuccefs; and the ingenious propofals of Leibnitz were not found fufficient. Harrifon's firft time-keeper, which he prefented to the Royal Society in 1736, was regulated by balancing rods placed crofswife over each other, with circular fprings at the ends, which refted againft two plates, which by the dilatation of the circular fprings, in confequence of heat, feparated, and on their contraction by cold approached each other. The friction alfo was leffened, and the time-keeper was fufpended like the mariner's compafs. In a voyage of twelve weeks, the error in going amounted only to 36 feconds. A fecond one, conftructed in 1749, which was fmaller and more convenient, furpaffed the firft. To a third, conftructed in 1753, he applied a balance with a fpiral fpring, and a compenfation rod of brafs and fteel. In 1761 he conftructed a fourth, which, on a voyage of experiment that lafted 81 days, erred only 1 minute $54\frac{1}{2}$ feconds. A fifth, finifhed in 1764, erred only 54 feconds in fix weeks. Dr. Mafkelyne, however, to whom it was afterwards referred for trial, did not give fo favourable a report of it. Berthoud and Le Roy made attempts alfo to conftruct time-keepers; but the firft were not fuccefsful. In the year 1741 Le Roy's time-keeper, which in fix weeks erred only half a degree, was preferred to that of Berthoud, which in the fame

fame time erred 34 minutes 36 feconds. The author gives an account alfo of Rivaz's experiments. The time-keepers of Arnold, Rendal, and Mudge, were found to be exceedingly correct. Mudge conftructed only three; the firft of which was tried, in the year 1774, by Dr. Mafkelyne, Hornfby, Count Bruhl, Von Zach, and Campbel. The other two were tried by Dr. Mafkelyne in 1777. One of them, in the courfe of 93 days, was found to have exceeded the mean time by only 1·8 fecond. It fhowed the longitude between London and Oxford within 1·6 fecond. The balance vibrated altogether independent of the wheel-work, and always received from the moving power a new impulfe, which at each vibration was uniform. The balance had two fpiral fprings, which produced a very uniform action. Befides this, the machine was furnifhed with a compenfation balance. In imitation of the nautical time-keepers, pocket chronometers were conftructed. Thefe ferve for determining the geographical longitude at land. In thefe the free fcapement of Mudge is employed. Emery, an artift from Neufchatel, made the firft, which, after a paffage of four weeks, gave the longitude of St. John's, Newfoundland, correct within fix feconds. Another, conftructed by Mudge, in a voyage of 14 weeks, erred only 17 feconds. Mudge never made but two pocket chronometers. Arnold made above 900, of various conftructions. His chronometers of the beft kind, with gold cafes, coft 120 guineas, with filver cafes, 100.

In the year 1500 the art of clock- and watch-making in Germany became a particular branch of bufinefs. The author here gives an hiftorical fketch of the origin and progrefs of watch-making in Swifferland from the year 1679 to the prefent time, particularly in Neufchatel and Valengin, which will be read with great intereft. In both thefe diftricts there are at prefent 3634 clock- and watch-makers. One Abraham Robert invented there an inftrument which ferves for adjufting the teeth of the wheels and pinions. The fame artift conceived the idea alfo of a dead fcapement. Perrelet invented the inftrument for placing wheels ftraight; and Recorder watches, which wind themfelves up. The moft celebrated watch maker in Chaux de Fond, where there are above 400, is James Droz, who has obtained great celebrity by the conftruction of fome very curious automata.

XI. *Machines moved by clock work.*—Under this head the author defcribes, with great minutenefs, all thofe works moved by clock-work which are not employed for meafuring time. The reader will here fee, with pleafure, how much can be produced by human ingenuity; but it is not poffible to give a proper idea of it in an extract. We shall only observe that the author here gives an account of planetariums; the most remarkable town clocks by Hahn, Mollinger, Dienel, and the works of Jacob, the celebrated Huttig, a weaver of Bunzlau, and of Droz; also the automata of Vaucanson, &c. together with waywisers, pedometers, and other objects of the like kind.

X. *Bibliography of clock- and watch-making.*—The author under this head mentions not merely the titles of books, but gives an extract of their contents with critical remarks.

LXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

THE following letter from Baron von Zach to the president Sir Joseph Banks, contains some interesting information respecting the planet Ceres Ferdinandea.

DEAR SIR, Seeberg Observatory near Gotha, Feb. 20, 1802.

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

1802.	Mean Time in Seeberg.	Ap. Right Ascen. observed.	App. Dec. observed.
Feb. 3	15h 40′ 35″ S.	188° 42′ 13·05″	12° 40′ 5″ N.
4	15 36 41·4	188 42 36·30	
5	15 32 45·1	188 42 30·15	12 50 25
9	15 16 43·7	88 38 3·90	13 14 18
19	14 34 46·7	187 58 27·90	14 20 3

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

Epoch for the beginning of the year to the meridian of Seeberg - - -	77°	27	36·5″
Aphelion } both sidereal -	325	57	15·0
Node } -	80	58	40·0
Greatest equation of the centre -	9	20	8·0
Inclination of the orbit - -	10	37	56·6

Logarithm of half axis major 0·4424742

Eccentricity of the orbit - 0·c814064

Mean diurnal heliocentric and tropical motion 769·7924

With these elements of the orbit, all the observations made by by Mr. Piazzi in Palermo, from January 1, till February 11, 1801, agree perfectly well, and within a few feconds; and my obfervations are reprefented by them thus:

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

As thefe elements agree hitherto fo well with the heavens, the following ephemeris, calculated upon them for the next month, will probably do the fame; fo I annex it here to point out to the Englifh obfervers the place where they have to look for the Ceres.

Pofition of the Ceres for Midnight Mean Time in Seeberg Obfervatory.

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

This planet will come in oppofition to the fun, March 17, in the afternoon. At the fame time this heavenly body will be be in its greateſt proximity to the earth = 1,6025, and therefore the moſt favourable time to look for its ſatellites, if there are any, to meaſure its diameter, and to examine its nebuloſity. About this time the planet will alſo be in its greateſt geocentrical latitude = 17° 9′, and a little later ſhe will have her greateſt retrograde motion, about 13′ in right aſcenſion per day. The north declination will increaſe till the beginning of April, and about the ninth of the ſame month the motion in declination will commence to the ſouth. It appeared to me that the Ceres has ſome change of light. I imputed it at firſt to our hazy atmoſphere this winter; but Mr. Schröeter of Lilienthal, and Mr. Olbers of Bremen, ſent me word that they have obſerved the ſame, and they believe that it is the planet which is ſubject to ſuch changes of light. Mr. Herſchel will tell us beſt whether it is ſo. I have ſome hopes to find the planet in antient catalogues of ſtars. Mr. Melſier was very near it in the year 1779. The famous comet of that year ran juſt over the northern wing of Virgo, as now, and the new planet was not very far diſtant. If the comet had attained two months ſooner the completion of Virgo, Mr. Melſier muſt infallibly have obſerved the Ceres then, becauſe he determined all the little ſtars in the vicinity of the comet; the planet would have been in the way of the comet, and ſo, of courſe, he would have catched the little planet in 1799.

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

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

On the 29th of April and the 6th of May, the reading of Count Bournon's paper on corundum was continued, and on the 13th of May was concluded. It furniſhes a complete and ſcientific mineralogical deſcription of that ſubſtance.

On the 6th and 13th of May there were read two papers communicated by Dr. Herſchel, giving an account of his obſervations during the late fine weather upon the two new celeſtial bodies diſcovered by M. Piazzi of Palermo, and Dr. Olbers of Bremen, and by them reſpectively called Ceres Ferdinandea, and Pallas. In our Magazine for March laſt, ſome account was given of a former paper by Dr. Herſchel, wherein he formed an eſtimate of the magnitude of Ceres by comparing

paring its apparent difc with that of the Georgian planet. It was then thought very extraordinary that the calculations, founded upon fuch a comparifon, and upon the moft probable diftance of Ceres from the fun, made the real diameter of that newly-difcovered celeftial body certainly lefs than 5-8ths of the diameter of our moon. By Dr. Herfchel's fubfequent obfervations, however, detailed in the firft of the two papers above mentioned, and which were made in much more favourable weather, the diameter of Ceres was found much fmaller than what he inferred it to be from his former obfervations of the 11th of February. His later obfervations confifted chiefly in his taking repeated meafures by means of his lamp-micrometer; an apparatus long ago contrived by Dr. Herfchel, and publifhed in the Philofophical Tranfactions, for eftimating the extent of angles much fmaller than a fecond of a degree. By fimilar obfervations he found the apparent diameter of Pallas to be lefs than that of Ceres; and from thofe apparent diameters, and the beft data for the diftances from the fun, Dr. Herfchel has computed their real diameters to be about 160 miles for Ceres, and about 80 for Pallas; from which it appears, that out of the quantity of matter which the planet Mercury contains 73,839 fuch bodies as Pallas could be formed. The exiftence of fuch relatively minute bodies in the heavens belonging to the folar fyftem, and moving according to the law of gravitation, is a circumftance which appears to us extremely interefting; the knowledge of which muft ftill further diftinguifh the prefent æra of aftronomy, which has already been rendered fo illuftrious by Dr. Herfchel's own labours in the field of difcovery.

In his paper of the 6th of May he gives an account of feveral obfervations of a fmall coma or hazinefs which furrounded both Ceres and Pallas, the appearance and extent of which feemed to vary according to the ftate of the air. An account is alfo given of the method he followed to fatisfy himfelf that thefe two ftars have no fatellites belonging to them.

In the fame paper Dr. Herfchel points out the advantage of diftinguifhing fuch new celeftial bodies from the planets and comets by fome appropriate name which will admit of a full and precife definition. By reafon of their differing fo much from planets and comets as fcarcely to be perceivable from minute fixed ftars, even by very good telefcopes, Doctor Herfchel has adopted the term *afteroids* to denote them; but referving the liberty of fubftituting afterwards any other appellation which he may think more appofite, and to which the precife and copious definition laid down in his paper may equally apply. He next remarks, that from the nature of

afteroids

afteroids the difcovery of fuch bodies in the heavens neceffarily requires a particular method of obferving, which hitherto aftronomers have but feldom purfued. On account of their minutenefs, they fo far lie beyond the power of good telefcopes to diftinguifh them, that Dr. Herfchel in his five different reviews of the zodiac detected none of thofe concealed objects; whereas, had they lefs refembled very fmall telefcopic ftars, he muft have infallibly detected them. It is therefore, he fays, only by a diligent and accurate attention to minute ftars, in the view of difcovering amongft them *fuch as may be in motion*, that afteroids can fuccefsfully be diftinguifhed from the multitudinous collection of telefcopical ftars which fo much abound in the heavens. A fearch of this kind, from its nature, cannot be carried on except in obfervatories furnifhed with fixed inftruments, which, it is well known, make no part of Dr. Herfchel's apparatus. In the conclufion of his paper, honourable notice is taken of the affociation of twenty-four aftronomers in Germany who have portioned out the zodiac into as many parts, in order more effectually to explore it. As this new method of fearching, in the hands of Piazzi and Olbers, fo quickly produced important difcoveries, Doctor Herfchel thinks it highly probable that more celeftial bodies of the clafs of afteroids remain concealed, which may fooner or later be found out in confequence of their obferved motions.

On the 20th, the reading of a paper, by Richard Chenevix, Efq. on the chemical analyfis of corundum, was begun. The chemical facts delivered in this paper, with Count Bournon's mineralogical account of the fame fubftance, already alluded to, will perfect our knowledge refpecting corundum.

BRITISH MINERALOGICAL SOCIETY.

Defcription of the Satin Spar, by A. Aikin.

The fatin fpar, characteriftic fpecimens of which have been prefented to the fociety's cabinet by the late Mr. Mohr, and Mr. Richard Phillips, is a mineral as yet peculiar to the neighbourhood of Alfton Moor, in Cumberland, where it lies fo near the furface as to be occafionally thrown up by the plough.

Its colour is a pure white, fometimes ftained by an ochrey yellow tint of more or lefs intenfity.

It occurs in thin ftrata from one to three or four inches thick, traverfed by thin irregular feams of pyrites parallel to the direction of the ftratum.

Its luftre is between pearly and fatiny, confiderably chatoyant.

Its

Its perpendicular fracture is ſtraight or ſlightly waved fibrous, diſcovering an obſcure fine ſtratification parallel to the direction of the whole ſtratum: hence the varying or chatoyant luſtre is obſerved only when moving it in the direction of the fibres. The croſs or parallel fracture requires a ſtronger blow to effect it than the former, and preſents a compact, broad, ſplintery ſurface.

When broken, it flies into flattened fibrous fragments.

In thin pieces it is ſemi-tranſparent.

Can juſt be ſcratched by the nail: is brittle: non-elaſtic: ſp. gr. 2·709 to 2·721. It may be poliſhed to almoſt a mirror-like ſurface, and is then exquiſitely beautiful.

Its proper place in a mineralogical arrangement ſeems to be between compact ſtalactite (*dichter kalkſinter*), and ſlaty ſpar (*ſchieferſpath*).

Analyſis of the Satin Spar, by Mr. H. Pepys junior.
Preliminary Experiments.

A piece of about 400 grains was expoſed to a red heat for two hours. It loſt near 1-4th of its weight, and was much altered in its appearance, having loſt its ſmooth feel. Upon immerſion in water, it imbibed it with a hiſſing ſound, became hot, fell to powder, and a portion of it was diſſolved. The ſolution was rendered turbid by carbonic acid. The undiſſolved portion effervеſced ſlightly with acids, and had obtained an hepatic ſmell from ſome adhering pyrites.

A portion of the crude ſpar, carefully freed from all adhering pyrites, diſſolved completely in nitric acid, with effervеſcence. This ſolution was examined with the following re-agents:

On adding ſulphuric acid to a portion of the ſolution, the two fluids became nearly ſolid, and much heat was extricated; proving the preſence of lime, ſtrontian, or barytes.

Strontian water gave a precipitate of a brown colour, which was again ſoluble in nitric acid.

Barytic water gave a precipitate of rather a darker hue than the ſtrontian: this was alſo ſoluble in the nitric acid.

Tincture of galls, pruſſiate of potaſh, and ammonia, gave ſlight traces of the preſence of iron.

A ſolution of ſulphate of ſoda gave the ſame granulated precipitate as with a ſolution of nitrate of lime.

100 grains of the picked ſatin ſpar were expoſed to the action of 200 grains of nitric acid of ſp. gr. 1·25 in a veſſel of a conical form furniſhed with a ſmall glaſs worm on the top, that, ſhould any fluid be carried up by the gaſeous product, it might be depoſited in its paſſage. This and the veſſel were

were previoufly weighed. A ftrong effervefcence enfued upon mixture: the gas produced was heavier than atmofpheric air; extinguifhed flame; produced a clouded precipitate in lime, ftrontian, and barytic water. 200 grains of the acid being found not fufficient for the folution of the fubftance, 100 grains more were carefully added: effervefcence again took place, and more of the fame gas was liberated. It was then expofed to a heat of 200 degrees Fahrenheit. Upon cooling, it had loft 46 grains.

250 grains of the moft perfect part of the fatin fpar were not foluble in 500 grains of nitric acid of the fpecific gravity 1·25: upon the addition of 250 grains more, there was obtained a folution nearly colourlefs. It was then diluted with diftilled water, and evaporated to the confiftence of a ftrong fyrup, without exhibiting any fymptoms of cryftallization, though the evaporation was fufpended, and the folution fet to cool, thrice during the procefs. The concentrated folution became flightly yellow.

100 grains in folution in nitric acid, left to fpontaneous evaporation in a warm, dry place, after ftanding two months exhibited no cryftals.

Analyfis.

A. *For the carbonic acid.*—250 grains of picked fpar were diffolved in 757 grains of nitric acid of the fpecific gravity of 1·25: the gas produced was received over mercury. It changed the colour of tincture of litmus red, and was abforbed within one-hundredth of the whole by barytic water, giving a copious precipitate, which was foluble, and effervefced upon the addition of diluted acid. 119 grains were given off by this treatment, equal to 47·6 per cent.

B. *For the calcareous earth.*—The nitric folution A was diluted with an equal quantity of diftilled water: upon the addition of diluted fulphuric acid it became nearly folid: it was then further diluted and filtered. The precipitate, dried in a heat of 212°, weighed 433 grains. It was then brought to a red heat, after which it weighed 330 grains.

C. *For the iron.*—The folution B was then treated with cauftic or pure ammonia, which gave a flight red florulent precipitate. This precipitate, being filtered and dried, weighed 3 grains, equal to about 0·3 of metallic iron.

D. The folution C was then evaporated, during which, previous to the formation of any cryftals of fulphate of ammonia, 7 grains of cryftals of felenite were depofited of a filky needle-formed appearance.

E. The felenite B and D. = 337 grains, were decompofed by by a ſolution of carbonate of potaſh. The carbonate of lime thus obtained, being ſeparated by the filter, and expoſed to ignition in a black-lead crucible in a furnace for two hours, gave 125·2 grains of lime.

Satin ſpar, 100 parts contain,

Carbonic acid, A,	47·6	Carbonate of lime -	97·680
Lime, B, D, E,	50·08		
Iron, C - - - - -			012
Loſs (probably water of cryſtallization) -			2·308
			100·000

Having repeated the experiments with a very perfect piece of the ſpar, I was not, during the whole proceſs, able to detect the ſmalleſt portion of iron. I did not, in this inſtance, decompoſe the ſelenite formed, as it rated when ignited at 49·5 per cent. of lime. The minute portion of iron detected in the firſt analyſis muſt therefore have been produced from ſome ſmall pyritic grains in the ſubſtance of the ſpar then employed. Its quantity, as may be obſerved, was only about a thouſandth of the whole.

Remarks on the Affinities of Lime, Strontian, and Barytes.

Previous to the foregoing analyſis it was neceſſary to inſtitute a few experiments on the affinities of the above-mentioned ſubſtance: as the reſults may perhaps be uſeful to others, they are here ſubjoined.

A ſolution of nitrate of lime in diſtilled water, is partially, but not immediately, precipitated by a ſolution of cauſtic ſtrontian. On joining the two, the mixture aſſumes a ſizey appearance.

A precipitation takes place alſo in a ſolution of nitrate of lime, when a ſolution of cauſtic barytes is added. The precipitate falls down much ſooner in this caſe, and in more conſiderable quantity, than when the ſolution of ſtrontian is employed.

Nitrate of barytes in ſolution is immediately decompoſed by pouring into it a ſolution of ſulphate of ſoda, which produces a copious precipitate.

Nitrate of ſtrontian in ſolution is alſo decompoſed by ſulphate of ſoda when a ſolution of the latter is poured into it, but much ſlower than the nitrated barytes.

A ſolution of nitrate of lime, upon having a ſolution of ſulphate of ſoda added to it, does not inſtantly give a precipitate, but in a very ſhort time after yields a granulated precipitate of a more conſiſtent form than any of the before mentioned.

The

The ſatin ſpar has been uſed in ornamental jewellery, ſuch as aer pendents, rings, &c.; and from its wavy and chatoyant appearance, with the brilliant poliſh of which it is ſuſceptible, ſeems well calculated to be employed as a faſhionable appendage to the fair ſex.

SOCIETY OF ARTS, MANUFACTURES, AND COMMERCE, LONDON.

This Society had its annual diſtribution of premiums and bounties for improvements in agriculture, chemiſtry, the polite arts, manufactures, &c. on the 25th of May, his Grace the Duke of Norfolk in the chair. About forty candidates were honoured with gold or ſilver medals, the ſilver pallet, or pecuniary rewards for uſeful improvements, or as badges of meritorious exertions in the different branches of the fine arts.

The meeting was numerous and ſplendid, many ladies of the firſt rank and faſhion being preſent.

Our limits do not allow us to inſert at this time a liſt of the ſucceſsful candidates: we cannot, however, cloſe the preſent article without mentioning that this ſociety, to which the nation owes ſo much for bringing forward numerous valuable diſcoveries and improvements, was the firſt to award, ſome months ago, an honorary and pecuniary recompenſe to Mr. Henry Greathead, of South Shields, for his conſtruction of a life-boat, by which the lives of many perſons ſhipwrecked have already been preſerved.

The life-boat is now coming into general uſe; and, from the exertions making by the gentlemen at Lloyd's, who have devoted 2000l., and other patriotic individuals, who are contributing their money and exertions to the eſtabliſhment of them on every part of our coaſts, we have the pleaſing proſpect of many thouſands of valuable lives being preſerved to their families and to the community by this ineſtimable contrivance. The reward of the Society to Mr. Greathead was their gold medal! and fifty guineas.

IMPERIAL ACADEMY OF THE SEARCHERS INTO NATURE.

The following prize queſtions have been propoſed for the year 1803:

I. Germany produces a conſiderable quantity of umbelliferous plants endowed with medicinal properties, which are as yet unknown, or known but imperfectly or from conjecture. Only a few of theſe have been admitted into the new diſpenſatories, and into Murray's *Apparatus Medicamentorum*. The number

number of the umbelliferous plants growing wild in Germany may be reckoned to be about 115: of these, Ehrhart, in the seventh volume of his Collections, names forty-four as officinal, which he has extracted from different dispensatories: but there are many of them the virtue and effects of which are not sufficiently or accurately determined, which were adopted in the old dispensatories, but which are no longer considered as medicinal. Murray, on this account, makes the number of those which ought to be retained twenty-eight; a number still lessened in the dispensatories of other countries, but which comprehends species which, in general, may be accounted medicinal. The academy wishes, therefore, to have determined—Whether there are any of the German umbelliferous plants not yet admitted into the *Materia Medica* which possess considerable virtue in medicine? What these virtues are? In what diseases, and in what manner, these plants, or individual parts of them, can be used? Those who employ themselves on these questions must select at least five species besides those mentioned by Murray, in order to ascertain, by chemical and medical experiments, the uses and purposes to which they can be applied. It will be agreeable to the academy, if those authors who have furnished any hints towards these researches be pointed out; but the writers must not content themselves with mere quotations.

II. The phænomena which have been observed in the effects produced by the Voltaic pile afford the most pleasing prospect of future discoveries in chemistry, physiology, and medicine, and even give reason to expect some conclusions in regard to the obscure secret of life and organization. Though we are still too far from having discovered the grounds of these phænomena to entertain a hope that such conclusions can be soon obtained—as an attempt to arrange and to reduce into a systematic form the many and various observations which have been made since the invention of the above apparatus by the industry and spirit of research, and particularly of the German chemists, might contribute to bring us nearer to the object in view, the Society proposes as the subject of a prize a treatise on this discovery, and requires:

1st, That the author will collect all the observations made before the beginning of the year 1803, and deduce from them a series of principles systematically arranged, quoting the names of the observers, and also the works which contain an account of their observations.

2d, That he will make new experiments in regard to those principles

principles not fully proved, and, if poffible, form from them fome decifive conclufion.

3d, That in eftablifhing thefe principles he will make decifive experiments in regard to the action of the Voltaic pile on unorganized bodies, on pure water, alkalies, acids, metals, &c.; then on organized bodies, and particularly on dead bodies (in promoting or preventing putrefaction); and on living, in regard to fhocks, pricking fenfation, tafte, luminous appearance, &c.

4th, That the author will attend in particular to the fuppofed identity of the galvanic power and electricity; and will mention and give an opinion on the explanations of the phænomenon offered by others, and particularly on thofe of M. Ritter. It will be agreeable alfo to the academy if the author will make experiments and obfervations of his own to confirm or refute the hypothefis of M. Ritter; and if he will employ Lavoifier's theory to explain thefe phænomena.

5th, That he will apply the principles and conclufions in the 1ft, 2d, 3d, and 4th articles, to the practice of phyfic in particular; and endeavour to determine theoretically how far, and in what manner, the ufe of the Voltaic pile in medicine may be falutary? That he will collect, arrange fyftematically, try, and give fome opinion of the obfervations already made by Grapengieffer, Hagenbach, Huber, &c. That he will make experiments of his own on the fubject, and defcribe their confequences, both negative and pofitive, with fidelity and without prejudice; and will always mention the mode of application, for example, the pofition and number of the metallic plates, and whether formed into one or more piles connected together, &c.

The papers, written in Latin, German, or French, according to the ufual regulations, muft be tranfmitted to the prefident of the academy, at Erlangen, before the 1ft of October 1803.

The prize is a gold medal of the value of 25 ducats, and will be adjudged to the fuccefsful candidate on the 5th of January 1804.

SOCIETY OF THE SCIENCES AT FLUSHING.

Some years ago, a ftone having been found at Domburg, in the ifland of Walcheren, containing an infcription in which mention is made of a certain female divinity named *Buronina*, the above Society have propofed the following queftions:

Who was the goddefs Buronina, who feems to have been worfhipped by the antient inhabitants, or by foreigners who

 refided

refided among them? Are there any traces of this deity ſtill remaining? The prize is a ſilver medal. The anſwers, written in Dutch, Latin, or French, muſt be tranſmitted, poſt paid, to A. Dryfhout, of Middleburg, before the 1ſt of January 1803.

LXIII. *Intelligence and Miſcellaneous Articles.*

ASTRONOMY.

IN our laſt we mentioned Dr. Olbers's diſcovery of a new celeſtial body, and laid before our readers its obſerved places on the 28th, 29th, and 30th of March, and on the 1ſt of April.

In the ſciences, when a way is once opened to great and important diſcoveries, it ſometimes happens that they ſoon follow each other in ſucceſſion. It is now only about twenty years ſince the celebrated Dr. Herſchel made a diſcovery of which we have no previous inſtance in hiſtory, viz. of a new primary planet. On the 1ſt of January 1801 another was diſcovered by Piazzi; and a year after, a third new one has excited the curioſity of the public. On the 28th of March an account was publiſhed in the *Reichſazeiger*, No. 98, that Dr. Olbers, of Bremen, equally celebrated as a phyſician and an aſtronomer, had been ſo fortunate as to obſerve a moving ſtar in the north wing of Virgo, perfectly ſimilar in its form and light to the Ceres Ferdinandea; ſcarcely diſtinguiſhable from a fixed ſtar of the ſeventh magnitude; without any nebuloſity, and retrograde like Ceres, but increaſing much more in its northern declination. After obſerving this ſtar for three days, and aſcertaining that it had a motion peculiar to itſelf, he ſent notice of his diſcovery to Baron von Zach, of Gotha. The latter immediately found this ſmall ſtar, obſerved it on the 4th and 5th of April at the obſervatory of Seeberg, and found Dr. Olbers's obſervations confirmed in the fulleſt manner. This ſtar, to which he was inclined to give the name of *Pallas* in order to diſtinguiſh it from *Ceres*, appeared to him to be ſomewhat leſs bright than the latter. Mr. Schröter, of Lilienthal, who, in conſequence of the information communicated to him by Dr. Olbers, obſerved this ſtar with his 13 feet teleſcope, conſiders it to be ſomewhat larger and better defined than Ceres: its diameter he eſtimates at 4' ſeconds, whereas that of Ceres is only 4. But the obſervations made by Dr. Olbers and Baron von

Zach are still too few in number to admit of any decisive opinion being formed respecting the nature of this interesting body. If it be considered as a comet, its regular motion and whole appearance oppose any idea of this kind; but there is every probability in favour of its being a planet, though its orbit must have an uncommonly great inclination to that of our earth. Dr. Olbers ventures to consider it as a new primary planet of our system, moving between Mars and Ceres in an orbit very much inclined to the sun. Its period of revolution he makes three years, its mean distance from the sun $2\frac{1}{17}$th of the distance of our earth from the sun, and the place of the ascending node $5^s\ 20°$.

Mr. Walker, lecturer on astronomy, has observed this body, and describes it as being of a pale red colour, very faint, and less brilliant than the Ceres, visible with a definable disc with a magnifying power of 100 times.

As the distance of this small body from the earth is increasing, and its light decreasing, astronomers must hasten to make good observations if they are desirous of determining its orbit in an accurate manner: it will otherwise be difficult to find it again.

The following notice from the astronomer Burckhardt contains some further useful information on this interesting discovery:

" The star discovered by Dr. Olbers on the 28th of March has so great a resemblance to a planet, that it was natural to suppose its orbit to be somewhat eccentric. I have placed it successively between the earth and Mars, between Mars and Piazzi's planet, and between the latter and Jupiter. I have employed the eccentricities of one and of two tenths (those of Mars and Mercury), but neither of these suppositions has succeeded. It is easy to make a very small eccentricity answer to the longitudes, but the latitudes do not indeed begin to approach each other but by supposing it to be very great, (four sixteenths).

" These researches seem to me to prove that we must suppose a very eccentric orbit; and I have begun to calculate a parabolic orbit as for a comet, and the elements I have found are as follows:

Inclination of the orbit - -		54°	58′	30″
Ascending node - -	5^s	26	45	34
Place of the perihelion -	3	23	52	$3\frac{1}{2}$
Perihelion distance - -		1·8432		

Time of the passage of the perihelion September 29, 1801, $16^h\ 48'$.

The motion direct.

 " These

"These elements will correspond to three observations, of the 29th March, 7th and 16th April. I have omitted nothing that could render this calculation as exact as possible; but the heliocentric arc being only four degrees, we can expect only to attain nearly to exactness. The observation, therefore, of April 9, which Mechain was so kind as to communicate to me, gives an error in longitude of $-35''$, and in latitude of $-11''$. I am, however, far from wishing to exclude elliptic orbits, and I shall continue my researches with them in proportion as a greater number of observations are obtained; but, in the mean time, I was desirous to gratify the curiosity which the possibility of another new planet seems to have excited in the public.

Floreal 1, year 10. BURCKHARDT."

METEOROLOGY.

At Derby, an uncommon phænomenon appeared in the air a few mornings ago: it consisted of four circles, and part of another, in the following order:—The smallest, which was nearest to the sun, might be fifteen degrees diameter, with luminous spots to the right and left, like what are called *parahelia*; the second encompassed the former, and was twice as large; the third the size of the second, and joined its circumference to the west; and the fourth circumscribed all the others, and was touched upon the western side by part of another of the same diameter.

VACCINE INOCULATION.

This useful practice seems to be gaining ground in Spain. Dr. Francisco Piguillem has published the following work in recommendation of it:—*La Vacuna en España, o Cartas Familiares sobre esta nueva Inoculacion, escritas á una Señora,* por el Dr. Francisco Piguillem. That is, The Vaccine in Spain, or Familiar Letters on this new Inoculation, &c. Madrid, 8vo. In Catalonia, above 7000 persons were inoculated between the month of December 1800, and September 1801. "On the highest mountains and amidst the severest cold, in the deepest valleys and amidst the greatest heat, (says the letter from which this account was taken,) the inoculation has been attended with complete success."

A letter from Portugal to Dr. Alibert at Paris, after mentioning the opposition which has hitherto been made to the vaccine inoculation in that country, says,—"Some one has found here, in a Portuguese work, that the vaccine inoculation was practised at Lisbon and in the neighbourhood so early as 1613, and was thence fpread to Gallicia; but that, for unknown reafons, it was afterwards abandoned."

GALVANISM.

A letter from Germany fays, the effect of galvanifm in curing deafnefs has been fully confirmed at Jever. We learn alfo from Caffel that M. Schaub, apothecary of that place, has cured a perfon who had been deaf for eighteen years by three weeks ufe of the Voltaic pile. Mart. Frifchefen, a Benedictine, and profeffor of philofophy at Salzburg, has performed a number of fuccefsful cures on the blind, lame, and deaf, by means of a pile confifting of 300 plates; which has encouraged feveral of the phyficians there to make experiments of the fame kind. This remedy has been employed alfo with fuccefs by Dr. Grapengieffer of Berlin, Dr. Martens of Leipfic, and others.

MINERALOGY.

About three years ago, C. Pontier met with fome fragments of chromated iron in the Lower Alps, but out of its place. Circumftances arifing from the war prevented his afcertaining its true pofition in the earth. He has at length found the natural place of this curious mineral in a quarry near Gaffin, in the road to Cavalaire. This metal is mixed with green ferpentine rock, which owes its colour, probably, to chrome. It fometimes forms maffes of five folid decimetres each.

ANTIQUITIES.

M. Baguerie, of Bourdeaux, has fent to the mufæum of that city a mummy found in one of the caverns at the bottom of the peak of Teneriffe. This mummy feems to be of a different kind from any hitherto defcribed, and to have been prepared in a different manner.

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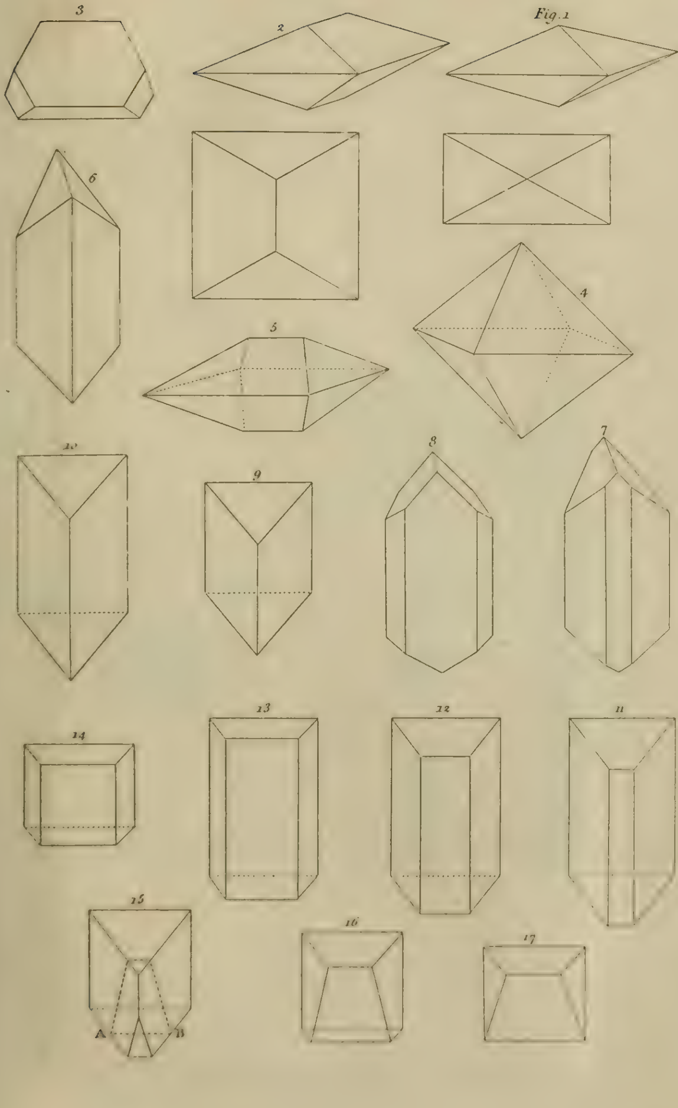
Vegetable

END OF THE TWELFTH VOLUME.

☞ The Binder is desired to place Dr. PULTENEY's Head at the Beginning of the Volume, and the other Plates at the End.

Printed by TAYLOR and WILKS, Chancery-Lane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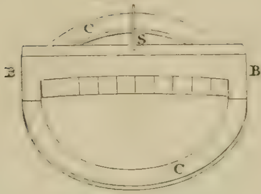

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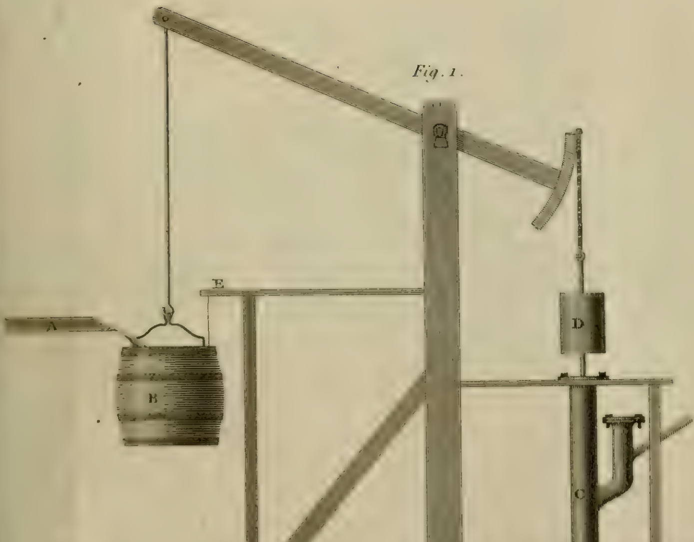

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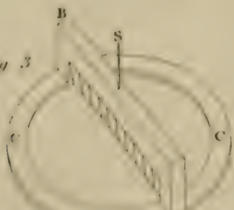
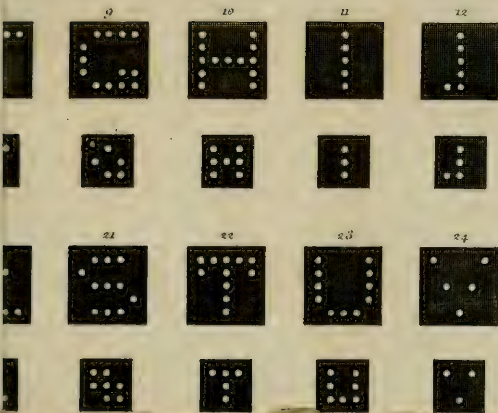


Lowry sculp.

Philo. Mag. Pl. III. Vol. XII.



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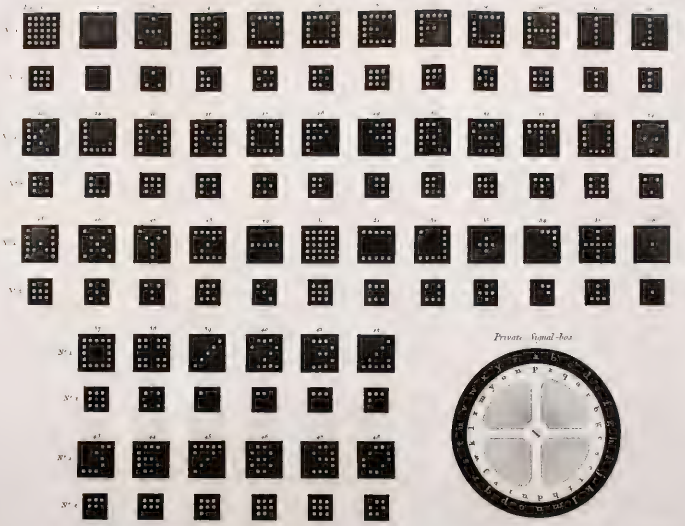

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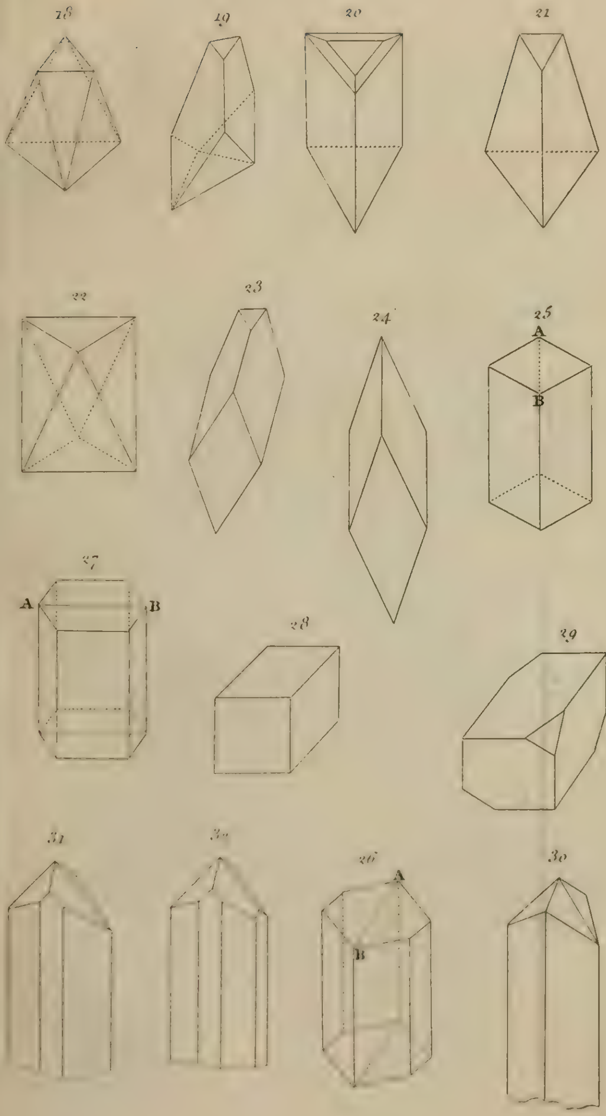

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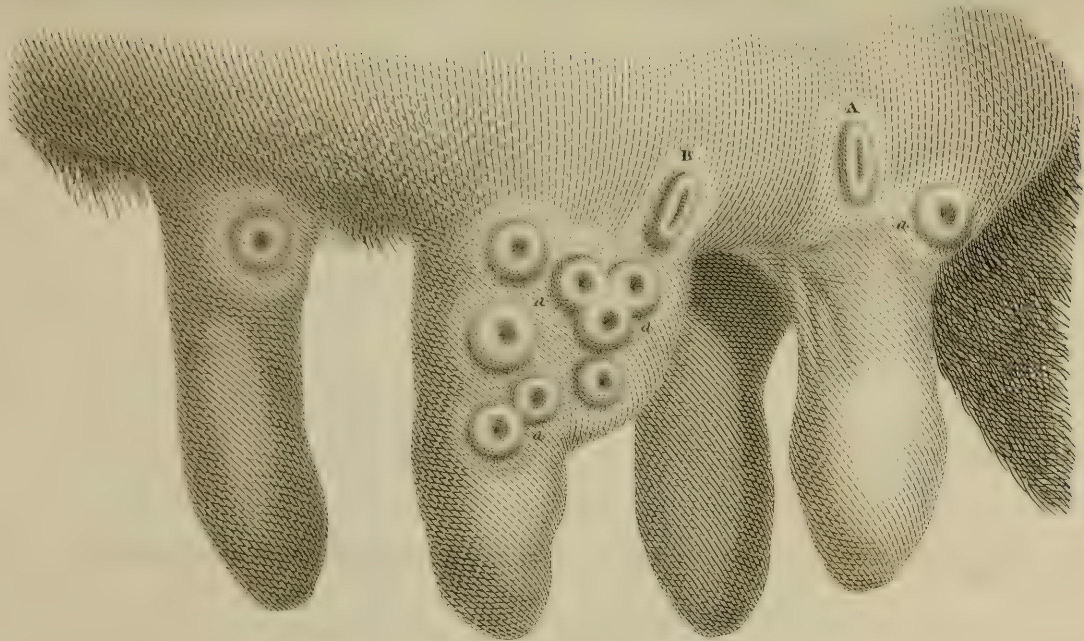

Lowry sculp

The Ibex, or Alpine Wild Goat.

Lowry sculp.