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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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ELEVENTH VOLUME.

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THE

THE

PHILOSOPHICAL MAGAZINE.

I. *A Dissertation on the History of Sugar. By Professor* BECKMAN*.

THOUGH Saumaise, Vossius, Deusing†, and other learned men have written on the history of sugar, yet no one who has read and examined the opinions, disputes, and arguments of these authors will deny that it is still very defective and obscure. For this reason, and as no article of commerce, whether we consider the immense quantity of it used in different parts of the world, the great number of persons employed in the cultivation of it, or in preparing and selling it, or the large sums expended annually in the purchase of it, seems more worthy of having the history of it cleared up, I resolved to try whether, after the attempts of so many writers, I could produce any thing new or certain on the subject; especially as many new observations, which may have escaped these authors, can now be drawn from books of travels, and works on natural history. It may also happen, that what the antients have said on the subject can be rendered clearer and more intelligible by a new revision; for the history of sugar has been involved, by learned men, in such a confusion of words and things foreign to it, that it costs no little time and labour to separate truth from probability.

As our sugar is the essential salt of that plant called by botanists *saccharum officinarum*, or *arundo saccharifera*, the question whether this plant was indigenous in that part of

* From the *Transactions of the Royal Society of Göttingen*, vol. v.

† Salmasii Exercitationes Plinianæ, p. 716. 916.; and Exercitationes de Homonymis, p. 109.

Is. Vossii Observationes ad Pompon. Melam: *Hagæ Comit.* 1658. 4to. p. 268.

Antonii Deusingii Fasciculus Dissertationum: *Groningæ* 1660. 12mo. p. 479. Theophrasti Hist. Plantarum illustrata à Joh. Bodeo a Stapel. *Amstelodami* 1664, p. 109.

 the

the world known to the antients, ought to be discussed before we inquire whether the antient Greeks and Romans were acquainted with sugar; for, if it could be proved that the sugar cane was transplanted in modern times from the new to the old continent, there would be no occasion for dispute. But this point, as might be easily shown, has been neglected by most of those who have attempted to write on the history of plants; and such an error has been lately committed by a man of learning, who entered into a discussion respecting the Greek and Latin names of the potatoe (*tuberosum solanum*), which, without all doubt, was transplanted into Europe from South America. In regard to the sugar plant, it is indeed certain that it grows spontaneously in many parts of Asia and Africa; for it can be established, by incontrovertible proofs, that sugar was made in Europe before it was known in America *. It is possible that the Greeks and the Romans may have been acquainted with a plant abounding with sweet and pleasant juice which grew spontaneously in various places; and as the word *saccharum*, which, without doubt, signifies some sweet juice, occurs frequently in the writings of the antients, it is worth while to inquire whether the *saccharum* of the antients was the same as that which we use at present.

What the antients called *saccharum*, *sakcharon*, *sakchar*, or *sakchari*, was a white substance, similar in consistence to salt, and which, like salt, broke under the teeth. It had a sweet taste, but less so than common honey. It was found in Arabia Felix, but the best was found in India; and it was brought also from the remotest parts of Ariace and the country of the Borygazi. It was not made by art, but was produced by a reed, in the form of gum, or adhering to the canes in natural concretions. The largest of these with which the antients were acquainted were in size equal to about that of a walnut. In the time of Pliny, Dioscorides, and Galen, it was employed only in medicine †.

This

* *Voyages de Monf. Chardin*, à Rouen 1723, 8vo. vol. iv. p. 34:—Quant au sucre, je crois qu'il y en a eu de tout tems aux Indes. Je sai bien que cela est fort contesté,—mais je tiens le contraire, fondé sur ce que le sucre croît partout dans les Indes abondamment, aisément, excellemment, et non pas comme les fruits que l'on tire des pais éloignez, qui ne viennent jamais si bien, lorsqu'ils sont transplantez loin de leur sol. Une autre raison, que j'ai encore plus forte, c'est que le sucre se trouve nommé et ordonné en cent endroits des anciens écrits de medecine Indiens, Persans, et Arabes.

† *Plinii Hist. Nat.* lib. xii. c. 8, edit. Hard. vol. i. p. 659: Saccharon et Arabia fert, sed laudatius India; est autem mel in arundinibus collec-
tum

This is all that the antients have left us in their writings respecting *saccharum*. It is indeed short and defective, but let us try what certain information can be drawn from it. The name itself is of no importance in determining the question, for antient names have often been applied to new things with which the antients were unacquainted. For this reason then, though the name be the same, there may be a great difference between our sugar and that of the antients. Nor can any arguments be adduced from the origin of the word, since it is altogether unknown, and as there are some who believe, and not without reason, that this appellation was given to various vegetable juices found naturally in a concrete state on the plants which produced them *.

The plant which produces our sugar, as well as that which produced the *saccharum* of the antients, may, in some measure, be referred to that class of plants called in general reeds (*arundines*), and grows in the same parts of the East Indies and Arabia from which the antients procured theirs. But their *saccharum* was not formed from the expressed juice, as is the case with that which we use at present, but was collected, in the manner of a gum, upon the reeds themselves. This circumstance, however, may even be called in question by those who wish to search only for truth, and to separate what is certain from what is doubtful. For Galen himself

tum gummium modo, candidum, dentibus fragile, amplissimum nucis avellanæ magnitudine, ad medicinæ tantum usum.

Dioscorides de Medica Materia, lib. ii. c. 104, edit. Saraceni, p. 112:— Καλειται δε τι και σακχαρον, ειδος ον μελιτος εν Ινδια πεπηγοτος και τη Ευδαιμονι Αραβια, ευρισκομενον επι των καλαμων, ὁμοιον τη συςασει ἁλσι, και θραυομενον ὑπο τοις οδουσι καθαπερ οἱ ἁλες. Est et quoddam mellis concreti genus, quod saccharum nominatur, quodque in India et Felici Arabia in arundinibus reperitur, consistentia sali simile, ac dentibus etiam salis modo fragile.

Galenus de Simpl. Medic. Facultat. lib. vii. c. 9. edit. Charterii, vol. xiii. p. 207: Και το σακχαρ καλουμενον ὁπερ εξ Ινδιας τε και της Ευδαιμονος Αραβιας κομιζεται. Περιπηγνυται μεν ὡς φασι, καλαμοις, εςι δε τι και αυτο μελιτος ειδος. ἡττον μεν ουν εςιν, η το παρ ἡμιν γλυκυ. Sed et saccharum, ut vocant, quod ex India atque Felici Arabia convehitur, concrescit quidem, ut aiunt, circa calamos, et ipsum mellis est species minus certe nostrate dulcis, sed assimiles ei vires obtinens, &c.

Arriani Periplus Maris Erythrææ, edit. Stuckii, p. 5: Εξαρτιζεται δε συνηθως, και απο των εσω τοπων της Αριακης και Βαρυγαζων, εις τα αυτα τα περαν εμπορια, γενη προχωρουντα απο των τοπων σιτος. και ορυζα——και μελι το καλαμινον, το λεγομενον σακχαρι. Ex intimis locis Ariaces et Barygazorum, in hæc eadem ulteriora emporia ex more solito devehuntur variæ res, ut frumentum oryza—et mel arundineum, quod saccharum dicitur.

* The celebrated Plaz, in *Program. de Sacchаro*, Lipsiæ 1763, adduces the opinion of Reiske respecting the name *succar*, viz. that it is of Persian or Indian and not Arabic extraction; and that it was brought, together with the thing signified, from India to Arabia.

ſeems to have entertained ſome degree of doubt when he ſays,—" it forms itſelf into concretions, *as we are told*, around the reeds." This account alſo may be rendered ſtill more ſuſpicious when we reflect upon the many ridiculous things which both the antients and moderns have related reſpecting articles brought from diſtant countries by merchants, who, for the moſt part, are intereſted in concealing the real nature of their merchandize under falſe accounts. Nor do I ſee any arguments to confute thoſe who might aſſert that Pliny and Dioſcorides were deceived by merchants; ſo that theſe two naturaliſts believed the *ſaccharum*, which was an expreſſed juice, condenſed by art and the help of fire, to be a certain kind of gum. This doubt I, indeed, cannot remove; but let us try whether we can produce any thing ſatisfactory by adopting Pliny's opinion, that the antient *ſaccharum* was the work of nature, and not of art. I ſhall therefore inquire whether our ſugar-cane ever produces ſugar ſpontaneouſly; which, indeed, ſeems not at all improbable, ſince we have camphor both in a natural ſtate and produced by art. This queſtion, indeed, may be determined by the teſtimony of thoſe who in India have made this an object of their attention.

Hernandez, in a book which he wrote on the plants of Mexico*, relates that he had heard that fragments of ſugar were found ſometimes adhering to the tops of the canes. Matthiolus † alſo ſays, that he had heard from men worthy of credit, who had viſited the iſland of St. Thomas and Madeira, that in thoſe iſlands the ſugar is found in a concrete ſtate, like gum, on the ſugar canes, by the exuding of the juice, and not unlike that made by art called *ſugar candy*. John Langius ‡ ſaw alſo ſugar canes, brought from

* *Hernandez Theſaurus Novæ Hiſpaniæ*: Romæ 1651, fol. lib. iv. c. 14. p. 111:—Audio naſci ſponte ſua apud Argentarium, quod vocant, flumen arundines ſacchari, arborum magnitudine, quibus (quod dixerat Plinius in veteri quoque orbe evenire) vi ſolis expreſſi ſacchari orbiculi ſuprema parte adhæreſcant.

† *Matthioli Opera*, edita à C. Bauhino: Baſiliæ 1674, fol. epiſtol. lib. i. p. 76:—Sed an quid pluribus de his diſputandum, cum multorum teſtimonio jam notum ſit in inſulis, quas Canarias et Divi Thomæ appellant, in iiſdem ſane calamis, a quibus ſaccharum exprimunt, illud etiam per ſe gigni, et aperto arundinis latere, manare, et gummi modo coaleſcere? quibus facile ſubſcribunt ii, qui hoc ævo in Indiam navigant. Etenim ad Bethecalam Indiæ utrumque jam vendi affirmant.

‡ *I. Langii Epiſtolarum Medicinalium Volumen tripartitum*, Hannoviæ 1605, 8vo. p. 320: Vidi quoque ex Madera in Hiſpaniam allatas arundines quibus ſaccharum purum accreverat; quod ſeparatum, ne reliqui decoctio vileſcat, non niſi magni veneunt.

Madeira to Spain, to which sugar was adhering in a pure concrete form. Amatus Lusitanus* writes, that the juice sometimes exudes from the sugar cane and concretes in the same manner as resin or gum, and not unlike lumps of sugar candy; and that the same thing happens not only in India, but in Madeira, the island of St. Thomas, and the Canaries. Bauhin † likewise tells us, that there are sometimes found in that part of the cane called by modern botanists the *eyes of the leaves*, grains or tears of sugar, of the size of peas, reduced to a concrete state by the heat of the sun, sometimes pellucid, and sometimes candied and not pellucid. John Costæus ‡ also believes, from works published in India, that the sugar is produced spontaneously on the canes at Bethecala in that country.

That no doubt may arise on account of the size of the concretions, which Pliny says were sometimes as large as a walnut, I shall here observe that this might be easily accounted for, since the drops adhering to the leaves, being melted by the heat of the sun, might fall down and be united into one mass. Manna concreted in this manner was seen by Niebuhr in Arabia §.

We have therefore a sufficient number of testimonies which seem to confirm the account given by the antients of their *saccharum*. I am acquainted, however, with no later authors who saw sugar in that form; and Rumph || denies that any of this kind is to be found in India: but that author, who examined with great attention the nature of the sugar cane, assigns a reason, not at all improbable, why sugar is never now formed spontaneously on the canes. The planters, he says, do not suffer the canes to attain to a sufficient age for the exudations to be produced, because the cane every

* *Amati Lusitani Enarrationes in Dioscoridis de Medica Materia Libros:* Argentor. 1554, 4to. p. 227:—Veteres saccharum dixerunt sal in arundinibus collectum; nec ab re, cum in ipsis arundinibus non quidem fistulosis, sed succo plenis, magno æstu, succus foris attrahitur, et ibidem ac resina vel gummi in ipsis arundinibus densatur ac concrescit, similis omnino saccharo cando dicto, hodie arte parato, vel sali dulci, unde multi illum salem Indicum appellabant. Nec tantum in India hoc evenire novimus; sed etiam hodie in insulis Maderæ et S. Thomæ ac Fortunatis quas Canarias dictitant.

† Casp. Bauhini Theatrum Botanicum: *Basiliæ* 1658, fol. p. 298.

‡ *Costæi Annotat. ad Mesuæ Libr.* 1. *Secret. Med. Distinct.* 1. in initio: Saccharum antiquorum ex eadem planta provenire, ex qua nostrum saccharum vulgare, disces ex Indiæ commentariis; adhuc enim apud Bethecalam Indiæ tale videre est, iisdem arundinibus sponte adnascens, ex quibus reliquum purius atque impurius conficitur.

§ Beschreibung von Arabien: *Copenhagen* 1772, p. 146.

|| Rumphii Herbarium Amboinense, pars v. p. 190.

year becomes more woody, and loſes its beſt juice. Beſides, he thinks it probable that the juice might exude from the canes more eaſily and more frequently in the warm regions of Arabia Felix and Indoſtan than in the iſlands of India, which are more expoſed to rains.

There are, however, ſtill other cauſes of doubt; for even if we allow that the ſugar cane ſometimes produces concretions, yet it is not probable that they poſſeſs the ſame properties as thoſe which the antients aſcribe to theirs. For example, theſe concrete drops, on account of their melleous nature, ought, according to every appearance, to be rather of a viſcid and ſticky nature than brittle; and this doubt appears to me of more importance than that propoſed by ſome learned men who have compared the medical properties of the antient and modern ſugar. For though ſome aſcribe the ſame qualities to our ſugar as thoſe for which the antient phyſicians extolled theirs, yet many aſſert that there is ſo great a difference, that they believe the two ſubſtances to have been totally different *. But, in my opinion, there muſt neceſſarily be a great difference between freſh ſugar formed ſpontaneouſly, and juice expreſſed by great force, and afterwards inſpiſſated by the help of fire, and purified by the addition of various ſubſtances. Many particles are no doubt deſtroyed by boiling, and many are thrown out with the ſcum, as the juice cannot otherwiſe be formed into a cryſtalline maſs, ſince the particles, which are too mucilaginous and oily, and in the ſeparation of which conſiſts the refining of ſugar, oppoſe cryſtallization too much. It is indeed not poſſible in any other manner to produce our common white ſugar, which is forced to cryſtallize; but the cryſtals are exceedingly ſmall, and, by their coheſion, form a continued maſs. Beſides, all thoſe who have made experiments on the nature of new ſugar found in it all thoſe medical virtues † extolled ſo

* Dioſcor. lib. c. Εςι δε ευκοιλον, ευςομαχον, διεθεν ὑδατι και ποθεν· ωφελουν κυςιν κεκακωμενην και νεφρους. καθαιρει δε και τα τας κορας επισκοτουντα επιχριομενον. Eſt ſaccharum alvo idoneum et ſtomacho utile, ſi aqua dilutum bibatur; vexatæ vero veſicæ renibuſque auxiliatur; quin et illitum ea diſcutit quæ pupillis tenebras offundunt.

Galenus, lib. c. Saccharum aſſimiles melli vires obtinens, quod ad abſtergendum, deſiccandum et digerendum attinet; porro quatenus nec inimicum ſtomacho eſt, ceu noſtras, nec ſitim afferens, ea tenus ab illo ſubſtantia differt. Conf. *Galeni Methodus Medendi*, lib. viii. c. 4. edit. Charterii, vol. x. p. 191.

† See *Voſſius ad Melam*, p. 271, where may be found the following quotation from Julius Landus:—Arundines incolæ (puto, in inſula S. Thomæ) maturas et recentes comedunt jejuni, voluptatis gratia, ac etiam his de cauſſis, ut inquiunt, quod ſolvunt ventrem, jecur refrigerant, ſitim auferunt,

so much by the antients. Those therefore who, from the reasons I have adduced, may be inclined to believe that the sugar of the antients was the same as that now in use, will be readily persuaded that the antient sugar differed in nothing from ours but in boiling alone, and despumation, according to the phrase employed by chemists.

In my opinion, the antients were acquainted with a certain sweet substance collected from reeds; but it cannot be proved in a manner sufficiently satisfactory that it was the tears of the same cane or plant from which our common sugar is procured; especially since there are many kinds of reeds found in India which exude melleous drops. Lest this conjecture should appear to be too remote from the truth, I shall relate what a jesuit who had travelled through the southern part of America wrote to Francis Mar. Piccolo: "That in India melleous dew was sometimes so common that it formed itself on reeds in great abundance, and might thence be collected with very little trouble. He tasted these drops, and found that they were perfectly similar to sugar in every respect except the colour *."

In relating the accounts given by the antients of *saccharum*, I purposely omitted the words of Alexander of Aphrodisias for two reasons: first, because they are wanting in all the Greek editions of that author, and are found only in the Latin translations; and therefore it is impossible to say what his opinion was †. In the second place, I omitted the passage

auferunt, albos etiam faciunt dentes. Nimirum res est suavissima, neque cum sacchari arundinibus quæ Romæ venundantur, comparanda. Puerperæ etiam utuntur offa ex pane primum torrefacto, deinde in succi arundinum ultima coctura posito. Superaddunt vitellos, quod is cibus amissas vires maxime reficiat, ac stomachum et intestina recreet ventremque paullatim molliat.

* *Lettres édifiantes* V. *Recueil*, p. 264: Dans le mois d'Avril de May et de Juin, il tombe avec la rosée une espece de manne qui se congele et qui s'endurcit sur les feuilles des roseaux, sur lesquelles on la ramasse. J'en ai goûté. Elle est un peu moins blanche que le sucre; mais elle en a toute la douceur.—We are told also that the same thing takes place in the kingdom of Chili. See *Vidaure in Compendio della Storia Geographica Naturale e Civile del Regno el Chile*; Bologna 1776, 8vo. p. 13: and the German translation, Hamburgh 1782, 8vo. p. 23.

† *Alexandri Aphrodisii Problemata, omnibus Studiosis non minus utilia quam jucunda, Græce et Latine Joannis Duccioni Studio illustrata*: Parisiis 1541, 12mo. lib. ii. § 78. p. 70. 6:—Quod sacchar Indi appellant, mellis coagulum est, sole cogente rores, convertenteque ad mellis dulcedinem. Quod idem monte Libani fieri certum est. Fit sacchar id proximum salis globulis, candidum, fragile, vim id quoque tergendi purgandique melli similem habet, et, coctum ritu mellis nostratis, non æque atque incoctum purgare potest. Cibo tamen ita assumtum aptius ad alendum est.—There

is

ſage here alluded to, becauſe it is evident that Alexander of Aphrodiſias thought *ſaccharum* to be the ſame as that ſubſtance called at preſent *manna*; for he ſays, *Mellis coagulum eſt quod ſacchar Indi appellant quod idem in monte Libani fieri certum eſt.* But Libanon produces fruit-trees, and not canes that yield ſugar. And we are told by Galen, that ſuch abundance of honey was found on the leaves of the trees and ſhrubs of Mount Libanon, that the huſbandmen uſed to ſing, "Jupiter rains honey *." Alexander of Aphrodiſias, therefore, alluded to manna, which he called *ſaccharum*, or *ſacchar*; but manna, though it has an affinity to ſugar, is a ſubſtance totally different.

But it is poſſible that the antients may have been acquainted with ſugar, to which they gave ſome other name; and there are even learned men who think that ſugar is to be underſtood in various paſſages of the Greek and Roman authors, where they ſpeak of honey procured from reeds, of Indian ſalt, and of a ſweet liquor obtained from the roots of canes. There can be no doubt that the antients conſidered their *ſaccharum* as a kind of honey. Whatever was ſweet, the antients called honey; and for this reaſon, latter ages beſtowed the ſame appellation on every thing endowed with peculiar ſweetneſs. In this manner, manna is called *aëromeli* by Athenæus †, or rather Amyntas: beſides, from manna diluted with water in the ſame manner as honey, they made a kind of beverage which they praiſed as being ſweeter than honey. No wonder then if they thought *ſaccharum*, which was found in ſpontaneous concretions upon reeds, and collected in that form, to be, as well as manna,

is preſerved in the library of our univerſity a very ſcarce French tranſlation, entitled, *Les Problemes d'Alexandre Aphrodiſé—traduits de Grec en Francois* par M. Heret, à Paris 1555, 8vo. where the preceding paſſage is thus tranſlated, p. 93:—Ce qu'on appelle ſucre d'Inde eſt un lopin de miel fait par le ſoleil coagulant la roſée, puis la converiſt en douceur de miel. Ce que certainement ſe fait au mont de Liban. Ce ſucre premierement eſt blanchi, puis devient friable, et preſque ſemblable aux motes de ſel. Auſſi ha il puiſſance de nettoier et purger ſemblable au miel. Mais s'il eſt cuit comme le miel de noſtre païs il ne peut purger ainſi que s'il n'eſtoit cuit; toutefois ſi on le mange en ceſte façon, il eſt plus propre à nourir. Comp. *Fabricii Biblioth. Græca*, vol. iv. p. 74.

* *Galenus de Aliment. Facultat.* III. 38. p. 119. tom. ii. Baſil. 1559:—Memini me aliquando cum æſtate ſuper arborum ac fruticum herbarumque folia mel quam plurimum fuiſſet repertum, agricolas velut ludentes cecinisse Jupiter melle pluit. Apud nos vero raro id contingit, in monte autem Libano quotannis perſæpe. Itaque coria ſuper terram extendentes ac arbores excutientes, quod ab eis defluit, excipiunt, et ollas ac fictilia melle implent, vocantque id mel roſcidum et aerium, δροσομελι και αερομελι.

† Lib. xi. Deipnoſoph. edit. Caſauboni 1597, fol. p. 500.; Theophraſti Opera, edit. Heinſii: *Lugduni Bat.* 1613, fol. p. 474.

a kind

a kind of honey. But when I confider every thing that the antients have faid of honey found on reeds, I can difcover no certain arguments by which it can be proved that it was the fame with our fugar.

Among the fragments of Theophraftus, there is one preferved which treats of honey. That author, as he generally does, has diftinguifhed with more care than other authors all the kinds of fweet fubftances known in his time. He, however, comprehends them all under the appellation of honey. "The firft (fays he) is that which is made on flowers; the fecond is that which falls from the atmofphere upon plants of any kind; and a third is formed upon reeds." The firft kind which the author here fpeaks of, is, without doubt, that honey extracted by bees from flowers; the fecond is a melleous dew, which, no doubt, belongs to the manna of the moderns; and the third feems to be the *faccharum* of Pliny and the Greeks, of which I have hitherto been fpeaking. But nothing can be drawn from this paffage to elucidate the hiftory of our fugar *. Megafthenes fays, according to Strabo, lib. xv. that honey is produced in India on reeds without the labour of bees. The words of Ifidore † convey as little information: "Honey formerly was a kind of dew, and was found on the leaves of reeds; and hence Virgil fays, *Hactenus aërii mellis celeftia dona.* Even yet, indeed, it is found in a concrete form, like falt, adhering to branches of fhrubs in India and Arabia." Seneca expreffes himfelf almoft in the fame manner:—"It is faid that honey is found in India on the leaves of reeds, and that it is produced either by the dew of heaven or by the fweet juice of the reeds themfelves. Our herbs alfo have a property of the like kind, and the honey is fought for and collected by infects fuited by nature for that purpofe ‡," &c.—Thefe words appear to me to be at any rate worthy of notice, as they confirm what I have already afferted, that the origin of *faccharum* was little known to the Greeks or the Romans, fince Galen and Seneca fay exprefsly that they related merely on report a circumftance which was therefore uncertain and lefs probable.

As Diofcorides compares the *faccharum* found on reeds to falt, and Paulus Ægineta § makes mention of *Indian falt*, which

* Bofe, formerly profeffor, agrees with me in this opinion. See his *Otia Wittembergenfia Critico-phyfica, Differtat. hab.* 1739, p. 16.

† Origines, lib. xx.

‡ Epift. 84. edit. *Lipfii*, p. 549.

§ *Pauli Æginetæ Opus divinum, Albano Torino Vitoduerfi Interprete,* Bafiliæ 1532, fol. lib. ii. c. 54, p. 95: Speaking of a foulnefs of the tongue, he which differed neither in colour nor substance from common salt, and which was said to be endowed with a melleous taste, many men of learning have been induced to believe that this salt is the same as that called *saccharum* by Dioscorides, Pliny, and others. But, even if we allow this to be true, we learn nothing from it that can illustrate Pliny's account of *saccharum*; for the Indian salt is recommended by Paulus Ægineta only to those whose tongues are rough and foul, to whom I do not know whether any sweetness can be of service, whether procured from reeds or from any other plant.

It now remains that I should consider that sweet liquor which the Indians are said to have made from the roots of reeds. This beverage is mentioned by various authors both Greek and Latin. Solinus, indeed, tells us that this reed was of a prodigious size; but Vossius very properly observes, that the size which he ascribes to the Indian reeds or canes could not be that of our sugar cane. It is therefore probable that Solinus either committed a mistake in ascribing to these large canes a juice which in reality was expressed from a much smaller kind of reeds, or that the liquor alluded to was the produce of a plant quite different from that which produces sugar. But some verses of Varro extolled by Isidore *, and

he says—Archigenes et sal Indicum huic usui adhibuit, quod sane colore et substantia nihil a communi sale dissidet, sapore vero meliteum est; lentis vero ejus salis magnitudo, vel ad summum fabæ quantitas comesta, affatim atque profuse humectare prodest.—The same author, lib. vii. p. 388, says: Quin etiam saccar quod vocant mel, quod ex Felici Arabia invehitur, non tam dulce quam nostras est, sed æquales ei vires possidet, neque stomacho molestum est, neque sitim conciliat. *Avicenna*, fen. i. can. 4. tract. 2. c. 231. speaking of foulness of the tongue in fevers, follows Paulus Ægineta: Aut teneat in ore salem qui asportatur de India, et est in colore salis, et dulcedine mellis; et sumat de eo, secundum quod dixit Archigenes, quantitatem fabæ unius.

* *Isidor. Origin.* lib. xvii. c. 7: In Indicis nasci arundines calamique dicuntur, ex quorum radicibus expressum succum bibunt, unde et Varro ait:

> Indica non magna nimis arbore crescit arundo:
> Illius e lentis premitur radicibus humor,
> Dulcia cui nequeunt succo concedere mella.

Nobody acquainted with Latin will deny, says Vossius, that there is something here in the first line which must offend every ear not vitiated. Saumaise increases instead of curing the fault, when he reads *Indica nam magna*. Vossius thinks that the reading ought to be:

> Indica non magna minus arbore crescit arundo.

To the same subject belongs the following line of Lucan, Pharsal. iii. p. 237:

> Quique bibunt tenera dulces ab arundine succos.

I shall subjoin also the words of Pomponius Mela, lib. 3. c. 7:—Tam pinguis and some expressions also of Strabo *, seem to prove that the Indian reeds were of an immense size; though some doubt may be entertained respecting the opinion of Varro, as the words are said to vary in different editions of the book. Nor can there be any doubt but that there are many kinds of reeds from which a sweet juice may be expressed equal in sweetness to honey. But the principal reason why I cannot assent to the opinion of those who believe that the liquor of the Indians was made from our sugar cane is, that no recent author has praised the melleous juice of the roots, which are slender and not very succulent. Nor did Rumph ever hear in India, that a sweet and potable juice was expressed there from the roots of either reeds or trees. The antient Indians, had they known our plant, would, without doubt, have expressed from the trunk or stem a potable liquor, as the narrow or spongy part is so full of juice that it may be sucked out with great ease. But I do not see how those could be refuted who might assert that the antients were deceived in this point by travellers, and falsely imagined that the juice expressed from the stem was contained in the roots. Nay, this opinion might be confirmed by this circumstance, that Rumph himself, who certainly examined and was well acquainted with every thing relating to the nature of the sugar cane, its form and size, imagined that the tops of the canes, which for the most part incline towards the ground, might have been considered by the antients as roots †. And this opinion acquires some weight when it is known that the part of the stem nearest to the root or the earth contains the largest quantity of juice, as appears from the account of Rauwolf ‡ and others.

This

pinguis alicubi et tam feracis soli, ut in ea mella frondibus deffluant, lanas silvæ ferant, arundinum fissa internodia veluti nevia, binos et quædam ternos etiam vehant.

* Strabo, lib. xv. ex Eratosthene: Τους δε ξυλινους καρπους, γενασθαι πολλους, και τας ῥιζας των φυτων και μαλιςα των μεγαλων καλαμων γλυκειας και φυσει και ἑψησει χλιαινομενου του ὑδατος τοις ἡλιοις, τουτ᾽ ἐκπιπτοντος εκ Διος, και του ποταμιου. Fructus autem arboreos multos gigni, radices stirpium, præsertim magnarum arundinum et natura et decoctione dulces esse, aqua a solibus tepefacta, tam cælesti, quam pluviali.—Those desirous of being acquainted with the various kinds of reeds known to the antients may consult *Theophrast. Bodæi a Stapel.* p. 480.

† Herbarium Amboinense, vol. v. p. 188.

‡ *Beschreibung der Reise Leonh. Rauwolffen*; Frank. 1582, 4to. lib. I. c. 4. p. 53. The author, speaking of Syria, says: "Sugar canes grow exceedingly well here, and in other places in the neighbourhood. The canes in size and height are not unlike our canes, but the interior part of them, and particularly at the bottom near the roots, where they have most

strength,

This essay might appear very defective if I passed over in silence the opinion of Saumaise * respecting the sugar of the antients, which has been adopted by many learned men. This author endeavours to prove, by a variety of arguments, that the antient *saccharum* was the same as the modern *sacchar mambu* of the Indians, and the *tabaxir* of the Persians and the Arabs. I shall not, however, adduce all the proofs which he employs, and which have been frequently quoted by others, but confine myself to a few observations. As I never had an opportunity of seeing, as far as I remember, the sugar called *mambu*, as none of it has been imported into Europe in my time, we must consider it to be that mentioned by those who have been in India, and who have often seen *tabaxir*.

The tree which produces it, and which is very common in India, is called by botanists *arundo bambus* †. Between its joints a sweet thick juice exudes, sometimes in a large and sometimes a smaller quantity. Every tree of this sort does not, however, exude that liquor; but those only which grow in Bisnaga, Betecala, and part of the province called Malabar; and for this reason many who speak of this tree, or reed, say nothing of its sugar. This liquor, when found in a concrete state, is sometimes blackish and sometimes of an ash colour, but on that account is not considered to be of a worse quality. According to the testimony of Indian, Arabian, Persian, and Turkish physicians, the *tabaxir* is of service in external and internal heats, and also fevers and dysenteries. In the time of Garcias ab Orto ‡, whose words I have hitherto used, this *tabaxir* was as dear as silver. The name *sacchar mambu* signifies the same as sugar of *mambu*, as the Indians give the name of *mambu* to the reeds or branches of the tree that produces it. Piso § derives the word *tabaxir* or *tabarzad*, for so it is read in Rhases, who lived in the ninth

strength, they are full of an agreeable excellent juice, and for this reason are purchased at a dear rate by the Turks. Before they begin to use these canes, they strip off the long leaves and cut away the dry part at the top, so that the remaining part, which is good, and full of sap, is scarcely an ell in length. Some of them carry about with them through the streets canes prepared in this manner as our boors do pipes, and cutting off bit after bit, and peeling away the rind, chew them publicly with great pleasure; for at the bottom near the root they are very sappy, and as tender between the teeth as a lump of sugar."

* Salmas. ad Solinum, p. 719.

† Pomet Aufrichtiger Materialist, *Leipzig* 1717, p. 114.

‡ Aromatum et Simplicium Medicamentorum Historia, *Antwerpiæ* 1574, p. 49.

§ Piso de Saccharo, p. 5.

ninth century, from the Persian language, and explains it by the expression (*lac lapidescens*), petrified milk; but Garcias calls it (*humorem lacteum*), milky juice.

I shall now proceed to the opinion of Saumaise, which I allow to be extremely probable. Tabaxir is a juice which spontaneously concretes in the manner of gum on very tall reeds; it is said to be sweet, and, like the *saccharum* of Pliny, to be useful only in medicine. The name *sacchar* seems to have been given by the Indians since the remotest periods to the juice of this reed, and latter ages have bestowed the same appellation on the juice of our sugar cane, because, when condensed by the help of fire, it approaches very near in appearance to the sugar of the *mambu*. Some of the young reeds of the *mambu* are so spongy and abundant in juice, that they are chewed with great avidity by the common people on account of their agreeable taste. But some very weighty reasons prevent me from agreeing in opinion with Saumaise: for, in the first place, that the reed called *bambu* is a tree of such a size that canoes may be formed of it, is no argument; for it can by no means be proved that the reed of which Solinus and Dioscorides speak, is the same plant from which they collected their *saccharum*: nay, Solinus says in express words that the Indians squeezed from the roots of this reed a potable juice. But more modern botanists and travellers, whose veracity is unquestionable, and who were well acquainted with the tree, deny that any sweet and potable juice can be expressed from its roots. Such of the antients as have spoken of the Indian reeds, and pressing juice from them, have never mentioned *saccharum*: on the other hand, those who expresly mention *saccharum* do not say a single word of squeezing juice from it. Besides, though the juice of the young plants be of a sweet and agreeable taste, yet the tears which exude near the joints, and are condensed by the heat of the sun into the form of white pumice stone (for such it appeared at least to Piso), have no sweetness at all, or rather a peculiar taste a little astringent, and similar to that of burnt ivory *. For this reason Rumph, who often tasted *tabaxir* in India, asserts that he found no

* *Gul. Pisonis Mantissa Aromatica, c. 10. quæ adjuncta est Pisonis Libris de Indiæ utriusque Re Naturali et Medica*; Amstelodami 1658. fol. p. 186: Arundines mambu adolescentes medulla levi, spongiosa et liquida (non adeo farcta ut vulgares cannæ sacchariferæ) refertæ sunt, quam vulgus grati saporis gratia avide exsugit. At ubi arundines hæ proceræ et annosæ factæ fuerint, liquoris contenti substantia, color, sapor, et efficacia mutatur (sicut in nuce coco adulta etiam fieri videamus), atque paulatim protruditur foras, et juxta internodia vi solis coagulatur.

sweetness

sweetness in it, and that it approaches much nearer to amyli than to sugar or salt.

In the last place, whatever kind of substance the *saccharum* of the antients may have been, it was not well known either to the Greeks or the Romans. It was not in common use, and employed only in medicine. Nor do we read of any other sweet substance being used for seasoning food, making sweetmeats, and preserving fruit, than honey, in which apples of all kinds and even flesh were preserved *. The antient physicians also mixed honey with their medicines in order to render them more palatable. But even this circumstance seems sufficient to prove that they were not acquainted with our sugar, which without doubt might have been employed much more conveniently both for food and in medicine. It is true, indeed, that many things used for seasoning were employed first in medicine, and were then, after a long interval, introduced into cookery. Thus, spirit of wine and grain were long recommended for the use of medicine under the name of *aqua-vitæ*; in the like manner, coffee, tobacco, and many other things that now minister to the luxury of our tables, or are in common use, were first known as medicines. Even oranges were introduced into cookery only at a late period among the Romans, and were not commonly eaten. Oranges, said Pliny †, are not eaten but as a remedy for poison; and they were put into wardrobes with clothes in order to give the latter an agreeable smell ‡. Nor does the use of oranges, which in modern times are so necessary in food, that the art of cookery would be at a loss could they not be procured, occur as seasoning in the work of Apicius. We have reason therefore to conjecture that *saccharum* was known and used as a medicine, but not as food. But as the antients prepared a beverage for which they before used common honey or aërial honey, called at present *manna* §, why should they not have tried *saccharum* in the same manner? for though they had not *saccharum* altogether purified, yet it is evident that the taste of even impure sugar would have been more agreeable than that of the above nauseous manna. Nor is it probable that the antients, had they known sugar, would have abstained

* *Columella*, lib. xii. c. 10: Illud in totum præcipiendum existimavi, nullum esse genus pomi quod non possit melli servari; itaque cum sit hæc res interdum ægrotantibus salutaris censeo, &c. See also *Apicii* lib. i. c. 8: Ut carnes sine sale quovis tempore sint recentes.

† Plin. Hist. Nat. lib. xii. c. 3.

‡ Athenæus Deipnos. lib. iii. c. 7.

§ Athenæus, lib. xi. p. 500.

from it on account of its dearness, when they spared nothing in their seasonings that could gratify the organs of taste. Pepper, for example, was sold in large quantities at more than its weight in gold or silver *: nevertheless, it was so much esteemed that it was the most common of all spices, and it is scarcely ever omitted by Apicius in six hundred dishes.—Our sugar was therefore unknown to the antient Greeks and Romans.

II. *Description of a Dwarf of a very remarkable Conformation.* *By Baron* F. Von Wurmb, *late Secretary to the Batavian Society of the Arts and Sciences in the Island of Java* †.

A FEW years ago there was seen at Batavia a native of the island of Bali ‡, named Kitip, who was so small, and at the same time diminished in so symmetric a manner, that he deserves to be considered as one of the rarest phænomena in the animal œconomy of nature.

His whole length amounted only to two feet eleven inches English measure, or about $33\frac{1}{7}$ Rhinlandish inches; and the reduction of his body on both sides so regularly alike, that the opinion of Buffon respecting the power of simple parts acting on both sides with equal force, from which arise the double parts on the expansion of the animal body, seems to be fully confirmed. The head, though by far too large for the body of this dwarf, and which might have suited one of six feet, exhibited in other respects nothing remarkable. His countenance, considering his race, was not disagreeable, as the features seemed to express a grave and not unpleasant disposition. His hair was long and black, but here and there a little gray, because he had already attained to the fortieth year of his age. Like most of his countrymen, he had no beard. His skin was of a brownish colour. His shoulders were broad, and his chest strong. The belly had the usual form, only that it was too much contracted towards the lower part. His arms were too long in proportion to the body, and a little twisted or turned outwards. The lower part of the arms was only half as long as that above the

* Plin. Hist. Nat. lib. xii. c. 7.

† From the Transactions of the above Society.

‡ Bali or Balei is one of the small islands lying on the east coast of Java, the inhabitants of which carry on a trade in slaves with Batavia, and procure in return such articles as they stand in need of. This island is called also Lesser Java.

 elbow:

elbow: the hands were short, broad, and of a round form before. The fingers consisted of round stumps, which were totally loose, without being strongly connected with the bones of the hand behind, and which were capable of no movement. They were furnished with nails, and consisted of the third row only of the finger joints, as the first and second were totally wanting. Each hand had six of these shapeless fingers; two standing close together in the place of the little finger, and the other four in the place of the thumb, the fore, middle, and ring finger. The bones of the hand were bent a little outwards, by which means the fist appeared round, and the back of it somewhat hollow. This small cavity was of great service to little Kitip on account of the shortness and immobility of his fingers, especially at the time of his meals, as he used the back of his hand instead of a spoon in order to convey rice and other food to his mouth. His legs were remarkably short; the feet seemed to have grown out from them quite solid; for, though their motion gave reason to suppose the existence of the bone of the leg and ischia, there was, however, no external appearance of them. The knees were entirely wanting. The feet were short and broad. Each foot had six toes similar in form to the fingers, and which had grown out in the like manner—but with this difference, that they were connected with the feet by a small part of their round surface, and that the two which stood in the place of the little toe adhered somewhat to each other.

This dwarf was born of well proportioned parents at Squoati, in the island of Bali, in the year 1740. He never had the smallest intercourse with the female sex, nor showed the least inclination for it. It appeared, however, that he was far from having been neglected by nature in this respect, and that his continence arose rather from an opinion he entertained, that if he denied himself the gratification of intercourse with the sex in this world, he would obtain a more perfect body in the next, but that on this side the grave he would remain a deformed dwarf. However much he was convinced that Nature had neglected him at his birth, he nevertheless believed she had given herself the trouble to preserve him by a miracle, which took place soon after he was born. He related, in this respect, that his mother, ashamed of having brought so deformed a child into the world, resolved immediately to destroy him, but found to her astonishment, when she attempted to commit this barbarous deed, that her child was completely invulnerable. This miraculous circumstance filled her with astonishment, and conducted her back to parental duty.

The

The following are the proportions of this dwarf, according to measurement:—The whole length 2 feet 11 inches. From the soles of the feet to the commencement of the thighs $9\frac{1}{2}$ inches. Length of the foot 3 inches, height of the foot $2\frac{1}{2}$ inches: breadth of the foot before 3 inches. Length of the arm 10 inches. From the elbow to the joint of the wrist 5 inches; from the joint of the wrist to the tip of the middle finger 2 inches. Length and circumference of the finger of an inch. Length and circumference of the thumb 1 inch. Distance from the tip of the thumb to the tip of the little finger 3 inches. Breadth of the shoulders 1 foot. Length of the head 9 inches. Circumference of the upper part of the arm 9 inches; circumference of the wrist 5 inches; of the neck 12 inches; of the body at the nipples of the breast 2 feet 6 inches; of the body at the navel 2 feet 2 inches; of the thigh 1 foot 7 inches; of the ancle 9 inches.

III. *Directions for Tanning all Sorts of Hides and Skin, according to the new Process introduced by Mr.* W. DESMOND. *Communicated by the Author.*

SIR, New Palace Yard, Sept. 26.

I AM flattered to find that you approve of the *Directions for Tanning*; it is certainly my wish that this process should be as generally known and practised as possible. The extent and success with which it is and has been carried on during the last three or four years by some of the principal tanners in Warwickshire, Staffordshire, and other neighbouring counties, may be sufficient inducements to all tanners to make the experiment, and convince themselves that leather, equal if not superior in quality to the best hitherto known, may be manufactured in about as many days as it generally requires months to tan it well in the usual way, and with less expense of labour.

Tanners in beginning the new process are apt to be startled at the quantity of bark which it *seems* to require; but a little reflection and experience soon satisfy them that, whatever mode they adopt, the same quantity of bark is necessary to tan a pound or any given weight of leather, and that the bark is much less liable to be wasted or lost in the new than in the slovenly old mode: they also find that, by proper management, they can give their hides an additional weight,

 equal

equal upon an average to ten per cent. upon each more than in the uſual way.

A hide, to be properly tanned, muſt have imbibed the quantity of *tanning* neceſſary to ſaturate it; and whether it receive that quantity in one or in eighteen months, it can make no difference in the quality of the leather, but a very material one to the manufacturer.

The opinion that ſo many months were neceſſary to tan hides, was founded on the clumſy and injudicious mode of throwing the hides and bark together in the pits; but this opinion is now rapidly giving way, and probably will, with your aſſiſtance and that of other ſcientific men, be totally exploded in leſs time than was hitherto required to tan a hide. Tanners will be much indebted to you for your endeavours to undeceive them; and the public, I think, muſt be ultimately benefited by the general adoption of this proceſs.

I am very reſpectfully,
Sir,
Your humble and obedient ſervant,
Alexander Tilloch, Eſq. WILLIAM DESMOND.

Directions for Tanning.

AS that part of the preparation of hides and ſkins, properly called tanning, conſiſts in impregnating and ſaturating them with a principle obtained from tan, by which they acquire ſtrength and firmneſs, preſerve a ſufficient flexibility, and become indiſſoluble and incorruptible in water either hot or cold; ſo the nature of this proceſs is, 1ſt, To aſcertain, by a ſimple and certain criterion, ſuch ſubſtances as contain the ſaid principle: 2dly, To extract it from them, to ſeparate it from every other principle by which its effect might be impeded or counteracted, and to give it the degree of ſtrength neceſſary for the intended purpoſe: 3dly, To diſpoſe the hides and ſkins for the introduction of this principle: and, 4thly, To impregnate and ſaturate them completely with the ſame in leſs than a tenth part of the time uſually employed in tanning; all which operations are performed in the following manner:

Provide five veſſels, called *digeſters*, of any convenient materials and dimenſions, with an aperture at the bottom of each: let them be placed near each other, and elevated upon ſtillages or otherwiſe, ſo that a pail or other ſmaller veſſel may be placed under them. Fill the digeſters with tan, that is to ſay, with the bark of certain trees, of oak for inſtance, cut ſmall, bruiſed, or ground to a coarſe powder. Pour water on the tan in the firſt digeſter, where it may ſtand ſome time, or be drawn off immediately. This liquor is to be poured on the tan in the ſecond digeſter, and drawn off as before; then on the tan in the third digeſter, and ſo on until it comes through the tan in the fifth and laſt. The liquor is then highly coloured, and marks from ſix to eight degrees on the hydrometer for ſalts. This liquor is to be uſed for tanning the thickeſt hides; and may, for want of a more characteriſtic name, be called the *tanning lixivium*. It has this peculiar property, that if you take a ſmall quantity of it in a glaſs, and pour on it a few drops of a ſolution of animal glue, the liquor, which before was clear, becomes turbid, and a whitiſh ſubſtance falls to the bottom of the glaſs. The precipitate thus obtained, by means of the ſolution of glue, is a ſure indication that the liquor contains the tanning principle; for this reaſon, that glue being of the ſame nature with the ſkins or hides of which it is made, whatever ſubſtance unites itſelf indiſſolubly with the former, will do ſo likewiſe with the latter.

This ſolution is made by diſſolving a little common glue in water over a moderate fire: by means of it, not only oak bark, but alſo the bark of ſeveral other trees, ſuch as the plane-tree, cheſtnut-tree, the American hemlock-tree, poplar, elm, willow, &c. as well as divers ſhrubs and plants, ſuch as myrtle, ſhumach, &c. all which I call *tan*, are found to contain the tanning principle; and by employing the ſolution as above, it will, in all caſes, be eaſy to aſcertain whether any given ſubſtance contains this principle or not.

In the courſe of theſe lixiviations two things will be obſerved. 1. The liquor running from the firſt digeſter, at length loſes its colour. If in this ſtate you take a little of it in a glaſs, and repeat the former experiment, the liquor no longer becomes turbid, but remains clear; which ſhows it contains no more of the tanning principle: but if you pour into the ſame glaſs a few drops of a ſolution of ſulphate of iron, the liquor becomes thick and black. This liquor is not to be poured on the tan in the ſecond digeſter, but is to be laid by, and uſed for the *depilation*, or taking off the hair or wool, as will be more particularly deſcribed hereafter. It is diſtinguiſhed by the name of *gallic lixivium*, becauſe it appears to contain the ſame principles as galls.

The liquid ſulphate of iron is obtained by diſſolving a ſmall quantity of iron in oil of vitriol diluted with water, or by diſſolving green copperas in water. This ſolution ſerves to aſcer-

 tain

tain such substances as contain the gallic principle. Lime water will also produce this effect.

When the liquor ceases to grow black by the mixture of the sulphate of iron, it will be useless to pour any more water on the tan in the first digester. This tan, being exhausted both of the tanning and gallic principles, must be removed, and new tan put in its place.

2. You will observe that the liquor, after running through all the digesters, at last grows weak. Add to your stock of tanning lixivium all the liquor that marks from six to eight degrees on the hydrometer. What proceeds afterwards from the last digester is to be poured on the new tan in the first. Then the fresh water is to be conveyed on the tan in the second digester, and the liquor of the first to be laid by, while it marks six or eight degrees on the hydrometer, and added to the tanning lixivium, which must always be carefully separated from the gallic. In this manner, the tan in all the digesters may be renewed, and the lixiviations continued.

The number of these lixiviations, as well as the mode of making them, may be varied at pleasure; the essential point is to repeat them so as to give the liquor a sufficient degree of concentration, which may be determined by the hydrometer, and proportioned to the quickness required in the operation, and the thickness of the hides and skins to be tanned; all which experience will soon teach. As all kinds of tan are not equally good, it will sometimes happen that six or more filtrations will be necessary to obtain a lixivium of six or eight degrees: in this case, as well as where a greater quantity of liquor is required, the number of digesters is to be increased, and the same method pursued as above; and when a weaker lixivium is wanted, three or four filtrations will be sufficient.

The person who directs these lixiviations should be provided with the solution of glue and sulphate of iron, already described, in order to ascertain the qualities of the different lixivia, as well as with a hydrometer or areometer, properly graduated, to determine their degree of concentration or specific gravity.

Cow Hides, Ox Hides, &c.

1. *Washing and Fleshing.*—They should be washed in running water, well cleaned, and fleshed in the usual way.

2. *Depilation, or taking off the Hair.*—Immerse the hides for two or three days in a vat filled with the gallic lixivium, and a mixture of sulphuric acid, or oil of vitriol, marking 66 degrees on the hydrometer for acids, and in the propor-

tion of 1 to 1000; that is, one part of vitriolic acid to a thousand parts of gallic lixivium; or 1 pint to 125 gallons. During this immersion, the hair is detached from the hides in such a manner, that you may easily know when they are to be taken out of the vat, that is, when the hair is quite loose. It is then to be scraped off with the round knife on the horse or beam.

3. *Raising.*—When raising is necessary, which is seldom if ever the case, immerse the hides for ten or twelve hours in a vat filled with water, and a five hundredth part of its volume of mineral acid, of the same quality as the former, and the operation of raising or swelling is done.

4. Wash the hides repeatedly, and use the round knife; then they are fully prepared for tanning.

5. *Tanning.*—The remaining part of the process consists in tanning, properly so called: for which purpose, steep the hides for some hours in a weak lixivium of only one or two degrees; to obtain which you may take that which runs from the second digester, or some that has been already used for tanning. They are then to be put in a stronger lixivium, where in a few days they will be brought to the same degree of saturation with the liquor in which they are immersed. The strength of the liquor being then considerably diminished, it must be renewed; and when the hides are completely saturated, that is, fully tanned, which is known by cutting off a bit of the edge, remove the leather, and let it dry slowly in a shady place.

Calf Skins, Goat Skins, &c.

1. Flesh them with the knife, and work them in running water, like the others.

2. Steep them in lime water, in which there should be more lime than the water can dissolve at once. What is not dissolved will subside to the bottom, but must be mixed with the water, by stirring it several times a day.

3. After two or three days, remove the skins: when the hair is found quite loose, scrape it off on the horse; wash and press the skins well, until the water running from them is perfectly clear, and the lime totally extracted.

4. Steep them first in a weak lixivium, then tan them as above; but observe that the tanning lixivium must not be near so strong as for the hides.

Lime is used for these soft skins instead of the mixture of gallic lixivium and vitriolic acid, for this reason, that the acid always swells the leather more or less, and because the lime may be more easily extracted from them, by washing and compreſſing them, than from the thick hides, which when limed are harſh and apt to crack, if the lime is not totally extracted before they are tanned.

Amongſt the different modes of immerſion, which may be practiſed in the courſe of theſe operations, the beſt way ſeems to be that of ſuſpending the hides and ſkins vertically in the lixivia, by means of tranſverſal rods or bars, and at ſuch a diſtance aſunder as not to touch each other in any one point. If they are laid out one over the other, according to the common practice, they will require frequent *handling*, in order that all the parts may be equally ſaturated, and to prevent the folds or plaits that would otherwiſe be formed in them: all this would occaſion a conſiderable loſs of time and labour.

In ſome caſes it will be found expedient to mix freſh tan from time to time with the lixivium; this and other modifications, ſuch as the various ſtrengths of the lixivium, the raiſing or not raiſing the hides, the uſe of the gallic lixivium, &c., which may be found neceſſary, will depend on the ſtate and quality of the hides and ſkins to be tanned, as well as on the purpoſes for which they are intended: all theſe conſiderations muſt be left to the judgment of the manufacturer, but do not in any way alter or change the principle on which this mode of tanning is founded.

Beſides the very great ſavings in point of time and labour, the leather tanned according to the above method, being more completely ſaturated, will be found to weigh heavier, to wear better, and to be leſs ſuſceptible of moiſture, than the leather tanned in the uſual way.

THE following additional explanations may be of uſe to thoſe to whom ſcientific terms and chemical operations are not familiar.

1. Reſpecting the number, the diſpoſition, and the dimenſions of the digeſters. Five or ſix of theſe veſſels are ſufficient to ſhow, by way of experiment, the nature of the proceſs, the different principles contained in the bark, how they are to be diſtinguiſhed and ſeparated from each other, &c. But when the buſineſs is conducted on a permanent and extenſive ſcale, a much greater number will be neceſſary, not only for the purpoſe of ſupplying the required quantity of liquor, but alſo for ſpending or completely exhauſting the tan, particularly in cold weather, or if hard water is uſed, and the bark not ground fine enough. I would recommend twelve digeſters at leaſt, fully to anſwer theſe purpoſes.

They may be diſpoſed in four rows of three veſſels each: the two middle rows placed cloſe to each other; the two ſide rows ſeparated

ſeparated from them by a paſſage for a ſmall cart or wheelbarrow to fill the digeſters occaſionally with new tan, and to remove it when ſpent. The bottoms of the four front digeſters, or thoſe neareſt the vats, ſhould be ſome inches higher than the edge of the vats, ſo that, when the liquor is ready, it may, by means of a ſhoot, and turning the cock in the digeſter, run of itſelf into the vat where it is wanted. Over each of theſe front digeſters is to be placed a ſecond, projecting a little into it, ſo that, by turning a cock, the liquor may run from the upper into the lower digeſter, without the trouble of pumping; and over the ſecond, a third placed in the ſame manner and for the ſame purpoſe.

By this diſpoſition of the digeſters all the labour of pumping is avoided, except from each of the loweſt to the uppermoſt one of the next row. The four inferior digeſters muſt conſequently be provided with an eye each, for the pump to ſtand in when neceſſary.

Beſides the cock or plug placed in the part of the ſuperior digeſters which projects into thoſe immediately under them, there muſt be another in the ſide for the purpoſe of conveying the liquor into the vats, when neceſſary, by means of a ſhoot.

As to the dimenſions of theſe veſſels, their depth ſhould not exceed two feet and a half, or three feet at moſt; but they may be as wide as the breadth of the ſhed or building, where they are erected, will admit; and, if large enough to contain half a ton of bark at a time, the better will they anſwer the deſired end. They ſhould be made of wood, and not pitched. Metallic or brick digeſters would greatly injure the liquor; no cement, or mortar made of lime, ſhould be uſed in them, becauſe lime deſtroys all the tanning principle with which it comes in contact.

Though theſe obſervations are applicable chiefly where new digeſters of the moſt convenient form are to be erected, yet even in old tan yards, where ſome of the pits already in uſe may be converted into digeſters, their number and depth ſhould be attended to. There ſhould be twelve of them, at leaſt, if poſſible, for the reaſons already aſſigned; that is, for ſupplying the neceſſary quantity of liquor, and completely exhauſting the tan: their depth ſhould not exceed three feet, that the preſſure of too high a column of bark may not prevent the water from diſſolving all its parts equally, and that the tan, by clodding, which it is apt to do, may not prevent the liquor from running. No more water ſhould be poured on the tan than is ſufficient to cover it, and therefore the eyes of theſe ſpenders ſhould be no larger than is abſolutely neceſſary for the pump to ſtand in.

It is ſcarcely neceſſary to add, that the bark as well as the infuſions ſhould be carefully protected from rain, ſnow, and hail, which would injure them materially; and even from froſt, if poſſible, becauſe the operation of the liquor when frozen is totally ſuſpended.

2. Five or ſix days are generally ſufficient for the immerſion of the hides in the ſame vats, as by that time they come to an equilibrium, in point of ſaturation, with the liquor; that is to ſay, they acquire all the ſtrength the liquor can give them. They are then to be ſhifted into a ſtronger infuſion, where they may remain the ſame number of days. In mild weather, if the liquor is of a proper ſtrength, three or four immerſions, of five or ſix days each, will be ſufficient to tan the hides, which in the old mode require eighteen or twenty months to be completely tanned. To keep them longer in the ſame liquor would be ſo much time loſt, as in making the infuſions it would be a loſs of time to let the liquor ſtand more than a couple of hours on the ſame tan. It is not the length of time during which the ſame water ſtands on the bark, but the number of waterings, that will completely ſeparate the tanning principle from it. As the heavieſt and beſt part of the liquor always falls to the bottom of the vats, it ſhould be ſtirred up from time to time.

3. The uſe of the ſolution of glue ſhould not be neglected. Since this ſolution has been made known as a teſt for aſcertaining the preſence of the tanning principle in the infuſions of different kinds of bark, a great variety of them have been already found, and many others may yet be diſcovered, to be excellent ſubſtitutes for oak bark; and in proportion as their uſe becomes general it will neceſſarily lower the preſent high price of the latter article, and ſave many young oaks.

Amongſt the barks already found, by means of this teſt, to contain the tanning principle in a greater or leſs proportion, the principal are thoſe of the following trees, viz. willow, aſh, hazel, Spaniſh cheſtnut, poplar, ſallow, cherry-tree, birch, ſycamore, plum-tree, beech, and elder.

4. In tan yards where there is not a ſufficient body of running water for ſoaking and waſhing the ſkins, in order to extract the lime from them, the preſent mode of extracting it in grainers, by means of pigeon's dung, hen's dung, or other alkaline ſubſtances, may be continued; but the ſkins ſhould never be put into the ſame liquor in which hides unhaired with the gallic and vitriolic acids have been tanned. The uſe of theſe acids is not abſolutely neceſſary either to ſhorten the time or produce good leather, but when uſed will be found to add conſiderably to its weight.

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5. By means of the hydrometer the comparative value of all sorts of bark may be easily determined. For this purpose take equal quantities of the barks to be compared; infuse them the same number of times in equal quantities of water, and the hydrometer will show their relative strength or value.

IV. *On the attractive Power of Bodies floating on Water.* *By Dr.* G. Carradori *.

If a few small dry bodies, such as bits of dry leaves or sawdust, be thrown into water and immersed at about the distance of an inch under each other, during the time of immersion you will see the bodies separate themselves a little from each other, owing, as may be readily conceived, to the effects of the motion occasioned by immersing them in the fluid. If you then bring one of these bodies near to another, the latter will every where follow the other, whether you immerse it deeper, raise it above the water, or move it horizontally; and both bodies will hang to each other, and thus exercise a kind of attractive force. This, however, is not the effect of the reciprocal attraction of the bodies; for, in the first case, that is, when you immerse the one body deeper, the other will endeavour to approach it, because a cavity or circular vacuity is produced, in which it must then follow the current; and the cause of this is, that the cohesive power of the water is not so strong as the attraction which it exercises over that body, and therefore its surface always yields more, and, keeping behind the body, pushes it forwards, and, as it were, swallows it up. This phænomenon is exhibited by all floating bodies which attract the water; for they follow the same movement, which, according to the adhesion which it has with the body, either rises upwards or sinks down.

As it has been observed that all bodies which contain a certain volatile or fixed oil, or resin, have an extensive power, whether they be fluid or hard, and, in the last case, that they fall into dust as soon as they are applied to the surface of the water, I am of opinion that all substances which exhibit such a phænomenon contain a common oil, or resin, which gives rise to it. I will not here mention that series of bodies, all of an oily or resinous nature, with which I made experiments; but I can assert that they all produced this effect in a greater or less degree. Milk for example, which, as every one

* From *Giornale Fisico-Medico*, by Brugnatelli, Vol. IV.

one knows, is an expressed juice, extends itself upon water, as well as the milky juice of the tithymalus and other plants of the like kind, though visibly with less force; and therefore there is no reason to doubt that the farina of fruits and other seeds is of the same nature as an expressed juice, as, both in a hard and fluid state, it extends itself on the water in the same manner as the milky juice of plants. The cause of the greater velocity with which farina spreads itself on water than milk and the milky juice of plants, ought, in my opinion, to be ascribed, without all doubt, to the resinous parts which form a difference between it and milk, as the latter contains a fixed oil. This is the case not only in regard to common meal, but also in regard to the tuberous roots, all the grasses, and, in general, all plants; and besides these, the farina of a great number of seeds of various kinds, both inflammable and incombustible, with which I made experiments: mealy fruits have this property in an eminent degree; and on this account we have reason to assert that all of them have a superabundance of resinous particles, and that they must afford nourishing food. There is reason to believe that the resinous part of corn or fruit consists in the gelatine, and therefore I imagined that starch, as a kind of mucilage, must be deprived of its property of extending itself in water; but it, however, possesses that property, though in a less degree than meal. We cannot therefore refuse to admit in it a resinous substance also. It must, however, be a substance more mucilaginous than gelatine itself, or resin; and we must ascribe the difference of these two substances to the proportion of this principle, and perhaps also to its different combinations.

Real gum, when divided into as small particles as possible, has an extensive movement on the surface of the water also. I therefore consider gum, resinous gum, and resin, to be almost degrees of one and the same substance.

When I was making experiments of this kind with different substances, a thought struck me to pound a piece of bread, made of dry grain, and to make experiments with it also, in order to ascertain whether it still retained the property of meal, and I found that it still exhibited the same phænomenon. This induced me to conclude that the resinous part of meal is destroyed neither by fermentation nor the process of baking.

Before I quit this subject it may not be superfluous to inform those who may wish to make experiments of this sort, particularly with a view to discover those substances which contain oil or resin, what cautions must be used to prevent them from falling into errors. Their first care, then, ought to be, that

that the water on which the substance with which they mean to make their experiments is thrown, may have a large surface, and the experiment will be decisive when it is performed in a vessel into which the water flows and runs out, or in a river or pond.

If the experiment be made in a vessel filled with water, oily impurities may easily adhere to the sides of it, and thus prevent the effect, as they will occupy the surface of the water.

Care must be taken also that no oily substance, of whatever nature, may have been previously adhering to the vessel; for, in that case, the experiment would fail.

The evaporation of the body may also occasion deception. I have myself observed, that when the hand is rubbed under the armpits, over the forehead, or on any other part of the body from which more oily evaporation proceeds than from others, and if a piece of earth be taken in this hand and placed on the water, an extensive movement immediately takes place. The same thing may happen, if the substance with which the experiment is made be held a long time in the hand. It is therefore necessary to wash the hands, and, after they have been well dried, to proceed to the experiment by throwing the substance on the water from a small spoon. It may thence be readily conceived what errors may arise if all these precautionary rules are not observed.

V. *Historical Sketch of the Institution and Progress of the Royal Society of London.*

BELIEF in the influence of the stars on the fortune and character of man, together with the attention always unavoidably given to the measurement of time, preserved some knowledge of astronomy among our ancestors even in the darkest ages of modern Europe, and rendered it one of the first objects of zealous study at the æra of the general revival of learning and science. The alchemical dream of the convertibility of all baser metals into gold, engaged many an enthusiast and many an impostor in experiments in chemistry, from the earliest period in modern history from which we have any information of the pursuits of the inquisitive and the learned. The uses of common life, and the restoration of this part of the science of the antients, encouraged and advanced the study of mechanics even in the fifteenth and sixteenth centuries. No other branch of physical science but these three had

had been much or successfully studied by the moderns before the days of Bacon.

It was the grand merit of Bacon, that he turned mankind from investigating in science "merely the relations of existence (in general very imperfectly known), and of words,"— "to acquaint themselves more fully, by the experiments of the senses and of auxiliary instruments, with existence in its different sensible modifications, and with the mutual relations of the various parts of material existence to one another." Science was thus at once reduced to "experience, and the arrangement of the facts which experience ascertained." The united inquiries, observations, and memoranda, of as many as possible of the intelligent and enlightened part of the human race, were henceforth understood to afford the only means for its true improvement. Men became now first sensible, that, by due investigation of the qualities and the phænomena of matter, they might discover modes of science unknown to the antients. And the necessity for the association of philosophers, in order to the accomplishment of the great purposes of philosophical inquiry, was from this time, by those who adopted the views of Bacon, warmly acknowledged.

It dwelt much upon the minds of the ingenious and inquisitive in England. Milton's plan for a new seminary and course of education, in his letter to Hartlib, seems to have been suggested by this fundamental idea. Cowley's proposal of a philosophical college, to be established, with an income of four thousand pounds a year, at Chelsea, was a fine model for the union of a school with a society for the advancement of physical knowledge. It was within a few years after the death of Charles the First, that a few of those persons, who were afterwards incorporated in the Royal Society of London, began at Dr. Wilkins's lodgings, in Wadham college, Oxford, those philosophical meetings which were to be in that society continued.

These were almost the first philosophical meetings in Europe for the advancement of physical knowledge. There were in Italy more than forty academies, for the refining of the Italian language and the social study of the fine arts. But, though the societies of architects, sculptors, and painters, might make some inquiries and observations relative to those parts of physics in which their respective arts were immediately concerned; yet the opprobrium of atheism, which was charged against the earlier natural philosophers in that country, had, with other causes, hitherto prevented societies, with express and exclusive views to physical discovery, from there arising. In France,

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an academy had been inſtituted at Paris for the refinement of the French language. A ſimilar eſtabliſhment began to be formed at Caen. At Aix, in Provence, the illuſtrious Peireſc, the Robert Boyle of France, not only correſponded with the moſt eminent and ingenious men in Europe on almoſt every intereſting ſubject in phyſics and in the ſtudy of antiquities, but engaged his friends often to aſſemble in his houſe for the mutual communication of their obſervations and diſcoveries as well in natural philoſophy as in every other part of knowledge. But the inſtitution of an academy of ſciences was reſerved to diſtinguiſh, at a period ſomewhat later, the miniſtry of Colbert, and the moſt ſplendid part of the reign of Lewis the Fourteenth. The profeſſors in the German univerſities occaſionally met and converſed about the ſubjects and ends of their ſtudies; but in theſe, phyſical ſcience had, hitherto, but ſmall ſhare; and there was not as yet, in Germany, any ſeparate company of philoſophers aſſociated for the inveſtigation of the laws of material nature.

Dr. Seth Ward, the Hon. Robert Boyle, Dr. Wilkins, Sir William Petty, Mr. Matthew Wren, Dr. Wallis, Dr. Goddard, Dr. Willis, Dr. Bathurſt, Dr. Chriſtopher Wren, and Mr. Rooke, were the principal perſons who aſſociated at the meetings in Wadham college. They met rather to perform experiments than to diſcourſe about them. Mechanics and chemiſtry were the branches of ſcience to which their firſt experiments chiefly related. In the year 1658 the ſcene of their meetings was transferred from Oxford to Greſham college, in London. Their numbers were here ſoon conſiderably augmented. Their meetings were, for a time, interrupted by the diſturbances which enſued between the uſurpation of the Cromwells and the reſtoration in 1660.

At the reſtoration, two Scotſmen, Sir Robert Moray, of Tibbermuir, and a Mr. Erſkine, who, having followed the king in his exile, had now influence at court, eagerly joined the philoſophical meetings at Greſham college, and perſuaded their ſovereign to take the riſing ſociety under his own ſpecial patronage. A royal charter was ſoon after granted, incorporating theſe philoſophers into a body, to conſiſt of a preſident, a council of one-and-twenty members, and an indeterminate number of fellows. William Lord Viſcount Brounker became their firſt preſident. The inſtitution was, for a time, very much an object of faſhionable attention at court. The king himſelf, as the patron and founder, the duke of Buckingham, prince Rupert, the earl of Clarendon, the firſt earl of Shafteſbury, Monk duke of Albemarle, duke Albert of Brunſwick-Lunenburgh, the earl of Peterborough,

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were enrolled at the head of a number of diſtinguiſhed names in the firſt liſt of the members—*Omne ignotum pro mirabili.* For a time, the moſt extravagant enthuſiaſm was excited in favour of the new ſociety, and from its exertions an improvement the moſt extraordinary of all the arts of life was expected at once to enſue.

The meetings of the ſociety continued after its incorporation by charter, as before, to be held at Greſham college. They took place every Wedneſday in term time, immediately after the lecture of the profeſſor of aſtronomy in that college, and were eagerly attended by the members. Dr. Croone, with the title of regiſter, had, at firſt, the care of recording the tranſactions at theſe meetings in journals. It was afterwards committed to Dr. Wilkins and Mr. Oldenburgh, the two firſt ſecretaries. Cowley had demanded a revenue of four thouſand pounds a year for his projected college at Chelſea; but the Royal Society was ſupported without any public fund, ſimply by the voluntary contributions of the gentlemen of whom it was compoſed.

The ſociety entered upon its reſearches with views at once magnificent and philoſophically modeſt. It propoſed but "to make faithful records of all the works of nature and art." By theſe it hoped to explode errors, to reſtore neglected truths, to apply philoſophy to the uſes of life, and to open up the avenues to future diſcovery. It profeſſed to admit into its aſſociation men of all religions, all countries, all conditions and employments in life. They propoſed to derive uſeful truth not only from the learned and profeſſed philoſophers, but from the ſhops of mechanics, the voyages of merchants, the ploughs of huſbandmen, the ſports, fiſh-ponds, parks, and gardens, of gentlemen Some heſitated whether Mr. Grant, a ſmall ſhopkeeper in London, the author of ſome excellent obſervations on the bills of mortality, ſhould be received into ſuch a company of philoſophers. But king Charles, on peruſing his obſervations, not only recommended his immediate admiſſion, but deſired that they would add to their number as many ſhopkeepers as poſſible of equal endowments.

In their reſearches they would not truſt the reports of others in any caſe where they could bring the matter under the examination of their own ſenſes. In ſome inſtances they allowed particular members to chooſe whatever ſubjects they ſhould think proper; to inſtitute, at pleaſure, any train of experiments on them; to defray the expenſe from the purſe of the ſociety; and then to report at the meetings the ſeries and the reſults of the experiments they had thus made.

Other

Other experiments were expreſsly ordered by the voice of the whole ſociety, and were intruſted in the performance, not to ſingle perſons, but to committees nominated for the occaſion. For inquiry concerning things at a diſtance, they had, very early, begun a correſpondence with the inquiſitive and enlightened in almoſt every different part of the world. At many experiments in the parks, in the royal gardens, and on the Thames, the king himſelf zealouſly aſſiſted in perſon. Mr. Huygens, the famous philoſopher in Flanders, received, ſoon after the inſtitution of this ſociety, its frequent aſſiſtance, and communicated to it all his diſcoveries and inventions; in particular, that noble one of the firſt application of the motion of pendulums to clocks and watches. The philoſophers of Florence, and eſpecially the grand duke's brother, prince Leopold, entered into friendly correſpondence with the Engliſh philoſophers, and acquainted them with every new obſervation that was made on nature in Italy. The Germans no ſooner heard of this inſtitution than they ſent many of their books to be ſubmitted to its cenſure, and contributed likewiſe preſents of various new inſtruments of their invention. The travellers, phyſicians, ſurgeons, anatomiſts, &c. of France were peculiarly early and frank in their communications to it. The Academy of Arts and Sciences at Paris was ſoon after embodied in imitation of this ſociety: and the example of the Engliſh was quickly, more or leſs, followed in almoſt every country of Europe in favour of the phyſical ſciences and the uſeful arts.

The experiments which king Charles the ſecond was the moſt attentive to, related chiefly to chemiſtry and mechanics. He would often amuſe himſelf at Whitehall in the execution of curious mechanical works. He had alſo in his palace a ſmall laboratory for chemical operations. He made aſtronomical obſervations, upon various occaſions, in Saint James's Park. He was exceedingly curious in directing the inquiries of the ſociety to every thing that concerned the improvement of the art of ſhip-building. He attended and directed different experiments at the Tower of London, as well as at Whitehall, to aſcertain the forces of projectiles, with their variations in different circumſtances.

In their early meetings, the members of the ſociety were at great pains to diſtinguiſh what objects were the worthieſt of their firſt inquiries. Some were directed to examine all "treatiſes and deſcriptions" of the natural and artificial productions of foreign countries: others were commiſſioned to diſcourſe with ſeamen, travellers, tradeſmen, and merchants, by whom thoſe countries and their productions had been perſonally

ſonally examined. From the union of the *vivâ voce* with the written intelligence papers were formed, explaining ſummarily what was known concerning every different country, and what remained to be yet inquired into. Theſe papers were read at ſubſequent meetings; and queries were then drawn from them, to be tranſmitted for anſwers to the ſociety's foreign correſpondents.

One ſet of queries which were prepared among the earlier labours of this ſociety, had for its object, to mark "what things were needful to be obſerved in order to the making of a natural hiſtory in general." They formed ſimilar queries reſpecting the hiſtories of the air, atmoſphere, and weather; of the production, growth, &c. of vegetables; of agriculture, &c. They marked out trains of experiments to be tried on rarefaction and condenſation; on the petrifaction of wood; on the loadſtone; on the diſcoveries wanted to complete the ſcience of anatomy; on injections into the blood of animals, and on the transfuſion of the blood of one animal into another: on the tides of the ſea; on the varieties of oyſters, and the manner of their nouriſhment; and on the phænomena of mines. They gave directions to ſeamen for the obſervation of the eclipſes of the moon, the eclipſes of the ſun by Mercury, and the ſatellites of Jupiter. They ſent abroad directions for obſervations and experiments to be made in India, China, the iſland of St. Helena, Teneriffe, Guinea, Barbary, Morocco, Spain, Portugal, Turkey, France, Italy, Germany, Hungary, Tranſylvania, Poland, Sweden, Iceland, and Greenland.

One of the firſt anſwers they received to their inquiries concerning foreign countries, was ſent from Batavia by Sir Philiberto Vernatti. It corrected an error, prevalent in Europe, "that diamonds and other precious ſtones were in the Indian iſles continually renewed, as by vegetation, in the quarries out of which they were taken." It informed them of the exiſtence of a volcanic mountain in Java, and another in Sumatra. It exploded a vulgar error, which one ſhould have thought to be, even otherwiſe, incapable of obtaining for a moment the aſſent of philoſophers—"that there was in the iſland of Sambrero, lying north from Sumatra, a ſenſitive plant that would, when pulled with the hand, ſhrink into the earth, and that had for its root a living worm." It brought one of the firſt accounts propagated in Europe of the exiſtence of the *boban upas*, the famous poiſon-tree of Java. It added a variety of other curious information concerning the moſt extraordinary objects and arts in the Eaſt. But of the queries ſent out, it left ſtill many unanſwered.

Mr.

Mr. Hooke was one of the moſt ingenious and indefatigable of the early members of this ſociety. He explained, in an admirable paper, the beſt method of making a hiſtory of the phænomena of the weather. He made many experiments, which were repeated, with many variations of circumſtances, by the ſociety, to prove, "that viſible and ſenſible fire was but the act of the diſſolution of heated bodies by the air: that this diſſolution of ſuch bodies took place in a manner perfectly ſimilar to the ſolution of iron, tin, and copper, by acids: that the evolution of heat and light was a neceſſary reſult of the ſolution of heated and combuſtible bodies in air: that the aſhes of a burnt body were a reſiduum of it, that was inſoluble in air." This chemical theory, as the learned reader cannot but perceive, differs from that which was propoſed by M. Lavoiſier, and is now deſervedly received as philoſophical truth, only in being expreſſed in the chemical dialect of that time, which is now obſolete; and in being, in the intention of its author, to a certain degree accommodated to other theories which then prevailed. In verifying the theory of Hooke, the ſociety made a number of experiments: to aſcertain how long a candle, a lamp, or ignited coals, would continue to burn in a cubic foot of common, of rarefied, and of condenſed air; to exhibit the ſudden extinction of flame by the affuſion of air already ſatiated with burning, and the impoſſibility of even the moſt intenſe and torrid heat to continue without a ſupply of freſh air; and to find what particular degree of heat was to be produced from the burning of every different combuſtible material. By other experiments, having a reſpect to the ſame theory, they aſcertained that flame was ſubject to be extinguiſhed by the air in a deep well. They made beſide theſe, and with a view to diſcern the varied phænomena of ignition and combuſtion, independently of Mr. Hooke's ſyſtem, a great number of other experiments on the bringing of copper into a ſtate of great combuſtibility, on the igniting of tin filings by the help of nitre, &c. &c.

They inſtituted a multitude of experiments to determine the nature, properties, and uſes of air. By the care eſpecially of Mr. Boyle, many trials were made to find, by means of the baroſcope, the different degrees of the preſſure or gravity of the atmoſphere at different heights and depths. They took much pains to diſcover the oppoſite limits of the rarefaction and the condenſation of air. They made a variety of experiments on the propagation of ſound in air: and what is, in a chemical point of view, highly remarkable, a number of their experiments were directed to aſcertain "the genera-

 tion

tion of air, by corrosive menstrua, out of fermenting liquors; and to determine the fitness or unfitness of such air to support combustion and respiration." Other experiments were made by them for the purpose of discovering how long a man might live by inspiring and respiring the same air; whether air contaminated by respiration might be again made pure and respirable; whether the unfitness of contaminated air for respiration were not a quality altogether independent of its temperature, as hot or cold. By other experiments they examined what quantity of air might be sufficient for the respiration of an animal for a given space of time; and in what manner air, previously applied to support combustion, became by that unfit to be respired. Others of these experiments respected the necessity of air to support vegetation; and it was the object of others to examine what use was made of air in the vital functions of fishes under water.

Water was the immediate subject of many of their first experiments. Some of these were made to ascertain the differences in the specific gravities of different sorts of water. The Torricellian experiment on the ascent of water *in vacuo* was repeated in almost every possible change of circumstances. Other experiments were made on the production and phænomena of steam. By others they endeavoured to find the differences in the heat of the waters of the ocean at different depths under the surface.

On stones and metals they instituted many experiments. Lead, diamonds, the Bologna stone, the separation of silver from lead, and especially the loadstone and the magnetized needle, were now first philosophically examined in regard to some of their most important qualities.

They examined the growth of vegetables in different sorts of water; the utility of steeping seeds; the inversion of the roots of plants set for growth; the decrease of the weight of plants growing in air; the reunion of the bark to the wood from which it had been stripped.

Their medical and anatomical experiments were numerous. They dissected a cameleon: they made injections into the veins of different animals: they made a number of observations and experiments with a view to determine how far there might be truth or falsity in the doctrine of the equivocal generation of insects: they made many trials on the meaner animals of poisons and antidotes: they tried what effects might ensue from the transfusion of the blood of one animal into the veins of another: and they made likewise some curious experiments on the hatching of silk-worms in rarefied air.

Their

Their experiments on the freezing of water in different circumſtances; on the production of cold by ſaline ſolutions; on ice, to evince that it was ſuſceptible of various degrees of cold more intenſe than that of ſimple freezing; and on the congelation of oils; were various, intereſting, and proſecuted with the moſt attentive accuracy. A curious train of experiments was made at the Tower, under the immediate inſpection of the lord viſcount Brounker, to aſcertain what changes might be produced on the weights of lead and copper by ſubjection to fire in a cupel. Both the copper and the lead were found to gain (by oxidation, no doubt,) an addition. The cupel ſuffered always a diminution of its weight when ignited, but not a diminution equal to the augmentation in the weight of the metals.

Among the inſtruments invented by the ſociety within a few years after its inſtitution were, an univerſal ſtandard meaſure of magnitudes by means of the pendulum; a wheel barometer, and other inſtruments, for finding the preſſure of the air; an auger for boring the ground, and fetching up parts of the ſtrata through which it ſhould paſs in their natural order; an inſtrument for meaſuring the ſwiftneſs and ſtrength of the wind; a diving-bell, and a pair of ſpectacles with which a diver might ſee any thing diſtinctly under water; ſeveral engines for finding and determining the force of gunpowder; ſeveral acouſtic inſtruments to aſſiſt and improve the ſenſe of hearing; a *chariot way-wiſer*, which would exactly meaſure the length of the way of any coach or chariot to which it was applied; an inſtrument for grinding optical glaſſes; a variety of teleſcopes, &c. &c.

A manufacture of glaſs had been, more than thirty years before, eſtabliſhed in Broad-ſtreet, in London, by a company of mercantile adventurers, among whom the moſt conſiderable perſon was admiral Sir Robert Manſel. Workmen and ſuperintendents were procured from Venice. It might have gone on ſucceſsfully, if the great civil war had not broken out. Soon after the reſtoration, the duke of Buckingham, at a vaſt expenſe, eſtabliſhed new glaſs-works in London; and the art was there, within a very few years, carried to ſuch perfection, that theſe works ſupplied better glaſs for microſcopes and teleſcopes than that which was to be had from Venice. The duke of Buckingham not only expended much money upon thoſe glaſs-works, rather as an experimentaliſt than a manufacturer, but took a warm and active intereſt in various others of the ſociety's purſuits; and it was under his immediate patronage that Dr. Spratt wrote his excellent Hiſtory of the Royal Society—the fineſt piece of Engliſh proſe that was produced in the ſeventeenth century.

[To be continued.]

VI. *Chariot Way-wiſer—Pedometer—known to the Antients.* *By Mr.* R. HERON.

To Mr. Tilloch.

SIR,

I PERCEIVE, by ſome excellent articles which have at different times appeared in your Magazine, that you are not averſe to illuſtrate the ſciences and arts by the lights of claſſical erudition. I cannot pretend that there is much erudition in having particularly remarked the following paſſage in the life of the Roman emperor Pertinax by Julius Capitolinus. But it certainly evinces at leaſt one ingenious mode of mechanical art, which I had ſuppoſed peculiar to the moderns, to have, on the contrary, had a diſtinct exiſtence likewiſe among the antients.

This paſſage occurs where the biographer has occaſion to relate, that Pertinax, to find money for a donative to the ſoldiers of the prætorian guard, ſold off, by a nine days auction, the ſumptuous dreſſes, furniture, and curioſities of art, with which Commodus had filled the palace. It is a part of an abridged inventory of the things ſold at that auction. It conſiſts of two clauſes: I ſpeak, for the preſent, chiefly of the latter.

"1. Vehicula, arte fabricata nova, perplexis diverſiſque rotarum orbibus, et exquiſitis ſedilibus,—nunc ad ſolem declinandum, nunc ad ſpiritus opportunitatem, per vertiginem: 2. Et alia, iter metientia, et horas monſtrantia."

"1. Carriages of a new conſtruction, with wheels of different diameters, and curiouſly connected with one another, and with ſeats of a peculiar and exquiſite contrivance; in which vehicles the rider could at pleaſure ſhade himſelf from the ſun, and was in no danger of getting out of breath; ſo eaſy was their motion at its utmoſt ſwiftneſs. 2. Alſo other carriages, which had contrivances to meaſure the diſtances over which they were driven, and to count the hours ſpent in the journey."

Here are two ſorts of carriages mentioned, both of new conſtruction. The former, remarkable for the eaſe of their motion,

motion, in consequence of having wheels of different diameters, and for covering the rider from the sun, may seem to resemble some of our present carriages more nearly than has been supposed, yet do not so strikingly differ from those which were common among the antients as to excite much surprise. But the latter appear, indeed, extraordinary: they possessed all the advantages, as it should seem, of the *chariot way-wiser*, which has been believed to have been first invented by some members of our Royal Society about the year 1662; and of the *pedometer*, which is at present manufactured, I think, under a patent.

I am, Sir,

Your obedient servant,

R. HERON.

London,
October 8, 1801.

VII. *Observations on the Spots of the Sun, and the Nature of the Light of that Luminary. By* M. VON HAHN *.

OUR experience of the various effects produced by the sun's rays, and particularly the doctrine of latent heat and specific heat, seem to confirm the opinion that light is an actual body which streams forth from the sun, and which is connected, in a variety of ways, with the bodies of our earth. The determination of the following question, which belongs peculiarly to chemistry, "Whether the sun be actually a burning body, or only attracts the illuminating matter from the surrounding expanse of the universe?" may receive some illustration from continued observations made on the body of that luminary.

One great difficulty here is, that the sun has a peculiar light, and we are accustomed only to consider objects which almost continually receive their light from without, and which commonly produce shadows more or less perceptible according to the different directions and strength of the light which falls upon them. Now our eyes, being accustomed to shadows and projecting images, by beholding an uncommon kind of illumination are thrown into embarrassment, and cannot bring the images in harmony with sensation, the basis of all perspicuity.

One may observe the sun a long time without discovering any other spots than those which are common. The first view, therefore, of the so called solar faculæ produces an

* From *Professor Bode's Astronomical Almanac for the Year* 1796.

agreeable ſurpriſe, as it exhibits ſomething very different from what we ſee in thoſe parts of ſurrounding nature which are neareſt to us. With a Herſchel's teleſcope of ſeven feet, one beholds with aſtoniſhment, and very diſtinctly, in thoſe ſpots in the middle of the ſun's orb, parts beſet with little hills ſeparated by valleys, and ſingle mountains, of a proportional height, which project ſtrong ſhadows. It is worthy of remark, that theſe objects have as diminiſhed an appearance as when one looks at things from a great height. The cauſe of this is to be ſought for in the ſituation of the light which falls on them, and belongs to thoſe optical deceptions that muſt occur ſo frequently in obſerving the ſun, and on a knowledge of them depends the whole ſecret of forming a proper judgment reſpecting the ſolar bodies.

The fineneſs of the delineation, the ſcenic appearance, and the regularity of theſe objects, induce one to believe that ſolar landſcapes are here actually repreſented; and totally deſtroy the idea of the ſun being a flaming body; for it is impoſſible that the irregular movement of flame could ever produce ſuch ſcenes. Theſe parts are, at times, of a more whitiſh colour; but it ought to be particularly remarked that they have a ſomewhat dimmer light than the reſt of the ſun's orbit, which is every where luminous. If the diviſion of the lights and ſhadows, the kind of illumination, and the gradation of the ſhades, be accurately obſerved and examined, we ſhall be obliged to admit that theſe faculæ in the ſun are landſcapes.

The moſt important obſervation here is, that when there are a group of ſuch landſcapes the ſhadows are almoſt always projected to one ſide, and are ſharp-pointed. This circumſtance ſerves to render clear to the eye a confuſed mixture of ſpots.

Let one ſearch out, in the neighbourhood of ſuch groups, ſmall, ſingle, inſulated ſpots, and obſerve whether there is ſeen near them a weak gradation of ſhade, or a luminous vapour which is very familiar to us, and which ſhows that the light illuminates an eminence on all ſides, but not with equal ſtrength. If the eye is once accuſtomed to the effect that hence ariſes, it can with great eaſe convert an often exceedingly compound mixture of dark parts and luminous ſpots into well arranged images.

There are certainly caſes in obſerving the ſun when draftſmen and painters only could be able to arrange properly the mixture of faint ſpots, and to obviate the deception which ariſes from half ſhadows. When an eminence, for example, is ſo ſtrongly enlightened by the luminous matter near it,

that

that one ſide of it has the ſame brightneſs as the ſolar light itſelf, we then no longer ſee an eminence, but only a faint ſpot produced by the half ſhadow on the other ſide. But if the direction of the ſtrongeſt half ſhadows towards any part of the ſun's diſk be once known, we may then venture to examine the greateſt collection of luminous faculæ, and diſtinguiſh them with the beſt ſucceſs. They then loſe their ſtrange appearance; and the eye becomes convinced that an aſſemblage of eminences and plains could exhibit ſuch an appearance only by a light proceeding from different ſides, though with various degrees of ſtrength. Beſides, it may ſometimes be the caſe that the light may be higher, nearer, or at a greater diſtance in one diſtrict than another; and therefore it is difficult, under all theſe circumſtances, to form a right judgment of an object in the ſun.

The ſun, therefore, muſt be incloſed in a luminous covering, or ſurrounded by an atmoſphere of light—an idea firſt ſtarted by Mr. Bode; but the peculiar quality of it remains to be more fully inveſtigated. A globular covering of boiling light ſeems to be too moveable to produce the above phænomena. The irregular ſpreading of the ſurrounding flame would every moment change the ſcene; as a mixture of luminous matter with the ſolar atmoſphere would give it an obſcure appearance, and be contrary to the ſmoothneſs of that line by which the ſolar diſk is bounded.

The ſun may be conſidered as an opake body of itſelf, and ſurrounded by an atmoſphere almoſt ſimilar to ours; but the luminous matter as an uncommonly fine fluid, which, like liquid naphtha, floats on this atmoſphere, and, on account of its great lightneſs, can never ſink down under any circumſtances whatever.

The atmoſphere of the ſun, by his rotation, being raiſed higher under the equator, will be depreſſed towards the polar regions. The luminous matter is attracted from the celeſtial expanſe; and this hypotheſis, which has been adopted by many, appears to be the true one, becauſe the phænomena on the ſurface of the ſun can be beſt explained by it. It muſt however be premiſed, that no more luminous matter is attracted than may be neceſſary to form almoſt a total covering to the ſun; and that, on account of the continual influx towards the poles, it muſt often happen that the quantity there will be diminiſhed. This is a conſequence of the thing itſelf; for, if it be admitted that the ſeparation of the luminous matter is performed by a chemical affinity, the effect of which is facilitated by the ſun's rotation, the attraction will ceaſe when the ſolar atmoſphere has imbibed a ſufficient quantity.

quantity. By obferving the fun, we perceive not only the luminous matter, but the light reflected, in a lively manner, from the fun's body; and at fuch a diftance they cannot be diftinguifhed from each other. Through a deception of our fenfes, we often take the effect for the caufe, and feek for the object in the light, and then again for the light in the object.

But as the atmofphere, greatly expanded at the equator, muft fink down fomewhere from well known caufes, the luminous matter flies off on all fides towards the lower parts, and illuminates the parts it left, no longer from above, but laterally. Here we difcover the folar faculæ; and if in thofe parts there be inequalities, there arife then half fhadows, which are almoft loft in the tint of the ground. If the obferver fails in his judgment of the angle of inflection, he fees only a mixture of faint fpots, and no landfcapes.

It will now be eafier to explain the proper fpots in the fun, as they are called. They are parts of the fun lying under full fhadows, which muft naturally arife when the illuminating fluid draws itfelf fo far back from the horizon of that neighbourhood, that no more rays can reach its centre. The contraft with the reft of the dazzling difk of the fun increafes their blacknefs, and gives them the appearance of a burnt coal, or cinder. We fhall be readily convinced that fome parts of the fun's furface may lie in a full fhadow, when we reflect on the great fize of thefe fpots, and confider the height of the luminous fluid to be fmall, or equal to about the diftance of our higheft clouds from the earth. It cannot be higher, elfe no perceptible half fhadows would be occafioned, becaufe the fhadows of bodies become fmaller in proportion to the height of the illuminating point. The irregularity of the fpots arifes partly from the different fituations of the luminous matter, and partly alfo from the inequality of the bottom. That remarkable vapour by which they are furrounded is occafioned by a kind of crepufculum, by which the fide light is conveyed to the moft diftant part from the centre of the fhadow. As the luminous matter now appears to be more fparingly divided in the folar atmofphere, a place may be totally deprived of light by its retiring: but near the boundary of the light it is directed towards the bottom, though fomewhat weakened by the refraction and reflection of the rays, as is the cafe in our morning and evening crepufcula; and the fpots by thefe means obtain their conical form. If we confider this vapour more attentively, the before-mentioned landfcapes can be clearly diftinguifhed according to the above method; and if the full fhadows have confiderable considerable inequalities, there arise forms of all kinds which exhibit no determined figure, until we become no more acquainted with the situation of the shadows. On the summits of such eminences may be clearly seen rows of luminous points, which, however, arise from a reflected light; but, on account of their liveliness, are taken for the luminous matter itself. They seem to be, as it were, separated portions of the solar atmosphere, which float about within the conical cavities. But on closer examination this deception vanishes.

Lastly, should we suppose an atmosphere filled with clouds, enlightened from above by the luminous matter, the great blackness of the spots, and the matter which surrounds them, would oppose such an idea. Such a continual phænomenon is founded on invariable causes, and cannot be reconciled with the changeable nature of clouds. A real solar landscape may be recognized to be the same before seen, even after some time, when the direction of the light which falls upon it has changed.

It is more than conjecture that the sun separates the luminous matter from the firmament, and partly appropriates it to itself and partly transmits it to other bodies, which, on account of their less magnitude, are unable to effect the same. As so many stars shine with their own light, it is probable that they all derive it from the same source; and that the immense space of the firmament contains this all-enlivening matter, which nature has dealt out with a sparing hand, and seems to have destined for the most important purposes.

The phænomenon of the zodiacal light may be very naturally explained according to these ideas. The separation of the light takes place more at the sun; it floats on its atmosphere in exceedingly small quantity; and, as it streams out immediately in all directions, the terrestrial bodies are secured from immoderate heat by this tendency to diverge. The attraction of the luminous matter from the firmament must extend to a considerable distance; for that set free at the sun is supplied by another quantity, which seeks for an equilibrium, and therefore forces itself towards the solar equator. It accumulates therefore in those parts; and, as it is half separated, on account of the attraction of the sun acting at that distance, this mean state produces the paleness of the zodiacal light. This conjecture seems to be confirmed by a remark of Cassini. At a time when the solar spots were invisible, no zodiacal light was to be seen. The absence of the spots shows a superabundance of the luminous matter on the sun; consequently none is separated, and these phænomena must cease. Were their seat in the solar atmosphere, they ſhow themſelves more ſtrongly the more widely the light is diſperſed over the body of the ſun.

Poſterity, perhaps, will be able to deduce the cauſe of the northern lights and other phænomena from theſe ſuggeſtions reſpecting the production of the luminous matter, and to anſwer the important queſtion—" By what circulation is that noble matter, which diffuſes light and heat throughout all nature, again conveyed back to infinite ſpace; or whether deſtined and employed for the formation and maintenance of organic bodies?"

VIII. *Experiments on Platina.* *By* L. Proust *.

I. *Of the foreign Bodies mixed with native or crude Platina.*

THE bodies generally mixed with platina are ſtony and metallic ſand, pyrites, gold, and mercury. The firſt kind of ſand conſiſts of ſmall rock cryſtals and fragments of a hyacinth colour; the ſecond conſiſts of ferruginous ſand of two ſorts: the one octaedral, and ſuſceptible of being attracted by the magnet, among which there are ſome that do not poſſeſs that property till after being heated: the other ſort is equally black and angular; but the angles are deſtroyed in ſuch a manner, that it is not poſſible to determine their figure, which ſeems to announce that they are not ſo hard as the preceding. The latter are not ſuſceptible of being attracted by the magnet, even after they are heated, and when analyſed they exhibit new ſubſtances combined with iron; phænomena by which we are authorized to conſider them in their union as a mineralization of a peculiar kind, as will be ſeen hereafter.

II. *On the Method of ſeparating the Sand from the native or crude Platina.*

Place three or four pounds of crude platina on a large table the edges of which are ſomewhat raiſed, and at the diſtance of a foot from one of the ſides; then ſpread it out, by means of a card or a rule, in ſuch a manner as to occupy the ſpace of about two feet, and blow upon it obliquely with a pair of hand bellows, to make the ſand, which is always lighter than the gold or platina, paſs to the oppoſite ſide. If this operation be repeated five or ſix times, taking care to

* From *Annales de Hiſtoria Natural*, Madrid 1799, No. 1.

blow

blow in different directions, the platina may be rendered ſo pure that, when examined with a magnifying glaſs, a grain of ſand will not be found in a pound of it.

III. *Of the Gold.*

All the gold contained in crude platina is ſeldom diſcovered, even with the help of a magnifying glaſs; and as mercury is the means employed to ſeparate them, it is evident that, if a few grains of gold remain, they muſt retain their portion of the mercury. Hence the gold acquires a white hue, which aſſimilates it to the colour of the platina, and prevents it from being perceived.

I do not know whether the workmen at the platina mines are acquainted with any certain method of aſcertaining whether the ſeparation be complete. A very ſimple proceſs is, to heat the platina in an iron ſpoon or crucible. The mercury becomes volatilized, the gold reſumes its colour, and the platina acquires that of calcined iron. The gold then, on account of its colour, may be very eaſily diſtinguiſhed from the platina.

In this manner I ſeparated 7 ounces of gold grains, or ſcales, from 100 ounces of ſome kinds of crude platina, and 13 from others. The mean product is 10 per cent., as I announced at the time to government.

Theſe ſcales are of different colours: ſome have the yellow colour of gold, others are of a pale or a green yellow. The laſt, tried with the blowpipe and borax, fuſe themſelves, tingeing the borax green, as iron would do; which ſhows that it is a green native gold, like that of the jewellers.

The nitric acid alſo, digeſted cold over theſe kinds of crude platina, ſeparates the mercury from the gold, but not ſo completely as heat.

Crude platina of a black colour, finer than the white, is very commonly brought from America. It ſtains the fingers, probably on account of the ſuperabundance of graphite or plombago which it contains. I never found gold in this kind, and I have not been able to obtain any information reſpecting its origin.

IV. *Of Sulphur as the Mineralizer of Platina.*

When crude platina is expoſed to the heat of the blowpipe on charcoal, a ſtrong ſmell of ſulphur is exhaled, accompanied with fumes which do not whiten gold, and which are not produced but by a degree of heat ſuperior to that which volatilizes mercury. Black platina grains give more fumes than the white.

Large grains of platina picked and heated, emit alſo an odour of ſulphur accompanied with fumes. A perſon employed by government to collect platina informed me, that the largeſt grains he ever ſaw never exceeded the ſize of a pea; and that he had given ſeveral of that ſize to the archbiſhop of Santa-Fe. In regard to the enormous grain ſaid to be depoſited in the cabinet of Vergara, the director of the Biſcayan ſociety aſſured me that he had no knowledge of it.

When crude platina is brought to a red heat in a covered crucible, no odour of ſulphur is perceived with the vapour of the charcoal, but at the moment when the crucible is uncovered this odour manifeſts itſelf very powerfully; but if the crucible be ſhut cloſe it ceaſes. It was long before I was able to diſcover the nature of this vapour; but the experiments, which will be deſcribed hereafter, proved to me that it is nothing elſe than concentrated ſulphuric acid.

From 1600 grains of white crude platina well purified, and heated in a retort, I obtained only half a grain of mercury and a grain and a half of water, without any kind of gas. Having broken the retort before it was quite cold, it exhaled a ſulphurous odour. The platina became black, and was found ſlightly cemented, and adhering to the glaſs.

A mixture of 400 grains of the ſame crude platina and an equal quantity of ſweet muriate of mercury, heated in a retort, gave, after the ſublimation of the mercury, a very light ſtratum of cinnabar, which adhered to the top of the retort, and, when ſcraped a little, aſſumed a vermilion colour.

Crude platina, heated in a crucible and immediately pour'd into a ſilver diſh, blackens the veſſel in the ſame manner as ſulphur. In a word, all ſolutions of platina form an abundant precipitate with barytes.

As ſulphurets of platina, both native and artificial do not ſuffer their ſulphur to eſcape when heated in covered veſſels, there is reaſon to believe that this metal cannot be entirely freed from it by an open fire; for the ſulphuret which occupies the centre of each grain, may then be conſidered as ſhut up in a veſſel.

Here, then, we have ſulphur as a conſtituent part of crude platina. It may here be aſked, with what combuſtible this ſulphur is united. If it be recollected that iron, and even copper, exiſts alſo in this ore, it will not be ſuppoſed that it is combined with the platina; and yet it is with this metal that the ſulphur is united, as will be ſeen hereafter. In the mean time it may be deduced, from the preceding experiments, that what is at preſent called, and what ought to be continued to be called, *crude platina*, is evidently nothing elſe

elſe than the mineralization or ſulphuret of that metal which it has been thought proper, in the modern nomenclature of metallic ſubſtances, to call *platina*.

V. *On the Oxidation of crude Platina.*

When native platina is brought to a red heat in open veſſels, it aſſumes the appearance of the oxidated iron of a gun barrel; but with this difference, that the oxidation effected at the ſurface is found at its *maximum*; for by rubbing it on paper, or triturating it with water in a porcelain mortar, a red powder is detached from it. Another proof of this is obtained by digeſting over it muriatic acid, which entirely diſſolves this powder, and with alkalies gives a precipitate of oxidated iron of 48 per cent. The platina which remains has recovered its whiteneſs.

I ſhall here give an account of an experiment made a good many years ago by C. Darcet, and at which I was preſent. That chemiſt put into a crucible, placed in the porcelain furnace of Serres, ſome ounces of crude platina: when taken from the furnace the platina was triturated with water, and ſeparated by theſe means from all the powder it contained. After this firſt ſubſtance was removed from it, it was put again into the furnace, and purified a ſecond time in the ſame manner; and this operation, being repeated, gave a new quantity of powder. After paſſing through the furnace in this manner, as far as I can remember, ſeven or eight times, the platina was totally changed into an oxide of a very dark brown colour. I was prevented, by particular circumſtances, from learning of Darcet what was the reſult of his laſt experiments.

An account of experiments made on platina in the porcelain furnace of Furſtemburg may be ſeen in the *Journal de Phyſique*; but as the author did not take care to remove the oxide as it was formed, he concluded, as Macquer, Baumé, and other chemiſts had done, that platina was indeſtructible. But let us return to the oxidation in queſtion.

The muriatic acid, of which we have ſpoken, had taken from the calcined platina three oxides, viz. that of iron, that of copper, and that of platina. This ſolution, united with the ſolution of muriate of ammonia, gave the yellow precipitate which is peculiar to platina.

Potaſh being employed to complete the precipitation from the liquor, the reſult was a ſecond precipitate of red oxide of iron, mixed with a little copper, which immediately manifeſted itſelf by muriate of ammonia.

If in this ſolution there had been found only a little oxide, which the muriatic acid might have ſur-oxygenated, the ſolution

lution of platina would have exhibited nothing aſtoniſhing. But as its deoxidation could not be aſcribed to ſuch a cauſe, it muſt be inferred, that when platina is combined with other metals, it becomes oxidated much eaſier than has hitherto been ſuppoſed. Platina, then, has this property, like all other metals, in which the ſtate of combination always favours oxidation.

The following is the detail of an experiment made with great care. Eight hundred grains of crude white platina, well purified from gold, ſand, and mercury, after having been calcined for the ſpace of an hour, had loſt only one grain. Having afterwards expoſed them to the action of muriatic acid, they loſt in the acid four grains and a half, and reſumed their whiteneſs; by which it is ſeen that the oxygen of the atmoſphere had almoſt replaced the weight of the ſulphur diſſipated.

The 794½ grains, being calcined a ſecond time, became black, and their weight was increaſed two grains, but the ſmell of ſulphur was no longer diſtinguiſhed; a circumſtance which renders the combination of oxygen more ſtriking. Having been again expoſed to the muriatic acid, they loſt 9½ of their weight: they were found to be black, and viſibly corroded.

The 788 grains which remained were boiled a third time in the ſame acid, and the reſult was a new loſs; but that which they experienced in freſh acid, after a fourth ebullition, was ſlow, and far inferior in the two laſt mentioned boilings: it did not exceed four grains and a half.

The weakened action of the acid made me believe that I ought to continue the oxidation in order to penetrate further into the interior of the platina, which induced me to proceed to a fourth calcination; but it was uſeleſs, for the grains experienced no change either in their weight or their colour: the application, therefore, of the muriatic acid was to no purpoſe. The exterior of the grains had then the appearance of pure platina, having a very ſtrong reſemblance to ſilver of a low alloy.

The total loſs ſuſtained by theſe 800 grains was 17½ grains, or a little more than two hundredths. A grain of gold, not perceived in purifying the crude platina, was found towards the end of the operations; and this grain proved to me that the muriatic acid alone produced the effects I have deſcribed, and that nitric acid had no ſhare in them.

The metallic ſolutions of which we have ſpoken contained each platina, iron, and copper: they became turbid with the muriate of ammonia; from which we may infer, that if the platina

platina be diſſolved in the muriatic acid, that it may be precipitated by that ſalt.

Let us now return to the crude platina. When treated with dulcified muriate of mercury, it is found at the bottom of the retort with all the ſigns of corroſion. The water immediately takes from it a light efflorescence of a yellowiſh white colour; and potaſh, poured into the liquor, ſeparates from it a red oxide of iron and of copper. In regard to platina, none is found in it; from which we may conclude that the iron and copper only had received from the muriate of mercury a ſufficient quantity of oxygen to effect their ſolution. The loſs of the ore was 12 per cent. We are therefore obliged to acknowledge three metals in platina ore. We ſhall ſhow hereafter, that beſides ſulphur it contains alſo two other combuſtibles *.

VI. *Experiments with the Nitric Acid on Platina.*

Nitric acid of 35 degrees, in which crude platina, purified from ſand, iron, and copper, was boiled, did not even alter the poliſh of the grains: it only ſeparated the mercury from the gold. The ſame acid had as little action on the platina grains blackened by calcination, and ſubjected to the ſame ebullition: they retained their cruſt, and the ſtate of the acid was in no manner changed. Margraf and Lewis, however, aſſert that nitric acid has ſome kind of an action on them. But their acid either contained muriatic acid, or the grains on which they operated were more ferruginous. This is very poſſible; for there are ſome kinds of crude platina which have a great many incruſted grains, and others cavernous, which contain red oxide. The black coloured crude platina in my poſſeſſion, and which belonged to Don Pedro d'Avila, is in that ſtate. I can now entertain no doubt that black kinds of platina are ſometimes brought to us. That ſent a few years ago to the king's cabinet, of which I have a ſpecimen given me by our tranſlator of Buffon, is of this kind. It may be whitened by means of the muriatic acid, but not

* The author of the Mineralogy of Spain, Don Chriſtian Herrgen, expreſſes himſelf thus in the ſeventh number of the *Annales de Ciencias Naturales*: " After having read the memoir publiſhed by L. Prouſt in the firſt volume of this collection, one is obliged to acknowledge that platina is never found pure in a native ſtate; it always exhibits a real mineralization, as this memoir proves. It is much to be regretted that we have not yet the leaſt geognoſtic notice reſpecting this valuable ſubſtance. Don Herrgen indicates at the ſame time the places where platina is found, which are, *Choco*, *Popayan*, *Quito*, *Rio del Pinto* in the province of Peru, *Carthagena*, and *Barbuda* in New Grenada.

 ſufficiently

fufficiently to acquire the brilliancy of the white kind ufed in commerce.

VII. *Of the Effects of the Nitro-muriatic Acid on Platina.*

I compofed this acid with the nitric and muriatic acids. I fhall mention hereafter the proportions beft calculated to effect this folution.

Four hundred grains of crude platina well picked, which had been tranfmitted to me by Don Cafimir Ortega, being treated in this acid until it had loft all its action, there remained a black matter, which, when wafhed, and collected together in a china cup, was found to weigh 10½ grains, or 2 pounds 10 ounces per cent.

The fame quantity (400 grains) of crude white platina, which I received from Don Domingo Fernandez, left fix grains of a black refiduum, or one and a half per cent.

An equal quantity of the fame kind, of which the largeft particles were felected, left 14 grains, or 3½ per cent., of a black refiduum. Thefe three experiments give a mean product of about 2½ per cent.

VIII. *Of the crude Black Platina.*

Four hundred grains of the platina of Don Pedro d'Avila left a refiduum of 7 grains, or 1 pound 12 ounces per cent.

Four hundred grains of the fame left a black refiduum of 9 grains, or 2 pounds and a quarter per cent.

An equal quantity of the largeft particles of the fame left a refiduum of 3 per cent. Thus the mean product of the black kind was 2 pounds 12 ounces per cent.

It is here feen that there is a very remarkable variation in the quantity of thefe refiduums: it arifes, firft, from the very variable action of the folvent, which more or lefs deftroys the fubftance of the refiduum; and, in the fecond place, from there being fometimes found in thefe refiduums particles of that metallic fand of which we have fpoken, and which are generally pretty large, and imbedded, as it were, in the heart of the platina. This fact has been obferved alfo by Guyton.

IX. *Of the Graphite of Platina.*

The refiduums of which we have fpoken are nothing elfe than graphite or plumbago: it burns flowly by the blowpipe, inflames nitre, and deprives the arfenic acid of its oxygen; it leaves fhining or gloffy traces on paper; when expofed to heat, it lofes neither its foftnefs to the touch, nor its property

property of ſtaining: in a word, it has as much luſtre as the beſt Engliſh pencils *.

That the aſſay by this method may be exact, the graphite, when heated by the blowpipe, ought to exhale no ſulphurous odour; for, if this odour is perceived, it indicates that it ſtill contains metal not diſſolved. I muſt however obſerve, that during the ſolution the platina appears to be reduced to a black powder; or, rather, a powder is detached which reſembles galena coarſely pulverized.

This powder, which is as heavy as platina, accumulates at the bottom of the retort: after the ſolution of the grains of the metal, it ſtill retains for a long time the ſame appearance; and it is only by renewing the nitro-muriatic acid that it diſappears entirely, leaving nothing in its place but graphite. The nature of this powder will be ſeen hereafter.

X. *Obſervations on Solutions of Platina.*

To free theſe ſolutions from the nitric acid employed in exceſs to oxygenate the muriatic acid, they muſt be concentrated till they acquire the conſiſtence of honey. The ſuperabundant muriatic acid, becoming oxygenated in its turn at the expenſe of the remaining nitric acid, acquires the quality of oxygenated muriatic gas combined with nitrous gas.

The ſaline maſs which remains is of a dark brown colour: it diſſolves in warm water if the deſiccation has not been too ſtrong; for in that caſe the part which reſiſts ſolution is a brown muriate containing leſs acid, in which platina, iron and copper are re-found, and from which the muriatic acid cannot be ſeparated even by ſeveral waſhings. In regard to the liquid ſolution, it is merely a muriate; for, when decompoſed by means of potaſh, nothing is produced but muriate of potaſh and a little ſulphate. It has an aſtringent taſte, but without any of thoſe metallic ſavours which are ſo diſagreeable in all ſolutions of ſilver, copper, &c.

If the ſolution in this ſtate be treated with potaſh, the ſaline precipitates common to all ſolutions of platina will be obtained, but not an atom of nitrate will be obtained by evaporation; from which it may be inferred, that the nitric acid of ſolutions not concentrated has no part in precipitations of this kind. It will be ſeen, in the courſe of this paper, that the ſlighteſt veſtige of this acid is not found in the pre-

* On fuſing crude platina with oxygen gas, a black ſhining ſcale forms on the outſide of the globule, which may be ſeparated by the hammer. This is frequently found to ſoil the fingers.—EDIT.

cipitates of the moſt nitric ſolutions. In a word, the reſult of ſolutions of platina in nitro-muriatic acid is, as in that of gold, a pure and ſimple muriate of platina. To what has been ſaid I ſhall add, that the difference between one kind of crude platina and another may be known by that obſerved in the colour of their ſolutions. Black platina grains, for example, diſſolved in the ſame quantity of acid as the white, give a colour much redder.

XI. *Solution of crude Platina on a large Scale.*

The phænomena obſerved in the ſolution of this mineral, when large quantities are employed, do not differ from thoſe effected by ſmall quantities. In the firſt of theſe operations, however, there are ſome effects more ſtriking, which I ſhall here point out.

When the nitro-muriatic acid, aided by a moderate heat, begins to attack platina, the firſt bubbles of gas become covered with a yellow powder, which retains them at the ſurface of the liquor. Theſe globules float in every direction, and at length adhere to the ſides of the retort, or vaniſh, to give place to others. Theſe phænomena being common to many ſulphurous metals, we are authorized to believe that this ariſes from the ſulphur which envelops theſe bubbles of gas. This powder, when thus depoſited on the ſides of the retort, is ſoon diſſipated; becauſe, being very minute, and in ſmall quantity, it is ſpeedily ſubjected to the action of agents which tend to convert it into ſulphuric acid.

But to return to the black powder: in proportion as the ſolution advances, this powder is ſeen forming itſelf, increaſing, and aſſuming, with the remainder of the platina, a leaden colour, ſimilar, as already ſaid, to that of pulverized galena. It is then proper to lay aſide this reſiduum, on which the ſolvent has only a very weak action, which cannot be continued but by repeated ebullition, and adding a large quantity of new acids. This ſolution, when poured on a new quantity of mineral, acts with a great deal of force; which proves that this reſiduum, though it ſtill holds abundance of metal, reſiſts the action of the ſolvent much more than freſh grains.

This powder has been remarked, but not examined, by thoſe who have operated on platina. To obtain it ſeparately, the reſiduum muſt be waſhed in a great deal of water; it muſt be agitated a great deal, and decanted very ſpeedily, becauſe its gravity is very little different from the mineral which remains to be diſſolved. What has not been carried off by decantation, when moiſtened, and rubbed between the fingers,

fingers, breaks, and ſtill gives a pretty large quantity of black powder. It is therefore neceſſary to again take from the bottom of the water the mineral found there; to rub it ſtrongly between the hands; to ſeparate the powder from it; and to unite it to the former. It is evident, from what has been here ſaid, that the friction and ſhocks experienced by the mineral, agitated by ebullition, and corroded by the acids, effect the ſeparation of the black powder.

If theſe grains, inſtead of being pounded, are left whole, and if they are examined with a magnifying glaſs, it is ſeen that they have retained their form, that they are eaſily broken, and that they are hollow, and covered with brilliant laminæ. The aperture through which the ſolvent has penetrated into the interior is often diſcovered; and as their colour is in nothing different from that of the powder, there can be no doubt that the latter is the reſiduum of the eroſion of the grains of the platina. In a word, theſe phænomena are more remarkable in black platina ore than in white, becauſe the principle which produces this powder appears to me to be more abundant in the former.

XII. *Experiments on the Black Powder.*

Two hundred grains of this powder, well preſſed together, being calcined in a wide crucible, exhaled ſulphuric acid fumes. Towards the end of the calcination I thought I perceived that the fumes, though ſtill acid, had no longer a ſulphurous character. The reſiduum was pretty black, and began to be agglutinated together: it had loſt 23 parts. The 177 which remained were diſſolved in nitro-muriatic acid; and the ſolution being precipitated by muriate of ammonia, the reſult was an ammoniacal muriate ſimilar to others, of which I ſhall ſpeak hereafter. The remaining liquor was found much leſs coloured than uſual. I treated it with potaſh, which, to my great aſtoniſhment, precipitated a white phoſphate of iron. I call it white, becauſe it is that which all iron oxidated to its *maximum* produces. We have here, therefore, on the one hand ſulphur, and on the other phoſphorus, combined in the reſiduum of our crude platina in greater quantity than in the metal itſelf.

As the remainder of the black powder weighed no more than 42 grains, it is evident that the acid had diſſolved 135 of the 177 grains of black powder calcined.

Theſe 42 grains of powder were ſtill very black; they no longer emitted any odour by the flame of the blowpipe. They were not attacked by the nitro-muriatic acid. They were nothing elſe but plumbago or graphite very pure. To

 aſcertain

ascertain with more certainty the nature of this powder, I thought proper to make the following experiment.

I dissolved 200 grains of black powder well dried, but not calcined, in nitro-muriatic acid of the same strength. Having washed and weighed the residuum, it indicated that the solvent had taken up only 24 grains. The solution being then treated with muriate of ammonia gave me common muriate of ammonia; and the last liquor produced, by the addition of potash, phosphate of iron, which gave me no new information.

But, if it be recollected that 200 grains of the black powder, which had lost 23 grains of sulphur and phosphorus, afterwards suffered to be taken up by the same solvent even 135 grains of platina, one will certainly be astonished to find that 200 grains of the same powder, which still retained its 23 grains of combustible matter, should have resisted the action of the solvent to such a degree as to give up to it only 24 grains of metal. This resistance must necessarily find its measure in the difference between the numbers 135 and 24, which here represent to us the quantities taken from the black powder during the first and second experiments.

I then suspected that the union of the combustibles accumulated in the platina, formed, perhaps, the greatest obstacle to the solution, both in these experiments and in those made on platina on a large scale. Several facts, which I had already observed in regard to the solution of metallic sulphurets, compared with that of their respective metals, confirmed me in this opinion, at least until other observations had enabled me to rectify what might not be founded on that conjecture.

I therefore collected the powder, after the small loss it had experienced in the nitro-muriatic acid, and exposed it to the flame of the blowpipe. At the same instant it exhaled fumes of sulphur, and perhaps even of sulphuric acid, as dense as that which it had produced before being subjected to the action of the solvent. If we suppose, then, that the black powder gives up to the solvent 135 parts instead of 24, at the moment when it is separated from the combustibles by torrefaction, there remains no doubt that these combustibles, on the one hand, oppose a strong resistance to the action of acids, and defend the platina from it; and, on the other, that œconomy requires, in operations on a large scale, that the black powder should be preserved separate, to be calcined before it is treated with nitro-muriatic acid.

If we reflect that the sulphurets and phosphurets of iron, which are the only ores found in small quantity in the mineral, are incapable of opposing such a resistance to the action of the nitro-muriatic acid, it will no longer be doubted that it is chiefly owing to the ſulphurated phoſphorized platina.

In ſupport of my conjecture, I ſhall ſay a few words reſpecting the ſulphurets of which I have ſpoken.

The ſulphuret of mercury, or cinnabar, contains the metal in its natural ſtate, and not, as is commonly ſaid, in that of oxide; for, notwithſtanding the diſpoſition of mercury to unite with oxygen, concentrated nitric acid does not attack cinnabar.

Copper takes up 28 parts in 100 of ſulphur, and never leſs: it can, however, diſſolve ſulphuret of copper in various proportions, as may be ſeen in my eſſays on black copper. If copper containing ſulphuret be expoſed to nitrous acid of 12 or 15 degrees *, the copper will be diſſolved; and ſulphuret, which will ſeparate itſelf under the form of a blackiſh blue powder, cannot be oxidated but in a very ſtrong heat.

I applied ſeveral times an acid of 35 degrees † to American ſilver ore containing ſulphurets of copper, lead, iron, antimony, zinc, mangaueſe, &c., and found that, if theſe mixtures were not heated, the greater part of the metals were attacked by the acid, the whole, or at leaſt the greater part, of the ſilver in the ſtate of ſulphuret excepted; from which we may conclude that this ſulphuret is one of thoſe which reſiſt the action of the nitric acid much more than the metal itſelf. It is very probable that this is the caſe with many others, which I do not at preſent recollect.

[To be continued.]

IX. *On Bleaching. By a Correſpondent.*

To the Editor of the Philoſophical Magazine.

SIR,

THE importance of the new mode of bleaching to the principal manufactures of this country renders it worthy the moſt minute and accurate attention. Before your publication of O'Reilly's Eſſay, no complete treatiſe on the modern ſyſtem had appeared. That of Berthollet, the diſcoverer, though in many particulars very defective, was undoubtedly the beſt adapted to inſtruct the practical manufacturer, who cannot be ſuppoſed intimate with the minutiæ of the laboratory. The in-

* If theſe are degrees of Baumé's areometer, as may be preſumed, they indicate a ſpecific gravity of from 1·089 to 1·114.

† Specific gravity of 1·314.

definite essay of Higgins presupposes too much previous knowledge in the reader, ever to enable the inexperienced to adapt or establish the new process; and the many other detached essays *, however valuable on particular parts, are yet far from forming a *whole*, whence the practical bleacher might gain instruction to adapt or improve this new system. The present essay not only presents a complete view of all that has hitherto been published, but adds many original facts to the illustration of the *modus operandi*, or theory and practice of bleaching. It is therefore with no inconsiderable deference that I venture to offer some remarks on the work of so experienced a chemist.

That there should still be such a diversity in the quantity of the ingredients for preparing the oxymuriatous acid may appear strange, when its nature and properties are so well understood. This may be accounted for only by the peculiar modes of preparing it, for the end of all is the same. The first attempts to prepare this acid fit for bleaching often fail, and the cause of such failures, owing, perhaps, only to the want of manual dexterity, is generally ascribed to a deficiency in the quantity of some of the ingredients. To remedy this supposed deficiency, an unnecessary quantity of some one article is added: hence the German practice of using 64 parts salt to 20 manganese, when most probably the converse quantities, properly managed, would have much sooner produced the desired effect. The use of salt in this process has, no doubt, hitherto been greatly over-rated. It does not appear that muriatic acid alone possesses any bleaching powers, but that of depositing a pale substantive yellow upon every other colour. To oxygen, of which it is only the vehicle, is justly ascribed the detersive property of oxymuriatous acid; and stuffs are whitened with greater or less celerity, according to their greater or less facility of combining with oxygen. The colouring matter undergoes a real slow combustion, which terminates by the formation of carbonic acid, which, escaping under the form of elastic fluid, produces what we call *bleaching*. Granting that combustion actually takes place, (in which there is no less of theory than experiment,) and the oxygen, uniting with the carbon, escapes in the state of carbonic gas, the muriatic radical and base of the colouring matter must unite and form a concrete substance; the residuary product of combustion, which literally dyes a leaden white, and which, like leaden paint, on exposure to the atmosphere, shortly assumes a dirty yellow colour. That this is really the case can scarcely be doubted; and that all the

* It is possible some of these may have escaped the writer's attention.

attempts

attempts hitherto have done no more than dyeing a leaden white, which soon becomes a pale yellow, that requires the joint efforts of oxygen and alkalies to remove, must, on a moment's reflection, be evident to every one. To this cause may be ascribed the failure of the learned editors of the Encyclopædia Britannica, who were no mean chemists. With the acid prepared according to Berthollet's mode they dyed yellow, but could not bleach white.

It is generally agreed, that the muriatic acid is no further useful to bleaching than by its power of retaining oxygen in water, and thus conveying it in a state fit to act, more or less, on both the external and internal parts of the stuff to be bleached. Hence it is evident, that the use of such a quantity of salt is not only injurious to the celerity of the process, but entails an embarrassing expense on this method of bleaching. However, even this more than triple quantity of salt may have been necessary to incorporate or retain the requisite quantity of oxygen to perform the whitening process, if the apparatus were not properly adapted. Condensers have been generally used, and Mr. O'Reilly proposes (Plate I. fig. 1. C.*) an *improved* one; but, if duly examined, they will be found not only inefficacious, but highly disadvantageous to the admixture of the oxygen with the water. It is well known how slight the union of oxygen with muriatic acid is; and whoever will try even this improved condenser will find an almost complete separation of the two gases before they are brought in contact with the water. So slightly indeed are they mixed, that the moment the heat and consequent motion is withdrawn from these condensers, that moment the gases begin to separate according to their specific gravities. At the top will be found tolerably pure oxygen gas, while the bottom contains muriatic mixed with some sulphuric acid gas. This last combination is considerably detersive, but is also somewhat corrosive, and has often damaged goods; a circumstance that may be avoided by having them previously wetted in water. It is less injurious, however, to cotton than linen goods; and to this is owing probably the great quantity of sulphuric acid used at Manchester, where it is diluted with double its quantity of water; which must excite such a rapid ebullition, that some of the sulphuric acid will be carried into the receiver. Sulphuric acid, diluted with *less* than its volume of water, will produce the purest oxymuriatic gas. By using condensers, much of the oxygen too is dissipated through the anti-absorbent tube (if I may so call it) at the top of the condenser. Under such circumstances it is not

* See Philosophical Magazine, vol. x.

surprising

furprifing that extraordinary quantities of muriatic acid fhould be required to retain a very fmall quantity of oxygen.

To obviate the lofs of the oxygen, and prevent the necesfity of ufing fo much falt, the alembic and great receiving vat, or vats, fhould always communicate immediately together, without the intervention of condenfers, which always tend to diminifh the capability of the muriatic acid to unite with and retain the oxygen in the water, and thus defeat the object of the procefs. Abforption might be guarded againft by having the communicating tube bent, like a double fyphon (℧), and in the lower angle to have an air-tight ftopper, that would yield on the firft preffure, fo that no water could ever reach the alembic. It ought to be remembered too, that if by neglect abforption ever takes place, the liquor will be rendered almoft totally inert, even with the beft condenfers. Various other modes of anti-abforption might be adapted equally efficient with thofe in ufe, and without their inconveniences.

When citizen Paul thinks proper to publifh his mode of impregnating water with oxygen, perhaps a ftill more fpeedy and effectual method of difcharging the colouring matter of cotton and linen may be found; and the theory or procefs of combuftion, adapted by Berthollet in his "Elements of the Art of Dyeing," and ably combated by Dr. Bancroft * in his moft ingenious "Refearches in the Philofophy of permanent Colours," be fuperfeded by the more fimple term *folution.* J. A. B.

Tokenhoufe-yard,
Sept. 9, 1801.

X. *Experiments on Dyeing Cotton with the Flowers of the Carthamus Tinctorius* (Safflower). *By Profeffor* BECKMAN †.

ABOUT feven years ago, I made various experiments on dyeing wool with the flowers of the carthamus; and two years after I added fome experiments on dyeing linen in the fame manner ‡. As thefe were approved by men well verfed in that part of chemiftry which relates to the art of dyeing,

* May it be permitted to afk this philofopher, through the medium of the Philofophical Magazine, when the world will be favoured with his promifed fecond volume of Refearches?

† From the *Tranfactions of the Royal Society at Göttingen for the Year* 1780.

‡ Novi Commentar. Societ. Göttin. iv. p. 89. and vi. p. 79.

 I refolved

I reſolved to make ſome experiments, alſo, on dyeing cotton with the flowers of the carthamus; eſpecially as no perſon, as far as I know, had ever before ſhown what effect might be produced by the beautiful red dye of this flower.

The celebrated Poerner * made, indeed, various experiments on cotton; but his only relate to the ſaffron-coloured dye of the plant; nor did he employ any alkaline ſalt to extract the red dye. Scheffer † alſo made experiments on the ſame ſubject; but my obſervations have not been anticipated by his, ſince he tells us, that the method of dyeing cotton and ſilk with theſe flowers is the ſame, though it may be eaſily proved that there is a very eſſential difference. But, before I give an account of my experiments, let me be permitted to make a few obſervations reſpecting the art of dyeing cotton in general.

No doubt can be entertained that it is more difficult to dye cotton than to dye wool or ſilk. Every thing made of wool readily imbibes the dye: ſilk imbibes it with more difficulty: but cotton rejects all dyes ſo much, that thoſe which immediately penetrate wool, and tinge it with a ſtrong colour, ſcarcely communicate any tint whatever to the latter, and even the dye which adheres to it may be eaſily diſcharged, and oft-times by water alone. For this reaſon, there is ſcarcely any colour on cotton, except that called the Turkey red, which is either agreeable to the ſight or of a durable nature. But, if we wiſh to inquire into the cauſe of this difference, we muſt attend to the nature of the two ſubſtances.

There can be no doubt that wool contains a larger quantity of fat, or inflammable matter, than cotton. We have reaſon likewiſe to conjecture, that wool, and the hair of animals, have more and larger pores, as well as more capacious fibres, than thoſe ſubſtances procured from vegetables. The great object, then, will be, to remove by art that difference which there is between animal and vegetable ſubſtances. That the ſmallneſs of the pores or fibres can be corrected by art, we have ſcarcely reaſon to hope: but we may make choice of colouring ſubſtances well diluted and ſoft, and as fluid and penetrating as poſſible, that is, rather ſaline than earthy; and may alſo prepare or macerate the cotton in ſuch liquors as are capable of rendering its fibres ſofter and looſer. Daily experience, however, ſhows that much cannot be effected in this manner; for though cotton, by means of ſalts, may be made to imbibe various dyes, yet they are eaſily

* Poerner Färberey-Verſuche, vol. iii. p. 138.

† Scheffer Chemiſche Vorleſungen überſezt von Weigel, p. 675.

washed

washed out and destroyed. Let us try, then, if a method cannot be discovered to render the nature of cotton more like to that of wool. This, indeed, will be the case, if we instil into it an oily substance so as to become a component part of it, which can neither be washed out with cold water nor soap, nor in any other manner.

But as the Turkish red possesses all those qualities which are requisite in stuffs that have been dyed, this affords a sufficient proof that cotton may be so prepared by art as to receive a constant and fixed colour. That Turkish dye, therefore, must be the pattern to which all our experiments are directed: for it stands the rays of the sun, as also the action of soap and common ley; though when often washed, and exposed to the sun, it undergoes some variation; but the colour is so far from becoming worse, that it is rather improved, and rendered brighter. If the cotton yarn, however, be digested long in a caustic ley, or rubbed in it too much, the colour is injured. Rectified spirit of wine (alcohol) does not hurt the colour of the yarn digested in it, nor does the spirit acquire any tincture from it. Spirit of sal-ammoniac, prepared by means of lime (caustic ammoniac,) is no less ineffectual. The colour is not injured by urine when undergoing decomposition; nor does microcosmic salt hurt, but rather makes the colour more agreeable, as is said; though that experiment I never tried. Mineral acids corrode the threads, but the colour itself remains. If digested for some days in nitric acid, or in *aqua-regia*, the colour becomes yellowish, and at length disappears.

These experiments, which I myself and many chemists have tried, seemed to preclude all hope of dissolving and discovering the nature of that dye used by the Turks, till J. C. Oettinger, formerly professor at Tubingen, who has deserved so well of chemistry, threw out some hints likely to lead to a knowledge of it. He, as far as I know, was the first person who had an opportunity of observing the great effect which oil of olives has upon that Turkey dye. He found, that if cotton yarns dyed with it were dipped in this oil, and then rubbed a little, they lost their colour, and that the whole colouring matter became so thoroughly dissolved, that other threads could be dyed in that oily solution; so that, by the help of oil, the dye on Turkish yarn could be transferred to other undyed cotton.

These observations afford grounds for various conjectures. The colouring matter which the Turks use, is perhaps of such a nature as to be soluble in oil in the same manner as resins; and perhaps the colouring matter adheres to vegetable

table ſubſtances by means of ſome matter diſſolved by the oil. But the conſtancy of the colour in alkaline ſalts oppoſes this conjecture; and it is more probable that the Turks may have diſcovered ſome method of aſſimilating cotton ſtuffs to thoſe made of wool, and of thus rendering them more capable than they naturally are of receiving the colouring ſubſtance.

The celebrated profeſſor Pallas proved, a few years ago, that this conjecture is not only probable, but even true. This gentleman, who, in travelling through the immenſe provinces of the Ruſſian empire, directed his attention not only to obſerving and collecting animals, plants, and minerals, but alſo to various uncommon proceſſes employed by the inhabitants, which might ſerve to improve the arts of Europe, was ſo fortunate as to acquire a knowledge of the manner in which the Armenians, who have retired to Aſtracan on account of the continual troubles in Perſia, dye cotton after the Turkiſh manner. This proceſs, which the profeſſor communicated to the public in 1776 in the Peterſburgh Journal, I ſhall not deſcribe at full length, but only relate a few circumſtances that may ſerve to confirm my opinion, and to be a guide to others in proſecuting experiments on this ſubject.

The cotton yarn is ſteeped by the Armenians in fiſh oil. It is then dried in the open air, and this proceſs is repeated for ſeven days and nights. The Armenians ſay that no fiſh oil can be uſed but that which aſſumes a milky appearance by the addition of an alkaline ſolution. I, indeed, ſuſpect that the fat of marine animals, and, in particular, fiſhes, is much ſofter and more ſubtle, and penetrates ſolid bodies much more eaſily than the fat of other animals; and it is for this reaſon, in my opinion, that the Armenians uſe only fiſh oil, which can be readily procured at Aſtracan.

The yarn which has imbibed a ſufficiency of oil is waſhed, dried, and dipped in an aſtringent liquor in which there is an abundant mixture of alum.

Cotton yarn thus prepared is dyed with the colouring matter of madder which has been mixed with calves blood. In my opinion, the dye of the madder is not only heightened by the redneſs of the blood, but, as the red part of the blood is of an oily nature, as is proved by the red globules, which, by the help of a microſcope, are ſeen floating on blood, it may facilitate the entrance of the colouring matter into the pores of the yarn, and the alkaline ſalt contained in blood acts ſtrongly on the colouring particles of the madder, which it extracts, as I have already proved by experiments with alkaline ſalts.

When

When the yarn is dyed, it is digested for 24 hours with a strong solution of soda in an earthen jar placed over a fire. By these means the colour is more strongly saturated, as the alkaline salt renders most red colours more obscure or yellow; and, if there are any remains of oil, they are entirely extracted by the solution of soda *.

What has been here said, is confirmed by the accounts given of the method of dyeing employed in the East Indies; for, though it may appear somewhat different from that followed at Astracan, yet the dyers there make use of animal oil or animal soap, which adheres to the dung of sheep and other animals †.

Though there is no doubt that the method of dyeing described by professor Pallas might be successfully imitated by our dyers, it is not my intention to make any trial on madder, but rather to apply the principles, which I have already explained, to the use of the carthamus. For this reason, I subjected to experiments a piece of cotton cloth commonly called *chintz*, which was white, and had never been worn. I washed carefully the flowers of the carthamus, that nothing of their yellow colouring matter might remain; after which I steeped them in an alkaline solution, and expressed the juice. This expressed juice, which contained all the red dye of the carthamus, and which I shall call the *alkaline liquor*, I tempered with the acid of lemon juice.

1. Having melted hog's lard, I steeped the cloth in it for two hours, and then washed it in cold water until scarcely any residuum of the fat substance was seen floating on the surface. I then steeped the cloth, after it was thoroughly dried, in the alkaline liquor, and, when it was well saturated, I threw it into lemon juice, and repeated the same process frequently. I proceeded in the like manner with chintz, which I had premacerated in water alone, and not with animal fat; but the latter imbibed much less of the colouring matter: and this experiment, therefore, proves the advantage of using fat substances.

2. In the like manner, I premacerated cotton cloth in a solution of common soap, and, after it had boiled for some hours, I dried it. This experiment also succeeded; for the

* For professor Pallas's account of the process followed at Astracan for dyeing Turkey red, see Philosophical Magazine, vol. i.

† Compare *Lettres édifiantes*, rec. xxvi. See also *Mémoires de Mathématique et de Physique présentés à l'Academ. de Paris*, tom. iv. p. 1. The process for dyeing described by Pallas is similar to that inserted in *Observations sur le Commerce et sur les Arts*, par Flachat, tom. ii. p. 405.

cloth

cloth thus prepared was much better dyed than that ſoaked in water alone.

3. But common oil of olives is much ſuperior to either hog's lard or ſoap. I boiled cloth in this oil for three hours; I ſqueezed out the oil, and waſhed the cloth in cold water till not a drop of oil ſeemed to be left in it, though the ſmell of it remained. I then dried the cloth either above a ſtove or in the open air; dipped it ſeveral times, alternately, in the alkaline liquor and lemon juice; and obſerved that there was a very eſſential difference between the cloth which had, and that which had not, imbibed the oil. The one had aſſumed a ſtronger and much better ſaturated colour; and the other one much weaker, and was, at the ſame time, covered in many places with various pale ſpots. The cloth, alſo, which had not yet imbibed oil, loſt much of its colour in drying; but the colour of the other in drying ſeemed to acquire ſtrength. I learned alſo, by experience, that the whole proceſs ſucceeds better, the oftener the cotton is thus dipped in oil and dried. It is certain, alſo, that the cloth extracts ſome colour from the oil, and therefore it is neceſſary that the latter ſhould be pure and limpid. The alkaline ſalt, by which the colouring matter of the carthamus is extracted, is of great ſervice in this proceſs; for the oil is ſo far from rejecting the watery colouring matter, which ſome might apprehend, that it rather ſtrongly attracts it, on account of the alkaline ſalt, which facilitates its entrance into the pores of the cotton.

4. As theſe experiments had ſucceeded, I endeavoured to imitate with the carthamus, as far as poſſible, the proceſs employed by the Armenians. With this view, I boiled a piece of cotton cloth ſeveral times in olive oil; then waſhed it thoroughly in cold water, and afterwards dried it. After this, I mixed water impregnated with the yellow colouring matter of the carthamus, or a yellow infuſion of the carthamus, with pounded galls and alum. I then took cloth which had been premacerated in oil, and dipped it in this ſolution, after it had boiled a little, and found that the cloth, when wrung, was of a yellow colour. Having dried the cloth, I dipped it in the cold alkaline liquor of the carthamus, and then immerſed it in lemon juice; and this being frequently repeated, the cloth appeared of a beautiful and full red. Cloth which had not been ſteeped in oil, but which in other reſpects had been expoſed to the like proceſs, was of the ſame colour, but a little paler; and, for that reaſon, I recommend this mode of dyeing to thoſe who do not chooſe to employ oil.

I ſhall now add in what manner I tried the conſtancy or durability

durability of this dye. But I ſhall ſpeak only of thoſe cloths which were premacerated in an aſtringent ſolution of alum. The cloth which had imbibed oil, and that which had not imbibed it, both ſtood alkaline ſalts; for theſe ſalts rendered the colour of both darker, without any other change. Nitric acid not diluted with water rendered the colour of both firſt brighter, then yellow, and at laſt deſtroyed it altogether. But ſtill there was a great difference: for the cloth which had been prepared with oil retained its colour much longer, and when waſhed in water, at the end of ſeven or eight minutes the colour returned, but of a violet hue; whereas cloth prepared without oil loſt its whole colour ſooner. The ſame acid diluted injured the latter, but not the former. Even the Turkey dye, prepared with madder, which is more conſtant, will not long withſtand the nitric acid; nor is it neceſſary that all our cloths ſhould be of ſuch a nature as to bear all acids. If a few drops of mineral acid fall by chance upon a piece of cloth dyed in this manner, a yellow ſpot is produced; but it entirely vaniſhes from both kinds, if the acid be immediately well waſhed out with water. It is needleſs to remark, that acid vegetable juices do this dye no hurt. It ſuſtains no injury from human urine, even if the cloth be left in it for ſeveral days. When expoſed to the rays of the ſun for ſome days, theſe cloths ſuffered no injury, though that dyed without oil ſeemed to have loſt more of its colour. But cloth which has even been prepared with oil, will not bear to be boiled with ſoap; its colour firſt becomes violet, and then totally diſappears.

Theſe few experiments ſo fully demonſtrate the great advantage of oil in dyeing cotton, that no doubt can be entertained of its utility. I muſt ſtrongly recommend it, therefore, not only to thoſe who dye cotton, but to thoſe alſo who dye linen. For, as the great power of the oil on the too fading dye of the carthamus is, that the colouring matter is ſooner abſorbed, and in greater abundance, by the cloth, and alſo retained longer by it, its power would doubtleſs be much greater upon dyes which, by their natural qualities, bear, without any aſſiſtance, ſoap, the rays of the ſun, and ſalts, by which the dye of the delicate carthamus is injured. I do not, however, imagine that I have found out the beſt method of employing oil; but I entertain a hope, that thoſe who may proſecute theſe experiments will diſcover a better. Thoſe, in my opinion, will be more ſucceſsful who can uſe fiſh oil, which, in many places, may eaſily be procured by dyers. Nor is it impoſſible that whale oil, called commonly train oil, may be ſo prepared as to be fit for the purpoſes of

 dyeing.

dyeing. Dyers, however, may reſt aſſured that the colour which they wiſh for will be more conſtant the oftener the cloths to be dyed are dipped in oil, or are boiled in pure ſubtle oil, and then dried.

XI. *Deſcription of a new Furnace for conſuming its own Smoke, and ſaving Fuel: invented by Meſſrs.* Roberton, *of Glaſgow.*

To conſtruct furnaces on ſuch a principle as ſhould enable them to conſume their own ſmoke, has long been a deſideratum; and we believe the public in general, but eſpecially thoſe who have been annoyed by the ſmoke of ſteam engines, founderies, and ſimilar erections in their neighbourhood, will be glad to learn that a furnace has been contrived which effectually gains this end.

The conſtruction is extremely ſimple, and will be eaſily underſtood by the following deſcription, and the plate to which it refers.

Fig. 1. (Plate 1.) repreſents a vertical ſection, and fig. 2. a front view of a ſteam-engine boiler, furniſhed with one of Meſſrs. Roberton's furnaces; and the ſame letters refer in both to the ſame parts of the conſtruction.

The opening A, through which the fuel is introduced into the furnace, is ſhaped ſomewhat like a hopper, and is made of caſt iron built into the brickwork H, H. From the mouth it inclines downward to the place where the fire reſts on the bottom grate B. The coals in this mouth-piece or hopper anſwer the purpoſe of a door *, and thoſe that are loweſt are by this means brought into a ſtate of ignition before they are forced into the furnace. Below the lower plate of the hopper K, *e* the furnace is provided with front bars G†, which not only ſerve to admit air among the fuel, but offer a ready way to force the fuel back, from time to time, from *c* to *d* ‡,

* In the management of this furnace, what is chiefly to be attended to is, that the hopper be kept full of coal, and either wholly or in part ſmall coal, to prevent, as much as poſſible, air getting in by that paſſage: it is alſo neceſſary at ſome times to uſe a ſhutter of thin plate-iron, to be applied to the mouth of the hopper to exclude the entrance of air by that paſſage.

† Theſe bars are, in fact, a grated door, kept in their poſition by a catch L. and which may be opened at pleaſure for cleaning the fire out. In ſmall furnaces an opening here is all that is neceſſary: the bars may be diſpenſed with.

‡ Between the back end, *d*, of the bottom bars and the breaſt brickwork *b*, is repreſented in the plate a ſection of a ſhutter, which is ſometimes opened for the purpoſe of getting out the refuſe of the fuel.

to make room for fresh quantities to fall into the furnace from the hopper or mouth-piece. By this arrangement the fuel is brought into a state of ignition before it reaches the further side of the bottom grate, where it is stopped by the rising breast, *b*, of the brick-work, so that any smoke liberated from the raw coals in the mouth-piece must pass over these burning coals before it can reach the flue FFF. But this, though it would cause a large quantity of the smoke to be burnt, would not completely prevent the escape and ascent of smoke up the chimney; for it is not merely necessary that the smoke should be exposed to a heat sufficient to ignite it before it escapes: unless, at the same time, a quantity of fresh air, able to furnish a sufficiency of oxygen for the combustion of the smoke, can be brought into contact with it, it will still escape in an undecomposed state. The judicious admission of fresh air, in such a manner that it can reach the smoke, without previously passing through the fire, and parting with its oxygen in its passage, and in such quantity as not to cool the bottom of the boiler, but merely to cause the smoke to burn, constitutes the chief merit of this invention; and to us it appears that it will fully answer the proposed end. Below the upper side of the mouth-piece or hopper, and at about the distance of 3-4ths of an inch from it, (this space being a little more or less, according to the size of the furnace,) is introduced a cast iron plate *a n*. This plate is above the fuel, and the space between it and the top of the hopper is open for the admission of a thin stream of air, which, rushing down the opening, comes first in contact with that part of the fire which is giving off the greatest part of the smoke, viz. the fuel that has been last introduced, mixes with it before it passes over the fuel in the interior, which is in a high state of combustion, and enables it to inflame so completely, that not a particle of smoke ever escapes undecomposed.

The quantity of air thus admitted to pass over the upper surface of the fire, is regulated by a very simple contrivance: The plate *a n* rests at each end on a stud, or pin, projecting from the cheeks of the mouth-piece A, or is furnished at each end with a pivot which works in the cheeks; the said pins or pivots being placed about mid-way between the outside and inside of the mouth-piece or hopper, so that, by elevating or depressing the edge *a* of the plate, the opening at *n* is enlarged or diminished. When that degree of opening which produces the best effects is obtained, which is easily known, the plate *a n* is kept in its place by means of a piece of iron introduced above it, and answering the purpose of a wedge.

Under

Under the grates is the aſh-hole I, the upper part of which is furniſhed with doors S S, which, when ſhut, prevent the heat from the front bars G from coming out into the apartment, and incommoding the workmen

Invited by an advertiſement, we went to Meſſrs. Bunnell and Silver, Bedford ſtreet, Covent Garden, to ſee one of theſe furnaces at work, and we were not a little gratified in obſerving that the ſmalleſt appearance of ſmoke could not be perceived iſſuing from the top of the chimney. The advantages of ſuch an improvement can hardly be better illuſtrated than by mentioning what had actually happened with this ſteam-engine. The ſmoke, before the improved furnace was employed, incommoded the neighbourhood ſo much, that it was ſtopped as an intolerable nuiſance. Now it is ſo far from diſturbing any one, that, without being admitted to ſee the engine, it would be actually impoſſible to know when it is at work.

Theſe furnaces, we underſtand, have alſo been adopted by many intelligent manufacturers at Leeds and at Mancheſter. At the latter place, if we may credit newspaper reports, ſeveral manufacturers have had their works indicted as nuiſances for not having adopted the improvement; the magiſtrates arguing, that, though the welfare of the place required that ſuch inconveniences ſhould be ſubmitted to while no poſſible cure for them was known, the health and comfort of the inhabitants equally demand, now that the evil can be done away, that ſmoking furnaces ſhould not be permitted in the place.

We earneſtly recommend to owners of ſteam-engines, and alſo to thoſe who are annoyed by them, to endeavour to bring this improvement into general uſe. Indeed, we entertain no doubt of its being univerſally adopted ſooner or later; for it yields advantages not only in point of cleanlineſs, comfort, and health, but alſo in point of *intereſt*—all the ſmoke uſually diſcharged at the top of the chimney, being, in fact, ſo much good fuel, that only wanted the contact of freſh air to inflame it under the boiler. It is a fact well known, that the flame which is often ſeen iſſuing from the chimneys of founders, &c. has no exiſtence except at the top of the chimney: while aſcending the flue it is only denſe ſmoke, conſiſting of the azote of the atmoſpheric air decompoſed in paſſing through the fire, of hydrogen, coal-tar, and carbonaceous matter, of ſuch a high temperature, that it only wants oxygen to make it inflame ſpontaneouſly: this it obtains from the atmoſpheric air into which it aſcends, and then preſents ſuch appearances as would make a haſty obſerver adopt the opinion that the flame had afcended, as flame, from the fuel in the furnace; which is by no means the cafe. A confideration of this fimple fact will convince any perfon that it is not an inconfiderable proportion of the fuel that is thus wafted. Nor is this the only lofs fuftained; the quantity of heat required not merely to render fuch a portion of the fuel volatile, but to give to it a temperature able to produce the effect of which we have taken notice, is itfelf furnifhed at the expenfe of an extra and unneceffary quantity of fuel! The whole wafte in many cafes is, we are perfuaded, not lefs than an eighth of the whole fuel employed.

XII. *Obfervations on the Onager of the Antients, or the real Wild Afs. By Profeffor* PALLAS *.

OUR cabinets of natural hiftory contain a multitude of animals, and other natural productions, from both the Indies, which are even fcarce in their native countries, and which, on that account, are fent as rarities to Europe: on the other hand, many fpecies common in diftricts not very remote from Europe are ftill imperfectly known, and therefore the exiftence of many of them is accounted doubtful, becaufe people do not give themfelves the trouble to examine the places where they are found. Of the truth of this obfervation, the wild afs, or onager, fo often mentioned by antient authors, is a remarkable inftance. From the filence of moft of the modern travellers in the Eaft refpecting it, one might almoft be induced to conclude that the race of this animal is become entirely extinct. An evident caufe of this is, that the Europeans have very little intercourfe with thofe diftricts from which the Romans procured wild affes for breeding mules. As our travellers are obliged, for the fake of fecurity, to go in large caravans when they pafs through the Afiatic deferts, where thefe animals refide, they can fee only a very fmall portion of the country. It is therefore not furprifing, that animals noted for their fhynefs and timidity fhould efcape their obfervation. I have been affured, in a letter from M. Niebuhr, the celebrated traveller in Arabia, that during the whole courfe of his travels he never faw a wild afs, not even in Syria, where, at the time when that country was vifited by Rauwolf †, who fpeaks of thefe animals, they muft not have been very uncommon. Befides this old naturalift, and by

* From *Neue Nordifche Beyträge*, by profeffor Pallas, vol. ii.

† Rauwolf's Reife, *Augf.* 1583, 4to. p. 65.

Pietro della Valle *, I fcarcely know one traveller in the Eaft who has fpoken of the onager from his own obfervation. The latter only mentions it in confequence of having feen one which was kept at Baffora as a curiofity; and Olearius † faw fome wild affes in a park in Perfia, where they had been collected for the purpofes of hunting. One of the oldeft European travellers, who penetrated to the deferts of Great Tartary, the monk Rubriquis ‡, firft makes mention of the Tartarian name *kulan*, under which the wild afs is at prefent known by the wandering tribes of thofe countries. In the journal of the two Englifh travellers Hogg and Thompfon, publifhed by Hanway § in his Travels, mention is made of whole herds, not only of ante'opes and wild horfes, but alfo of wild affes, which they met with in the neighbourhood of the lake Aral, and in the courfe of their travels through the diftricts lying to the eaft of the Cafpian fea.

This imperfect information is almoft the whole of what is found in the modern travellers refpecting the exiftence of the wild afs in Afia, where this animal, in the time of the Romans, was generally known, as far as Leffer Afia, Syria, and Arabia ||. Refpecting thofe in Africa our information is equally deficient; and I can adduce no other teftimony of their exiftence in that country, than what is to be found in Leo Africanus and Marmol. For thofe faid to have exifted formerly in the Canaries in great abundance, were produced from tame affes fuffered to run wild, and are now entirely deftroyed ¶. The cafe was the fame with thofe which, according

* Voyage de Pietro della Valle, *Amft.* 1766, part iii. p. 137, part vi. p. 105.

† Olearius Reife nach Perfien, *Schlefwig* 1656, p. 526.

‡ Allgemeine Gefch. der Reifen, vol. vii. b. 17. c. 2.

§ Hanway's Hiftorical Account of the Britifh Trade on the Cafpian fea, vol. i. p. 349.

|| Varro and Pliny fpeak of the onager as an animal very common in Leffer Afia. Xenophon, Suetonius, and Ammianus Marcellinus, give the fame account in regard to Mefopotamia, Perfia, and the kingdom of Parthia. Tacitus in his account of the Ifraelites fays, that during their journey through the Arabian defert under the direction of Mofes, they often followed the wild affes, by which means they were enabled to difcover fprings. The Scripture alfo fpeaks of the wild afs as an animal very common in the deferts bordering on Paleftine. None of the antient authors, however, except Oppian, has left us a precife defcription of the onager; but it clearly appears from Oppian, that under this name the antients underftood the wild afs properly fo called, which I have here defcribed; and, in my opinion, none but Philoftorgius applies the above appellation to the zebra.

¶ See on this fubject the teftimony of Aloyfius Cadamofto in the collection of Ramufio, part i. p. 98: and what Glafs fays, in his Defcription of the Canary Iflands, of the general hunting match which the inhabitants of cording to Dapper's account, were found wild formerly in ſome of the iſlands of the Archipelago, but of which, at preſent, no traces remain.

By the accounts which I have received from ſome of the wandering Aſiatic tribes, and from Ruſſians and Tartars who eſcaped from the ſlavery of theſe people, and alſo from Bucharian merchants who had travelled with the caravans, it appears certain that in the diſtricts of Great Tartary there are abundance of wild aſſes, which theſe people call *kulan*. Theſe animals proceed from the ſouth in innumerable herds, and ſpread themſelves in the mountainous naked deſerts lying to the north and eaſt of the lake Aral, where they feed during the ſummer, and in autumn unite in hundreds, and even thouſands, in order to ſeek for a warmer winter reſidence in the ſouthern diſtricts on the borders of India and Perſia. From a paſſage in Barboza's Travels * it appears that theſe emigrations extend even to the ſouthern parts of India; but it is certain that Perſia is the moſt common winter reſidence of the wild aſs, and every year they are found in the mountainous diſtricts around Caſbin. Reſpecting their emigrations, in the diſtricts of Tartary, eye-witneſſes have aſſured me that traces of large herds, from two to three hundred fathoms in breadth, may be often ſeen on the plains. But as they remain at a conſiderable diſtance from the Ruſſian frontiers, and ſeldom proceed northwards beyond the 48th degree of latitude, it was never poſſible for me, during my reſidence on the borders, notwithſtanding all my exertions, to obtain a deſcription of this animal from the Kirgiſians. I therefore requeſted profeſſor Gmelin, whom on my return to Aſtracan I found on the eaſtern coaſt of the Caſpian ſea, ready to ſet out on a ſecond tour to Perſia, to procure all the information he could reſpecting the *kulan*, and to make us at length acquainted with the nature of the aſs in its wild ſtate: but he was not able to obtain from the interpreters a real wild *kul n*. We are, however, much indebted to him for two animals of this kind, bred at Caſbin from *kulan* colts which had been caught wild, and which he brought with him from Perſia. The ingenious pupil of Gmelin, who accompanied him on his ſecond tour to Perſia, M. Hablizl, has given a careful deſcription and meaſurement of the *kulan* ſtallion which died in the courſe of the ſea voyage to Aſtra-

of the iſland of Forteventura were obliged to undertake on account of the great increaſe of wild aſſes, and during which 1500 of theſe animals were deſtroyed.

* See the account of Odoard Barboza in *Ramuſio*, part ii. p. 300. where wild aſſes are mentioned in the mountains of Malabar and Golconda.

can,

ean, together with an accurate drawing of it. The female was brought alive to Peterfburgh, and was delivered to me with the above defcription; and from thefe the prefent defcription of the wild afs or onager of the antients has been formed.

The Perfians pronounce the name of the wild afs *kurhan*, as it is written by Olearius. They call it alfo in the Turkifh dialect *dagha-ifchaaki*, or the mountain afs, becaufe it prefers living in the drieft and moft mountainous deferts. Among thefe as well as fome Tartar tribes it is an object of the chace, and hunted in various ways. The Kirgifians lie in wait for the *kulan*, and endeavour to fhoot it in order to feaft on its flefh, for the roafted flefh of this animal is one of their dainties; and indeed muft not be ill tafted, fince we learn from Pliny that the flefh of the young onager was much fought after by the Romans *. On the other hand, the Perfians rather try to catch the wild afs alive in pits carefully covered fo as to conceal them, which they dig in the ground; and, that the animal may not be injured, they fill the pits to a certain height with hay. The animals are driven towards the places where fuch pits have been made, by a company of hunters who affemble for that defign, and the young colts are fold at a high price to the principal men of the country for the purpofes of breeding. From the wild colts caught in this manner are produced thofe beautiful and active riding affes employed even in Perfia, and in Arabia and Egypt, for travelling through the deferts, and which in the latter countries are fold fometimes for 75 ducats. According to the account tranfmitted to me by M. Niebuhr, there are many of thefe riding affes which, in point of colour, have a perfect refemblance to the wild afs here defcribed. Tavernier fays that, in Perfia, beautiful faddle affes are dearer than the beft horfes, and often coft a hundred dollars †. He makes an exprefs diftinction between them and the common breed of affes, which abound in Perfia as well as in Bucharia and China, but are employed only for the draught and for carrying burdens; and from the Perfian cuftom which he mentions of painting thefe faddle affes red, which is done alfo in Egypt with the *kanna*, we may perhaps be able to explain the fabulous account given by Ælian of the red-headed onager

* *Plin. Hift. Nat.* lib. viii. c. 44:—"The beft onagers are found in Phrygia and Lycaonia. The foals, as dainties, are known under the name *lalifiones*, and come chiefly from Africa." This author fays foon after (cap. 45.) that Mecenas, at the Roman entertainments, introduced the flefh of young mules inftead of that foreign venifon.

† Voyage de Tavernier, liv. iv. c. 3.

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in India; to which he likewise adds a horn on the forehead *. Le Bruyn and Adanson † praise also these riding asses produced from wild asses, and they are extolled by all oriental travellers. They possess all the good properties of the wild race, from which they are sprung; that beautiful figure on account of which Martial gives to the onager the epithet of *pulcher*—great agility and fleetness in running. Besides, they are highly valued because on journeys through the desert countries they hold out much better than the Tartar horses, and advance with an uniform pace at a faster rate than even the camel ‡.

The she ass brought to Petersburgh, a representation of which in two positions is annexed, (see Plate II.) had not attained to her full growth, and in all probability continued so weakly because she had been caught young and badly taken care of. She, however, was able to perform the journey from Astracan to Moscow, in summer, which is more than two hundred German miles, continually trotting behind the post-waggon without resting more than two nights; and though she had suffered considerably by sometimes falling down and being dragged behind the carriage, she had sufficient strength, after a short stay at Moscow, to travel two hundred miles further to Petersburgh: when she arrived there, indeed, she was exceedingly meagre and in bad condition, and could scarcely stand on her legs. She died towards the beginning of autumn; but rather in consequence of the cold, and dampness of the climate, together with change of food and the means employed to cure an eruption, which had broken out on her skin in consequence of being overheated on the journey. Notwithstanding this malady, she recovered her strength so far as to show, in part, her former agility, and that she possessed properties very superior to common asses. But the dampness of autumn was evidently injurious to her; for when she went out into the wet fields her hoofs soon split, and at length dropped from her feet.

All the Tartar tribes consider the wild ass as one of the swiftest of animals, and unanimously assert, that the fleetest of their horses are unable to keep pace with this light and slender animal. We are told by Xenophon that the wild

* Ælian. Hist. Animal. lib. iv. c. 52.

† Le Bruyn Reize over Moskovie door Persien en Indien, p. 405: Adanson Voyage au Senegal, p. 118.

‡ Niebuhr estimates the distance which a riding ass can travel with an uniform pace in half an hour to be 1750 paces—double those of a man; whereas the largest camels can travel only 975, and the dromedary 1500 at most. See his *Reize in Arabien*, p. 311, 312.

asses

affes of Mefopotamia, when hunted, ftopped fometimes as if to take a nearer view of their purfuers, and then fprang forward again with fuch velocity, that the beft horfes could not overtake them. All the antient authors extol their fpeed in running; and their Hebrew name, *paræd*, is derived from this property. As they are fond of feeding on the cold rocky hills and mountains, nature has endowed them with the power of going with the greateft velocity over the rougheft ground and along the narroweft paths; and this property has even remained with the common fluggifh afs, and from it has been tranfmitted to the mule. The fhape of the animal feems as if deftined for this purpofe; the body being very flender, while the feet ftand near to each other, and the fmall round hoofs, which are exceedingly hard, are fharp on the edges.

As the wild afs frequents the dry fouthern diftricts, and never wanders fo far north as the wild horfe, herds of which are found under the 56th degree of latitude, the tame afs does not thrive fo well in the damp northern diftricts as the horfe; and this, together with want of care, exceffive labour, and bad nourifhment, may have occafioned the degeneration of the common afs, and brought this breed into difcredit. But who judge of the noble breed of our beft horfes from the wretched animals in the poorer diftricts, which are fcarcely fo good as affes? With proper care and management, might not as good riding affes be bred in the temperate and fouthern parts of Europe, as in the Eaft? and might not the breed of this animal be gradually improved as we have improved the horfe, the wild breed of which are far inferior in fize and beauty? In regard to the vicioufnefs of the afs, and particularly its obftinacy, which is inherited by the mule, it appears to me very probable that this is owing to the too wide conformation and fenfibility of the organs of hearing which nature has beftowed on the wild race, which, being deftined to live in extenfive deferts, have need of being able to difcover danger at a greater diftance. The noife which the afs muft hear in the neighbourhood of men evidently ftuns it; and, on that account, it is cuftomary in England to crop the ears of the affes employed at mills, becaufe it is known that by thefe means they become much more tractable and obedient. Means might eafily be difcovered of weakening their hearing, and accomplifhing the fame end, without fuch a mutilating operation.

By improving the breed of affes, which, after the example of the antients, might be done fooneft, and in the moft complete manner, by introducing riding affes from the Levant, or colts of the wild breed, the breed of mules, fo ufeful for carrying burdens, would alfo be much improved. Varro and other antient authors, who treat of agriculture and the breeding of cattle, unanimoufly affert that, in their time, the beft mules were produced from a mare and the onager, or wild afs. The Perfian mules, the ftrength and courage of which are extolled by Le Bruyn *, are in all probability produced in the fame manner. Thefe qualities, perhaps, might be obtained in a higher degree if the *dfhiggetai*, or great Mogul wild afs, which exceeds the onager in fize and beauty †, and perhaps alfo in fleetnefs, could be once tamed, and employed for this purpofe.

The wild afs, in its habits and mode of life, approaches near to the *dfhiggetai* and wild horfe. They rove about in herds, which confift of young colts of both fexes, under the protection of a ftallion. The fame account is given by Oppian and Pliny, and by thofe who have copied them. But it appears that at the periods of their migration the ftallion lays afide his jealoufy, and feveral herds then unite together. The feafon of covering is then paft, and the females are big with young; yet the ftallions, even then, are accuftomed to bite and kick each other very much.

Their hearing and fight are fo acute, and the weather is fo fine, that in the open fields it is not poffible to approach them. The Kirgifians, therefore, endeavour to conceal themfelves in the diftricts through which they pafs, or in the neighbourhood of the falt lakes, which they frequent; but the *kulans* feldom drink, and often not in the courfe of two days. The afs which I had in my poffeffion frequently abftained from drinking for two days, and in particular when a heavy dew, or light fhower of rain, had fallen. She was

* Le Bruyn fays in his Travels above quoted, p. 139, that in the fouthern parts of Perfia there are mules which, from natural inftinct, like fome fpirited horfes, and the ftallions among the wild horfes, attack bears and other ravenous animals with great courage, and knock them down with their fore-feet. The unfortunate fwine, which in thefe diftricts are black, and covered with very rough briftles, fall fometimes a facrifice to this impulfe of the mules, which are not always able to diftinguifh them from dangerous animals. A circumftance of a fimilar kind is mentioned by Varro, lib. ii. c. 8:—" I once faw the horfe of a Calmuc Tartar, while the rider was on his back, rufh into a field and kill a breeding buftard, which he, no doubt, took for fome favage animal." This kind of inftinct feems to be common alfo to the wild affes, which, according to the account given by the Kirgifians, attack wild animals in a body. When one of them obferves a fnake, it immediately gives the fignal by a loud cry; upon which all the reft affemble, and each of them tries which fhall firft deftroy the dangerous reptile. They do the fame in regard to fuch ravenous animals as they are able to overcome; but the tiger is too much for them.

† For a defcription of this animal fee Philofophical Magazine, vol. ii.

fonder

fonder of faline than of frefh water; but fhe would never touch any that was mixed with bran, or that was turbid. Bread rubbed over with falt feemed very grateful to her; and fhe would often eat whole handfuls of falt. Mr. Hablizl faid, that while he kept her at Derbent, fhe ufed always to run to the Cafpian fea to drink, though fhe could have found frefh water much nearer. Plants which contained faline particles, fuch as the different kinds of kali, or the foda plant, goofe-foot, and plantain, were moft agreeable to her; and next to thefe, bitter milky plants, fuch as dandelion, the fow-thiftle, and the like; and after thefe, the different kinds of clover, lucern, all leguminous plants, efpecially when given her with the grain, and dog's grafs. She was fond alfo of green cucumbers; and fome vegetables, fuch as different kinds of pulfe, which fhe did not relifh when green, fhe ate readily when dried. On the other hand, fhe had an averfion to all fweet-fmelling balfamic plants which are eaten by the tame afs, fuch as mudwort, ranunculus, nettles, and all hard and prickly plants, and even thiftles. In Perfia, the young *kulan* colts, when caught, are firft tamed by feeding them with rice, oats, wheat ftraw, and bread. The wild afs eats with great avidity a plant very common in the fouthern part of Tartary, and to which the Bucharians give the name of *büirogan*.

The afs I here fpeak of was exceedingly tame, and, like a dog, followed without any compulfion thofe who fed her and gave her drink. She could be made go any where merely by the fmell of bread; but if any one attempted to lead her by a halter, contrary to her inclination, fhe fhowed as much obftinacy as a mule. She would not fuffer any one to ftand behind her; and when touched with the hand or a ftick on the back or thighs fhe threw out her heels, with a loud fnorting, like that emitted by a ftallion when he kicks.

The male which died on the journey from Perfia was much larger. According to Mr. Hablizl's meafurement, his length from the ears to the rump was 4 feet 10½ inches; the height before, 4 feet 2 inches 8 lines; behind, 4 feet 6 inches 6 lines; the length of the head 2 feet; of the ears, 11½ inches; and the tail, with the bufh at the extremity, 2 feet 1½ inch. The length of the female, however, was only 3 feet 10 inches; the height before, 3 feet 4 inches 8 lines; behind, 3 feet 6 inches; length of the head, 1 foot 6 inches; the tail, 10½ inches; the bufh at the end of the tail, 8 inches 5 lines; the ears, 7 inches 5 lines; and fhe weighed, when the bowels were taken out, only about 165 pounds apothecaries weight. The male was alfo much more ftrongly made in the neck, limbs, breaft, and rump; and was diftinguifhed from the female by a ſmall ſtripe over the ſhoulders croſſing that on the back, which was wanting in the female, the latter having only the ſtripe on the back. This croſs is much more common among the tame aſſes, and is obſerved, in particular, on thoſe which are of a bright colour. The Kirgiſians ſay, that among the wild aſſes the croſs ſtripe is found ſometimes double.

The onager is much taller and finer limbed than the common aſs. The female in my poſſeſſion was ſo narrow in the cheſt and rump, that behind ſhe had the appearance of a young filly, as may be ſeen in the plate. Like young fillies, alſo, ſhe could ſcratch her head and neck with the hind foot, which a full grown horſe cannot do. On the fore-legs ſhe appeared very weak; but ſhe could carry on her back the heavieſt man, and even run with him. She always held her head erect, in a more graceful manner than the common aſs; kept her ears always upright, even during illneſs; and in all her motions ſhowed great agility.

The head of the onager is higher and larger than that of the *dſhiggetai*, and yet I found the ſkull, when cleaned, to be remarkably light. The head is much bent, like that of a ram: the fore-part between the eyes is flat; but above the eye-ſockets, which are as ſtrong as thoſe of old horſes, it is elevated in a flattiſh round form. The lips are very thick, and to the very edges cloſely covered with ſtiff hair like briſtles, which towards the round part of the mouth lie crooked. The noſtrils do not form that excreſcence-like elevation which is peculiar to the *dſhiggetai*. The pupil of the eye is yellowiſh brown. The ears at the points are quite black, covered on the inſide with curled hairs that croſs each other, which grow partly on both edges of the ear, and partly on three ridges which run along the hollow of the ear lengthwiſe.

The colour on the muzzle and the greater part of the body is a beautiful white, with a kind of ſilvery appearance; but the upper part of the head, the ſides of the neck, and the rump, are of a pale iſabella colour. This colour does not extend along the fore-thighs; it however covers the haunches; although on the ſides of the belly a white ſpace, of about a hand breadth, ſeparates the colour of the belly from that of the thighs: a white band, alſo, runs along each ſide of the ſtripe on the back, and unites itſelf to the white part of the lower belly. Oppian has expreſſed very accurately this white ſeparation, in his poetical deſcription of the onager. The mane, which is blackiſh brown, begins between the ears and extends to the ſhoulders; it conſiſts of ſoft woolly hair from three to four inches in length, and ſtands erect exactly as in

newly-

newly-foaled horſes. The ſtripe on the back begins at the mane, and ends at the commencement of the tail: it is almoſt of a coffee-brown colour, broader towards the *os ſacrum*, pointed towards the tail, and every where covered, even in ſummer, when the animal is almoſt perfectly ſmooth, with thick matted hair, which ſeems very different from the reſt. The hair which forms the buſh at the end of the tail is almoſt as ſtrong as that of the mane of the horſe, and a full ſpan in length.

The ſort of ſcar in the inner ſide of the fore-feet, which in the common aſs is round, I found in the wild aſs to be longiſh, but not perfectly oval. The hoofs of all the four feet have in the place of the ſpur an elevation covered with a horny ſkin. The hoofs are almoſt round, and have deep cavities on the ſoles.

The hair of the onager, and particularly in the winter, is much ſilkier and ſofter than that of the horſe: it may be compared to the beſt camel's hair. The winter wool is matted, has an oily touch, and where the iſabella colour prevails, that colour is much paler, a few places excepted, where it is ſhaded in a different direction, and particularly according to the lines expreſſed in the figure.

In the onager I counted only ſixteen articulations of the tail; the others were in number equal to thoſe of the common aſs. It had only thirty-two teeth, viz. in each jaw ſix grinders very much uſed, and in each ſide row five cheek-teeth. In the interior parts I could obſerve no further difference between them and thoſe of the common aſs than are often to be found in animals of the ſame ſpecies. In the great gut I found very large worms (*aſcaris pollicaris*), ſuch as are frequently found in horſes; and here and there in the ſmall guts ſome aſcarides, ſmaller than thoſe common in man. Thoſe writers alſo who aſſert that the aſs is never infeſted by external vermin, muſt not extend this obſervation to the wild aſs; for the one here deſcribed was ſo covered with a ſort of ſmall lice, that the hair ſeemed as if ſtrewed over with them.

I have already ſaid that the Kirgiſians prefer the fleſh of the *kulan* to all other game, and even to that of their young horſes, of which they are ſo fond. The ſame taſte ſeems to prevail among the Arabs *; and the writers of that nation, in regard to this food, make the ſame diſtinction between the wild and tame aſs as the Hebrews made between both animals;

* See Bochart's *Hierozoicon*, lib.iii. c. 16; and Forſkäl's *Obſervat. Zoolog.* p. 5. where the onager is mentioned under the Arabian name *djaar*.

mals; and they consider it unlawful to mix the onager with the tame ass, as they do all mixed breeds of other animals.

The skin of the wild ass is highly valued by the Bucharians for making shagreen. Rauwolf says the same thing of the Syrian wild ass, the skins of which are brought to Tripoli for sale. Those, however, who have hitherto believed that the skin of the wild ass is grained of itself, and like a natural kind of shagreen, or that shagreen can be made of no other skin than that of the wild ass, are much mistaken; yet this erroneous opinion is to be found in some modern writers, and has been adopted even by Buffon. As I had an opportunity at Astracan of seeing the process used for the preparation of the fine shagreen, in order to destroy this false idea I gave a description of it in a former volume of this work *. The Bucharians, however, convert whole skins of the wild ass into a kind of coarse black shagreen, of which they make boots of a singular form, but remarkably durable, and having the soles entirely covered with nails: these boots are used also by the Kirgisians, and purchased at a high price: the fine shagreen, on the other hand, is made chiefly in Persia, and from a particular part of the hides of horses. Respecting the stones found in the wild asses, and of which Bauhin, in his Latin Treatise on the Bezoar, distinguishes two kinds, I can say nothing: they are the stones, perhaps, found in the common ass or mules, which, in order to give them greater importance, are pretended to be those of the above animal, in the same manner as stones cut from horses are brought from Persia and India under the name of Garmandel or Coromandel bezoar. I shall pass over in silence all the fables related of the onager which may be found in Gesner, Aldrovandi, and Bochart. The mistake of some modern writers, who consider the onager of the antients as the zebra, or who have confounded it with the *dshiggetai*, is unworthy of refutation. To be fully convinced that the wild ass here described is beyond all doubt the onager of the antients, nothing will be necessary but to compare this description with those of Oppian and other writers; and with the passage of Luitprand, bishop of Cremona, quoted by Bochart.

* For this description see Philosophical Magazine, vol. vi.

XIII. *Account*

XIII. *Account of some Improvements on Water-Wheels.* By Mr. ROBERTSON BUCHANAN, *Engineer, Glasgow.*

To Mr. Tilloch.

DEAR SIR, Glasgow, Oct. 20, 1801.

A CORN MILL in which I had an interest having had about two feet of its fall unoccupied, owing to the overshot wheel having been too small in diameter, I was desirous to apply this unoccupied part of the fall in some way to increase the power of the mill. Thinking over various expedients for this purpose, in February 1798 an idea of a water-wheel occurred to me, which I thought might be applied with great advantage not only to the situation I had in view, but in all other cases of low falls. Some circumstance, however, prevented me at the time from putting it in practice; but I had afterwards, at different times, two models of it made; and in November 1799 sent up the accompanying description of it to my friend Mr. Mundell, of Fludyer-street, requesting him to give it to the public. Upon some after reflection, however, I conceived it best, for some time, to delay its publication; and accordingly till now have allowed it to lie dormant.

Soon after finishing the drawing referred to in the description, I thought the particular form of buckets there represented might be a little altered, so as to make them still more advantageously contain the water. Upon showing the alteration I proposed to my ingenious friend Mr. JOHN ROBERTON of this place, he was at considerable pains to investigate the subject, and pointed out the form of buckets represented in fig. 3.

Allow me further to direct your attention to the construction of the arms shown in the figures. It is applicable to any kind of water-wheel; and since 1790, when I first constructed a wheel with arms on that principle, a considerable number of large wheels have, in Scotland, been erected on that plan. I shall annex a short description of this construction of arms.

I remain,
Dear Sir, yours,
ROBERTSON BUCHANAN.

Copy of the Description sent to Mr. Mundell.

It has been ascertained, from the most accurate experiments on water-wheels, that under the same circumstances an an overſhot produces about double the effect of an underſhot wheel.

Hence it is obvious, that where water can be made to act on a wheel by weight, it is much more effectual than when the ſame water is made to act by impulſe.

But where the fall is leſs than about half the diameter of a wheel with buckets made in the uſual form, the difficulty of filling them, and the ſhort time they are able to retain the water, are ſuch great defects, that in ſuch caſes wheels with open float boards, on which the water acts partly or altogether by its impulſe, have been found in practice to be more advantageous.

It occurred to me however, that, by adopting another form of the buckets, they might be ſo made as to be eaſily filled, and at the ſame time capable of retaining the water in a ſituation to produce nearly its full effect, altogether by weight, on a low fall.

A wheel of the conſtruction I mean is repreſented in the figures. Contrary to the uſual practice, the water muſt here be poured into the buckets *from within* the ſhrouding *.

How the filling of the buckets from within can be accompliſhed, may not at firſt be obvious; but a ſlight inſpection of the figure will ſhow that it may be done without the penſtock interfering with the arms of the wheel.

The buckets in the figure empty themſelves by means of apertures the whole length of the buckets, but no wider than juſt ſufficient to diſcharge the water before they begin to aſcend.

References to the Figures.

Fig. 1. (Plate III.) elevation of the water wheel.

A, A, the penſtock.

From B to C a part of the wheel is repreſented with the ſhrouding removed, to ſhow the form of the buckets and the ſituation of the water in them.

a a a the apertures by which the water eſcapes from the buckets.

Fig. 2. horizontal ſection of the wheel, and plan of the penſtock.

b the aperture by which the water enters from the penſtock to the buckets.

ROBERTSON BUCHANAN.

Rotheſay, Nov. 9, 1799.

* The ſhrouding is a flat ring, generally of wood, of the diameter of the wheel, ſerving to form the ends of the buckets, or to connect the floats, expreſſed by the letter B, (Plate III.)

Deſcription

Description of the Construction of the Arms.

Fig. 1. and 2. It is evident that arms, as commonly fixed in mortised shafts, are weakest in one direction, and that commonly the direction of the strain. To remedy this defect, the *feather-pieces* FF are applied all round, having their broadest ends towards the centre of the wheel, and being at right angles to the breadth of the principal arms. In order to unite them firmly to the principal arms, and connect the whole more firmly, a ring of iron, R, is applied on each side: blocks of wood, W, put in the vacant space between, and the keys KK, bring the whole home to the shaft.

N. B. In some cases it may be easiest to introduce the water in the direction XY, as it was in one of the models, instead of that on the shaded part of the plan.

XIV. *Proceedings of Learned Societies.*

ROYAL ACADEMY OF SCIENCES AT BERLIN.

On the 6th of June the academy held a public sitting to celebrate the anniversary of the king's birth-day.

Prize Questions.

The Physical Class had proposed the following question for the year 1801:

"Has electricity any influence on matters which ferment? If it has, what is its action? Is it favourable to fermentation, or does it prevent it? Does it produce changes in the products of fermentation? What advantages may be derived from the development of this matter to improve the art of making wine; and those of brewing, making vinegar, and distilling spirits?"

But as it received only one memoir on the subject, which was not satisfactory, the class proposes the same question for the year 1803, with this express condition, that "those who attempt to answer it, will do so by a series of essays and experiments calculated to produce evident and certain results."

The Philosophical Class proposed for the year 1803:

"Can a moral appreciation of actions be taken into consideration in the establishment and application of penal laws? and, if it can, to what extent?"

The Class of the Belles Lettres again proposed the following question for 1803:

On the Goths and Gothicism.

"1. Among the nations who contributed to hasten the downfall of the Roman empire, did the Goths distinguish themselves above others by any peculiar qualities in regard to their constitution, their laws, customs, and usages; or, more particularly, in regard to their literature and arts?

"2. Are not the terms *Gothic* and *Gothicism* denominations created at posterior periods to denote the state in which the sciences, letters and arts were, after the decline of the Roman empire, and during the middle ages?

"3. If this be the case, at what period did these terms begin to be used in this more general acceptation?"

The Mathematical Class has also repeated, for 1802, the following question, with a double prize:

"As there still remain, notwithstanding the labours of the ablest astronomers, several points to be cleared up in regard to the variation of the obliquity of the ecliptic, the academy invites the learned to employ themselves again on this object, and will adjudge the prize to that memoir which shall contain the most interesting researches and the most important illustrations of this point.

"It invites the learned of all countries, except the ordinary members of the academy, to turn their attention to these questions. The prize, which consists of a gold medal of the weight of fifty ducats, will be adjudged to those who, in the opinion of the academy, shall have given the best answer. The papers, written in a legible hand, must be addressed (post paid) to the perpetual secretary of the academy. Those which, in the opinion of the academy, have not sufficient merit to be entitled to the prize, cannot be returned to the authors, as the originals must be deposited among the archives of the academy: all those, therefore, who transmit memoirs as candidates for the prize, are requested to take copies of them before they transmit them to the academy.

"The term for receiving the memoirs is fixed at the 1st of May of the above years, after which period none can be received, whatever reason may be assigned for the delay."

FRENCH NATIONAL INSTITUTE.

In the public sitting of the Institute on the 7th of October, the following papers were read:

An account of the observations made on the perpendicular chains (plumb lines) suspended from the dome of the Pantheon, the object of which is to ascertain whether any warping has taken place in that edifice: by C. Prony;

On that kind of pavement called by the Greeks *lithostrata*, and by the moderns *mosaic:* by C. Gibelin;

An

An hiſtorical account of the life and ſervices of H. F. Gilbert: by C. Cuvier;

On public ſpirit: by Toulongeon.

The following notice of the labours of the Claſs of Literature and the Fine Arts was read by C. Villars:

Antiquities.

The names of two celebrated cities, Herculaneum * and Pompeii †, have often inflamed with a noble emulation the amateurs of antiquities and antient monuments. Hitherto the total diſappearance of theſe two cities has been aſcribed to the famous eruption of Veſuvius in the firſt year of the reign of Titus, and the 79th of the Chriſtian æra. According to the obſervations of C. Du Theil, this epoch is not certain; for theſe two cities roſe again from their ruins under the reign of that prince; and they ſtill exiſted, with ſome remains of ſplendour, under Hadrian. The beautiful characters of the inſcription traced out on the baſe of the equeſtrian ſtatue of M. Nonius Balbus, ſon of Marcus, are an evident proof of its exiſtence at that period. They were found under the reign of the Antonines; for the account of Trimalchio's feaſt, in the ſatirical romance aſcribed to Petronius Arbiter, furniſhes ſeveral evidences of the exiſtence of Pompeii, and of ſome of the edifices of Herculaneum, under the laſt of theſe princes. In the geographical monument known under the name of Peutinger's chart, which is of a date poſterior to the reign of Conſtantine, that is to ſay, in the commencement of the fourth century, Herculaneum and Pompeii were ſtill ſtanding, and then inhabited; but in the Itinerary improperly aſcribed to Antoninus, neither of theſe two cities is noticed; from which it may be conjectured, with ſome degree of foundation, that the entire ruin of Herculaneum and Pompeii muſt have taken place in that interval, between the time when Peutinger's chart was conſtructed and that when the above Itinerary was compoſed.

The eruption which took place in 471 occaſioned the moſt

* Herculanum, otherwiſe Herculanium, Herculaneum, Herculeum, an antient city of Italy in Campania, on the ſea coaſt, oppoſite to Veſuvius. *Plin.* lib. iii. c. 5. places it between Naples and Pompeii. *Paterculus*, lib. ii. c. 6. as well as *Florus*, lib. i. c. 6. ſays that it was conquered by the Romans during the war of the allies.

† Pompeii, in Campania, four miles from Naples towards the eaſt. Under the aſhes of Veſuvius was diſcovered the village called *Torre del Greco*—the Greek tower. It is generally ſuppoſed that the city of Pompeii was buried in that ſpot.

 dreadful

dreadful ravages. If we can give credit to Marcellinus, the aſhes thrown up by Veſuvius covered all Europe. They were carried as far as Conſtantinople; where, according to our chronicler, an anniverſary feſtival was inſtituted in remembrance of this ſtrange phænomenon. It is very probable that the cities of Herculaneum and Pompeii diſappeared at that period, and that no more traces of them were left. This conjecture is ſupported by a letter written by Caſſiodorus in the name of Theodoric, who reigned between 493 and 526. On this authority C. Du Theil is inclined to think that, after the fatal eruption of 471, ſuch of the inhabitants of Pompeii as had the good fortune to eſcape alive, retired to Nola in Campania, and that thoſe who eſcaped from Herculaneum took ſhelter at Naples. The quarter where the latter eſtabliſhed themſelves was detached from the other parts of the city.

This may ſerve to explain the denomination *Regio Herculanenſium*, or the quarter of the Herculaneans, which has been obſerved on ſeveral monuments of ſtone dug up at Naples, and alſo in ſeveral inſcriptions, which have been collected and publiſhed by learned Neapolitan antiquaries.

After Du Theil had read this memoir, he communicated a notice reſpecting ſome objects of antiquity lately found near the ſmall town of Azai-le-Nideau, on the left bank of the Indre. The account of them was tranſmitted to him by C. Biencourt. Some workmen, when digging a ditch, diſcovered a coffin containing a young child, as was concluded from its teeth, cranium, and ribs; but the reſt had ſuffered ſo much from the injuries of time, that it fell into duſt when expoſed to the air. At the ſide of this body were the following objects:—1. Two poniards, one of iron, the other of ivory, half deſtroyed: 2. A gold bulla ſhaped like an acorn—a badge worn by the children of Roman patrician families: 3. A ſmall jaſper ring ſet in gold, on which are engraven two veiled figures: 4. A ring entirely of rock cryſtal. It has engraven on it two rams yoked to a car: a ſmall Cupid ſitting in the car urges them on full ſpeed. This engraving diſplays much grace and ſpirit. 5. A cryſtal ring imitating a row of pearls ſurmounted with a ſhell: 6. A priſm of twenty planes, of rock cryſtal, very regularly cut: 7. Four lachrymatories of glaſs: 8. Several figures ſculptured in ivory, and almoſt decompoſed: 9. A young Cupid alſo of ivory, pretty well pulverized. In the ſame tomb were found ſeveral works compoſed of reſinous ſubſtances, which time ſeems to have reſpected. 1. A lion holding under his paws a dog, the head of which he is devouring: 2. Another piece of ſculpture repreſenting a car drawn by an old goat. A Cupid ſitting in the car makes a ſign to the goat to advance, and threatens him with a whip which he holds in his hand. Before the car a young woman is ſeated without veſtments, and behind it is an old man clothed in a long robe. This piece ſhows a great deal of expreſſion. 3. A ſilver cup, the bottom of which is oxidated: rings and broaches of metal, on which are filed reſinous perfumes. The deſcription of theſe articles, which C. Biencourt will ſoon tranſmit to the Inſtitute, muſt be highly intereſting to artiſts and antiquaries.

C. Mongez, always employed in reſearches reſpecting the coſtume of the antients, has been endeavouring to diſcover and deſcribe the different ſubſtances which they employed for their veſtments, arms, &c. Particular reaſons have induced him to detach from his work an eſſay on the uſe which the antients made of hemp. Heſiod and Homer do not mention this vegetable. Herodotus ſays that it reſembles flax, from which it differs only in ſize and in height. The appearance of theſe two plants, however, is marked with ſuch a ſtriking difference, that the father of hiſtory, ſince he ſpeaks ſo incorrectly, muſt have never ſeen hemp. He nevertheleſs tells us, that the Thracians procured from the northern countries of Europe the hemp which they employed to make cloth for their veſtments.

Ariſtophanes ſpeaks of a hemp merchant; and we know that the Greeks uſed hemp for cordage, and for caulking their veſſels. But they did not then cultivate this production, for it is not mentioned by Theophraſtus; and Hiero procured hemp from the northern countries of Europe; that is to ſay, from the banks of the Rhodanus, which throws itſelf into the Viſtula, and which he calls, improperly, the Eridanus. It is ſtill from Ruſſia and Livonia that Europe is ſupplied with hemp: it is very probable that it was not cultivated by the Greeks till about the commencement of the Chriſtian æra.

The Romans, like the Greeks, employed this ſubſtance for cordage, and for caulking their veſſels; but neither of them manufactured ſail-cloth of it, if we are to judge from the Greek and Roman writers whoſe works have been preſerved. C. Mongez has been obliged to deſcend from Herodotus, in his paſſage reſpecting the Thracians, to the 12th century, to find a paſſage which makes mention of hempen cloth. It is ſtill in a northern country, viz. England, that he found it.

In the 13th century, and the following ones, hempen cloth

 became

became very common in the middle and southern parts of Europe. There can be no doubt that the remains of this cloth, employed with those of linen for making paper, then introduced into Europe, contributed to preserve the antient authors whose works were effaced; that the same parchment might be employed for the books of the church and treatises of theology. Mongez concludes his researches by comparing the passages in antient authors, where mention is made of torrefied hemp-seed, with the smoke of which barbarous nations got intoxicated, and the practice still common in Egypt and Asia of chewing these seeds for the like purpose.

The Class of the Mathematical and Physical Sciences had proposed in the year 8, as the subject of a prize, to be adjudged on the 7th of October 1801, the following question:

"To point out the earthy substances, and the processes proper for making earthen-ware capable of standing a sudden transition from heat to cold, and which may be within the reach of all classes of people."

This question was accompanied with the following notice: —The art of manufacturing that valuable pottery known under the name of *porcelain* has been brought to a degree of perfection in France which leaves nothing more to be desired; but the case is not the same with the common pottery employed for daily use: the latter is far from being carried to that degree of increase and prosperity so necessary for the wants of the great body of the people; while some neighbouring nations, who do not make so fine porcelain, manufacture very useful pottery, the properties of which are far superior to that made in France. The Institute requires that the competitors will examine the composition of these excellent kinds of pottery, the earths which may be employed in the composition of them, and the artificial mixtures which might be substituted in their stead: the manner in which these earths ought to be treated to give them the necessary qualities; the art of baking them; the degree of heat; the form of the furnaces most advantageous; and, in particular, the processes for glazing pottery without oxides and pernicious metals.

The class received only one memoir on this subject, accompanied with several specimens of pottery, and of the earths of which they were made. It found that the memoir was written in a clear and methodical manner; that it contained principles acknowledged by philosophy and chemistry; and that the details, serving to illustrate them, announced long practice in the art of pottery, together with great theoretical

retical knowledge: but as the ſpecimens which accompanied this memoir did not anſwer the object pointed out by the claſs, it has prolonged the term for adjudging the prize to the 3d of October 1802, ſtrongly recommending to the candidates to apply with particular care to this branch of manufactory.

This double prize, of the value of 6800 francs (above 280*l.* ſterling), will be adjudged in the ſitting of the Inſtitute at the above period.

The Claſs of Literature and the Belles Lettres propoſed in the year 8, as the ſubject of a prize in antiquities, the following queſtion:

"What are the ſtudies which form, and the kinds of knowledge which characterize, the antiquary? What advantages ought to ariſe to ſociety from ſuch knowledge?"

On account of the importance of this ſubject, the claſs has thought proper to prolong the period of receiving memoirs till the 3d of October 1802, as before.

The prize will be a gold medal of the value of five hectogrammes, and will be adjudged in the public ſitting of January 5, 1803.

XIV. *Intelligence and Miſcellaneous Articles.* *October* 1801.

MEDICINE.

THE *Oberdeutſche Allgemeine Literatur-Zeitung*, July 23, 1801, under the head "Literary Notices," contains the following article:

"In the 7th number for the year 1779, of that generally eſteemed journal *Der Rathgeber für alle Stande*, the queſtion, Whether friction with oil can be employed for preventing and healing the ſmall-pox, having been anſwered in the affirmative, both by the Rev. Charles Gottfried Bauer, of Frohburg, and the learned editor Dr. Daniel Collembach; and as they produce ſeveral important grounds to prove their aſſertion, an individual who has the good of mankind at heart wiſhes to ſee it confirmed by facts. For this purpoſe, he offers a prize of twelve ducats to the phyſician who, before the end of the preſent year, ſhall have ſucceſsfully employed friction with oil on the greateſt number of variolous patients. The ſmall-pox, however, muſt have been of the malignant confluent kind, and accompanied with ſymptoms conſidered as fatal.

"Any phyſician alſo, who ſhall prove, by the greateſt number of facts, that he has preſerved whole families, by friction with oil, from the infection of this diſeaſe, will receive a like prize of twelve ducats. We therefore expect to be made better acquainted with the rules for employing this remedy ſo much extolled, and which Dr. Fothergill in England, and other eminent phyſicians, found uſeful in putrid fevers."

MINERALOGY.

M. Chenevix has had under examination a ſpecimen of *manachanite* from Botany Bay. It differs from the Corniſh in containing a white quartzy ſand. It yielded on analyſis,

Silex - -	11
Oxide of iron -	49
Oxide of titanium -	40
	100

It is difficult to obtain this metal reduced and pure; but by mixing the oxide of titanium with glaſs of phoſphorus and charcoal, and then employing a flux of borax, a button of phoſphuret of titanium may be obtained.

If the mineralogical riches of New Holland may be judged of from the valuable productions with which we have become acquainted already, we may look forward to many intereſting diſcoveries: already a metal is found to be a native of thoſe climates which has not long been known, and has been but rarely met with in Europe.

D'Andrade, in his travels through Sweden and Norway, has added to the catalogue of minerals the following ſubſtances, of which he intends publiſhing a particular account: Acanthicone, ſpodumene, ſahlite, ichthyophthalme, coccolite, aphrizite, allochroite, indicolite, wernerite, petalite, chryſolite, ſeapolite.

Klaproth has completed the analyſes of the fluated alumine called the *chryſolite of Greenland.* It contains

23·5	of alumine
36·0	of ſoda
40·5	of fluoric acid and water.
100·0	

He conſiders the diſcovery of ſoda in ſtones as very remarkable. It is the firſt inſtance of ſuch a quantity of ſoda being preſent in an inſoluble ſubſtance.

Vauquelin

Vauquelin has repeated the analyfes: he finds

Alumine - -	21
Soda - - -	32
Fluoric acid and water -	47
	100

Upon a careful examination of the alumine he thinks it contains 5 of filex.

Klaproth has alfo difcovered a native phofphate of copper; and is at prefent employed in analyfing this new combination of the phofphoric acid.

CHEMICAL NOTICES.

Some experiments made by Mr. Carlifle prove that fulphurated hydrogen gas is not evolved from eggs unlefs they are heated in water, and this by the decompofition of the water ufed.

A perfon in the colour line, on the continent, having mixed coarfe Pruffian blue and nut oil for a blue colour, covered them with water, and left them till wanted. On going to ufe it, he found it changed to white: thinking it required more Pruffian blue, he fpread it upon a ftone, when the colour came again of itfelf: he then covered it with oil inftead of water, but with no better fuccefs. As an experiment, fome was laid on white paper, fome on wood, and fome on a wall. That on the white paper was the laft that returned to the blue colour; but they all became a ftrong blue.

Phofphorus expofed in hydrogen gas is diffolved in proportion to the temperature. If this is done in a light place, the phofphorus will fublime on the infide of the glafs. If the glafs be darkened in parts where the light cannot enter, the phofphorus will not have fublimed. This adhefion to the fides of the glaffes never takes place but when light is prefent.

Mr. Cruickfhank has publifhed a fecond paper on the gafeous oxide of carbon, in which he propofes a method of analyfing thefe gafes by the oxymuriatic acid gas, which abforbs and decompofes the hydrogen and the hydro-carbonate, leaving the gafeous oxide. In this cafe, not more than a double quantity of the oxygenated muriatic acid gas fhould be ufed. The gafeous oxide itfelf may alfo be decompofed by the oxygenated muriatic acid gas, provided a fufficient quantity be expofed to its action. Hydrocarbonate or hydrogen may be immediately decompofed by the action of oxymuriatic gas and the electric fpark; but the gafeous oxide of carbon and oxymuriatic acid gas are not fired by the electric fpark.

Mr.

Mr. Cruickfhank gives the following as an entertaining experiment:—Fill a veffel, of about the capacity of three pints, with oxymuriatic gas; fee that it is drained from the water in it; then throw into it about a dram of good ether, and cover the veffel with a piece of paper. In a few feconds a white vapour will be obferved moving circularly: this will be followed by an explofion and flame, and a large quantity of carbon will be depofited.

GALVANISM.

Mercier has quoted a work, publifhed 27 years ago, which contains the principle of galvanifm. Twelve years before, the celebrated Sulzer difcovered this principle, and obtained the fame refult. "If you join," fays he, "two pieces of metal, one of lead and the other of filver, in fuch a manner that their edges form one plane, and if they be placed upon the tongue, a tafte of the vitriol of iron will be felt, while each piece feparately leaves no trace of tafte."—*Smith's Pofthumous Works,* part ii. p. 308.

NATURAL HISTORY.

A very ftrong leaden pipe, which conveyed water into a bafon in the garden of the palace of the Confervative Senate, the ci-devant Luxembourg, and which has not been in exiftence for 30 or 40 years paft, has lately been difcovered. This pipe contained a fediment depofited by the water, which, hardened by time, had become ftone, or rather freeftone, fufficiently clofe to ftrike fire with fteel without the lead which contained it being in any manner altered. Had the workmen taken the precaution to remove the lead without touching the matter, they would have preferved a curiofity unique in its kind; a natural pipe from 2500 to 3000 feet long, of the moft compact freeftone, and an inch and a half in thicknefs. In its whole diameter there were counted about fifty concentric ftrata or coats.

COMETS.

"In the *Journal de Paris* of May 28, Caigné the notary announced, that he would give 600 francs to the perfon who fhould difcover a comet. On the morning of July 12, C. Pons, keeper of the obfervatory of Marfeilles, difcovered one, and in the evening it was obferved alfo by three able aftronomers at Paris. The Board of Longitude being requefted to adjudge the prize, thought that C. Pons had a kind of priority, though he did not make his obfervations known till the evening: befides, the three aftronomers of Paris confented, and C. Pons had

had the prize. I ſolicit the government to propoſe prizes in the ſame manner, to accelerate our knowledge of comets, the only branch of aſtronomy which has not made progreſs. It is, however, ſo eaſy to find comets, that thoſe not aſtronomers may diſcover them when they chooſe.

LALANDE."

ASTRONOMY.

"Who can decide, when doctors diſagree?"

"For ſome years the ſpots of the ſun had become rare; but on the 23d of September, in the morning, I obſerved a beautiful round and well-defined one, and found it to be 22 degrees to the north of the ſolar equator. This is a parallel in which very beautiful ones have been ſeen; which confirms me in the opinion which I advanced, (*Memoires de l'Academie* 1776 and 1778,) that there are eminences in the opake and ſolid nucleus of the ſun, where ſome ſcoriæ, which float on this ocean of fire, are ſtopped from time to time. This does not agree with the idea of Dr. Herſchel; but this celebrated obſerver is more to be admired for his obſervations than his hypotheſes. LALANDE."

CULTURE OF POTATOES.

Extract of a Letter from Sir John Methuen Poore, Bart. to Sir William Pulteney, Bart.

SIR, Ruſhall, April 4, 1801.

I can prove, not by theory, but practice, the benefit of planting potatoes on fallows. In the pariſh where I reſide, the whole of which, except five acres, is my property, there are thirty cottages, containing 131 poor people. I have, for five or ſix years paſt, allotted, free from rent, four acres of land, intended to be ſown with wheat the following autumn, for the cottagers to plant with potatoes; by which means each raiſes from ten to fifteen ſacks (equal to 240 pounds per ſack), yearly, in proportion to the number of their children: each has not only ſufficient for his family, but he is enabled alſo to fat a pig. They declare, was I to give among them a hundred pounds, it would not be of ſo much benefit to them; and it is not one ſhilling out of my pocket, for I have as good, if not a better crop of wheat from this land, as I have from the other part of the field.

The method I take is this:—The latter end of November I plough the land; the froſt during the winter mellows it: the beginning of March following, I plough it again, and harrow it; at both which times I have little to do with my horſes: I then divide it into lots; a man with a large family has

has a larger lot than a single person, or one who has only two or three children, allowing about five perches (of $16\frac{1}{2}$ feet square) to each in a family: they then plant it, and put over their potatoes what manure they have collected the year preceding (for every cottager has more manure than necessary for this from their fires, and a variety of other things); and during the summer, after their day's labour is done, they and their wives hoe them; and as every man works more cheerfully for himself than for another, they do not suffer a weed to grow. In October they dig them up; and it is the most pleasant thing imaginable, to see the men, their wives and children, gathering the produce of their little farms, which is to serve them the ensuing winter. Was this plan generally adopted, the labourers would consume but little corn; which would supply the manufacturing towns, and we should have no occasion to import. As four acres are sufficient for thirty families, it would take but a small quantity of land from every farm in the kingdom. The way practised here, is to plant the potatoes in furrows, eighteen inches apart, and a foot apart in the rows. The land about me is of different qualities: on the hills, rather light; in the vale, near the parish, inclining to clay; but all fit for turnips: the potatoes are planted in the low land, being nearer home. The poor, at present, will not live entirely without bread, as many do in Ireland, though potatoes daily get into use more and more; and I am persuaded, was my plan generally adopted, in two or three years the labourers in the country would consume but little or no corn. Thirty years ago, the poor in this part of the country would not eat potatoes, if they could get other roots or vegetables.

SINGULAR PHÆNOMENON AT COLNEBROOK DALE.

On Sunday, the 7th of September, a most tremendous fall of rain, accompanied with thunder, raised the river Colne to such a height, as to make it reach one of the blast-furnaces, belonging to the Colnebrook Dale company, by the overflowing of the dams. The instant the water entered the furnace, a dreadful explosion took place, and a column of melted and red-hot mineral was discharged into the air, in a perpendicular direction, upwards of 150 feet. The explosion was repeated at intervals for two or three times, accompanied each time by an extremely brilliant column, the heat of which was so intense, as to be felt several hundred yards distant. The roofs and floors of the contiguous buildings were displaced by the concussion. It is remarkable, that the prodigious quantity of matter, upwards of 2000 cubic feet of

iron-

iron-ſtone, lime-ſtone, and coals, part in fuſion, emitted from the mouth or top of the furnace, is entirely diſſipated; and that, notwithſtanding the principal part of it was large pieces of iron ore and limeſtone, yet not a particle of either can be found any where near the ſpot, nor is a veſtige of them to be diſcerned on the tops of any of the adjacent buildings. It is alſo wonderful, that the aperture at the top of the furnace (an opening not exceeding a few feet in diameter), through which the contents were exploded, is not in the leaſt rent, but is now as ſound as ever, together with the furnace itſelf.—It is ſuppoſed a water-ſpout muſt have burſt near the ſpot, as part of the works was in a few minutes completely inundated, and the water attained to the amazing height of 17 feet in a very ſhort ſpace of time.

PARTICULARS RESPECTING THE SAVAGE OF AVEYRON:

Extracted from the Report made to the National Inſtitute by Citizen Degerando.

When the young creature, known by the name of the *Savage of Aveyron*, was diſcovered in the foreſt of Canni, and brought to Paris by the profeſſor Bonaterre, the public, for a conſiderable time, echoed with this intelligence. It occupied the idle, attracted the curious, and gave riſe to a multitude of diſcuſſions, which were at leaſt premature, as they could then have no foundation but conjecture.

In the mean time, the public, who ran in crowds to ſee this child, were aſtoniſhed to behold in him only a being, nearly inſenſible, which appeared neither to hear nor to ſee, which gave no ſigns of attention, and appeared to have no ſort of active principle. Thus the intereſt which he excited became extinct as ſoon as it was raiſed.

The ſpirit of ſyſtem then paſſed a new ſentence on him. Some perſons, according to whoſe opinions the ſavage ſtate is not only the primitive, but alſo the moſt perfect ſtate of man, were ſurpriſed that this child of nature correſponded ſo ill with their ideas; and, dreading that he would completely overturn their hypotheſis, they haſtened to ſecure themſelves from any concluſion that might be deduced from him by declaring that he had been born a fool.

Superficial minds confirmed this ſentence. The ſavage of Aveyron reſembled a fool; it was therefore eaſy to conclude that he was a fool; and, what lent a ſtrong confirmation to this opinion was, that Pinel, a phyſician, who has acquired a high reputation by his ſucceſſes on perſons of diſordered intellects, having accurately examined all the circumſtances connected

connected with the physical and moral state of this boy, and having compared them with those of the idiots confined at the hospital of the Salpetriere, found such a perfect coincidence between them, that he thought himself justified in declaring this creature a natural idiot.

But a few philosophers still opposed themselves to a decision so precipitate and severe. They thought it possible that the solitary and brutal life of the savage of Aveyron might have produced a sort of habitual idiotism, the appearances of which might be similar to those of natural idiotism; and they held it extremely unjust to condemn the creature for ever, and extremely unwise to leave so extraordinary a phænomenon totally unexplained; at the same time they suggested the means that they conceived would be most effectual to rouse his faculties and unfold his understanding, if in reality he had any. Locke and Condilhac had already given the idea of these means. Previous to any attempt to produce connection of ideas, the ideas themselves should be created; in order to create the ideas, the attention should be fixed; and, in order to fix the attention, the wants and necessities of the person should be interested. They did not wish to teach him the use of signs, before he could have acquired those notions which these terms are intended to express; they wished to work on his sensibility, to direct it to its proper objects, and, by the formation of new habits, to counteract those depraved ones by which he had already been enslaved. They saw that a long time would be required to excite in him attention to a world in which he was a stranger, and regard for objects in which he had been, and was yet, totally uninterested: but they resolved to apply themselves with industry, and to await the effect with patience.

The boy was committed to the care of C. Ytard, physician of the National Institution for the Deaf and Dumb, in order that, by the combination of physical and modern remedies, the double incapacities under which he laboured might be more effectually removed. C. Ytard's exertions have already been crowned with a degree of success which is almost prodigious: he has published the particulars, which he has dedicated to the National Institute.

He proceeded nearly in the following order:—The sense of feeling seemed to be entirely paralysed in the child: he showed no sensibility either to heat or cold; his smell and taste were plunged in a similar sleep. A repetition of warm baths soon unfolded his nervous sensibility: in a little time after, his feeling acquired a considerable degree of delicacy; he became nice in the choice of his food; he made use of a selection and a cleanliness a cleanlineſs in it to which he had been before a ſtranger: his choice was directed by the ſmell.

The eye of this child was wild and wandering: he ſaw, without doubt, but he never dwelt on the object. The loudeſt noiſes appeared ſcarcely to ſtrike his ear; a piſtol ſhot would not make him turn his head. Superficial obſervers would have concluded that he was deaf; but C. Ytard was aware that, even when the ſenſe is perfect, no perception is produced unleſs the mind is attentive; and he was not aſtoniſhed that the violence of this ſound made no impreſſion on a being whom it could not intereſt. He found a new proof of the juſtneſs of his obſervation in the attention which his pupil beſtowed on the ſmalleſt ſound which could intereſt him, ſuch as the cracking of a nut, or the turning of a key.

In the mean time new habits were formed in the boy; a number of new neceſſities aroſe; food, dreſs, reſt, and walking out, were ſo many new means of augmenting his dependance. Finding himſelf under the neceſſity of availing himſelf of thoſe about him, he has begun to feel the force of moral affections, and has conceived a particular attachment to his governeſs. His ideas have been multiplied and connected; ſome efforts have been made to amuſe him, and it is contrived to unite inſtruction with amuſement. He has been exerciſed at compariſons; they have accuſtomed him to compare objects with their images, and in theſe compariſons he has only been conſtrained to uſe the united powers of judgment and of memory. C. Ytard thought this a favourable moment to teach him our written characters, and he made uſe of the method employed in the inſtruction of the deaf and dumb: he wrote the name of the object on the image, and then, by effacing the image, he hoped that the name would remain connected with the remembrance of the object: but this method proved unſucceſsful. Then other means were uſed, which are detailed in C. Ytard's publication, the effect of which was as happy as could be hoped. The boy now diſtinguiſhes the characters of the alphabet, and places them in their order; he pronounces the words *lait*, *ſoupé* (not ſoup), in the common tone, and then brings the proper letters, and forms theſe words. In this manner, he every day acquires a new word; he already begins to emerge from his ignorance; he has entered on the territory of reaſon; he is in poſſeſſion of ſome of our terms of ſpeech, and will ſoon be enabled to give us ſome information reſpecting his early condition; a ſubject which, of all others, muſt be moſt intereſting to curioſity.

It muſt be obſerved, that he finds great difficulty in the formation of articulate ſounds; from the effect of long diſuſe of his organs of ſpeech, there are only a few words that he can pronounce perfectly; but it is hoped that the ſame perſeverance which conquered the firſt difficulties that ſtood in his way, will alſo help him over the others.

LUNAR RAINBOW.

About ſeven o'clock on Saturday night, the 24th October, a beautiful *lunar iris* appeared at Edinburgh in the weſtern part of the horizon. As the moon was then very near the full, and her altitude above the horizon not very great, the red, yellow, and violet colours of the *iris* could be eaſily diſtinguiſhed, and the bow approached pretty near to a complete ſemicircle. This phænomenon continued half an hour; and, as the weſtern part of the horizon was during that time covered with an obſcure haze, which rendered it excellently adapted for the reception of the colours, it preſented a moſt beautiful ſpectacle to many who had never, till then, the pleaſure of obſerving it. Ariſtotle informs us, that he was the firſt who obſerved the *lunar iris*; and adds, that they are never ſeen but at the time of full moon. It has often appeared a matter of wonder to philoſophers, how this phænomenon ſo rarely occurs: but if we conſider, that it never appears unleſs the moon be near the oppoſition—that ſhe comes into this ſituation only thirteen times a year; whereas the ſun is in a ſtate fitted for their production twice every day; and that the light of the moon is incomparably more faint than that of the ſun; our ſurpriſe will quickly vaniſh, and we ſhall rather be aſtoniſhed at the frequency of their production.

Dr. Garnett, having fitted up an elegant and commodious lecture-room in Great Marlborough-ſtreet, intends, we underſtand, to begin his lectures on natural philoſophy and chemiſtry immediately. The introductory lecture will be delivered on the 2d of November, at eight o'clock in the evening. The lectures on experimental philoſophy will be delivered every Monday and Friday, at the ſame hour; and thoſe on chemiſtry every Tueſday and Thurſday, at one o'clock P. M. The medical lectures will not commence till January 1802.

XVI. *On the Equilibrium of Arches.* By Mr. JOHN SOUTHERN, *Engineer, Soho, Birmingham* *.

SIR, Soho, Birmingham, Nov. 2, 1801.

AT the present moment, when the grand project of throwing a single arch over the Thames (proposed by Messrs. Telford and Douglas) is in contemplation, whatever tends to render the theory of the equilibrium of arches more easily to be understood, will, it is reasonable to suppose, be acceptable to many of your readers; and if you think the following paper of this description, you will, by inserting it in the Philosophical Magazine, oblige,

Sir,
Your obedient servant,

Mr. Tilloch. JOHN SOUTHERN.

1. LET the right lines, fig. 1. ZBCDEF be situate in a vertical plane, and moveable about the angles B,C,D,E as joints, and also about the points Z and F; let ZB be horizontal, and let such weights be laid upon the joints BCDE as will keep the whole in equilibrio, the points Z and F not yielding: required what such weights are, and what the forces exerted in each line.

From any point B in the line ZB draw B*p* perpendicular to that line, continue the said line to any distance, *a*, whence draw the line *a*M parallel to B*p*; by the equilibrium of forces, the weight (represented by the line) *a*M laid upon the joint B will be counterbalanced by a force *a*B in the direction ZB, and a force MB in the direction MB, and of course the point B remain at rest. But action and reaction being equal and contrary, the force *a*B will not only be exerted against B, but also be returned and act against the point Z; also the force MB will be returned, and act against the joint C. Continue BC to *b*, so that *b*C = MB, and from *b* draw the line *b*N parallel to B*p*, meeting the line CD in N; then will the force *b*C, in the direction BC, be just balanced by a weight *b*N laid upon the joint C, and a force NC in the direction NC. In like manner continue the lines CD and DE to *c* and *d* respectively, making the lines *c*D = NC, and *d*E = OD; and from the points *c* and *d* drawing lines parallel to B*p* meeting the line DE and EF in O

* Communicated by the Author.

and P; so will the weight cO laid upon the joint D, and the force OD, balance the force cD in the direction CD; and the weight dP laid upon the joint E, and the force PE balance the force dE acting in the direction DE. The force PE will, by its reaction, be exerted upon the point F.

Make $BA = aB$, and draw Am parallel to BC; Bm will therefore $= aM$, and $Am = BM$; and consequently the lines AB, Bm, mA, will represent the forces and their directions of action which keep the joint B in equilibrium. Draw An parallel to DC, then because $bC = MB = mA$, and bN is parallel to mn (included in Bp), the triangle Amn is equal and similar to the triangle CbN, and consequently the lines Am, mn, nA, represent the forces and their directions of action which keep the joint C in equilibrium. So, Ao and Ap, being respectively drawn parallel to DE and EF, the triangle $Ano = DcO$, and $Aop = EdP$; and consequently their sides represent the forces which keep the joints D and E respectively in equilibrium.

2. It is obvious that the weight pressing on any joint is as the *difference of the tangents* of the angles which are formed by each of the legs of that joint with the horizontal line: for the weight on the joint D is as $cO = no$; but Bn is the tangent of the angle nAB (to the radius AB) = the angle which the leg DC of the joint forms with the horizontal line AB; and Bo is the tangent of the angle oAB (to the same radius) = the angle which the other leg DE of the joint forms with the horizontal line AB; consequently, no is the difference of these tangents.

3. It is equally obvious that the force exerted through any leg of any of the joints, and which may be called the *compressing force*, is *as the secant* of the angle which that leg forms with the horizontal line: for the force through the leg FE is as pA, the force through ED is as oA, through DC as nA, through CB as mA, and through BA as BA; respectively the secants of the angles, with the radius AB, which each leg makes with the horizontal.

4. The horizontal force, or compressing force at the crown, (which is constant,) is AB = radius; because the leg at the crown is horizontal, and therefore the angle is nothing there, the secant of which is equal to radius.

5. If the weights which are disposed upon any *number of contiguous joints* be united and laid upon one joint, and if the legs which form this joint have the same declination from the horizontal line, and the same forces exerted through them that the *extreme* legs of the two extreme joints of the said contiguous number have, the equilibrium will still be exact.

 Continue

Continue the line BC to G and *e*, and the line FE to G; make G*e* = MB = the force acting towards G from B; draw *eg* parallel to B*p*. By construction, G*e* is parallel and equal to A*m*, and *g*G is parallel to *p*A; therefore the triangles G*eg* and A*mp* are equal and similar, and represent the forces which keep the joint G at rest; but the force or weight *eg* = *mp* = *mn* + *no* + *op* is equal to the sum of the weights upon the angles CDE; and, by construction, the legs FG and BG of the joint G have the same declination from the horizontal that the extreme legs (FE,BC) of the two extreme joints (EC) of the contiguous joints CDE have.

6. It is hence evident that the *sum* of the weights upon *any number* of successive joints (whether they be numerous or few), beginning at the vertex, is *as the tangent* of the angle formed by the *ultimate* leg of the last joint with the horizontal line.

7. Consequently, if the legs be extremely numerous and short, so as to form a curve, or a line differing insensibly from a curve, the whole weight pressing on that curve must be *as the tangent of the angle* which a tangent to the curve at the lowest point makes with the horizontal line, because the said tangent will coincide with the ultimate leg of the curve.

8. If the curve be the arch of a circle, the *angle*, which a tangent to the curve forms with the horizontal line, will be equal to that at the centre, contained between the vertical radius and that drawn to the point whence the tangent is drawn, and may be measured by the arch itself.

Let *adf*, fig. 2. be the arch of a circle from the centre *z*, and *za* vertical; draw *a*F horizontal, a tangent to the arch at *a*, which is consequently at right angles to the radius *az*. From any point in the arch, as *e*, draw *em* a tangent to the curve at *e*; the angle *em*F is therefore that which the tangent to that point makes with the horizontal line; and because *em*, one of its legs, is at right angles to *ez*, and F*m*, (the other leg,) by construction, at right angles to *az*, the angle *eza* is equal to the angle *em*F, and may be measured on the arch from *a* to *e*. So the angle *dza* may be proved to be equal to that which a tangent to the arch at *d* would, if drawn, make with the horizontal. And so of any other point.

9. Let the arch *adf*, in the last figure, represent the intrados of a bridge; if it be required what weights should be laid upon any given portions of it to effect the equilibrium, draw through the given points *bcdef* radii from the centre *z* to the horizontal line *a*F; so are the portions *a*B,BC,CD, DE, and EF, proportional to the weights required to be laid

 on

on the reſpective portions *ab*, *bc*, *cd*, *de*, and *ef*, of the arch. For E*a* is (7.) proportional to the whole weight on the arch *ea*, D*a* is proportional to the whole weight on the arch *da*, and therefore ED is proportional to the weight on that portion of the arch *ed* correſponding with it, and is as the difference of the tangents of the angles formed by tangents to the arch at the points *e* and *d* with the horizontal. (2.)

10. The horizontal force, or compreſſing force at the crown, is as radius (4.) = *za*; the compreſſing force at any point, *c* or *d*, is as *z*C and *z*D reſpectively (3.); and, of courſe, the preſſure on the abutment in direction of a tangent to the arch there, as *z*F.

11. It is eaſy, from what has hitherto been demonſtrated, to calculate from tables of tangents the actual weights which ſhould be laid upon given portions of arches, provided the weight upon any one portion be given. Suppoſe, for inſtance, an arch to contain from the ſummit or crown to the abutment 30°, and that on the firſt degree 10 tons be laid, (including the weight of the arch, road, &c.) then ſay, As the tangent of one degree is to 10 tons, ſo is the tangent of two degrees to the weight upon two degrees, from which ſubtract the weight upon the firſt, and there will remain the weight due to the ſecond degree; and ſo on.

Thus, as tang.	1°	- co. ar.	1.75808
is to	10 tons	- -	1.00000
ſo is tang.	2°	- -	8.54308
Weight upon 2°	20,006 tons	-	1.30116
Subtract	10 the weight upon the firſt degree.		
	10.006 tons upon the ſecond degree.		

Again, as tang.	1°	co. ar.	1.75808
is to	10 tons	-	1.00000
ſo is tang.	15°	- -	9.42805
Weight upon 15°	153.51 tons	=	2.18613

As tang.	1°	co. ar.	1.75808
is to	10 tons	-	1.00000
ſo is tang.	16°	- -	9.45750
Weight upon 16° -	164.28 tons	-	2.21558
Subtract weight upon 15°	153.51 tons		
Weight upon the 16th degree	10.77 tons		

Again,

Again, as tang.	1°		co. ar.	1.75808
is to	10 tons		-	1.00000
ſo is tang.	30°	-	-	9.76144
The whole weight upon 30° = 330.77 tons			-	2.51952

Double this will be the whole weight of the bridge.

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12. It was ſhown (3.) that the compreſſing force of any leg in a ſyſtem of angles was as the ſecant of the angle which that leg formed with the horizontal line. The leg at any point of a curve will coincide with the tangent at the ſame point, and therefore the compreſſing force at any point of the arch of a circle is as the ſecant of the arch between that point and the vertex (8.) Thus the compreſſing force at 30° from the crown, is as the ſecant of 30°; and this is the force exerted upon the abutment in direction of the tangent to the arch there. The compreſſing force at 2° from the crown is as the ſecant of 2°, and at the crown (10.) as the ſecant of 0 degrees.

Thus, as the tang. of	1°	-	co. ar.	1.75808
is to	10 tons		-	1.00000
ſo is ſecant of	0°	-	-	10.00000
The compreſſing force at the crown = the *horizontal* force, or thruſt, or puſh, againſt the abutment	572.90 tons		-	2.75808

Again, as the tang. of	1°	-	co. ar.	1.75808
is to	10 tons		-	1.00000
ſo is ſecant of	30° *		-	10.06247
The compreſſing force exerted againſt the abutment in direction of the tangent to the arch at that point	661.53 tons			2.82055

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13. This ſeems to be all that is requiſite to be known of the theory of the equilibrium of arches of a circle, ſo far as it relates to practice: the builder will naturally deſire to conſtruct his bridge with the feweſt materials conſiſtent with ſtability, and will probably firſt determine the dimenſions and weight of thoſe at the crown, which he will fix at a minimum, and from that, by the rules juſt given, the weight

* To ſome of thoſe who may not have a logarithmic table of ſecants it may be uſeful to ſay, that the logarithmic ſecant of an angle may be had by ſubtracting the co-ſine of the angle from 20.00000.

over other parts of the arch: in moſt caſes wherein the ſpan of the arch is large, the ſpandrils will require to be hollow, and the weights, deduced as above, will ſhow him how much. To carry the theory a little further,

14. Let A*m*B, fig. 3, be any curve equilibrated by the wall ABCD built upon it, and ſuſtained by a horizontal force applied at A the crown, and a tangential one at B; let S be the centre of curvature at A and SAD vertical. It was ſhown (9. 10.) that the weight of incumbent matter on any portion of the arch of a circle, is to the horizontal force as the tangent of that portion of the arch is to radius. Let A*m* be a very ſmall portion of the curve, which may therefore be ſuppoſed not to differ ſenſibly from the arch of a circle of the radius SA, nor indeed from the tangent of that arch. Therefore the weight upon A*m* will be to the horizontal force as A*m* is to radius AS: but as the weight is proportionate to the altitude AD of the wall, ſo is the horizontal force alſo as that altitude; and as the former may be expounded by the ſmall area A*n* (*mn* being parallel to AD) = A*m* × AD, ſo may the latter be expounded by AS × AD: in other words, the horizontal force, puſh, or thruſt, is equal to a wall whoſe length is AS and altitude AD. Make BG a tangent to the curve at B, and draw BP horizontal; alſo draw *sad* parallel to SD, and make *sa* = SA, and *ad* = AD; draw *ab* and *dc* horizontal, and *sb* at right angles to BG. The angle *bsa* is therefore equal to the angle GBP, which is the angle made by the curve at B with the horizontal line; and as *ab* is the tangent of this angle with the radius *as* (= AS), the area *abcd* is equal to ABCD (7.) = *ab* × *ad*.

Draw FI parallel and very near to BC, and FH a tangent to the curve at F; alſo at right angles to FH draw *sf*, and from *f* parallel to *bc* draw *fi*. It follows, from what has been ſaid, that the ſmall trapezium BI is equal to the ſmall parallelogram *bi*.

15. Hence the *general equation* for the altitude of the wall above of all curves of equilibration, which are ſo poiſed by the matter acting upon them *vertically only*, as if ſtanding on vertical pillars cloſe together upon the arch, viz. $a\dot{\imath} = w\dot{y}$; wherein

a = height of the wall at the crown.

$\dot{\imath}$ = the fluxion of the tangent of the angle GBP, formed by the tangent to any point of the curve (as B) with the horizontal line, the radius being AS.

w = the altitude of the wall above the ſaid point (as BC), which determines the figure of the extrados.

$\dot{y}$ =

$\dot{y}$ = the fluxion of the ordinate to the ſame point of the curve = the horizontal diſtance of the lines BC and FI.

16. Let the curve be the arch of a circle, and let it be required to find w. Let r = radius, and $\dot{y}$, $\dot{z}$, and $\dot{t}$, the contemporary fluxions of the ſine, the arch, and the tangent, (ſee fig. 4.); alſo, let s = the ſecant, and u be drawn parallel to $\dot{z}$.

then, $\dot{y} : \dot{z} :: r\ (= AS) : s\ (= Sb)$

$\dot{z} : u :: r : s$

$u : \dot{t} :: r : s$

therefore, $\dot{y} : \dot{t} :: r^3 : s^3$ and $\dot{t} = \frac{s^3\dot{y}}{r^3}$.

Subſtitute this value for $\dot{t}$ in the general equation above, and it will be,

$$\frac{as^3\dot{y}}{r^3} = w\dot{y}; \text{ therefore}$$

$$w = \frac{as^3}{r^3}.$$

$\frac{a}{r^3}$ being a conſtant quantity, w is proportional to s^3, as found by others.

17. Let the curve be the parabola, fig. 5.
A the vertex or crown.
B the point on which w is required.
BK a tangent to the curve at B.
x = AP (and by the nature of the parabola $2x$ = PK).
y = PB; other letters as before.

$$y : 2x :: r : t$$

$$t = \frac{2xr}{y}$$

$$\dot{t} = \frac{2ry\dot{x} - 2rx\dot{y}}{y^2};$$

and ſubſtituting this value for $\dot{t}$ in the general equation $a\dot{t} = w\dot{y}$), we have

$$\frac{2ary\dot{x} - 2arx\dot{y}}{y^2} = w\dot{y}, \text{ hence } wy^2\dot{y} = 2ary\dot{x} - 2arx\dot{y}.$$

By the nature of the figure $y^2 = px$, where p the parameter = $2r$ (r being the radius of curvature at its vertex)

$$\text{therefore } y^2 = 2rx \text{ and } x = \frac{y^2}{2r};$$

$$\text{alſo } \dot{x} = \frac{y\dot{y}}{r}.$$

Subſtituting theſe values in the equation juſt above,

$$w y^2 \dot{y} = 2ay\dot{y} - ay^2\dot{y}$$

$w = a$; ſo that the extrados and intrados are preciſely ſimilar and equal figures, having their vertical diſtance conſtantly the ſame $= a$, the altitude of the wall at the crown.

18. Let the extrados be a horizontal line DC, fig. 6, required the intrados, a being given, and x, y, w, as before.

By the figure $w = a + x$, and the general equation will be

$$w\dot{y} = \overline{a + x}\,\dot{y} = a\dot{t};$$

$$\text{therefore } \dot{y} = \frac{a\dot{t}}{a + x}.$$

Alſo, $\dot{y} : \dot{x}$ (ſee figure) $:: r : t$.

$$\text{Therefore } \dot{y} = \frac{r\dot{x}}{t} = \frac{a\dot{t}}{a + x}$$

$$a t\dot{t} = ar\dot{x} + rx\dot{x},$$

$$at^2 = 2arx + rx^2,$$

$$t^2 = \frac{r}{a} \times \overline{2ax + x^2},$$

$$t = \frac{r^{\frac{1}{2}}}{a^{\frac{1}{2}}}\sqrt{2ax + x^2},$$

$$\dot{y} = \frac{r\dot{x}}{t} \text{ (as above) } = \frac{r\dot{x}}{\frac{r^{\frac{1}{2}}}{a^{\frac{1}{2}}}\sqrt{2ax + xx}} =$$

$$= \sqrt{ar}\frac{\dot{x}}{\sqrt{2ax + xx}},$$

$$y = \sqrt{ar} \times \text{hyp. log. of } \overline{a + x + \sqrt{2ax + xx}} + C.$$

But when $x = 0$, $y = 0$, and $C = -\sqrt{ar} \times \text{hyp. log. } a$.

The correct fluent is therefore

$$y = \sqrt{ar} \times \text{hyp. log. } \frac{a + x + \sqrt{2ax + xx}}{a},$$

which is the ſame that Dr. Hutton makes it, page 44, new edition of his Principles of Bridges, wherein Q there $= ar$, here $=$ the horizontal force,

If a, x, and y, be given in any caſe, as, for inſtance, the altitude of the wall at the crown above the intrados and the altitude

altitude and half ſpan of the arch; then r may be found as follows, and thence the ordinates to any other point.

Let $c =$ hyp. log. $\left(\frac{a + x + \sqrt{2ax + xx}}{a}\right)$.

Then $y = \sqrt{ar} \times c$,

$y^2 = arc^2$,

$r = \frac{y^2}{ac^2}$.

Example. Suppoſe the ſpan of the arch to be 100, the altitude of it 40, and the height of the wall above the crown 6 (ſame caſe as Dr. Hutton's, page 47, Principles of Bridges),

$a = 6$, $x = 40$, $y = 50$, therefore

$\frac{a + x + \sqrt{2ax + xx}}{a} = 15.268$, the hyperbolic logarithm of which is 2.7257 * $= c$, $c^2 = 7.4297$, $ac^2 = 44.578$, and $\frac{y^2}{ac^2} = \frac{2500}{44.578} = 56.081 = r =$ the radius of curvature of the arch at the crown.

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19. The WEIGHT, of the incumbent matter on a curve loaded to the equilibrium, contained between the crown and any point B, fig. 3. and which may be expounded by the area AC of the wall above it, has been ſhown (14.) to be always $= ab \times ad - at$. Let A = ſaid area AC.

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20. If the curve be the arch of a circle, and T = the *tabular* tangent of the number of degrees contained in the arch, then will $t = r$T, and conſequently A $= ar$T.

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21. If the curve be the parabola, ſee fig. 5;

then $y : 2x :: r : t = \frac{2xr}{y}$
and $y^2 = px = 2rx$ - - } by the nature of the figure.

Therefore $t \left(= \frac{2rx}{y} = \frac{y^2}{y}\right) = y$,

and A $= ya$,

* The hyperbolic logarithm of a number may be found by multiplying the common or Briggs's logarithm of the ſame number by 2.302585, &c, the product is the required hyperbolic logarithm.

22. If

22. If the top or extrados be a horizontal ſtraight line, ſee fig. 6;

$$t = \frac{r^{\frac{1}{2}}}{a^{\frac{1}{2}}}\sqrt{2ax + xx},\ (18.)\ \text{therefore}$$

$$ta = \text{A} = r^{\frac{1}{2}}a^{\frac{1}{2}}\sqrt{2ax + xx} = \sqrt{rax \times \overline{2a + x}}.$$

Example. Take the caſe of Dr. Hutton's laſt mentioned, wherein r was found equal to 56.081; therefore

$rax = 13459$, and $2a + x = 52$.

$\sqrt{13459 \times 52} = 836.60 = \text{A}$.

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23. It is well known to mechanicians that if a heavy body be ſuſtained by two forces, their directions muſt meet either at the centre of gravity of that body, or in a *vertical* line which paſſes through it. Let A, fig. 7, be the body, g its centre of gravity; ea a ſtring by which it is ſuſpended, which again is ſuſtained by the ſtrings ab, ac. ac is of neceſſity vertical, and, if continued, muſt paſs through the centre of gravity g of the body. If this ſtring were either longer or ſhorter, the point a would ſtill be at reſt, and the ſtrings ab and ac under the ſame tenſion. If the body were removed vertically to the ſituation dotted, it would be ſuſtained in the ſame poſition by the ſtrings bf and cg, which would ſuffer the ſame tenſion as when they were united in the point a. If inſtead of ſtrings, props hi, kl, applied in the ſame directions on oppoſite ſides of the body, were ſubſtituted; or if, inſtead of theſe props, others parallel to them were applied, which, if continued, would meet in the vertical line, (as mn and op, which meet in g, do,) the body would ſtill be ſupported at reſt and in the ſame poſition, and the props would ſuffer the ſame force of compreſſion in theſe latter caſes, as the ſtrings on the oppoſite ſides ſuffered tenſion in the former.

24. Hence the CENTRE OF GRAVITY of the weights upon the joints E, D, C, (fig. 1.) muſt be in a vertical line paſſing through G, becauſe they are ſuſtained in equilibrium, or at reſt, by two forces in the direction BC and FE, which lines, continued, meet in G. Alſo, the centre of gravity of all the weights, ſay on B, C, D, and E, muſt be in a vertical line that paſſes through b, becauſe they are ſuſtained by two forces in the direction FE and ZB, which, continued, meet in b.

25. Hence alſo the centre of gravity of the materials upon a curve in equilibrium will be in a vertical line that paſſes through the point of interſection of the tangents to the extreme points of the curve.

Let

Let *abc*, fig. 8. be the curve loaded to the equilibrium; let *ad* be a tangent to the curve at *a*, *cd* a tangent to the curve at *c*, and *be* a tangent to the curve at *b* (the crown). The centre of gravity of the whole materials is in a vertical line which paſſes through *d*; and the centre of gravity of the materials over the arch *a,b*, is in a vertical line that paſſes through the point *e*; becauſe the points *d* and *e* are reſpectively the points of interſection of the tangents, drawn from the extreme points of the portions in queſtion of the curve.

26. To find the horizontal diſtance *af* of the centre of gravity of the materials contained between the *crown* and the abutment, from the latter. Drop a vertical line *ef*, from *e*, upon the ordinate *y*. Let $af = d$, and $\dot{x}$ and $\dot{y}$ be parallel to, and cotemporary fluxions of, *x* and *y* reſpectively. Then it is evident that $\dot{x} : \dot{y} :: x\ (= ef) : d\ (= af)$;

$$\text{therefore generally } d = \frac{x\dot{y}}{\dot{x}}.$$

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27. If the curve be the arch of a circle, *e,b* (ſee laſt figure) is evidently the tangent of half the arch, which ſubtracted from half the ſpan, leaves *d*, = ſine of the arch — tangent of half the arch.

28. If the curve be the parabola,

$$y : 2x :: \dot{y} : \dot{x} = \frac{2x\dot{y}}{y}, \text{ by the nature of that fig.}$$

$$\text{therefore } d = \frac{x\dot{y}}{\frac{2x\dot{y}}{y}} = \tfrac{1}{2}y.$$

29. If the curve be the equilibrial curve with a horizontal extrados.

It was ſhown (18) that $\dot{y} = \sqrt{ar}\left(\frac{\dot{x}}{\sqrt{2ax+xx}}\right) =$

$$\sqrt{\left(\frac{ar}{2ax+xx}\right)}\dot{x};$$

$$\text{therefore } d\ \left(= \frac{x\dot{y}}{\dot{x}}\right) = \sqrt{\frac{arxx}{2ax+xx}} = \sqrt{\frac{arx}{2a+x}}.$$

Example. Take the caſe of Dr. Hutton's mentioned before, wherein *r* was found = 56.081, $rax = 13459$, and $2a + x = 52$,

$$d = \sqrt{\frac{13459}{52}} = 16.088.$$

XVII. *Ob-*

XVII. *Observations and Experiments upon Dr.* JAMES'S *Powder, with a Method of preparing, in the humid Way, a similar Substance. By* RICHARD CHENEVIX, *Esq. F.R.S. M.R.I.A.**

AFTER the obfervations and experiments made by Doctor Pearfon, to inveftigate the nature of Doctor James's powder, and prefented by him to this fociety, very little remained to be effected or defired towards a further knowledge of the fubject. But thofe very experiments ferved to fuggeft, that the mode of preparation was far from being the beft that the prefent improved ftate of chemical knowledge might afford; and that, in all probability, a lefs defective compofition might refult from a procefs more conformable to fome improvements, which of late have been advantageoufly applied to pharmaceutic chemiftry.

It may be laid down as a general principle, that, in delicate experiments, whether analytical or fynthetical, fire (that potent and once believed to be univerfal agent) is too precarious in its means, and too uncertain in its application, to be employed with full and conftant fuccefs. And, if it is ftill recurred to, the advantage of promptnefs, and a remnant of antient cuftom, are the principal reafons. But, where other methods can be devifed to effect the fame combinations, (and the humid way offers many,) every perfon converfant in chemical knowledge will allow the benefit of adopting them. The recent improvement in the mode of preparing calomel, is a ftriking example of fuch falutary corrections being fuccefsfully introduced.

A few obfervations upon the formula according to which Dr. James's powder, or the *pulvis antimonialis*, is prepared, and upon fome properties of antimony, will place this affertion in a more prominent point of view.

In order to prepare this powder, we are told to take equal weights of bone or hartfhorn fhavings and crude antimony, and calcine them together, at a high temperature: in other words, to take phofphate of lime, which already contains a great excefs of lime, and add to it an oxide of antimony. In this procefs, it has been fuppofed, that the phofphoric acid of the bone or hartfhorn will faturate, not only the lime with which it was originally combined, but, in addition to it, a new portion of metallic oxide, and a new portion of lime. For, what little fulphuric acid might, during the procefs,

* From the *Philofophical Tranfactions for* 1801.

have

have been formed by the combuſtion of the ſulphur of the crude antimony, is diſſipated at a much lower temperature than that to which the powder is expoſed.

Every oxide of antimony with which we are acquainted is volatile at a high degree of heat: it would therefore be hazardous to aſſert, that it is poſſible to preſerve always the ſame proportion of antimony, whatever care may be employed in directing the operation; and a diſſimilarity in the chemical reſult muſt neceſſarily be attended with uncertainty in the medical application.

To this property may be added another, no leſs conducive to error. That portion of oxide of antimony which is not volatilized, becomes, in a great meaſure, inſoluble in all the acids. What the effect of the gaſtric juice may be upon a ſubſtance which reſiſts the action even of nitro-muriatic acid, it is not my purpoſe to determine. It is ſufficient for me to ſay, that, as the quantity of inſoluble matter in a given quantity of Dr. James's powder, prepared at different times, may vary, the effect of any doſe alſo may differ according to the proportions of ſoluble and inſoluble matter.

I look upon it as a fortunate circumſtance, that thoſe experiments and obſervations which I mentioned in the beginning of this paper, exiſted as a ſtandard to which I might refer my own attempts, and by which I might eſtimate their validity. Dr. Pearſon has proved, (as by my own experiments I have found,) that in Dr. James's powder about 28 per cent. reſiſted the action of every acid. In examining ſome of the *pulvis antimonialis* of the London pharmacopœia, I found the average quantity of inſoluble matter to be about 44 per cent. This proportion, however, was liable to conſiderable variation *.

The powder here treated of is denominated by Dr. Pearſon a triple ſalt, or a real ternary combination of a double baſis (lime and antimony,) with phoſphoric acid. What I have mentioned with regard to the quantity of acid contained in bone or hartſhorn, as being too ſmall to ſaturate a new portion of theſe baſes, may throw ſome doubts upon the poſſibility of any ſuch combination in the preſent caſe. But I have made ſome more direct experiments, which tend to prove that no ſuch combination does exiſt.

I took ſome white oxide of antimony, (formerly called *algaroth*

* I find, from the information of ſeveral medical gentlemen, that the *pulvis antimonialis* is generally conſidered as ſtronger than Dr. James's powder. This ſeems rather extraordinary, when we conſider that the quantity of inſoluble matter is greater in the former than in the latter; and would almoſt lead us to ſuſpect it to be the active part of the medicine.

algaroth powder,) precipitated by water from muriate of antimony, and heated it for a long time with phosphoric acid. I decanted the liquor, and washed the powder that remained. No antimony could be found in the liquor, nor could any traces of phosphoric acid be detected in the residuary oxide of antimony. I then took a solution of muriate of antimony, and divided it into two equal parts: into one I poured distilled water; and into the other, a solution of phosphate of soda. In each liquor, a copious precipitate was formed; which precipitates, after being well washed, were dried. The weight of both was the same; whereas it is evident that, had any phosphoric acid been combined with the oxide, there would have been an augmentation of weight in that which was precipitated by the solution of phosphate of soda. This precipitate likewise, upon examination, gave no traces of phosphoric acid. From these experiments it appears that there exists no combination which can be denominated a phosphate of antimony.

To attempt an explanation of the real nature of the powder here spoken of, I had recourse to some experiments of M. Berthollet. By detonating sulphuret of antimony and nitrate of potash in a crucible, he obtained a mass, which he reduced to powder, and washed. The liquor gave, upon evaporation, a crystallized salt, which M. Berthollet terms an *antimoniate of potash*. I never could succeed in any attempt to form a similar combination between the above white oxide of antimony and potash, owing, I believe, to the small quantity of oxygen contained therein, compared with that which is combined with the oxide obtained by detonation. I cannot therefore say, that the powder in question is, in any degree, what M. Berthollet would call an *antimoniate of lime*.

But be the state, whether of mixture or of combination, what it may, my purpose is to endeavour to produce a substance which, from its more certain mode of preparation, may be more equal and constant in its effects.

Dissolve, together or separately, in the least possible portion of muriatic acid, equal parts of the forementioned white oxide of antimony and of phosphate of lime *. Pour this solution

* In order to procure the phosphate of lime, I dissolved, in muriatic acid, a quantity of calcined bone, and precipitated by ammonia, in its state of greatest causticity. By this means, the excess of muriatic acid, which held in solution the phosphate of lime, is saturated, and the phosphate is precipitated; but no muriate of lime is decomposed, if the ammonia is quite free from carbonic acid. This is the most direct method of obtaining phosphate of lime pure. This salt is not decomposed, as some have asserted, by muriatic acid, but merely dissolved by it. I have been induced

tion gradually into distilled water, previously alkalizated by a sufficient quantity of ammonia. A white and abundant precipitate will take place, which, well washed and dried, is the substitute I propose for Dr. James's powder.

The theory of this precipitation is so clear and simple that it does not require any comment. It may be useful, however, to those who wish to make this preparation, to remark, that it is absolutely necessary that the solution of phosphate of lime and of oxide of antimony, in muriatic acid, should, after being well mixed, be poured *into the alkaline liquor*, in order to obtain a precipitate homogeneous throughout the operation. For, should the alkaline liquor be poured *into the acid solution*, the water of the former would act upon the entire mass of oxide of antimony, while the alkali would precipitate the phosphate of lime only as it saturated the acid which held that salt in solution: thus the precipitate would contain more antimony in the beginning; and, towards the end, the phosphate of lime would be predominant. For the same reason, too, a pure alkali is preferable to its carbonate; for the carbonic acid disengaged would retain in solution a portion of phosphate of lime.

Whether this composition be a chemical combination or a mixture, I will not take upon me to determine; but, for the reasons above mentioned, in speaking of Dr. James's powder, I belive it to be merely a very intimate mixture. At all events, it must be more homogeneous than any that can be prepared in the dry way. It is entirely soluble in every acid that can dissolve either phosphate of lime or oxide of antimony separately; and, to have it constantly and uniformly the same, no further address in preparing it is required, than to avoid the errors I have mentioned.

As, after some medical trials of the powder, it was suggested to me, that it might be advantageous to render it somewhat stronger, I prepared another portion, by taking two parts of oxide of antimony and but one of phosphate of lime, and precipitating as above described. The medicinal power was then considerably increased.

Dr. James's powder is a medicine which has been so long in use, and is so deservedly ranked among the most valuable we possess, that every attempt to render the process for preparing it more simple and more certain, must be allowed to be of

duced to state fully these particulars, because, from the beneficial effects of this salt in the treatment of rachitis, as proposed by M. Bonhomme, (*Annales de Chimie*, vol. xviii. p. 113,) it may become of general use. The oxide of antimony. I obtained by precipitating, by water, the common butter of antimony of the shops.

some

ſome importance. But whatever reaſon there was to think, by arguing upon its chemical properties, that I had really ſucceeded in improving its medicinal virtues, it ſtill remained to be proved, by actual experiment, that the hoped-for ſucceſs was not merely conjectural. To aſcertain this, I gave ſome of my powder to Dr. Crichton, Dr. Babington, and Mr. Abernethy; gentlemen whoſe extenſive practice and acknowledged ſkill ſufficiently enabled them to judge of its medical properties. They all concur in opinion, that, in its general effects, it agrees with Dr. James's powder and the *pulvis antimonialis*; but that it is more mild, and conſequently may be given in larger quantities, ſeldom producing nauſea or vomiting in doſes of leſs than eight or ten grains.

XVIII. *Obſervations and Experiments undertaken with a View to determine the Quantity of Sulphur contained in Sulphuric Acid; and of this latter contained in Sulphates in general. By* RICHARD CHENEVIX, *Eſq. F.R.S. M.R.I.A.**

IN a paper which I had the honour to preſent to the Royal Society of London, and the ſubject of which was the analyſis of ſome arſeniates of copper and of iron, I had occaſion, in examining many pyrites, matrices of thoſe ores, to remark the very great inequality which prevailed in the reſults of repeated experiments, made with a view to determine the proportion of ſulphur. But I ſoon perceived that the inaccuracy was cauſed by a partial combuſtion and acidification of the radical, through the means of the nitric acid employed to diſſolve the ore.

Having therefore, in the uſual manner, aſcertained what quantity of that ingredient remained untouched, I was forced to ſeek the reſt of it in the liquor which had waſhed the various precipitates. To obtain it, I poured a ſolution of nitrate of barytes into thoſe waſhings when all the other ſubſtances had been carefully ſeparated, and was thereby enabled to precipitate, in a ſtate of purity, the ſulphate of barytes, formed by that earth, and by the portion of ſulphur originally acidified in the firſt treatment of the ore by nitric acid. To come at the knowledge of the proportion of ſulphur contained in a given quantity of ſulphate of barytes, I had recourſe, in the firſt inſtance, to the quantity of ſulphur ſaid by Lavoiſier to be contained in ſulphuric acid; and, in the

* From the *Tranſactions of the Royal Iriſh Academy.*

 next,

next, to the proportions of the latter, announced in the synoptic tables of Fourcroy, as entering into the composition of sulphate of barytes. According to the former of those chemists, 100 parts of sulphuric acid contain 71 of sulphur and 29 of oxygen; and again, in adopting the proportions of the latter, we have 33 per cent. of acid in sulphate of barytes. But if 100 contain 71 of sulphur, 33 must contain 23.43. Consequently, for every 100 parts of sulphate of barytes, I was to allow 23.43 of sulphur. But, by the results of my analyses, I had such quantities of sulphate of barytes as induced me to doubt the accuracy of one or other of the statements by which I estimated the quantity of real sulphur contained in the ore.

No person is better acquainted than our celebrated president with the many difficulties that occur in the analysis of salts in general, particularly with regard to the quantity of real acid they may contain. It has been a work of trouble to the ablest chemists, and they have not always agreed in their results. The proportions announced by Fourcroy may therefore be doubted, in common with those of the other learned operators to whom I have alluded.

The real quantity of acid produced by the combustion of any acidifiable basis, can be determined by one or other of the following methods only: by direct combination in some salt, the proportions of which are already known; or by obtaining, in a state perfectly free from water, the acid resulting from such combustion. To the former method, the general objections against all analyses of salts must apply. The latter is still more defective. It is by no means certain that we have ever yet obtained any acid in a state of perfect siccity, unless we except the phosphoric and the arsenic; for even the crystallized vegetable acids retain a portion of water in their crystallization. It is not that I absolutely deny our having obtained them so; but I say merely that we have no proof. It would indeed be setting narrow bounds to the perfection of nature to assert, that no combustible body could, when saturated with oxygen, assume of itself the state of liquidity; or that the oxide of the particular substance called *hydrogen* must be present to confer that property. Doubtless sulphuric acid may, as well as water, contain in itself so just a proportion of specific heat, as to remain liquid at the temperature of our globe, and under the pressure of our atmosphere. But both water and sulphuric acid being easily volatilized, and having a powerful affinity for each other, it is not easy, if even possible, by distillation, to separate them with sufficient accuracy in experiments of delicate inquiry.

A ſecond ſource of error, therefore, remained open in this branch of the calculation, which gave the proportion of 23.43 of ſulphur, as that contained in 100 parts of ſulphate of barytes.

However, before I would allow myſelf to call in queſtion ſuch authorities as thoſe I have quoted, I inſtituted the following experiments:—In a tubulated glaſs retort I put 100 parts of purified ſulphur, and poured upon them ſtrong nitric acid. A quilled receiver, plunging into a Woulfe's apparatus, was adapted to the retort; and all being well luted, I proceeded to diſtil. The liquor which came over was poured back ſeveral times upon the ſulphur, until the whole was diſſolved. The water which had come over, and that through which the nitrous gas, produced during the operation, had paſſed, were eſſayed for ſulphureous acid, and no traces of it could be found. No ſulphur had been volatilized, therefore no ſuſpicion could remain that all was not converted into ſulphuric acid. The liquors which were in the various parts of the apparatus were united, and to them was added a ſufficient quantity of nitrate of barytes. The whole was evaporated gently; becauſe, though I am well acquainted with the very little ſolubility of ſulphate of barytes, I well know that nitric acid will retain a ſmall portion of it, particularly when formed in a liquor where that acid abounds. In a firſt experiment I obtained 694 from 100 of ſulphur; in a ſecond, 348 from 50; and, in a third, 347 from the ſame quantity. But the ſimple rule of three reduced theſe quantities to 14.6 or 14.4 per cent. of ſulphur contained in ſulphate of barytes; a difference wholly to be neglected. If, therefore, we take 14.5 as the average for the quantity of ſulphur contained in 100 parts of ſulphate of barytes, we ſhall not be far from the truth. From the accordance of theſe experiments, repeated and varied, I had now no doubt but concerning the ſource where I was to ſeek the error, which gave 23.43 as the juſt proportion.

To aſcertain this point, I operated in the following manner:—I prepared ſome lime as pure, I believe, as chemical means can procure it. I digeſted white marble in muriatic acid; and, by leaving an exceſs of the earth, was certain that, by the ſuperior affinity of lime for that acid, nothing elſe had been taken up. Upon trying the ſolution with ammonia, no precipitate took place. By means of carbonate of potaſh, I ſeparated the lime in the ſtate of carbonate; and, after well waſhing the precipitate, expoſed it in a platina crucible to a violent heat till the weight no longer diminiſhed. I am acquainted with no more efficacious method to prepare lime fit for the delicate purpofes of fcientific chemiftry.

One hundred parts of this lime were diffolved in dilute muriatic acid, in the fame platina crucible, previoufly weighed; and then fulphuric acid was added in fufficient quantity. Sulphate of lime was precipitated; and the veffel was expofed to a heat, at firft gentle, to evaporate the liquor; and then, by degrees, raifed to a temperature which could expel every thing but the combined fulphuric acid, and leave the fulphate of lime completely calcined. The crucible with the falt was then weighed, and the augmentation was 76. It appears to me, that, if we admit (and I fee no reafon that we fhould not admit it) that calcined lime and calcined fulphate of lime are wholly exempt from water, it muft be clear that the 76 additional weight were fulphuric acid; and that the fulphuric acid muft in this ftate, more than in any other, approach nearer to what may be termed abfolutely real acid. One hundred parts of calcined fulphate of lime contain, therefore,

Lime - -	57
Sulphuric acid -	43
	100

By the former experiments (thofe made upon fulphur converted into acid, and then united to barytes,) we had the quantity of fulphur contained in fulphate of barytes. By the latter (thofe made by directly combining lime with fulphuric acid) we had the proportion of real acid contained in calcined fulphate of lime. Confequently, by knowing the ratio that fulphate of barytes bears to fulphate of lime, with regard to the acid in each, we fhall arrive at the knowledge of the quantity of fulphur contained in real fulphuric acid. For this purpofe, I attempted to diffolve, in water, 100 parts of fulphate of lime. But finding in this method of proceeding a confiderable inconvenience arifing from the great quantity of liquor neceffary to effect the folution of that falt, I had recourfe to the following expedient:—Upon 100 grains of calcined fulphate of lime, I poured fome oxalic acid, which attracts the bafis with an affinity fuperior to that exercifed by fulphuric acid. Oxalate of lime was here formed; but oxalate of lime is foluble in a very fmall excefs of any acid. A little muriatic acid operated a complete folution; and thus a great quantity of fulphate of lime required but little water to diffolve it. Into this liquor, muriate of barytes was poured, and fuffered to remain fome time, gently heated. By thefe means any oxalate of barytes that might have been formed, was retained in folution by the original

 excefs

excefs of acid; and the entire quantity of fulphate of barytes was depofited. Of the exactnefs of all thefe methods, which I ufed as the inftruments by which I afcertained thefe refults, I convinced myfelf by various preliminary experiments. After the ufual filtration, wafhing and drying at the gentle heat of a fand-bath, I obtained in one experiment 185, in another 183, and, laftly, in another 180. This difference does not exceed the limits of what all perfons converfant in analytic chemiftry will allow to experiments of this nature. We may therefore take 183 as the mean proportion; confequently we fhall fay, that 183 of fulphate of barytes contain the fame quantity of fulphuric acid as 100 of fulphate of lime; and 183 : 43 : : 100 : 23.5. Therefore 23.5 is the proportion of acid in 100 of fulphate of barytes. But we have before feen that 14.5 of fulphur, acidified by nitric acid, form that portion of fulphuric acid contained in 100 of fulphate of barytes, viz. 23.5. We muft now fay, that 23.5 : 14.5 : : 100 : 61.5, and the fourth term will be the proportion of fulphur = 61.5, which combined with 38.5 of oxygen will form 100 of real fulphuric acid.

In neither of the proportions, whether it be of the acid contained in the falt, or of the combuftible bafis contained in the acid, do I agree with the two chemifts whom I have quoted. This juftly excited fome doubts in my mind, and led me to repeat my experiments. Nor fhould I yet be thoroughly fatisfied, if I could not, upon other grounds than by fuppofing inaccuracy in them, account for the apparent differences. We muft ever expect to fee the errors of our predeceffors corrected by men much inferior in abilities, but who, by poffeffing more certain means, fupply the want of genius and invention. At the time in which the experiments were made that determined the proportion of 33 per cent. of fulphuric acid in fulphate of barytes, it was not known that we had never obtained any barytes pure; and that a confiderable portion of carbonic acid refifted the action of every degree of heat that had been applied to carbonate of barytes. The fact was, I believe, firft obferved by Pelletier, but the method of avoiding the inconvenience was pointed out by Vauquelin. He decompofes nitrate of barytes by fire, and a moderate degree of heat is fufficient to expel all the acid and the water. The chemifts I have mentioned performed fynthetic experiments by combining, directly or indirectly, fulphuric acid, and fuch barytes as they imagined to be pure. The conftant fimilarity of their refults is fufficient to prove the accuracy of their operations; but, working upon an impure fubftance, they muft have been contented with a fimilarity of error.

Three

Three causes may exist which are capable of accounting for any variation, whether in *plus* or in *minus*, that might have appeared in the experiments, by which Lavoisier determined the quantity of sulphuric acid obtained by the combustion of sulphur in oxygen gas.

1st, A part of the sulphur may be volatilized during combustion.

2d, All the sulphur may not be converted into sulphuric acid, but part may remain in the state of sulphureous acid.

3d, In rectifying, some acid may come over along with the water; or, *vice versâ*, some water remain with the acid. These considerations will excuse me for having proposed a doubt, where the authority of so great a man exists against the experiments which I have related.

The method which I had used to ascertain the quantity of sulphur in an ore had been practised by Tassaert (*Annales de Chimie*, No. 82: Analysis of Cobalt from Tunaberg); but he calculated according to the proportions of Lavoisier and of Fourcroy. In another paper by Thenard, (*Annales de Chimie*, No. 96,) he states the proportions of sulphuric acid, obtained by treating sulphur with nitric acid, to be

Sulphur	- -	55.56
Oxygen	- -	44.44
		100.00

but in the extract given by Guyton in that number of the *Annales de Chimie*, the mode of operation is not described. Calcined sulphate of barytes is estimated in the same paper to contain

Barytes	- -	74.82
Sulphuric acid	-	25.18
		100.00

which proportions are as near to what I had found as can be expected; for sulphate of barytes does not contain more than 3 per cent. of water of crystallization, and they must be deducted from the quintal.

Having determined with accuracy the proportion of acid in any insoluble sulphate, it is easy to proceed to the determination of that contained in any other sulphate. The docimastic art, or analytic chemistry, in general, cannot however expect to derive such advantages from the knowledge of soluble salts, as of those which, from their insolubility, may be used with accuracy, in delicate experiments, to determine the proportions of the constituent parts of bodies.

But if with this mode of operating we compare the quantities of real acid, ſaid (in thoſe excellent tables with which Mr. Kirwan has enriched the ſcience) to be contained in ſulphuric acid of different ſpecific gravities, each will ſerve as a proof of the validity of the other; and perhaps demonſtrate, that ſulphuric acid, without the intervention of water, may enjoy liquidity at the temperature and preſſure which act upon our globe.

XIX. *Experiments on Platina.* *By* L. Proust.

[Continued from p. 55.]

XIII. *Of the Cryſtallization of Solutions of Platina.*

THE ſmall yellow, red, and ſand-like grains which are depoſited at the bottom of the united ſolutions, and which ſometimes aſſume the octaedral form, are generally taken for muriate of platina. The quantity of theſe cryſtals, which is always very ſmall, does not increaſe by concentration; which, however, ought to be the caſe, if theſe cryſtals were really muriate of platina. No more of them appear in ſolutions from which they have been once ſeparated; and their ſeparation is even complete a long time before the moment when the real muriate of platina begins to cryſtallize.

Theſe cryſtals are a double ſalt, compoſed of muriate of platina and potaſh, as will be ſeen hereafter: they are formed from the ſmall quantity of potaſh which, by the inadvertency of the workmen, remains in the neck of the retorts, when preparing nitric acid, and from that alſo which is carried over in the diſtillation itſelf; for nitre is always found, after the rectification of aquafortis, at the bottom of the retorts. The greater part of thoſe who have operated on platina having in general employed the acids ſold in the ſhops, we need not be ſurpriſed that they ſhould have found cryſtalline depoſits of the kind here alluded to, and that none of them are ever ſeen in ſolutions made on purpoſe with acids well purified.

If ſome of theſe cryſtals be heated in a glaſs tube cloſed at one end, their nature may be ſoon diſtinguiſhed: becauſe the muriatic acid, charged with the oxygen of the metal, is volatilized in gas; while the platina, reduced to the metallic ſtate, and mixed with the muriate of potaſh, remains at the bottom of the tube.

Lewis, in my opinion, is the only perſon who has accurately obſerved the cryſtallization of platina. The ſolution was

was accidentally at that degree of ſaturation which could conduct to this diſcovery; and this circumſtance, as will be ſeen, is the firſt of thoſe neceſſary for the ſucceſs of the cryſtallization in queſtion.

The ſecond condition conſiſts in not leaving an exceſs of acid; and this Lewis accompliſhed; for he ſays (p. 148) that the ſavour of theſe cryſtals was harſh and diſagreeable, but much leſs corroſive than he expected, on account of the great quantity of acid which he had employed. Indeed, if this ſolution be prepared in the ſame manner as prepared by Lewis, it will be found to have the ſavour of ſour vegetables; and whatever diſagreeable taſte it has, ariſes from the iron and copper.

The muriate of platina, when pure, eaſily cryſtallizes; but it is difficult to make it cryſtallize when it is united with foreign muriates not ſuſceptible of cryſtallization, and which oppoſe this tendency. In this there is nothing which diſtinguiſhes it from the moſt cryſtallizable ſalts, and therefore we muſt endeavour to diſcover means for diſengaging it from theſe obſtacles. We ſhall now examine thoſe which may remove the exceſs of acid from a ſolution of platina.

Every ſolution which has been ſubjected to ebullition and concentration is not charged with muriatic acid, and the cauſe may be eaſily perceived: but the caſe is not the ſame with the nitric acid, which ſuffers itſelf to be concentrated; and though it is not attracted by the oxides, which the muriatic acid ſeizes upon excluſively, it adheres to them with ſufficient force; but if it be difficult to expel it, it may be eaſily decompoſed.

To accompliſh this, a little muriatic acid and crude platina muſt be added to the ſolution before it be diſtilled. This acid, which cannot act upon the platina until it has been oxidated, at the expenſe of the nitric acid, determines the decompoſition of the latter, and facilitates the ſeparation in the form of nitrous gas. This operation may be repeated, and when no more gas appears you may be aſſured that you have a ſolution diſengaged from every exceſs of acid. When the ſolution is brought to this term, nothing but muriates different from that of platina can retard the cryſtallization: theſe muriates are thoſe of copper and iron oxydated to a *maximum*.

To ſeparate them, it is indiſpenſably neceſſary to concentrate the ſolution in ſuch a manner, that it may form in cooling a maſs, which moulds itſelf in the retort, but of ſuch a conſiſtence, that, when placed on its ſide, it ſhall ſuffer a portion of the liquor to escape. If the concentration has not come to that point, and if the residuum presents only a thick substance like honey, it must be again placed over the fire. But if it has been carried too far; that is to say, if the separation of the liquid part cannot be effected, you must then add to the mass, which we here suppose to be four or five pounds, some spoonfuls of water, and redissolve it by the help of heat and ebullition. This method, which is very easy for those acquainted with the practice of the laboratory, is the same as that employed for refining sugar, and separating from it the molasses.

After having surmounted this obstacle, the rest becomes easy. Nothing then is necessary but to add a quantity of water nearly equal to the volume of the muriates that have thus been separated; to expose the whole to heat; to suffer the retort to cool in the furnace; and to proceed to two or three successive distillations, to separate the crystals of the muriate of platina.

This method has a double object—the crystallization of the muriate, and its purification. It is evident, and cannot be doubted, that, by repeating this process a sufficient number of times, platina may be as completely purified as by any of the other means hitherto known. But while we admit all the utility of this method, we must allow that it is tedious and troublesome, both in regard to the crystallization of the muriate, and its purification. The others therefore, in our opinion, ought to be preferred; for, to obtain the crystals with the greatest ease, it will be sufficient to concentrate a solution of platina purified by a process which we shall speak of hereafter. If any one, however, wishes to carry this method of purification to the highest degree, he may be certain of having attained to it by making the following experiment:

Dissolve, in a certain quantity of water, a little of this muriate, and try it with ammonia. If the common yellow precipitate of the red oxide of iron be obtained, there still remains some muriate of the latter combined with the platina; but the muriate of platina is pure if no precipitate manifests itself.

It is of importance that the solution should be well diluted, otherwise the ammonia would form with the platina that kind of saline precipitate which is produced by the muriate of ammonia.

At the moment of their concentration these solutions of platina experience a bubbling, which would endanger the retort if the person who conducts the operation persisted in continuing

continuing the fire. These movements arise from the union of a yellow powder, which as yet is only the potashed muriate of platina of which we have just spoken, and which it is indispensably necessary to separate. In regard to the most expeditious means of heating a retort which contains 12 or 15 pounds of matter, and of maintaining the heat, a support of wire ought to be preferred to sand-baths, which I have long banished from my laboratory, even in the preparation of æther, the rectification of acids, &c.

But when it is necessary to operate on a great scale with a certain number of vessels, it is evident that sand-baths placed in a row in a gallery will in this case be attended with more certainty.

XIV. *Of the Composition of the Nitro-muriatic Acid for the Solution of crude Platina.*

No author has said any thing positive in regard to the proportions which will yield the strongest nitro-muriatic acid for this kind of labour. This uncertainty, which stopped me during my first researches, induced me to endeavour to discover these proportions by direct experiments.

As the strength of acids varies in the different manufactories, it appeared to me proper to take them at that degree of concentration at which they are commonly found in the shops. I confined myself therefore to aquafortis of 35 degrees by Baumé's areometer, and muriatic acid of 15 degrees, without giving myself any trouble about the nitric acid which might be present in the muriatic acid, or the iron and the sulphuric and sulphurous acids with which common muriatic acid is often contaminated: for, if I had employed in my researches rectified acids, it is evident that my results could not be applied but with restriction to operations on platina on a large scale, in which case it will always be necessary to employ the acids of commerce.

Exper. I. One pound of nitro-muriatic acid, composed of
8 ounces of nitric acid and
8 ounces of muriatic acid,
dissolved 13 drams of platina out of 24 which I employed.

Exper. II. Nitro-muriatic acid, composed of
8 ounces of nitric acid and
10 ounces of muriatic acid,
dissolved 15 drams of platina.

Exper. III. An acid, composed of
4 ounces of nitric acid and
10 ounces of muriatic acid,
dissolved 17 drams and 54 grains of platina.

Exper.

Exper. IV. An acid, compoſed of
2 ounces of nitric acid and
14 ounces of muriatic acid,
diſſolved 13 drams and 38 grains of platina.

Theſe four experiments were made at the ſame degree of heat; that is to ſay, in four retorts, diſpoſed according to my mode on wire grates, and placed on a furnace. That the reſults may be more eaſily compared, I ſhall here give them in the following table:

Exper.	Black crude platina.	Nitric acid of 35°	Muriatic acid of 15°	Platina diſſolved.
I.	6 ounces	8 ounces	8 ounces	13 drams
II.	6 ounces	6 ounces	10 ounces	15 drams 24 grains
III.	6 ounces	4 ounces	12 ounces	17 drams 54 grains
IV.	6 ounces	2 ounces	14 ounces	13 drams 38 grains

Let us now ſee the conſequences which may be deduced from this table.

1ſt, In the three firſt experiments it is obſerved that the quantity of platina diſſolved increaſes in proportion to that of the muriatic acid, from which it reſults that this acid is the real ſolvent of platina.

2d, If we ſuppoſe that the twelve ounces of muriatic acid, found in the four ounces of nitric acid the quantity of oxygen neceſſary for diſſolving the platina, it is ſeen that a conſiderable exceſs of the latter was uſeleſsly expended in the two preceding experiments; that is to ſay, 4 ounces too much in the firſt, and 2 in the ſecond; and, by the ſame reaſon, the ſolutions of the firſt and ſecond experiments were more acid than thoſe of the third and fourth.

3d, Acids, the ſtrength of which is in a ſimilar proportion, or nearly three parts of muriatic acid for one of nitric acid, muſt form the propereſt proportion for diſſolving crude platina, and conſequently purified platina.

4th, The fourth experiment proves to us that the nitric acid, which was in the proportion of only 1-7th of the muriatic acid, was not able to furniſh to the latter the quantity of oxygen neceſſary to make it charge itſelf with all the platina which it ought to diſſolve; hence it reſults, that in this laſt proportion there is a leſs ſolution of platina, and a loſs of muriatic acid.

5th, As

5th, As the muriatic acid conſtitutes the ſtrongeſt part of the ſolvent of crude platina, in the commencement of the labour care muſt be taken that the heat does not attain to the point of ebullition; becauſe, 1ſt, At that temperature the muriatic acid eaſily acquires aëriform expanſion; 2d, The tendency which it has to ſeparate itſelf from the mixture is ſtill ſtronger when it begins to be oxygenated. From what I have obſerved, the ſolution ought never to begin at a heat ſuperior to 60 or 70 degrees of Reaumur (135° to 157° Fahr.), and it is then effected, without loſs of acid, with all the rapidity poſſible. In a word, the liquor ought not to be boiled till the effervefcence decreaſes.

The five conſequences which we have here deduced, and which are nothing but the reſult of the ſame experiments, give us the true principles of the art of diſſolving platina.

If we now compare the proportion of platina with the nitro-muriatic acid, we ſhall find that the quantity diſſolved riſes ſomewhat above the eighth part of the weight of the ſolvent. This proportion, however, is ſubject to ſome variation; for the ſame acid employed in ſimilar circumſtances diſſolves ſometimes a leſs quantity, though in general it deviates very little from this proportion.

XV. *On the Utility which may be derived from the Products of this Diſtillation.*

In operations on a large ſcale, œconomy being one of the moſt important objects, it may be readily conceived that the acid products in queſtion ought to be carefully collected, and that the concentration ſhould be carried as far as poſſible to increaſe theſe products; for the expenſe of theſe ſolutions will be ſo much more moderate, if the ſame quantity of acids can be employed for a greater quantity of platina. That I might be better enabled to aſcertain the advantages in this reſpect, I made the following experiment:

I diſtilled, to the conſiſtence of concrete honey, a ſolution ſimilar to that of the third experiment, the product of which I carefully collected; and by theſe means I was enabled to obſerve in this operation three different periods.

In the *firſt* the diſtillation, which began at a heat of nearly 70 degrees (or about 150° Fahr.), was accompanied with effervefcence and a diſengagement of nitrous gas, which, as Lavoiſier obſerves, deſerves to be examined, and eſpecially the moment when the oxide of azote or azotic oxide manifeſts itſelf.

In the *ſecond period*, the effervefcence was followed by ebullition; and here no nitrous gas was diſtinguiſhed; a weak colourleſs lourlefs acid only is feen to rife, while the folution makes no other progrefs than that of approaching towards concentration.

When the heat had made the greater part of the fuperfluous liquids pafs over into the receiver, the nitrous gas again fhowed itfelf; and it appeared to me at this *third period*, that the portion of the free acids, which had attained to a certain degree of concentration, exercifed a reaction on the crude platina. But I foon perceived that I could not depend on this advantage; for, after having diluted in water the faline mafs, to feparate the excefs of the crude platina, I found that the pound of nitro-muriatic acid had diffolved only 16 drams and 12 grains of platina; a quantity which differs by more than a dram from that of the third experiment: but notwithftanding this variation, it is not, as I have faid, an eighth above the other.

The nitrous gas of this third period gives us reafon to believe that it does not differ in its origin from that obferved in the concentration of metallic nitrates, when the latter approach condenfation. The augmentation of heat, therefore, only decompofes a remainder of the nitric acid, from which the red vapours in queftion refult.

Let us now return to the diftillation. The liquor found in the receiver weighed 10 ounces and 1 dram: it had no colour, and by the areometer indicated only 9 degrees; while the nitro-muriatic acid, which had produced it, indicated 22 before it was employed. The liquor is fometimes feen flightly coloured, though the operation may have been conducted with the ufual precautions; becaufe the movement of the ebullition is no doubt fufficient to throw a few drops of the folution into the neck of the retort. This, on the whole, is only a little platina, as is indicated at the moment by the cinnamon-coloured precipitate, occafioned by the hydrofulphurated water. Thefe acid products have no fenfible action on the platina; but, to know the advantage which ought to be taken of them, I divided them into two parts, of five ounces and half a dram each.

I added to one of thefe parts 2 ounces of muriatic acid, to the other an equal quantity of nitric acid; and thefe two new mixtures were treated, as ufual, with three ounces of the fame crude platina.

The firft diffolved 4 drams and 16 grains of platina, which would have approached 8½ drams if I had employed the 10 ounces and 1 dram of the product with the addition of 4 ounces of muriatic acid.

The fecond could diffolve only 48 grains, that is to fay, 1 dram

1 dram and 24 grains, had I employed the same product with the addition of 4 ounces of nitric acid. Let us now examine the consequences which may be deduced from these results.

If we suppose that the first mixture could dissolve 8 drams and a half of platina, the muriatic acid added must have found a sufficient quantity of nitric acid to oxygenate it; but 4 ounces of my muriatic acid not being able to dissolve so strong a dose of platina, it thence follows that, in the total product of the distillation, there must have been present two ounces, at least, of the same acid. Since 12 ounces of muriatic acid were indeed employed in the third experiment to dissolve 17 drams of platina, it is evident that to dissolve the half of this quantity, or 8½ drams, 6 ounces of the same acid would have been required; and since we had in the receiver the quantity of 2 ounces of muriatic acid, there is no doubt that there remained in the retort about 10: in a word, that these 10 ounces of acid at 15 degrees, strengthened by the oxygen of the nitric acid, can only dissolve 16 or 17 drams of black crude platina.

Let us now proceed to a valuation of the nitric acid which might be contained in the product of our distillation.

It is seen by the third experiment that 12 ounces of muriatic acid require not less than 4 ounces of nitric acid to enable it to dissolve 17 drams of platina. There were therefore 2 ounces at least in the product of our distillation, since, with the assistance of 4 ounces of muriatic acid added, it could have dissolved 8½ drams of platina, or the half of 17.

It thence follows, that there pass into the receiver, during the whole course of the distillation made with the proportions of the third experiment, nearly 2 ounces of nitric acid at 35°, and an equal quantity of muriatic at 15°, diluted in 6 ounces 1 dram of water; which quantities, united, form the product, being 10 ounces 1 dram. It thence results also, that the 17 drams were really dissolved by 10 ounces of muriatic acid, oxygenated by the decomposition of only 2 out of the 4 ounces of nitric acid employed. Should it however be asked, on seeing this estimate, why 16 or 17 drams of platina could not be dissolved in a mixture of 10 ounces of muriatic acid and 2 ounces of nitric acid? we reply, that the heat necessary to assist the action of this solvent may have soon changed the proportions of the composition of the acid, as in the experiment we have analysed.

In regard to the first acid, formed by the product of the distillation, it is certain that it contains a sufficient quantity

 of

of nitric acid to oxygenate the 2 ounces of muriatic acid which we found in it: but the great quantity of water in which they are diluted deſtroys all their action; and if the addition of 4 ounces of nitric acid reſtores to them ſome ſtrength, it is only, in all probability, becauſe that quantity of acid augments, to a certain degree, the concentration of the muriatic acid.

From theſe data let us try to determine what is the largeſt quantity of platina that can be diſſolved in nitro-muriatic acid prepared according to the proportions above indicated.

To diſſolve firſt the 17 drams and 54 grains, and then the 8 drams and 32 grains, making together 26 drams and 14 grains, we employed 12 ounces of muriatic acid and 4 of nitric acid, and afterwards 4 ounces of the former, that is to ſay, 20 ounces of mixture in the whole. Calculation ſhows, neglecting the 14 grains of platina, more or leſs, that only 5 pounds 11 ounces* of nitro-muriatic acid are requiſite to diſſolve a pound of black crude platina, which is that I employed in theſe experiments. If we conſult the chemiſts, we ſhall find that the greater part of them have fixed at 16 parts the quantity of nitro-muriatic acid which muſt be employed to diſſolve one of platina. If I am not miſtaken, Lewis and count Sickingen were the only perſons able to diſſolve it with 8 parts of ſolvent. But they were obliged to perform two operations to diſſolve that quantity. White crude platina is the kind moſt ſoluble; for I find, in a note, that a pound of nitro-muriatic acid, compoſed according to the proportions indicated, diſſolved 18 drams and 24 grains.

XVI. *Solution of Platina by the Means of a Nitro-muriatic Acid compoſed of Nitric Acid † at 31° and Marine Salt.*

Bergman found that an acid compoſed in this manner diſſolved platina exceedingly well. It will be ſeen by the following experiments that, in an operation on a great ſcale, this ſolvent may be preferred to thoſe compoſed with pure acids. To be able to diſcover the moſt advantageous proportion of acid and ſalt, I made a ſeries of experiments, but I ſhall give an account of thoſe only which can throw light on ſolutions of this nature. As the method of employing this kind of nitro-muriatic acid is not different from that

* We are here, no doubt, to underſtand the Spaniſh pound, which is nearly a ſeventeenth leſs than the French pound; but as it is divided into 16 ounces, or 128 drams of 72 grains, the proportions remain the ſame.

† The degrees here mentioned are thoſe of Baumé's areometer; 31° indicate a ſpecific gravity of 1·279.

followed

followed by others, I ſhall immediately proceed to a comparative table of the reſults:

Exper.	Black crude platina.	Nitric acid at 35°	Marine ſalt.	Platina diſſolved.
I.	6 ounces	6ou nces	6 ounces	20 drams 48 grains
II.	6 ounces	16 ounces	7 ounces	24 drams 12 grains
III.	6 ounces	16 ounces	8 ounces	21 drams 52 grains
IV.	6 ounces	16 ounces	9 ounces	21 drams 36 grains

As the reſult of No. 2. appeared to me the moſt important, I thought it my duty to repeat it, and I obtained 24 drams 16 grains of diſſolved platina.

In No. 4. there was an exceſs of ſea ſalt, which had attached itſelf to what remained of the crude platina.

I made my experiments with purified marine ſalt; but as in operations on a large ſcale the common kind is employed, I repeated them with common ſalt, and found the ſame reſults in equal quantities of the ſolvent; with the ſame variation, however, in the quantity of the platina diſſolved, which might have been remarked in the operation performed with the acids. I repeated alſo the ſame trials, making ſome changes in the two laſt columns:

Exper.	Black crude platina.	Acid at N° 35.	Common ſalt.	Platina diſſolved.
I.	6 ounces	16 ounces	6 ounces	20 drams
II.	6 ounces	16 ounces	8 ounces	22 drams
III.	6 ounces	16 ounces	10 ounces	20 drams
IV.	6 ounces	16 ounces	12 ounces	19 drams

An exceſs of marine ſalt was found here alſo in Nos. 3 and 4.

It is ſeen by theſe two tables, that the quantity of platina diſſolved experiences a diminution whether the proportion of 7 ounces of ſalt per pound of acid be increaſed or diminiſhed. It however appears, that in the experiments where more than 7 ounces were employed, the quantity of platina diſſolved ought to have been, if not greater, at leaſt equal; ſince the nitric

nitric acid ought to have disengaged and oxygenated the same quantity of muriatic acid. I confess that I cannot conceive how an excess of marine salt or nitrate of soda, found in the mixture, could prevent the solution of a greater quantity of crude platina, unless the explanation of this phænomenon be found in an observation I shall here make, or my occupations allow me to undertake new researches on this subject. The observation I allude to is as follows: The residuum of all the solutions, that is to say, the remaining crude platina, exhibits an aspect very different from that of the residuums left by nitro-muriatic acid composed of free acids. [The author promises a continuation of these experiments.]

XX. *On the Analysis of Wine.* *By* C. Chaptal*.

In all wines we distinguish an acid, alcohol, tartar, extractive matter, aroma, and a colouring principle; the whole diluted or dissolved in a portion of water more or less abundant.

1st, *Acid.*—An acid exists in all wines; I never found any which did not present some traces of it. The sweetest and most luscious wines redden test paper, if suffered to remain in them for some time; but they are not all acid in the same degree. There are some the principal character of which is a natural acidity. Those made from grapes not perfectly ripe, or produced in cold climates, are of this sort; while those produced by the fermentation of very ripe and saccharine grapes exhibit very little acid. The acid, then, appears to be in the inverse ratio of the alcohol, which is the result of the decomposition of the sugar.

This acid exists in great abundance in verjuice; and is found in must, though in a smaller quantity. All fermented liquors, such as cyder, perry, and beer, as well as fermented farinaceous substances, also contain this acid: and I have found it even in molasses. Indeed it is to saturate it completely, that lime, ashes, or other earthy or alkaline bases, are obliged to be employed in the purification of sugar. Without this precaution, the existence of the acid would oppose the crystallization of the sugar.

If wine be concentrated by distillation, the extract which results from it in general has a sour and pungent taste. To dissolve and separate the acid, nothing is necessary but to pour water or alcohol over the extract. This acid has a pungent

* From the *Annales de Chimie*, No. 109.

taste,

taste, an odour slightly empyreumatic, leaves a sensation of harshness on the tongue, &c. When well filtered and left in a jar, it suffers to be precipitated a considerable quantity of extractive matter. It then becomes covered with mouldiness, and in that state seems to approach the acetous acid. It may be purified by distillation, which separates from it a large quantity of extractive matter; and it is then less liable to be decomposed by putrefaction.

This acid precipitates the carbonic acid from its combinations: it dissolves with facility the greater part of the metallic oxides: it forms insoluble salts with lead, silver, and mercury; and separates the metals from all their solutions in acids.

This acid forms also an insoluble salt with lime. If abundance of lime water be mixed with wine, it precipitates the acid, which carries with it all the colouring matter. This acid, then, is of the nature of the malic acid. It is always mixed with a little of the citric acid; for, when digested on oxide of lead, besides the insoluble precipitate which is formed, there is produced a citrate, which may be shown in it by the common means.

This malic acid disappears by the acetification of the wine: in well made vinegar, nothing exists but acetous acid. This transformation of the malic acid into acetous acid naturally explains why wine which has only begun to turn sour cannot be employed in manufacturing acetite of lead: in this case there is formed an insoluble precipitate, with the production of which I was greatly embarrassed till I learned the reason of it. For a long time, C. Berard, my associate in my manufactory of chemical productions, added nitric acid to sour wine to give it the property of forming a soluble salt with lead. I was then of opinion that the acid of wine was by these means oxygenated, while the effect really produced was to hasten the decomposition and conversion of the malic acid into vinegar.

The existence of the malic acid in wine in different proportions, serves to throw some light on a phænomenon of the highest importance in regard to the distillation of wines, and the nature of the spirit produced from them. Every body knows, not only that all wines do not give the same quantity of spirit, but that the spirits produced from them are far from being of the same quality. No one is ignorant that beer, cyder, perry, and fermented farinaceous substances, give little spirit, and always of a bad quality. Careful and repeated distillation may, indeed, correct these faults in a certain degree, but never destroy them completely. These constant results,

results, established by long experience, have been referred to the greater quantity of extractive matter contained in these weak spiritous liquors: the combustion of a part of this principle by distillation, ought, it would appear, to be an immediate effect of it, and the acrid and empyreumatic taste, a very natural consequence. But, when I examined this phænomenon more closely, I found that, besides the causes depending on the abundance of this extractive principle, it was necessary to admit another, viz. the presence of the malic acid in almost all these cases. Having distilled indeed, with great care, these different spiritous liquors, I always obtained acidulous spirits, the taste of which was altered by that essentially belonging to the malic acid: it is only by confining one's self to extracting the most volatile liquor, that it is possible to separate a little alcohol free from all alteration; and still it retains a disagreeable odour, which does not belong to pure spirit.

Wines which contain the greatest quantity of malic acid give spirits of the worst qualities. It even appears that the quantity of alcohol is less, as the quantity of the acid is more considerable. If this acid be laid hold of by means of lime water, chalk, or fixed alkali, very little alcohol can be obtained by distillation; and in all these cases the spirit assumes a disagreeable taste of the fire, which does not contribute to improve the quality.

The difference of the spirits arising from the distillation of different kinds of wine depends then, chiefly, on the different proportions in which the malic acid is contained in these wines; and no certain means have yet been obtained of destroying the bad effect which this acid produces by its mixture with the spirit.

This acid, which we have found in grapes at every period of their increase, and which does not disappear from wine till the moment when it degenerates completely into vinegar, would deserve to be distinguished by the name of the *vinous acid*; but, to avoid innovation, we shall retain that of the *malic acid*.

2d, *Alcohol*.—Alcohol forms the real character of wine. It is the product of the decomposition of the sugar, and the quantity of it is always in proportion to that of the sugar which has been decomposed *.

Alcohol,

* I shall not enter into the question, whether alcohol is completely formed in wine, or whether it is a product of distillation; or, in other words, the result of fermentation or of distillation. Fabroni has adopted the latter sentiment, because, having mixed a hundredth part of alcohol with new

Alcohol, then, is more or less abundant in all wines. Those of warm climates furnish a great deal; those of cold climates give scarcely any at all. Ripe and saccharine grapes produce it in abundance; while the wines arising from green aqueous grapes, which contain little of the saccharine principle, afford very little.

There are some wines in the south which furnish a third of spirit: there are several in the north which contain only a fifteenth.

It is the proportion of alcohol that renders wines more or

new wine, he was only able to separate, by means of potash, the same quantity of alcohol. But this experiment appears to me to prove at most that the foreign alcohol added to the wine does not enter into so exact a combination as that which exists naturally there: it remains in the state of simple mixture. We observe a similar phænomenon when highly concentrated alcohol is diluted by the addition of a greater or less quantity of water; for it is known in commerce that this weakened alcohol has not the same taste, which marks, however, the same degree of spiritosity. I consider alcohol, then, in wine not as existing there in an insulated state, and disengaged from all combination, but as combined with the colouring principle, carbon, alkali, extractive matter, and all the other constituent principles of wine: so that wine is a super-compounded whole, all the elements of which may be extracted by chemical means; and when, by the application of heat, these principles are separated, the most volatile rise first, and what first passes is a very light compound, forming alcohol, then water, &c.

Distillation, by successively extracting all the principles of wine, according to the invariable laws of their gravity and of their affinities, breaks and destroys the primitive combination which constitutes wine, and exhibits products which, when united again, cannot reproduce the primitive body, because the heat has entirely disunited and separated the compound into principles which may exist in an insulated state, and which have scarcely any affinity.

In a word, it is of little importance to art whether the alcohol exists or does not exist in wine; the distiller still has invariable principles both in regard to the quality and to the quantity of alcohol which each kind of wine can furnish. Whether the heat, therefore, combines the principles of the alcohol, or merely extracts them from a mass where they are combined, the manner of operating and the results of the operation can receive from them no modification. We see in the distillation of all vegetable matters, and their products, a repetition of the phænomena exhibited by the distillation of wine.

Distillation by heat is not the only means of extracting the alcohol from wine. 1st, The carbonic acid gas which is disengaged by fermentation carries with it, and in a state of solution, a very considerable quantity of alcohol, as I have already shown. 2d, The gas which escapes from champagne carries off almost all the alcohol contained in that wine. 3d, Very spiritous wines, when shaken in the bottle, suffer to escape a very sensible flavour of alcohol. 4th, The wines which furnish most spirit are thought to be most spiritous to the taste. All these facts cannot be reconciled with the hypothesis of the formation of alcohol by distillation, and seem to prove that it exists quite formed in the wine.

The reader on this important subject may recur to the opinion of Fourcroy, published in *Annales de Chimie*.

less generous; it is it which disposes them to, or preserves them from, acid degeneration. Wine turns sour with more facility the less alcohol it contains, the proportion of extractive matter being supposed to be the same in both.

The richer wine is in spirit, the less it contains of malic acid; and this is the reason why the best wines furnish, in general, the best spirit, because they are then freed from the presence of that acid which gives them their disagreeable taste.

It is by distilling wines that all the alcohol they contain is extracted.

The distillation of wine has been known for several centuries; but this operation has been successively improved, and at present has been brought to such a state of perfection, that the spirit trade must derive great benefit from it, and that the process may be applied with advantage to every other kind of distillation. The alembics long used for distilling were boilers with a long narrow cylindric neck, having at the summit a hollow hemisphere, from which proceeded a tube, of no great size, destined to convey the liquor into the worm. Arnold de Villanova seems to have been the first who gave us any accurate notions in regard to the distillation of wines; and to him we are indebted for the first description of this long-necked alembic, models of which are still found among our perfumers.

The idea entertained, that the product of the distillation was the more delicate, subtile, and pure, as it was raised higher, by making it pass through very small tubes, directed the construction of these stills. But distillers were soon convinced that it was not so much the obstacles opposed to the ascent of the vapours as the art of graduating the heat in a proper manner, which rendered the product of the distillation more or less pure. It has been found that, in the first case, the force of the fire changes the nature of the spiritous principles by communicating to them an empyreumatic taste; while, in the second, pure spirit arises, and passes into the worm without alteration. On the other hand, œconomy, an important consideration in the arts, has caused all the changes made in the old processes to be adopted.

Thus the column perpendicular to the boiler has been successively lowered, the capital enlarged, the boiler made wider, and the following general forms have been gradually adopted.

The stills, at present, are a kind of flat-bottomed boilers, the sides of which rise perpendicularly to the height of about twenty-two inches. At this height a contraction takes place, which reduces the aperture to eleven or twelve inches. This aperture

aperture is terminated by a neck some inches in length, in which is adapted a small cover called the *head*, which increases in diameter towards the upper part so as to have the form of a truncated cone inverted. From the angle of the base of this capital proceeds a small tube destined to receive the vapour of the spirit, and to transmit it to the worm with which it is connected. The worm consists of six or seven circumvolutions, and is placed in a cask kept full of water to facilitate the condensation of the vapours: these vapours, when condensed, run down into a tub destined to receive them.

The boilers, in general, are fixed in mason-work up to the part where the contraction takes place: the bottom alone is exposed to the immediate action of the heat. The chimney is placed opposite to the fire-hole; and the ash-hole, which is not very large, is separated from the fire-place by an iron grate.

The boiler is charged with five or six quintals of wine. The distillation is completed in eight or nine hours, and during each operation about sixty pounds of coals are consumed.

Such is the process which has been long used in Languedoc; but though old, and generally adopted, it presents imperfections, which must strike every man acquainted with the principles of distillation.

1st, The form of the boiler produces a column of liquor exceedingly high and not broad, which being exposed to the action of the heat only by its base, is burnt in that part before the upper part becomes warm; bubbles then rise from the bottom, which, being obliged to traverse a colder mass of liquid, are condensed, and again dissolve in the liquor. It is only when the whole mass has been heated gradually that the distillation is established.

2d, The contraction at the upper part of the boiler and the internal bending which it presents in that place, still hurt the distillation: for this part, not being covered by mason-work, is continually exposed to the air, which maintains there a colder temperature than in other parts, so that the vapours which rise are condensed partly against the interior surface, and fall down in drops, or run back in striæ into the body of the still. In this case, the same thing happens which we daily see take place in distilling with a sand-bath: the vapours which rise, striking against the uncovered surface of the retort, which is always the coldest, are condensed, and fall down in striæ into the bottom; so that the same portion of matter rises, falls down, and is distilled several times; which occasions loss of time, expense in fuel, and hurts the quality of the product, which in some cases is altered and decomposed. These phænomena may be rendered very sensible by cooling the upper part of the retort of the sand-bath at the moment when the distillation is most active: the vapours successively become visible in the inside, and drops are condensed on the sides, which soon run down and join the liquor contained in the retort.

Besides, the contraction at the upper part of the boiler forms a kind of eolipile, where the vapours cannot pass without difficulty, which necessarily requires a more considerable force of ascension. This fact has been properly explained by Baumé.

3d, The capital is not constructed in the most advantageous manner: the head acquires almost the temperature of the vapours, which, being strongly dilated, press on the liquid and confine its ascent.

4th, The method of managing the fire is no less improper than the form of the apparatus: the ash-hole is every where too small, the fire-place too large, the door badly shut, &c. so that a current of air is established at the door, and is precipitated into the chimney, passing over the coals. Consequently, a violent fire is required to heat the boiler moderately. The grate is choked up with a thick stratum of combustibles, so that they become nearly useless for want of a free circulation of the air.

[To be continued.]

XXI. *Account of a Descent into the Crater of Mount Vesuvius by eight Frenchmen on the Night between the 18th and 19th of July* 1801.

TO ascend to the summit of mount Vesuvius, which is elevated 3600 feet above the level of the sea, is an enterprise of great difficulty, as it is necessary for nearly half the height to climb an exceedingly steep declivity up to the knees in ashes. Some philosophical men of eminence, however, as Spallanzani, Dolomieu, Dr. Moore, &c. have overcome all these difficulties. Sir William Hamilton, who caused a great many views of Vesuvius to be designed during his long residence at Naples, ascended to the summit of it sixty-two times; but no one, at least since the eruption in 1779, ever ventured to descend into the crater of this volcano, not even Sir William Hamilton, who considered it under so many points of view, and who visited it so many times. It was reserved

reserved for eight Frenchmen to hazard this dangerous enterprise, and to succeed in it completely, notwithstanding the timidity of their guides, the impossibility which the Neapolitans attached to it, and the instances they mentioned of rash travellers who had lost their lives in the attempt, and been swallowed up by the volcano.

To be able to appreciate the danger of this enterprise, it will be necessary to have a correct idea of the form and position of Vesuvius, and of the matters which it throws up. This volcano has the form of a truncated cone, and a part of its base, which is altogether three leagues in circumference, is washed by the Mediterranean; its mouth, or upper base, which is a little inclined to the axis, is 5722 feet in circumference. The earth from the base to half the height consists of vegetable mould mixed with lava and stones which have not been attacked by the fire, tufas, pumice, and calcareous stones, different in their nature and colour according to the different degrees of impression which have been made on them by the fire.

The half of the height next the summit is composed chiefly of pure ashes, but coarser than our common ashes. Till the present time, there have been twenty-four eruptions recorded in history. The first took place in the year 79 after the Christian æra: by these eruptions volcanic matters have been successively accumulated, but by that of 1779 the situation of the crater and of the aperture was entirely changed. The focus or crater is now sunk 200 feet below the upper edges of the mouth of the volcano.

To arrive at the crater, and to observe the numerous spiracles, long crevices, and fires which issue from them in several places, and also the variegated and still smoking matters of which the crater is composed, it was necessary to pass over this space of 200 feet.

The inner sides of the volcano are nearly perpendicular, or exceedingly steep, and composed of ashes, lava, and large calcareous stones; but these lava and stones, as they form no connection with the ashes, cannot serve as any point of support; and when any one is so imprudent as to adhere to this kind of rock, the least motion, the least displacement of any part, makes the whole crumble to pieces. Besides, from the summit of Vesuvius to the crater, the declivity, being exceedingly rapid, cannot be traversed but on all fours, and suffering yourself to glide down amidst a torrent of ashes and lava. But the most dangerous obstacles are those awful excavations, which cannot be passed over without great trouble and difficulty.

Difregarding the terror with which the Neapolitans endeavoured to infpire us, after having received their adieus, as if our feparation had been likely to be eternal, we fet out in a carriage, at half after eleven at night, on the 18th of July, from the hotel of the French ambaffador, fourteen in number, furnifhed with ropes and other articles which we fuppofed might be neceffary, and all in a ftate of the higheft fpirits, which never forfook us, even at times of the moft imminent danger. We arrived about midnight at the foot of Vefuvius; and, having quitted our carriage, mounted well experienced mules, and proceeding one after the other, with adjutant Dampierre at our head, amidft the thick darknefs of night, reached half way to the fteep fummit of the mountain. We had a numerous body of guides, and their lighted torches gave to our expedition a myfterious and folemn air, which formed a ftriking contraft with the mirth and gaiety of the company.

When we had afcended about half way, we were obliged to alight, and to clamber up the fteepeft and moft difficult part of Vefuvius, wading through the afhes up to the knees, till, exhaufted with fatigue, and covered with fweat, we reached the fummit at half paft two in the morning.

The firft thing that ftruck us, as foon as the morning began to dawn, was a moft magnificent fpectacle—a fuperb view of the city and port of Naples, the beautiful hills which furround them, and the vaft extent of the fea by which they are wafhed. After walking round part of the aperture of the volcano, that we might choofe the moft commodious place for defcending, adjutant Dampierre and C. Wickar firft defcended, without any accident, at the determined point. When they had got about a third of the way they were fuddenly ftopped by an excavation of fifty feet, which it was neceffary to pafs. As they found that it was impoffible to obtain any fixed point of fupport on afhes fo moveable; and being convinced that the friction of ropes would have foon deftroyed both the point of fupport and the neighbouring maffes to a great diftance, they refolved to return. Befides, while deliberating on the means of defcending, fome ftones rolling down from the fummit occafioned a general agitation wherever they paffed: adjutant Dampierre found the ground on which he ftood fhake beneath his feet; and he had fcarcely quitted it, calling out to C. Wickar to follow him, when it difappeared. Soon after, indeed, the whole place where they had ftood, and all the neighbouring fmall eminences, crumbled down fucceffively in the courfe of half an hour, and were precipitated to the bottom of the crater with an awful noife.

Before

Before we renounced our enterprise to return to Naples, dejected on account of not having succeeded, we once more walked round the mouth of the crater, and at last discovered a long declivity, pretty smooth though very steep, which conducted to the focus. Without examining the precipices which it might be necessary to pass before it could be reached, C. Debeer, the ambassador's secretary, accompanied by a lazzaroni, set out first to attempt the passage. When they had got half way, amidst a torrent of ashes, which the impression of their feet made to roll down along with them, they found means to fix themselves on the edge of a precipice twelve feet in height, which it was necessary to pass before they could reach the lower declivity. The lazzaroni, frightened, refused to proceed; but, being promised a double ducat, avarice got the better of his timidity; he speedily made the sign of the cross over his whole body, and, having invoked the Madonna, and St. Anthony of Padua, threw himself along with C. Debeer to the bottom of the first precipice: soon after they arrived at another, but being of less height it was passed with more ease. At length, amidst a continual torrent of falling lava, ashes, and stones, they arrived at the bottom of the crater, and stretched out their arms to us, sending forth shouts of joy, which we returned with the utmost satisfaction and enthusiasm.

C. Houdouart, engineer, immediately followed C. Debeer; and after encountering the same difficulties, and passing dangerous precipices, joined him at the bottom of the crater. Being there both convinced of the almost insurmountable difficulty of ascending, they threw themselves into each other's arms, like two friends reduced to the necessity of terminating their lives together in a desert island without any hopes of escaping from it.

They then began, but with cautious steps, to walk round this immense furnace, which still smokes in several places. The intrepid Wickar, who was very desirous to participate in their fate, called out to them to send some one to assist him in passing the two cliffs; but seeing no one coming, and growing impatient, he rushed forward, and rolled down towards them amidst a torrent of stones, ashes, and volcanic matters. Adjutant Dampierre, C. Bagneris physician to the army, Fressinet and Andras French travellers, and Moulin inspector of posts, soon followed, and arrived at the crater after having incurred the same dangers.

Wickar immediately sat down on a heap of scoriæ, and, with that superiority of talents for which he is distinguished, sketched out in profile, with a perfect resemblance, the portraits

traits of the eight Frenchmen who had descended. Each then formed a small collection of the different volcanic matters which appeared to be new or curious, and endeavoured to make a few observations.

Had we been allowed to depend on success, had we not been retarded in our preparations by our timid guides, and if some of us, having only just arrived at Naples, had not been straitened in point of time, our descent would certainly have been much more useful, and the results more satisfactory. However, though ill furnished with means, the following are the observations we were enabled to make:

Reaumur's thermometer, the only instrument we possessed, stood at 12 degrees, on the summit of Vesuvius: the air was cold, and somewhat moist: in the crater the quicksilver rose to 16 degrees, and we experienced the mildest temperature.

The surface of this place, which, when seen by the naked eye, looking down from above, appeared entirely smooth, exhibited, when we were at the bottom, nothing but a vast extent of asperities. We were constantly obliged to pass over lava exceedingly porous, in general pretty hard, but which in some places, and particularly those where we entered, was still soft, and yielded under our feet. The spectacle which struck us most was the numerous spiracles, which either at the bottom of the crater or the interior sides of the mountain suffer the vapours to escape. When we arrived at the crater, we were desirous to ascertain whether these vapours were of a noxious quality: we walked through them, and inspired them several times, but felt no inconvenience from them. The thermometer placed in one of these spiracles indicated 54 degrees, in another it rose only to 22. In all these experiments our instrument was covered with a humid matter, which was soon dissipated in the open air without leaving any traces.

In traversing the surface of the crater, we perceived a focus half covered by a large mass of pumice stone, and which, from its whole circumference, emitted a strong heat. The thermometer placed at first at the entrance of it, and then immersed to as great a depth as the nature of the ground and the heat would admit, never rose higher than 22 degrees. This singularity surprised us, but we were not able to explain it.

The volcanic productions which we observed in the whole crater were lava, exceedingly porous, and which the fire in certain places had reduced to scoriæ. It was of a dark brown colour, and sometimes reddish, but it is rare to find any white. The substances nearest the spiracles are all covered or impregnated with sulphur. This mineral is found very often

often in a ſtate of oxygenation. It is ſometimes white, and ſometimes of a yellowiſh colour, and the ſharp and pungent impreſſion it leaves on the tongue ſufficiently indicates the ſtate in which it is. The burning focus, of which we have ſpoken, produces the ſame reſults. Some baſaltic lava is alſo found, but in ſmall quantity; one ſpecimen only, of a conſiderable weight and beautiful poliſh, attracted our attention.

On the north ſide of the crater there are two large fiſſures, one of which is 20 feet in depth, and the other about 15. They are ſhaped like an inverted cone. The matter with which they are covered is entirely ſimilar to that on the reſt of the ſurface. They emit neither ſmoke nor heat; yet ſome ſulphurous productions plainly ſhow that the fire in theſe places has not long been extinct.

When we had finiſhed theſe few obſervations, it was neceſſary that we ſhould think of returning. The deſcent is far leſs laborious than the aſcent; for it is difficult to climb eminences where the points of ſupport are ſo moveable. Beſides, people cannot aſcend but one at a time in ſucceſſion, after long intervals, for fear of burying under a torrent of volcanic matters thoſe who follow, as the foot, when moved, diſplaces the aſhes, &c. to the diſtance of thirty feet round.

When we arrived at the two precipices, we were obliged to aſcend by mounting on the ſhoulders of a man placed at the bottom, and laying hold of a ſtick held by another at the top, and to reſt our feet no where but in a very gentle manner. At length, by prudence and caution, we reached the ſummit of Veſuvius without any accident, but exhauſted with fatigue, and ſo covered with aſhes and ſmoke, as to be ſcarcely diſtinguiſhable. Our ſix companions, who had not deſcended into the crater, were overjoyed when they ſaw us again, and ſupplied us with ſome refreſhments, of which we had great need.

When one grand difficulty is ſurmounted, inferior ones are overlooked, as of little importance. In leſs than twenty-five minutes we again deſcended, having confirmed, after examining various ſtones, this obſervation, that Veſuvius is the only known volcano which throws up from its bowels primordial ſubſtances, without being altered by the fire, and ſuch as are found at preſent in banks and veins.

At half after eight in the morning we arrived at Portici, the inhabitants of which were much ſurpriſed to ſee us return all ſafe. Their delicious fruits, and their excellent wine called *lacryma Chriſti*, ſoon made us forget our fatigue, and we then proceeded to Naples, which we reached in ſafety.

The

The result of this excursion, which was only an experiment, can be of no further use than to show the possibility of reaching the crater, and to open the way to it to philosophers, naturalists, and chemists, who, by exploring this immense furnace of nature at their leisure, will find a variety of matters which will afford an ample field for the application of their chemical knowledge, and may enable them to make discoveries interesting to the arts and the sciences.

The names of the eight Frenchmen, in the order in which they descended, are as follow: Debeer, secretary to the ambassador Alquier; Houdouart, chief engineer of bridges and causeways attached to the army of Italy; Wickar, painter; Dampierre, adjutant-commandant; Bagneris, physician to the army of observation; Fressinet and Andras, French travellers; and Moulin, inspector of posts.

XXII. *Observations on a destructive Kind of Insects found in some Parts of the Russian Empire, and in the Bannat of Temeswar.* By *Professor* PALLAS *.

WE are indebted to my friend Brunnich for the first account of this destructive insect, hitherto observed only in the Bannat of Temeswar in Europe. Linnæus, in the short description which he gave of it † from an account transmitted to him, compares it to that small gnat (*culex equinus*) which infests horses. Its principal colour is blackish; its breast projects in a round form, and the hinder part of the body is oval; the legs are pretty long; the thighs towards the body are white, but black underneath; the covering of the thighs mostly white, at the extremity blackish, the joints of the legs black, and the wings transparent.

According to this description, I found the greatest similarity between the gnat of the Bannat and the Russian *moschkara*, or stinging gnat ‡, which, after an attentive comparison, I find to agree perfectly with the creeping gnat (*culex reptans*) of Linnæus; and I suspected, from what I knew of the abundance and destructive nature of the Siberian insects, that the gnat of the Bannat might belong to the larger and more pernicious kinds in the warm mountainous districts of that country. I was, however, deceived in my conjecture, by

* From *Neue Nordische Beyträge*, vol. ii.

† *Culex Lanio* Linn. *Mantiss. Plantar.* ii. p. 541.

‡ *Bibio sanguinarius*, Pallas *Reise*, part i. p. 193; Append. p. 475; by the Russians called *moschka*; in Siberia improperly *mokriza*; on the Volga *moschkara*.

by several circumstances mentioned in Griselini's Natural History of the Bannat*, and his description of this gnat, which is totally unlike that of Brunnich. According to Griselini, it is larger than the common gnat, and perfectly like it in the conformation of its body; its head is also armed with feelers, and it has a small proboscis; the breast is blackish at the top, somewhat hairy, and on the lower part white; the fore legs are short, and the hind largest; the extremity of the body consists of lead-coloured rings, each of which is surrounded by a black line Griselini says nothing of white rings on the feet, and at the end names the insect, contrary to the expression of his own description, a gad-fly (*œstrus*).

In this uncertainty, the last circumstance led me to conjecture that Griselini, perhaps, might not be a very experienced entomologist; and as he described his insect in the time of harvest, and as the real destructive gnats appear on the other hand in the spring, I suspected that Professor Brunnich might have described the proper insect, and that Griselini had only seen some of the other kinds, which appear in large swarms. My doubts on this subject induced me to apply to Baron von Born, to whose information I had often been before indebted; and by his means I obtained a small collection of real Columbach gnats, among which I distinguished, on the first view, the Russian *moschkara* of a considerable size, and even an insect which I had so often wished to procure when collecting plants and insects in the Altai mountains, and on the Volga.

They are small flies, as thick but much shorter than the common gnats, and which, according to their characteristic marks and form, have a greater resemblance to the *bibiones* of Geoffroy than to gnats. Their breast, which is strong, projects in a round form; the head is flat, and at the top ends in a sharp oblique edge, has at the sides oblong eyes, and below tapers to a proboscis, which ends in two sharp points, not unlike the sting of the gnat; the feelers are of a brownish yellow colour, thick and sharp pointed, placed near each other on the forehead; the legs are much shorter than those of the gnat; the thighs and part next the joints of the legs are all white, and only blackish towards the ends; on some the thighs towards the body were whitish; the extremity of the body, which appears very dry and contracted, is surrounded with rings of brown, but the breast appears black,

* *Griselini Versuch einer politischen und naturlichen Geschichte des Temeswarer Bannats*. Vienna, 1780; part ii. p. 125.

shaded

ſhaded with grey; the wings, which are pretty large and broad, appear whitiſh, with their veins, and lie over each other.

Such I found the Columbach gnat, and I have ſeen the Siberian *moſchara*, or that common in the neighbourhood of the Volga, perfectly ſimilar, only that it was ſomewhat ſmaller, and had not ſo broad white rings on the legs. I entertain no doubt, therefore, that they are both of the ſame ſpecies. I am alſo convinced that the *culex reptans* of Linnæus is nothing elſe than the ſame inſect which is found in the northern countries, ſomewhat ſmaller and not in ſuch abundance.

Linnæus found great multitudes of the latter in the mountainous diſtricts of Lapland, whereas in Sweden they are more uncommon. According to his obſervations, they attack people principally towards ſun-ſet, crawl over their whole bodies, even into the mouth, noſe, and eyes, and cannot be driven away like other gnats, by ſhaking or blowing. If his deſcription in the *Fauna Suecica* be compared with ours, it will be found very little different.

Theſe troubleſome flies are found here and there in the northern foreſts of Ruſſia, but ſingly, and for the moſt part they run about among the hair of the cattle. But on the Volga, below Kaſan, where that river begins to flow between woody mountains, and approaches a warmer diſtrict, particularly from the neighbourhood of Simbirſk to Saratof and Kamyſchenka, they are met with from the end of May to the beginning of June in ſuch aſtoniſhing multitudes in low buſhy places, and the woody hills ſheltered from the wind, that they ſeem to fill the atmoſphere, like hail, falling blindly and with violence againſt the face, which is affected as if ſand were thrown againſt it; they fly into the eyes, noſe, and mouth, adhere pertinaciouſly to the ſkin, and with their blunt trunks pierce it often in ſuch a manner as to occaſion pain, ſo that a bloody puncture remains, though without itching. Fiſhermen, hunters, and all thoſe who, by their employments, are expoſed to the open air, or who travel, furniſh themſelves about the above period with a large net-cap ſoaked in birch oil, becauſe it has been obſerved that the *moſchka*, however blindly they ruſh againſt every thing, never venture to fly againſt thoſe open nets which have been ſoaked in the above ſtrong-ſmelling oil. Without theſe means it would often be impoſſible to open the eyes. When the inſect has an opportunity of faſtening itſelf on the ſkin unperceived, and of ſucking the blood, it fills its belly till it appears like a blown-up bladder, and cannot be removed but

by

by killing it. As people cannot open the mouth without several of them getting into it at once, it often happens that they bite or squeeze them between the teeth in spitting them out, and one then feels, without wishing it, that their entrails have a sweet melleous taste. At the end of June they almost all disappear, and are not again seen till August; but at that period they are far less abundant.

These flies are equally numerous in the Ural mountains, but they are still more so in the neighbourhood of the woody mountains of the southern part of Siberia beyond the lake Baikal. In the month of June, people are tormented with these vermin, even on the mountains, till they reach the cold summits, where there are no woods. In the latter end of summer, however, they are not to be seen in those districts. Travelling from Jakusk to Ochozk, they are to be again found in immense multitudes as soon as one has passed the Aldaan, and perhaps they are to be found also in North America.

It is commonly reported, in the Siberian and Ural mountains, that these small insects, with the assistance of the *gad flies*, which abound about the same period, torment horses, and other cattle, in such a manner as to occasion their death when they run about in the forests, and have no opportunity of escaping to the open country, or to a fire, the smoke of which would drive them away. Instances of this, however, are not so frequent in these countries as, according to every account, they must be in the Bannat of Temeswar. The cause of this, perhaps, is to be ascribed to the great size of these gnats in the latter country, and perhaps also to some poisonous quality connected with their sting; for I have observed, even in Siberia, that their sting sometimes occasioned on the human body large tumours, which scarcely disappeared at the end of forty-eight hours.

I shall not here repeat what Griselini, whether true or false, has related of these insects. I should have considered many of the particulars which he states, as containing too much of the marvellous, had not Baron Born, in his Letters, confirmed the accounts given of the destructive nature of these vermin in that country. "They come to the Bannat of Temeswar," says he, "by millions, as soon as the bloom appears on the trees in the spring; fall upon the cattle, creep through the rectum, the nostrils, and the ears, into the most interior parts of their bodies, and kill them in four or five hours. When the animal is opened they are found adhering in swarms to the lungs and to the bowels, which are completely inflamed. They continue three or four weeks, and are followed by immense clouds of dragon flies, *libellula* *grandis* and *ærea,* which devour every thing, and are in their turn devoured by the ſwallows, which find them in ſwarms. About the end of July or the beginning of Auguſt the ſecond brood appear, and accompanied with the like circumſtances."

XXIII. *Memorandums, Hints, Precepts, and Recipes, for the Uſe of Artiſts, Manufacturers, and others; including various ſhort Proceſſes either new or little known.*

[To be occaſionally continued.]

On ſome Kinds of Gilding.

THE value which has always been ſet on articles made of gold, has induced artiſts to deviſe means for giving to works of other metals, by covering them with a thin ſtratum of gold, a greater value than they really poſſeſs. Hence the origin of gilding.

In real gilding, the metal is either immediately covered with leaf gold, or an amalgam is formed of gold and quickſilver, with which the metal is rubbed over, and the quickſilver is then ſublimated by means of heat.

In gilding, a great deal depends on cleaning properly the ſurface of the metal to which the gilding is to be applied, becauſe the union of the gold with the metal becomes then more intimate. Silver, copper, braſs, and pinchbec, may be eaſily gilded, either by means of gold leaf, or of gold amalgamated with mercury; but iron and ſteel with difficulty; nor can ſo durable a gilding be given to them as to other metals by any proceſſes yet known. The cauſe of this is, that the ſurface of iron or ſteel during the proceſs of gilding cannot be kept thoroughly clean.

Gilding Iron or Steel with Gold Leaf.

In this proceſs the metal muſt be heated before the gold leaf can be applied, which expoſes it to the riſque of having its ſurface oxidated, beſides the danger of having the temper at the ſame time brought too low, when ſword-blades, daggers, and the like, are ſubjected to the proceſs.

Gilding with Amalgam and Nitrate of Mercury.

In gilding iron or ſteel by means of an amalgam, the difficulty and the danger of miſcarrying are ſtill greater; for, as the metal has no affinity for the mercury, an agent muſt be employed to diſpoſe the ſurface to receive the gilding. For this

this purpose a solution of mercury in nitrous acid (*aquafortis*), or what the workmen call quicksilver water, is applied to the parts intended to be gilded: the acid, by a stronger affinity, seizes on a portion of the iron, and deposits in the place of it a thin coating of mercury, which will not refuse a union afterwards with the gold amalgam that may be applied; but by this process the surface of the metal is injured by the nitrous acid, and the union of the mercury is very slight, so that a bright and durable gilding cannot be obtained.

Gilding with Amalgam and Sulphate of Copper.

Sometimes a solution of sulphate of copper (*blue vitriol*) is applied, with a camel's hair pencil, to the parts of the steel intended to be gilt. By a chemical action exactly similar to what we have described as taking place when a solution of nitrate of mercury is employed, a thin coating of copper is precipitated on the metal. Copper having an affinity for mercury, a kind of union may by this means be effected between the amalgam and the iron or steel, as the case may be.

In whichever of these ways the amalgam be brought into union with the steel, the surface is injured by the action of the acid employed, and still a heat sufficient to volatilize the mercury must be afterwards employed. Most artists, therefore, follow the first process, that of applying gold leaf, and burnishing it down on the article while hot; which, though a laborious process, is attended with less risk of the surface being injured.

An improved Process for gilding Iron or Steel.

This process, which is less known among artists than it deserves to be, may prove useful to those who have occasion to gild iron or steel. The first part of the process consists in pouring over a solution of gold in nitro-muriatic acid (*aqua regia*) about twice as much æther, which must be done with caution, and in a large vessel. These liquids must then be shaken together: as soon as the mixture is at rest, the æther will be seen to separate itself from the nitro-muriatic acid, and to float on the surface. The nitro-muriatic acid becomes more transparent, and the æther darker than they were before; the reason of which is, that the æther has taken the gold from the acid. The whole mixture is then to be poured into a glass funnel, the lower aperture of which is small; but this aperture must not be opened till the fluids have completely separated themselves from each other. It is then to be opened; by which means the liquid which has taken the lowest place by its greater gravity, viz. the nitro-muriatic acid, will run off; after which the aperture is to be shut, and the

the funnel will then be found to contain nothing but æther mixed with the gold; which is to be put into well-closed bottles, and preserved for use. In order to gild iron or steel, the metal must be first well polished with the finest emery, or rather with the finest *crocus martis* or *colcothar of vitriol* and common brandy. The auriferous æther is then to be applied with a small brush; the æther soon evaporates, and the gold remains on the surface of the metal; the metal may then be put into the fire, and afterwards polished. By means of this auriferous æther all kinds of figures may be delineated on iron by employing a pen or fine brush. It is in this manner, we believe, that the Sohlinger sabre blades are gilded. As every artist has not an opportunity of procuring a recipe for the preparation of the nitro-muriatic acid and æther, we shall here give processes for both.

Preparation of the Nitro-muriatic Acid.

In a sufficient quantity of nitrous acid dissolve as much sal-ammoniac as it can take up in a cool place. Into this nitro-muriatic acid put the gold, after it has been reduced to fine powder by being filed or beat into thin leaves, and deposit it in a warm place until it is dissolved. This solution has a golden-yellow colour, and gives a purple tint to animal bodies.

Preparation of the Æther.

Put into a large retort one pound and a half of the strongest spirit of wine, and pour gradually into it two pounds of the strongest sulphuric acid; shaking the retort a little each time the acid is poured in. The liquid will become hot, and the vapours, disengaged with force, will diffuse a penetrating and agreeable odour. When you have poured in all the sulphuric acid, rinse the neck of the retort by pouring in half a pound more alcohol; and having mixed the whole thoroughly, suffer it to stand for some time well closed up.

Then place the retort with the mixture in a sand-bath, and adapt to it a capacious receiver, and make a fire under the retort. The heat however must be slow, and so moderate that the receiver may never be heated by it. The distillation must be continued until a sulphurous odour, instead of the agreeable one, is observed at the mouth of the receiver. For this purpose the receiver may be now and then emptied, by which means the odour can be at all times observed.

When the distillation is ended you will have obtained an æther; which however always contains partly a little acidulous water, and partly some spirit of wine.

To obtain it perfectly pure, or to rectify it, put it again into a retort; and having added to it a little dissolved alkali, in order to absorb the acid, place it in a very gentle sand-bath, and adapt to it a receiver. If the half of it be then driven over at a very gentle heat, you will obtain very pure æther.

What remains after the first distillation consists, for the most part, of sulphuric acid impregnated with sulphurous gas and with carbonaceous matter furnished by the decomposition of the alcohol.

This residuum may be again employed for the preparation of æther. For this purpose pour over it good spirit of wine, but always a third less than before, and proceed to distillation: if you then rectify, you will obtain good æther.

Another good Process for Gilding.

Æther is proper, as has been above remarked, for taking up gold from the nitro-muriatic acid. Those to whom this preparation of æther, for the purpose of gilding, appears too difficult, may use in its stead essential oils, such as spirit of turpentine, oil of lavender, and the like; which will also take gold from its solution.

To prepare the Alcohol.

As it is sometimes difficult to procure alcohol ready prepared, the following method of rectifying weak spirit of wine will be found very advantageous to artists:

Take well dried potash and pour over it spirit of wine: the latter will not unite with the potash, but the water which it may contain will be taken up by that alkali. The spirit of wine is then to be poured into another glass, and subjected to the same operation as before. This process is to be repeated till it is observed that the potash is no longer very moist. Such spirit of wine is exceedingly strong, but rendered a little impure by the potash, as will appear from its yellowish colour. It must therefore be poured into a retort, having a receiver adapted to it, and distilled to a fifth part over a slow heat. What comes over is alcohol.

Cold Gilding of Silver.

Besides the above methods of gilding, there is also a cold gilding of silver, which can be speedily done, and with little trouble.

Dissolve gold in the nitro-muriatic acid, and dip some linen rags in the solution; then burn them, and carefully preserve the ashes, which will be very black, and heavier than common.

mon. Rub theſe aſhes on the ſurface of the ſilver either with the fingers or a piece of leather or of cork, by which means the fine particles of gold they contain will be left on the ſurface of the metal. If the ſilver be then waſhed, ſo as to remove the remaining part of the aſhes, the ſilver will have ſcarcely any appearance of gilding; but on being burniſhed it will aſſume a beautiful gold colour. This method of gilding is very eaſy, and requires very little gold.

Gilding of Braſs.

Fine inſtruments of braſs, in order that their ſurface may be kept longer clean, may be gilded in the following manner:

Provide a ſaturated ſolution of gold, and, having evaporated it to the conſiſtence of oil, ſuffer it to ſhoot into cryſtals. Theſe cryſtals muſt then be diſſolved in pure water, and the articles to be gilded being immerſed in it are then to be waſhed in pure water, and afterwards burniſhed. This proceſs may be repeated ſeveral times till the articles have been well gilt. A ſolution of gold cryſtals is preferred to a mere ſolution of gold, becauſe in the latter there is always a portion of free acid, which will not fail to exerciſe more or leſs action on the ſurface of the braſs or copper, and injure its poliſh.

Lackering of Braſs.

Various articles made of braſs have ſometimes an appearance as if they were gilded. This appearance, however, is produced by means of a ſolution of gum-lac in ſpirit of wine, with which they are rubbed over. As long as the lac laſts, they retain their ſplendour. Theſe articles, however, are attended with this inconvenience, that they muſt never be cleaned with a ſtrong bruſh, or ſcoured with chalk and the like, but only be wiped with a ſoft rag; for as ſoon as the lac is rubbed off they loſe their brilliancy. A varniſh of this kind may be prepared in the following manner:

Diſſolve two ounces of very pure and fine gum-lac in 48 ounces of alcohol, and place the ſolution in a ſand-bath expoſed to a moderate heat. To prevent the too abundant evaporation of the ſpirit of wine, as well as the burſting of the glaſs, a piece of bladder ought to be bound over the latter, and a few holes made in it with a needle. In another glaſs diſſolve, in the ſame quantity of ſpirit of wine, an ounce of dragon's blood in grains. When both the ſolutions are completed, mix them together; then put three grains of yellow wood into it, and ſuffer it to remain there twelve hours in a moderate heat: after which ſtrain the liquor through

filtering

filtering paper, and preserve it for use in a clean glass bottle. To give this lac-varnish a high gold colour, yellow wood is preferable to every other substance. If the varnish is intended to be pale, and not to change the colour of the brass, the yellow wood may be omitted; but if a stronger colour be required, a half more of the yellow wood may be added.

XXIV. *Letter from Professor* PICTET, *F. R. S. Professor of Natural Philosophy at Geneva.*

To Mr. Tilloch.

SIR, Paris, October 23, 1801.

DURING my stay here, on my return from England, I have had the satisfaction of meeting often the celebrated Italian professor Volta, who, in company with his friend Brugnatelli, professor of chemistry at Pavia, has undertaken a philosophical journey to this metropolis to give and to collect information. I have been repeatedly a witness to a most ingenious and interesting series of experiments, by which professor Volta demonstrates the evident identity between what has been called the *galvanic fluid* and common electricity. A short exposition of the leading facts will not, perhaps, be unacceptable to your philosophical readers.

He begins by showing, with the help of his conductor and a common electrometer made with two hanging bits of straw, a new and fundamental fact in electrics, viz. that different metals (and even other substances) mutually impart to one another, *by a mere dry contact*, a certain quantity of electricity: after this exchange, made rather by a kind of impulse than by affinity, they are found to possess opposite states of electricity, easily demonstrable by the common test with sealing-wax.

He shows afterwards, that if a system of two different metals be disposed in the following order—copper, zinc, copper—without any intermediate conducting liquid, no change of the electric state takes place in that case; because the influence of the copper on zinc on one side, is balanced by a similar and contrary influence on the other. But if a piece of wet paper be put between, then the electrical equilibrium is immediately destroyed by the contact, and the phænomena appear.

The better the conducting liquid happens to be, the greater is the effect. Thus brine or acidulated water act more strongly than

than pure water, *merely as better conductors*. This fact he also proves by direct experiments.

On these three fundamental facts he then builds his whole fabric. Let the apparatus be a pile, a series of cups, or a trough, his theoretical positions answer equally well. He explains, among many curious phænomena, why the Voltaïc pile charges a large Leyden jar, by a single instantaneous contact, much more than the electrical machine in the same circumstance.

He thus transfers, from a pile of 100 or 200 couples, to a very large Leyden jar, and even to a battery, by a single contact, which lasts not above 1-10th of a second, the same degree of electrical charge which the electrometer shows to exist in the pile, and the faculty of giving a shock equally strong. Those experiments require the most perfect continuity in the conductors; metallic *chains* must therefore be excluded from the circuit.

The frogs and other animals put in motion after death by what has been hitherto called the *galvanic influence*, are in his theory but mere electrometers. He shows, in particular, that a half frog, disposed in the common way, is almost indefinitely susceptible of muscular contractions by merely inverting alternately its position relative to the direction of the electrical current: that is, when blunted, for instance, by a number of discharges passing from the right leg to the left, it recovers its whole susceptibility if it be but half turned round, so as to reverse the course the fluid takes; and this repeatedly for a long while, till bordering on putrefaction.

A commission chosen among the ablest electricians in the National Institute, has been named to examine both the theory of the learned professor and the experiments on which it is grounded. He intends to collect soon, in one single performance, the most essential facts and inferences he has published at different times on the subject; and in the mean time my friend Mr. De la Metherie prepares for the *Journal de Physique* an account more circumstantial than the slight sketch I am now forwarding to you, rather fit, I confess, to excite than to gratify your curiosity.

I am, &c.

M. A. PICTET.

XXV. *Letter to the Editor of the Philosophical Magazine.*

Respected Friend,

THE subjoined paper has appeared to me, on perusal, sufficiently interesting to deserve publication in the Philosophical Magazine. It is another proof (which I was not possessed of at the time my essay on the same subject appeared) of the attention which this question had obtained on the continent; and as, notwithstanding a considerable similarity in the introductory part, our inquiries appear to have been directed to distinct though closely-connected branches of the subject, each of these papers may prove to the reader a useful commentary on the other. The insertion of it will therefore be acceptable to

Thy Friend,

Plaistow, 16th of the Tenth Month, 1801. L. HOWARD.

On the Influence which the Sun has on the State of the Barometer. By J. J. HEMMER *.

WHEN philosophy, in modern times, emerged from that lamentable state of darkness in which it was for so many years involved, and philosophers employed the more rational and certain way of observation and experience for examining the nature of things, all the assertions and opinions of the antients respecting the influence of the heavenly bodies were subjected to more accurate investigation. Some, as being contrary to experience, were totally rejected; others were retained, and placed beyond all doubt, as they evidently coincided with nature; and others were classed among uncertainties, as experience furnished nothing decisive for or against them. Among the number of the latter, according to most modern philosophers, is the opinion that a part of the wonderful variations which we observe in the barometer are to be ascribed to the effects of the sun and moon, and other heavenly bodies. This opinion, indeed, after a long series of experiments and researches, appeared to Pascal, Garcin, Wallis, Halley, De la Hire, Mariotte, Woodward, Leibnitz, Mairon, Bernoulli, Muschenbroek, De Luc, &c. so little supported by proofs drawn from nature, that they did not think proper to mention it. This, however, is much to be wondered at in regard to De Luc, as he says in his work on the atmosphere, that it is attracted with more or less force by the planets, according to their different distances.

* From the *Transactions of the Electoral Academy of Sciences at Erfurt*, vol. vi.

 Some

Some celebrated philoſophers, however, of the preſent day have confidently believed, that if the variations of the barometer were carefully compared with the different poſitions and diſtances of the heavenly bodies, and particularly of the ſun and moon, it would be found that many conſiderable variations of the ſtate of the mercury in the Torricellian tube are effected by them. For, as the flux and reflux of the ſea depend on the ſun and moon, it cannot be doubted that in the atmoſphere of the earth, which is much lighter than the water of the ſea, movements of the ſame kind, which have an influence on the barometer, muſt take place. The firſt experiments on this ſubject were made about fifteen years ago by the celebrated Lambert, who employed for that purpoſe eleven years obſervations of the barometer made by Doppelmayer of Nuremberg, and compared them with the apogeum and perigeum of the moon. He was not able, however, to obtain any certain reſult, and conſidered the period of the obſervations as too ſhort.

Toaldo, who was furniſhed with much more abundant materials, reſumed the ſubject, and by means of the forty years obſervations of the Marquis de Poleni and ſon, with eight years of his own, had a ſeries of forty-eight years obſervations. After a laborious compariſon of theſe with the apſides, ſyzigies, and quadratures of the moon, and with the paſſage of the moon and ſun through the ſigns of the zodiac, he thought he diſcovered undoubted proofs of a perceptible influence of the moon and ſun on the barometer. M. Friſi of Milan endeavoured to controvert this aſſertion, which had been the reſult of ſo much labour, and maintained that the variations of our atmoſphere have no ſimilitude to the flux and reflux of the ſea, and can admit of no compariſon, as the latter is regular and invariable, the former only temporary. The flux and reflux of the ſea, in regard to time and duration, are ſo certain, that there are tables of them for all the principal ſea-ports, which never err; whereas the variations of the atmoſphere are at the ſame time in different places, and in the ſame place at different times, ſo great, that where it rains at one period for three or four months it is ſometimes as long without rain. It ſufficiently appears at preſent, from mathematical calculations, that during the paſſage of the moon or ſun, from the horizon to the meridian, the daily height of the barometer cannot be changed more than $\frac{1}{5}$ of a line, Paris meaſure, by the effect of the former, and about $\frac{1}{7}$ by the effect of the latter.

To many, however, Friſi's reaſons did not appear ſo concluſive as to make them give over their reſearches on this ſubject;

ject; and in particular, moſt of the members of our meteorological ſociety, among whom M. Steiglehner, Planer, and Chiminello, have taken great pains. The firſt ſays, that he found, by ſeveral comparative obſervations, that the greateſt fall of the barometer does not happen in very remote places at the ſame time, but that it is earlier towards the weſt, and later towards the eaſt, and that the difference of the time is nearly equal to the difference of the meridians of the places; an aſſertion which indeed deſerves to be more accurately examined.

M. Planer obſerved the barometer for a whole year, ſix times every day, or every fourth hour of the natural day, at two, ſix, and ten in the morning, and at the ſame hours after noon; and found, in general, that the barometer, between ten in the morning and two in the afternoon, and between ten at night and two in the morning, was leſs in its riſing, and greater in its fall; and that the contrary was the caſe between the hours of ſix and ten in the evening and morning. M. Chiminello obſerved the barometer twenty-two times a day for three years, but he left a chaſm in the night which he ſupplied by calculation. The principal poſitions which he thence deduced, are, that the barometer falls towards noon, as well as towards midnight *.

* In examining a twelve-months' Regiſter, kept by —— Dunbar, Eſq. near the banks of the Miſſiſippi, in N. lat. 31° 28′, and long. 91° 30′ W. of Greenwich, which, for the greater facility of compariſon, I have laid down, with others, on a ſcale in the manner I have heretofore exhibited, there occurs a remarkable inſtance of a diurnal variation. For the ſpace of about four days before, and ſix days after the ſummer ſolſtice, the barometer regularly riſes from about 9 P. M. to about 6 A. M. then falls till the return of the former hour in the evening, then riſes again as before, &c. in alternate periods. In the firſt four days the direction is *aſcending*, and the elevation of a line drawn through the mean is about $\frac{6}{100}$ of an inch. In the latter ſix days the mean line is perfectly horizontal, the elevation each night amounting to $\frac{2}{100}$ and the depreſſion each day to the ſame, but occupying double time. The times above given are thoſe at which the obſervations were made, but it is probable that the maximum and minimum each day correſponded rather with the times of ſun-riſe and ſun-ſet. The firſt period of four days was dry, with a temperature of 92° in the middle of the day. This ended in a thunder ſtorm, on the 21ſt of the month, with 0,82 inches of rain. The barometer, after this, roſe $\frac{4}{100}$ in the night, then remained moſtly ſtationary, with cloudy weather, until the evening of the 26th, beginning a ſecond period of ſix days, during which briſk winds at S. S. E. and S. W. prevailed with rain every afternoon or evening, amounting, in all, to 1,7 inches. Temperature 85°. The whole occupied juſt the ſpace between full and new moon; and there are traces of the operation of this periodical influence in other parts of the Regiſter.

I ſhall not preſume, at preſent, to aſcribe this variation entirely to planetary influence; but the facts are worthy examination in that reſpect. L. H.

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The obſervations of theſe three men afford ſome indications of the influence which the heavenly bodies have on the barometer, and to which M. Planer aſcribes part of the effects: but he is far from proving this; for the number of the places the obſervations were made at, which M. Steiglehner compared, were not above three, and conſequently too few to determine the queſtion. The caſe is the ſame with the ſix obſervations of M. Planer, made daily, as has been obſerved by M. Chiminello. And might not what is aſcribed to the period of four hours have happened in the laſt hour, or the laſt half-hour? Chiminello's experiments are of more importance, but for want of night obſervations they are ſtill defective and imperfect.

In this reſpect, however, I was more fortunate, as I had an eaſy and ſure method of following all the variations of the barometer, and even the ſmalleſt, by night as well as by day. I had in my poſſeſſion, at that time, a barometer, belonging to the Electoral Phyſical Muſeum, invented lately by M. Changeux, and called a barometrograph, which being connected with a clock ſhows the ſtate of the mercury every four minutes, by means of a point impreſſed on a moveable plate. I began on the 21ſt of May 1785 to make uſe of this excellent inſtrument, and it appeared to me, by repeated obſervation, that a continual variation took place towards noon as well as towards midnight. By more accurate obſervation, I found that this variation in the mercury conſiſts, at leaſt, in an inclination to fall. But that I might be more certain in treating a queſtion of ſo much importance, I reſolved to compare all the obſervations from the firſt day to the end of the year, which are printed in five volumes of our Ephemerides. In that time I found that the paſſage of the ſun over the meridian by day and by night had taken place 446 times, and conſequently as many variations of the barometer were to be conſidered. From the moſt accurate compariſon, the following three rules preſented themſelves:

1ſt, When the ſun paſſes the meridian, the barometer, if in the act of falling, continues to fall, and the falling is accelerated.

2d, When the ſun paſſes the meridian, the barometer, if in the act of riſing, falls, or becomes ſtationary, or riſes more ſlowly.

3d, When the ſun paſſes the meridian, the barometer, which is ſtationary, falls, if it has not riſen before or after being ſtationary; in which caſe it uſually becomes ſtationary during the ſun's paſſage.

The firſt rule had only one exception, the ſecond none, and

and the third fix. The laſt were all in the night-time except one. On this occaſion, the barometer during the ſun's paſſage over the meridian ſtood ſtill, though it fell before and after. When theſe ſeven exceptions are deducted from the 446 variations there remain 439, which accord with the rules. When I ſay, however, that theſe variations took place during the ſun's paſſage over the meridian, I do not mean by that expreſſion the very moment when the ſun is exactly in the meridian, but the whole period when he is near it.

All theſe variations took place between eleven o'clock and half after one, conſequently within the ſpace of an hour and a half; except ſix, five of which happened half an hour or a quarter before eleven, and one about an hour after noon; which, indeed, muſt be aſcribed to a particular cauſe, which either haſtened or retarded the effect of the general cauſe. Of the above-mentioned 439 variations, 54 took place after twelve o'clock, and all the reſt before one.

As it is therefore certain that the barometer during the paſſage of the ſun over the meridian is always inclined to fall, it cannot be doubted that the cauſe of ſo invariable a phænomenon, which follows ſo remarkable laws, and recurs at the ſame time under the like circumſtances, muſt be conſtant and invariable. It is at preſent well known that the principal atmoſpheric cauſe of the falling of the barometer is heat and evaporation. But it appears, on cloſer examination, that neither of theſe can produce the phænomenon in queſtion. What M. De Luc, however, ſays *, that the leaſt height of the barometer, when no particular cauſe intervenes, always takes place about the hotteſt part of the day, which in every ſeaſon is about $\frac{3}{4}$ of the artificial day, might lead to a ſuſpicion that the falling of the barometer about noon proceeds from the heat. For, as there is a conſiderable increaſe of heat about noon, a perceptible falling of the mercury about the ſame time muſt be connected with it, until, in the above-mentioned part of the day, the mercury reaches its loweſt ſtate, and the heat attains to its maximum. But if we admit that the greateſt heat commonly takes place about the $\frac{3}{4}$ of the day, experience forbids us to aſcribe to the ſame period the greateſt falling of the mercury. I have therefore conſtructed a table in which the loweſt ſtate of the mercury for each day of the month, between noon and the evening, or night, is given from the above-mentioned barometrographic obſervations.

* *Recherches ſur les Modifications de l'Atmoſphère*, vol. ii. p. 94.

obſervations *. This ſtate of the barometer takes place when the mercury ceaſes to fall and afterwards begins to riſe, or before riſing is ſtationary. When it does not riſe after noon, but ſinks continually or by intervals, the loweſt ſtate after noon muſt be at midnight. The caſe is the ſame when it firſt riſes after noon, and afterwards falls towards the end of the natural day. But when it continually riſes after noon, though this movement may be interrupted by its once becoming ſtationary, the loweſt ſtate when it falls early, or when this does not happen, is to be conſidered as taking place about twelve at noon. When in this caſe a frequent falling takes place in the morning, I have obſerved that it was neareſt noon and the $\frac{3}{4}$ of the day: when the quickſilver roſe and fell ſeveral times between noon and evening, I marked ſeveral hours, without further examining at which the ſtate of the barometer was loweſt.

Now it appears from theſe tables, that in 222 days the loweſt ſtate of the barometer correſponded only ten times with the $\frac{3}{4}$ of the day; on the other hand, it deviated from that period ten times about half an hour, at other times a whole hour, and often half a day; and conſequently De Luc's rule, at leaſt according to my obſervations, is not correct.

We may therefore aſk thoſe who aſcribe the falling of the barometer to heat, Why ſhould this cauſe be ſo cloſely confined to the period between 11 in the morning and the middle of the firſt hour after midnight? Why does it not take place earlier or later, when the mercury riſes continually the whole day, or ſeveral days either fall a little towards noon, or at leaſt become ſtationary or riſe ſlowly? and, Why does not this rather take place ſeveral hours after noon, the time at which the heat generally is the greateſt?

But that heat is not the cauſe of this phænomenon is proved by the falling of the mercury at midnight; which is no leſs common and certain than the falling at noon: on account of the cold, it ought then rather to riſe or to fall.

We may aſſert the ſame thing of vapour as of heat, as theſe have the cloſeſt connection with each other. As a certain accumulation of vapours diſſolved in the atmoſphere makes the air lighter, and obliges the barometer to fall, the power with which it acts on the inſtrument muſt be in proportion to its quantity. When it obliges the barometer,

* This table has been omitted in this tranſlation, becauſe the reſults here given are ſufficient.

therefore,

therefore, to fall towards noon, why does the latter ſo often riſe afterwards? What then leſſens the quantity of theſe vapours when the heat, which by its nature promotes their ſolution, ſtill increaſes, or at any rate does not become leſs? How does it happen that the barometer falls in the night? as about that time the vapours are, for the moſt part, precipitated from the atmoſphere. Why do they exerciſe the greateſt force exactly at midnight? and, By what law of nature are they more connected with that than with any other period?

It may here evidently be ſeen, that the conſtant falling of the barometer about noon and midnight can be occaſioned neither by heat nor by the effect of vapours.

Beſides theſe two cauſes, and the winds, we are acquainted with no others in our atmoſphere which can make the barometer to fall. But as the wind often becomes calm about noon and midnight, or, when it blows, is of ſuch a nature that it does not make the column of air which preſſes on the barometer lighter, it is evident that the falling of the mercury about theſe times cannot ariſe from the wind.

What then remains but to ſeek without our atmoſphere for the cauſe of this phænomenon? It is evident that it is to be found in the ſun; for, as by its attraction it moves the water of the ocean, it muſt exerciſe a ſtill ſtronger action on the column of air; and from what is known reſpecting the flux and reflux of the ſea, the whole phænomenon may be eaſily explained. In the firſt place, as the atmoſpheric flux and reflux themſelves muſt depend on the ſun's paſſage over the meridian, it muſt alſo take place both at the diurnal and the nocturnal paſſage of the ſun; becauſe the flux and reflux of the atmoſphere, like thoſe of the ſea, muſt happen at the ſame time on oppoſite ſides of the terreſtrial hemiſphere. In the laſt place, the atmoſpheric flux which produces the falling of our barometer is only an effect of the flux which takes place between the tropics, where the air during the paſſage of the ſun aſcends, and therefore proceeds thence from us, as happens in all the tides of the ſea which lie beyond theſe circles. That theſe tides, which ariſe by communication, are tranſmitted more ſpeedily and by a longer duration in the atmoſphere than in the ocean, may be concluded from this circumſtance, that the particles of the air have leſs gravity and leſs mutual adheſion.

XXVI. *Letter*

XXVI. *Letter from* L. W. Dillwyn, *Esq. to* W. H. Pepys *jun. Esq. respecting the Effects of the Oxymuriatic Acid on the Growth of Plants**.

My dear Friend, Higham Lodge, Sept. 7, 1801.

THE few experiments I made on the effects of the oxygenated muriatic acid on the growth of plants, I conceive to be too insignificant to merit much attention: however, at thy desire, I here relate them, and give thee the few thoughts that have occurred respecting them.

From what M. Humboldt asserts respecting raising seeds so very rapidly by means of the oxymuriatic acid, the question occurred to me, whether it might not be successfully used for raising early crops of mustard and cress, radishes, &c. and therefore early in March I sowed two patches of each (as well as of some other plants) in a sheltered situation in the open air, and kept one of each constantly moist in the common way, and the other with a mixture of one large tea-spoonful of oxymuriatic acid to a pint of water; and in no instance did there appear to be any difference with respect to the time of germination.

I then tried the same experiment on two pots of balsams in a hot-bed, and also on some coxcombs. Those balsams moistened with the diluted oxymuriatic acid all came up in 90 hours, whilst none of the others appeared till near 30 hours after; and the combs came up about in the same proportion. I shall next give the particulars of an experiment I made in warmer weather in the open air. On the 2d of May I set four different lots of garden beans, with three in each lot, in the following manner:

Lot 1. I soaked in 24 drops of oxymuriatic acid, diluted with two large tea-spoonfuls of water, for seven hours, and, immediately before I took them out, added six drops of acid more.

Lot 2. I soaked in twelve drops of acid and two spoonfuls of water for the same time, and, immediately before I took them out, added three drops more.

Lot 3. I soaked for the same time in plain water.

Lot 4. were not soaked at all.

These were then all planted at the same depth in a sheltered border, and lots 1, 2, and 3, kept constantly moist with oxymuriatic acid diluted with water in the proportion of one large tea-spoonful to a pint.

On May 13, one of No. 2 came up, and on the morning of the 14th one each of Nos. 1 and 3, of which No. 1 was

* Communicated by Mr. Pepys.

forwardest;

forwardest; and in the course of the day the others of lots 1, 2, and 3, appeared. On the 16th they all looked finely; but those of lot 1 were largest, and those of lot 2 larger than lot 3: those of lot 4, which were kept watered with plain water, did not come up till the 18th, or perhaps one of them on the evening of the 17th. On June 5 they were all nearly fully grown; but those of No. 1 were considerably stunted, and those of lot 2 somewhat so, and neither of them were so fine as those of lots 3 and 4.

In the latter end of spring and beginning of summer I tried the same experiments on some other plants, but, from not being constantly at home, could not make them with sufficient accuracy; however, enough so to determine that those moistened with dilute oxymuriatic acid came up first.

In the experiment on the beans, we find that those soaked and moistened with the dilute acid came up some days before those that were only watered in the common way, and that those which had the most acid at first were the forwardest, and apparently most flourishing plants, though afterwards they the most rapidly declined; and therefore I conclude (though more experiments are wanting on this subject positively to determine), 1st, That the diluted oxymuriatic acid *only* in a certain temperature hath the effect of promoting an earlier germination of seeds than would otherwise take place: and, 2d, From the plants at first growing more rapidly than those watered in the common way, but afterwards declining so as to be surpassed by them, that the oxymuriatic acid possesses no nutritive quality, but acts merely as a stimulus, and therefore cannot be applied advantageously to the purposes of horticulture. I am very truly and respectfully

Thy sincere Friend,

L. W. Dillwyn.

XXVII. *Observations on the Question, whether mixed Metals can be distinguished by the Smell.* By M. Bœttiger*.

Werner†, Wiedemann, and other chemists, place smell among the external characters of fossils. They divide minerals into odorous and inodorous, and make the first class to consist of swine's stone, some other earthy bitumens, and sulphureous and arsenical pyrites, which when rubbed or struck

* This paper first appeared in the *German Mercury*, published by Wieland, March 1800: it was afterwards inserted in the *Magasin Encyclopédique*, No. 10, an. 10, from which we have translated it.

† Sur les Caractères extérieurs des Fossiles, § 204, p. 2080. Lenz: tom. i. p. 19.

ſtruck exhale a certain odour. The antients were acquainted with the ſmell of the ſemi-metals, experiments on which were made by Boyle. But I do not find in any of the mineralogical works of the moderns that the purity or alloy of the noble metals, ſuch as gold and ſilver, can be aſcertained merely from the external character of ſmell. Among the antients, however, ſmell was a principal character of metals. The fineſt compoſitions were diſtinguiſhed merely by their odour. Arrian on Epictetus * ſays expreſsly that an expert money-changer (banker) muſt neceſſarily have the ſenſe of ſmelling in an exquiſite degree. "Behold," ſays Epictetus, "in money affairs, when our intereſt is concerned, the art which has been invented, and the means which the changer employs to try the money. He tries them by the ſight, by the touch, by the ſmell, and even by the hearing †; for he throws

* I. 26. p. 110. edit. of Schweighauſer.

† And why not by the taſte? to go through the whole circle of the organs of ſenſe. This method was alſo known to the antients. The uſe of the touchſtone, *lapis Lydius* (ſee Theocritus, xii. 36; and Pliny, xxxiii. 8. with obſervations by M. Schneider, *Analecta ad Hiſtoriam Rei Metallicæ*, p. 3.), was, however, the moſt common method of trying gold, which when it had undergone this teſt was called *obryzum*. Pliny ſpeaks alſo, in the place above quoted, of trials by fuſion. There is a curious paſſage in Herodotus, vii. 10. reſpecting the true meaning of which mineralogiſts, in my opinion, have not yet agreed. Artabanus ſaid to Xerxes before the Perſians aſſembled, that pure gold could be diſtinguiſhed by rubbing it againſt other gold. Weſſeling, p. 312. 30. conſiders this idea ſo abſurd, that inſtead of *to rub* (παρατριψωμεν) he prefers reading *to compare* (συγκρινηται). Larcher, in his tranſlation of Herodotus, vol. v. p. 272, adopts, in part, the opinion of Weſſeling on this paſſage: he ſays the word in the Greek text is παρατριψωμεν. But it is not by rubbing gold againſt gold that we can diſtinguiſh which is the beſt; I do not therefore heſitate to adopt the reading of Sancroft's manuſcript, επεαν δε ἑτερω συγκρινηται, which indicates the only manner of aſcertaining pure gold before the uſe of the touchſtone was known. I confeſs, however, that παρατριψωμεν has always appeared to me preferable, becauſe συγκρινηται has the appearance of an interpolation, and may have been ſubſtituted by ſome one who did not underſtand the other word. My opinion is confirmed by C. Girod-Chantram, who, when conſulted on this paſſage, declared that it might be retained without any change, becauſe, pure gold being ſofter than that alloyed with copper, the leaſt pure metal muſt make an impreſſion on the other, and thus afford the means of aſcertaining it. The proof of this aſſertion was eſtabliſhed at the time when it was propoſed to the National Convention to coin new money in France without any alloy. The advantages of this method were found to be counterbalanced by ſome inconveniences, one of which was, that friction is more deſtructive to pieces of pure metal than to thoſe the hardneſs of which is increaſed by alloy—*Rapport par Loyſel*, Sept. 15, 1793. The National Convention charged the Academy of Sciences to make experiments, which fully confirmed the effect in regard to continual friction on pure and alloyed metals. "It thence reſulted," ſay the commiſſioners in their report, "that the loſs experienced

throws the money on the ground and remarks the sound. He is not even contented with sounding it once or twice, and the great attention he employs renders him a musician." However, as the antients generally employed the purest gold for coining money *, it appears that the proof by smelling did not relate to pure gold and silver, but to the ignoble metals which might have been mixed with them. For all the passages of antient authors which speak of the smell of metals † allude to an artificial composition which could be distinguished by its odour. It appears that it was in particular the composition so foolishly sought after by the Romans, to which, by way of preference, they gave the name of Corinthian brass, and which they made use of for drinking-cups and their most valuable table services. We must no doubt rank among this number the pretended *vasa Batiaca* which Alexander the Great found among the effects of Darius, and which being composed of bronze brought from India could not be distinguished from gold but by the smell ‡.

I am acquainted with two other passages which this remark may serve to explain. Martial, in the 60th epigram of his ninth book, describes a swaggering fellow, who having a great desire to purchase, but an empty purse, harassed the toymen of the Campus Martius or Septum at Rome by always cheapening and never buying. The poet, besides other things, says of him, "He consults his nose respecting the smell of Corinthian brass §."

Cicero, in his Paradoxes, speaks of the connoisseurs of his time who purchased Corinthian bronzes with incredible avidity, and in examining them employed all their knowledge in the arts. He exhibits one of these haughty Romans in the attitude of examining with great earnestness a night-chair of Corinthian brass ‖; for night-chairs were made of the most precious metals in those periods, when gold was

perienced by pieces of fine silver in circulation compared with that of alloyed silver would be nearly in the ratio of 3 to 2; and that of pure gold compared with alloyed gold would be as 7 to 3."—*Rapport de Loysel*, p. 19.

* At the commencement of the 17th century, Louis Savot explained exceedingly well the metallurgic part of numismatics in his *Discours sur les Médailles antiques*, inserted in the 11th volume of Grævius's *Thesaurus*. See Eckel.

† *Doctrina num. vet.* tom. i. p. 22. See Casaubon on Suetonius in the Life of Vespasian, c. 23; Beckman on Aristot. Mirabil. auscult. c. l. p. 99, 100; and his History of Inventions, vol. iii.

‡ Aristot. Mirabil. auscult. c. l. p. 97.

§ Consuluit nares, an olerent æra Corinthon.

‖ *Parad.* v. ii. Si L. Mummius aliquem istorum videret matellionem Corinthium cupidissime tractantem.

 sometimes

sometimes cheaper than silver *, and when Corinthian brass sold at a higher price than either. The wit of this passage is entirely lost, if we do not suppose that the connoisseur examined the value of the bronze and of the vessel, by holding his nose over it.

This circumstance, on various accounts, deserves to be carefully examined by our metallurgists. The first question would be, to know whether copper, mixed with other metals, can really be distinguished by the smell. A late French traveller ascribes to the inhabitants of one of the Kamtschatdale isles so acute a smell, that they were able, he says, to distinguish by it immediately copper alloyed with gold †. The second would be, to know the nature of this alloy.

The question respecting the manner in which the Corinthian brass was mixed and fused with gold or silver, has never yet been finally determined. Wiegleb ‡, and other chemists, made experiments, by fusing some pieces of antient bronze; but even if they could show with precision the manner of mixing them, their experiments would still be doubtful, because the antients were acquainted with several bronze mixtures §; and consequently, when they fused antient bronze, they were not certain of its being Corinthian brass.

Addition by the French Translator.

Having thought it of importance to give particular interest to this paper, by submitting it to some celebrated metallurgist at Paris, I begged C. Gillet Laumont to communicate to me his ideas on the question proposed by M. Bœttiger: "Whether copper, mixed with other metals, can be distinguished by the smell, and to determine the nature of that mixture." He was so obliging as to give me the following answer:

"Several metals have a smell which is peculiar to them: that of *iron*, *lead*, *tin*, and in particular *copper* ||, may be distinguished; and there can be no doubt that alloys composed of these metals must emit different odours. But the sense of smelling among mankind in a civilized state being less perfect

* *Plin.* lib. xxxiii. 12. See Caylus *Recueil d'Antiquités*, vol. ii. p. 309.

† See Croset *Nouveau Voyage à la Mer du Sud.* Paris, 1783, p. 258.

‡ Acta Academiæ Moguntinæ, 1777, p. 50.

§ For example, *orichalcum*, *electrum*, &c. There still exist a great many medals of *electrum*.

|| It is often necessary, in order to call forth their odour, to rub them, or at least to examine them at a degree of heat equal to that of the human body. Care must be taken, in particular, to leave a sufficient interval between each experiment, that the organs of smell may be entirely freed from the sensations produced by the preceding.

and

and lefs conftant, there are certainly many emanations which man cannot catch, or which would require a particular ftudy which it is feldom his intereft to apply to.

It is poffible, alfo, that fome perfons, highly favoured by nature, might acquire, by long habit, an exact knowledge of the quantity of copper that exifts in any alloy; but it is certain that thefe perfons could not communicate to others at a diftance, or perhaps even prefent, the kind of fenfations which they experience, and their different modifications, according to the nature and proportion of the mixtures.

The means of diftinguifhing alloys, and the proportion of alloys, by the help of fmell, appear then to me to form a fecondary character, which ought not to be neglected, and which, in certain individuals, may, by habit, be brought to perfection, but which, as it cannot be eafily communicated, can never become an effential and comparative character.

It is poffible that, in the time of Alexander, among the means eafily put in practice, that of fmell was the fureft for diftinguifhing the Indian vafes of bronze from thofe of gold.

In regard to the paffage of Herodotus, where it is faid "that pure gold was diftinguifhed by rubbing it againft other gold," it appears to me that the word παρατριψωμεν muft be retained, but the word *againft* ought to be changed into *by the fide of*. The paffage then becomes clear, and only fuppofes the employment of a hard body proper for retaining the traces of thefe metals. This explanation alludes to that kind of proof ftill practifed by our goldfmiths, with plates of metals alloyed in known proportions, which are called teft plates; but to be certain of their affay, they make ufe of a touch-ftone; that is to fay, a common black ftone, not fufceptible of being attacked by the acids which they pour upon it, to afcertain the degree of alteration in the traces left by the alloys.

XXVIII. *Some Obfervations on the Agitations of the Lake of Derwent Water, and its floating Iflands. By a Correfpondent.*

To the Editor of the Philofophical Magazine.

Kefwick, October 7, 1801.

PASSING through this place with little time to fpare from purfuits not philofophical, I can only obtain imperfect information about fome curious phænomena hitherto but little noticed; by fome as little regarded as tales of enchantment,

 and

and as little underſtood by others as the moving of the waters of the pool of Betheſda.—I mean the agitation of the lake of Derwent water, and its floating iſlands. Pennant ſays, the water of Derwent water is ſubject to violent agitations, and often without any apparent cauſe, as was the caſe this day: the weather was calm, yet the waves ran a great height, and the boat was toſſed violently with what the people in the neighbourhood called *a bottom wind.* Dalton ſays, Darwent lake is agitated at certain times during a calm ſeaſon, by ſome unknown cauſe. Mr. Croſsthwaite has been aſſiduous in obſerving any circumſtance that might lead to a diſcovery of the cauſe; but his reſult is, that nothing has occurred yet to throw light on the ſubject. He then gives various dates when the lake was diſturbed; amongſt the reſt he ſays,

Aug. 9, 1789. The lake in very great agitation, white breakers upon large waves, &c. without wind.

Oct. 1792. The water much agitated.

My informant, a boatman of the name of Walker, ſays he has frequented the lake for about 27 years, and heard of perhaps 13 iſlands, though previous to that time for 20 years none had appeared. One roſe laſt year, and two this year, which came up within two days of each other. The firſt roſe the 11th of September, and part ſunk the 27th of the ſame month. Sometimes one comes up and is down again in 24 hours, and ſometimes they ſtay two months. One of them in the year 1798 was 180 yards long, 50 yards wide, and ſtaid at the top of the water ſix weeks. One was pierced, and found ſeven yards thick. They frequently burſt, and are rent ſo wide, that boats can ſail up and land paſſengers at the edges of them, to walk about on the iſland. One was a foot high perpendicular of land above the level of the water. They are connected always by one ſide to the graſſy turf at the edges of the lake. When, within a few days after their firſt appearance, a pole is run three to ſix feet into them, and drawn out again, the air for ſeveral ſeconds bubbles up like a pot boiling violently; a ſmell ariſes like gunpowder, and it has been ſaid that with a candle you might light a bottle-full that was once collected. I was this day rowed to one, a part of which continued above the water, which gradually deepened all round it, and found it of an oval ſhape, about 10 yards by five yards. The top was a fine firm mud, thick ſet with a young aquatic plant I did not know. We puſhed the boat-hook about four feet into it. The firſt part, about two feet, appeared all mud, then leſs firm, and full of partially-decayed leaves and roots of trees and plants; the water bubbled up for

a few

a few seconds about two inches high, occasioned by the air that followed the hook out.

The rising of these islands is always accompanied with a bottom wind; yet the bottom wind appears frequently without them; and generally after a month or six weeks of dry weather the lake is sometimes partially, sometimes wholly agitated, accompanied by a roaring noise (the probable effects of echo in a calm in that situation). The waves are not long and rolling, but irregular shaped like mountains, 20 inches to two feet in height, like water jumbled in a tub; they frequently strike the boat like a rock, and break in spray from head to stern, not always when it is perfectly calm, but also with a gentle breeze, barely enough to ripple the surface. They frequently indicate a change of weather from fair to rain, and the greatest degree of agitation is always in the deepest water.

Having thus minutely described all the particulars I have time to collect, I shall leave it to others to find a better explanation than the following. When we consider that half the bottom of the lake is covered with a fine mud impervious to air, resting upon half-putrid vegetable matter; that being attached to the sloping banks, all air that is generated will endeavour to rise to the highest part, but cannot then get out; that great part of the bottom is a clean bed of pebbles and stones pervious to air; that the islands begin to rise generally in from 9 to 12 feet water, and are of considerable surface; that they have been measured 21 feet thick, with clear water under them; and that the most violent effects have been in the summer months—Is it not probable that the air set at liberty by putrefaction is enough to raise these large masses, so thick, and so little heavier than water itself; to contract and expand their extremities enough to give the violent and irregular motion to the superincumbent waters, by their flowing off, their reflux, and their breaking through—and that, more or less, whether they rise to the surface, or only part of the way, to find their equilibrium in the surrounding fluid? Wishing our ignorance of the cause of this curiosity of our own country may soon be removed, and wishing success to every investigation of the phænomena of nature, I will conclude with the signature of

D. I. P.

XXIX. *Description of a new Water Engine, invented by Dr. West, of the Island of Jamaica*.*

THE screw-pipes *a* and *b* (see Plate V. fig. 1.) turn in different directions, and the area of the lower must be to that of the upper in the *inverse ratio* of the perpendicular heights, and as much more as will be necessary to overcome the friction. The pipe *b* ought to widen a little from below upwards, in order to accommodate itself to the falling water.

Working of the Engine.

The valve of the pipe *a* at its upper orifice *c* being opened, (while the cup or bason *f* is supplied by any stream of water, a part of which is wished to be raised to a higher elevation,) the pipe *a* must be filled with water. It will continue full, though its lower orifice be open, if the valve fit close; and, water continuing to be poured into the cup *f* from the pipe *d*, if a motion on the axis be then communicated to the pipes in a direction opposite to that of the steam issuing at *e*, the whole will continue to move round so long as water is supplied by the pipe *d*, and part of the water will ascend in *a*, and be delivered into the upper circular gutter *g*, *g*.

XXX. *Description of a new Hygrometer.*

SIR, *To Mr. Tilloch.*

I HEREWITH enclose a scheme for an hygrometer, which appears to me to be an improvement on the common (oat-beard) hygrometer in some respects, though, being more complex, it is more likely to be out of order. I have made several of them, and find they act tolerably well. Should you think the invention worth noticing in your publication, you are welcome to make what use of the paper you think proper. Two other kinds of hygrometers I have also made, but they do not answer so well as I think, with some improvement, they might be made to do. In one the index moves under the spirals of a watch spring, by which the number of revolutions may be reckoned; on account of the stiffness of the steel, the index does not move with freedom. In the other the index moves between the coils of a piece of paper cut in a spiral form, on which paper are figures marked to register the place

* Communicated by Mr. Robertson Buchannan, engineer, Glasgow.

it

it points to. Both theſe inventions are for the ſame purpoſe as the winding hygrometer, which is to avoid the confuſion occaſioned by the oat-beard having more than *one* turn or twiſt in its courſe from wet to dry. I remain, Sir,

Your very humble Servant,

B. M. FORSTER.

Deſcription of the Winding Oat-beard Hygrometer.

The principal differences in this hygrometer from thoſe uſually made of oat-beards are the following:

The graduated circle (Plate V. fig. 2.) is numbered *completely round*, inſtead of *half round* each way, as uſual. On the top of the oat-beard * is cemented a circular piece of paper A, on which is fixed a tubular piece of ſtraw B, which is capped with another piece of paper C. D is a ſupport to keep the beard upright, made of card paper. On the ſtraw tube, or little cylinder, is faſtened a piece of fine ſilken ſtring, on which is hung a pea, to ſerve as a weight to keep the ſtring ſtretched. As the oat-beard untwiſts with moiſture, the index (made of ſtraw) moves the ſame way round as the hand of a watch, and, thus moving, coils the ſtring round the ſtraw tube or axis; by which means the number of revolutions from any time obſerved may be known; and thus the confuſion will be avoided which is occaſioned by the index moving more than once round, which it does in paſſing from extreme dryneſs to wet.

The ſtring may be ſo placed as to wind *up* when the index moves the contrary way, that is, from moiſt to dry, if the maker ſo chooſes; and in this caſe, the circle muſt be numbered the contrary way from the above.

The oat-beard is *fixed*, and the index is not to be turned or ſet to a certain point, as is the caſe with the common hygrometer, by a contrivance behind the caſe.

The method of keeping a regiſter of this hygrometer will be thus:

When on the upper part of the axis there is				
One coil, and the index points at 6, ſet down				1—6
Two coils	-	-	6 -	2—6, &c.

If the circle be divided into 100 diviſions, the reckoning will be,

No coil, and the hand at 10, ſet down					10
1 coil	-	-	10	-	110
2 coils	-	-	10	-	210, &c.

which, if the circle be large enough, will be a very convenient mode of regiſtering.

* The beard of the *avena ſterilis* (of Linnæus), which, on account of its ſize, is preferable to the beard of the common oat.

XXXI. *Historical Sketch of the Institution and Progress of the Royal Society of London.*

[Continued from p. 38.]

EVEN within a few years after its institution, the Society had acquired a considerable library, and a good collection of natural curiosities. A Mr. Colwal was, in these things, one of its first and most munificent benefactors. Mr. Hooke, the first curator of the Society, had the care of the books and curiosities; and they were arranged according to a classification of all the species in nature, contrived by the famous Dr. Wilkins. Mr. Henry Howard bestowed on the Society the whole Arundelian library, consisting of some thousands of printed books, with several hundreds of choice manuscripts. From the time, also, of the great fire, after which the buildings of Gresham-college were again occupied as an exchange, Mr. Howard afforded the Society a place of temporary accommodation in Arundel house.

Among the communications to this Society, within the first seven years after its incorporation by charter, were many curious theoretical discourses. Such were an hypothesis of the motions of the moon, and of the sea; a theory of fire and flame; an hypothesis of the form and spring of the air; a discourse of the possible height of the air, and its rarefaction upward; a discourse about improving wood for dyeing, and for fixing colours; discourses upon several mercurial experiments; a discourse of annealing and tempering steel; discourses about cyder and coffee; a discourse of the possibility of the retardation of the motions of the heavenly bodies, and of their going slower and slower the longer they last;—with a multitude of others, which it were inconvenient and tedious here to enumerate in detail.

As their views were more expresly and directly than those of their present successors in the Royal Society to the improvement of the arts, they quickly formed accurate histories of many of the most useful of these. Among their papers, within the first seven years after the Society's institution, were to be found, an history of English mines and ores, two separate histories of tinneries and tin-working, histories of iron-making, of lignum fossile, of saffron, of alkermes, of verdigrease, of the bleaching of wax, of colours, of the making of alum, of the preparation of salt from salt water, of enamelling, of engraving, of varnishing, of dyeing, of refining gold, of making potashes, &c. The processes of making bread,

bread, of making cloth, of tanning with the other parts of the manufacture of leather, of paper-making, of hat-making, of brewing, were accurately related according to the best practice of them at that time, in different memoirs by the first members and correspondents of the Society. Prince Rupert communicated two papers of extraordinary value, explaining the process for the manufacture of gunpowder, and an improvement of that process, by which gunpowder might be made which should have twenty times the strength of that which was commonly in use. Mr. Henshaw produced to the Society an ingenious history of the discovery and the manufacture of salt-petre. Many papers illustrative of the history of the useful arts were communicated by Sir William Petty, one of the most truly honourable founders to whom any family among our British nobility can trace the origin of its wealth and nobilitating distinctions.

But none of all those original members did more than Dr. (afterwards Sir) Christopher Wren to advance the purposes of the Institution. He proposed certain curious theories of motions, extending and correcting the doctrine of Des Cartes, which, however they may have been since exploded, were imagined with great ingenuity, and illustrated by a multitude of happy experiments. He contrived to annex to a weathercock an apparatus, which, of itself, registered for every twelve hours the changes of the wind; and invented also a thermometer, to register for itself the variations in the temperature to which it should be exposed. He was the inventor of an instrument for measuring the quantity of the rain that falls. He was the first who explained, that from the pendulum might be produced a natural standard of measure for vulgar use. He first exactly measured and delineated the spheres of the humours in the eye; and thence explained how reflection conduces as much as refraction to vision. He was the first author of the noble anatomical experiment of injecting liquors into the veins of animals. He made a variety of experiments to ascertain the powers of the magnet; and many to explain the nature of the powers by which sailing is performed, and to fix what fabric of a ship might be most suitable to the uses for which it is intended. He made many improvements upon telescopes, and a prodigious number of astronomical observations, particularly on the planet Saturn, and on the moon.

He invented a curious and very speedy method of etching. He suggested a number of contrivances for the improvement of water-works. He found out certain perpetual lamps and registers of furnaces, by which artificial heat might be so kept up and graduated as to imitate nature in the hatching of

chickens

chickens and insects, the vegetation of plants, the production of certain fossils, and the preservation of an equality in the motions of watches, in order to the discovery of the longitude, &c. All these things he accomplished in a diligent pursuit of the objects of the Society: and as his discoveries were made, he communicated them, but with the greatest diffidence and modesty, at its meetings.

And yet, even this variety, activity, and success in its researches, could not satisfy the tyrannical impatience of public expectation. The cavillers against the Society derided its members as idle dreamers, or interested impostors, who excited hopes which they could not gratify, and wasted their own lives and fortunes in pursuits having no rational attainable end. Others conceived prejudices still stronger against these studies; accusing them as atheistical, and adverse to the study of purely moral and intellectual truth; because they traced to second causes phænomena, which had been usually referred to the immediate agency of the Divine Power; and withdrew attention from the logic, ethics, and theology, which had hitherto reigned in the schools. By others they were abhorred as incapable of alliance with elegance and grace, with the charms of poetry and eloquence, with the refinement of the language, or the improvement of the popular parts of literary composition. To many that tolerance was extremely odious with which they received all men of ingenious inquiry into their correspondence, without distinction of nation or religion; and the admirers of the ages of classical antiquity derided, with the proudest contempt, the very idea upon which the Institution was founded,—that it was possible to extend the empire of human knowledge beyond those limits within which the antients had left it confined.

The Society, not unmoved by such censures and complaints, yet not diverted by them from the prosecution of its views, advanced, though slowly, yet with steady and vigorous diligence, in its experiments and inquiries. Dr. Spratt's apologetical history contributed to silence many prejudices. The imitation of the design of this Society, by so many similar institutions arising after it in foreign countries, evinced the general conviction of mankind to be in its favour. Its memoirs and transactions were, from time to time, printed; and, among discoveries and inquiries which could interest none but philosophers, failed not to contain, likewise, others of general utility and importance so striking and palpable, that even the attention and approbation of the vulgar could not be refused.

In pure mathematics, the successive publications of the Society,

Society, to the end of the seventeenth century, exhibited a variety of inventions exalting the power of that science, and furnishing it with new arms to triumph over difficulties of investigation to which it was before unequal. The discoveries of Newton had been prompted by that spirit of mathematical and physical research which the institution of the Society excited. Their communication did honour to the meetings of the philosophers, and enriched their volumes. Wallis, Gregory, De Moivre, and Halley soon communicated a number of valuable problems, theorems, and new demonstrations, important both in the new display of beautiful, and in their susceptibility of application to the improvement of the mathematical arts. In mixed mathematics the optical discoveries of Newton, Hevelius, and Huygens were, in great part, made first known to the world through the medium of the Royal Society. Leeuwenhoek and the illustrious Dr. Robert Hooke published most of their microscopical discoveries, in the first instance, through the same channel. A multitude of the most important observations in astronomy, the communications chiefly of Flamsteed, Hevelius, Cassini, Halley, Gregory, Bullialdus, Auzout, fill the early volumes of these Transactions. Bernoulli, Huygens, Hooke, and Wallis communicated some highly curious papers in mechanics and acoustics. Mr. Boyle, M. Homberg, Dr. Papin, Dr. Reiselius, and an inferior philosophical society at Oxford, communicated various experiments on the gravities of different fluids, their superficial figures, and their laws of movement. Mr. Thomas Savery, in the year 1699, made known to them an engine of his invention for raising water by steam.

Flamsteed, Borelli, Halley, Mercator, Cassini, Bullialdus, and Greaves, gave, for the improvement of geography and navigation, many observations of the longitudes and latitudes of different places, of the variations of the compass, &c. &c.

In architecture and ship-building, their Transactions exhibit the valuable papers of Leeuwenhoek "on the differences in timber as it grows in different countries, and is felled in different seasons;" of Mr. Bulteel "on the sheathing of ships with lead;" of Dr. Lister, Dr. T. Robinson, and Dr. Wallis, "on bridges, arches, and chimneys, &c." Wallis and Salvetti communicated some interesting papers on the theory of music. Dr. Sherard gave a receipt for making china varnishes.

In physiology, meteorology and pneumatics, the researches of this Society in the last thirty years of the seventeecth century were no less diligent and meritorious. The barometer, the hygrometer, the thermometer, were first put to important use in philosophical observation by its members.

The general phænomena of the weather in different seasons and latitudes were by them first recorded with philosophical accuracy. Of the winds, rains, snow, hail, lightning, thunder, meteors, exhalations, their papers exhibit an assemblage of facts, to which much has indeed been since added, but which still form a large and important part of our present science of atmospherical phænomena. They collected a prodigious variety of observations to illustrate the physical history of water, as it existed in seas, lakes, springs, mineral and salt-springs, &c.; and to explain the manufacture of salt from sea water or from that of salt-springs.

In those early volumes are a very great number of papers in mineralogy: the facts which they contain laid the foundation of this science. The history, in particular, of animal substances, and of vegetables found in a fossil state; of some volcanic eruptions; of marble found in Ireland; of the formation of peat-earth in Scotland; of the strata of pit-coal; of rock crystal, iron, and copper-ore; of amber; of the electrical capacities of amber, gum lac, and diamonds; was illustrated in a number of curious papers, the produce of much laborious inquiry, and of many observations made with the greatest diligence. Mr. Gill, Sir Robert Moray, Dr. Brown, and others, communicated interesting papers on damps in mines. Dr. Lister gave some mineral maps, and an account of the true way of making steel. A catalogue of electrical bodies was received from Dr. Robert Plot: Mr. D. Colwal favoured the Society with accounts of alum works and green copperas works. The lead mines in Somersetshire, the quicksilver mines in Friuli, the silver and gold mines in Hungary, were made particularly known to the Society by communications from Mr. Glanville, Dr. W. Pope, and Dr. E. Brown. A curious paper from Mr. M. Septalius acquainted them with the existence of mercury in certain plants. The collection of their papers presents, beside these, a prodigious variety of other information relative to subjects in the mineral kingdom.

The power of the magnet; the use of the magnetic needle; the declination of the needle, its variations in different places, with the theory of those variations, engaged much of the Society's attention; and were very ably illustrated by the papers of Dr. Halley, Mr. Hevelius, Mr. Auzout, Mr. Cunningham, and others.

To the improvement of the science of botany, and the practice of agriculture, they continued to devote much of their care. Rye, turnips, potatoes, maize, saffron, orange-trees, vines, are among the plants whose culture and œconomical uses were successfully explained in the papers read before this Society during the laſt century. The nature of vegetation, the circulation and deſcent of the ſap in trees, the reunion of bark to trees from which it had been ſeparated, &c. were finely explained by Dr. Beale, Dr. Liſter, Mr. Reed, M. Malphigi, Mr. Ray, &c. The uſe of marle, of ſea-ſand, of ſalt and of brine for manures, are taught in others of thoſe papers. Mr. King communicated a method of improving the bogs and loughs in Ireland by drainage. Dr. Beale explained the fertilizing powers of froſt. The preparation of oak for tanning, of ſugar from the maple-tree, of vinegar, of pitch, tar, reſin, and turpentine, are the ſubjects of ſome other ingenious papers of the ſame period. Dr. Liſter inveſtigated with curious pains the botanical hiſtory of the muſhroom. Hemlock, opium, ſnake-root, aloe americana, the amomum of the Philippine iſles, and many other plants adapted to be of uſe in the materia medica, were examined in their growth and qualities, under the Society's directions. Dr. Liſter proved the poſſibility of fertilizing a ſandy ſoil by the addition of clay. Sir Robert Moray gave a valuable paper on the proceſs of malting. Dr. Beale and Mr. Dale taught how to make bread from turnips and potatoes.

In zoology, the papers of this period are likewiſe numerous, and illuſtrative of many of the moſt intereſting ſpecies in the animal kingdom. The hiſtories of the ſilkworm, the whale, the cochineal inſect, the pearl-muſcle, the bee, were, on account of the relations of theſe animals to the uſeful arts, examined with extraordinary care. Many accounts of monſtrous births and figures of animals were from different parts tranſmitted for the Society's conſideration. Waller's poem "on the Summer Iſlands" appears to have had for its foundation a narrative concerning whales and whale-fiſhing at the Bermudas, which was communicated to the public in the Tranſactions of the Royal Society. Mr. Ray, examining into the nature of ants, diſcovered and made known to the Society that which has been ſince named the *formic acid.* Others, with much curious pains, illuſtrated the hiſtory of the ſpider. Dr. Liſter found an acid juice in another inſect beſide the ant. Mr. Boyle and others examined and explained the anatomical ſtructure of fiſhes. In regard to the human body eſpecially were theſe philoſophers wonderfully induſtrious in reſearch. The natural accidents to which it is ſubject, and its anatomical ſtructure, were never before ſo well illuſtrated as by the facts which they collected. Concerning the ſtructure, external parts, and common teguments of human bodies; the head; the neck and thorax; the abdomen; the humours and general affections

tions of the body; its bones, joints; their Transactions present an assemblage of facts such as must, in comparison, put to shame the industry of later anatomists.

In chemistry, they investigated the nature and composition of phosphorus, of the Bologna stone, of vegetable and mineral acids, &c. An engine to consume smoke was made known to them by M. Justel. A method of imitating the pottery of China appears also among their papers. Sir Robert Southwell communicated an account of the method of dressing buck and doe skins, which was practised by the Caribbees in the West Indies. The phosphorescent qualities of wood, putrid flesh, the surface of the sea, &c. were particularly examined in papers communicated by Mr. Boyle and Dr. Beale.

Nor were their endeavours confined to the improvement merely of physical and mathematical science. Mr. Lodwick gave an essay towards an universal alphabet. Dr. Wallis invented a method of teaching persons deaf and dumb to speak and understand language. Mr. Edward Lloyd communicated some valuable observations in philological science. Another gentleman gave a paper of highly curious observations and conjectures concerning the Chinese characters. The collection of their papers affords likewise some valuable illustrations of difficulties in chronology. Roman, Grecian, Saxon, Runic, Egyptian, and Persian antiquities have also a variety of new lights thrown upon them in these papers. The first account of the discovery of the famous ruins of Palmyra appears here in two communications from Mr. Timothy Landy and Mr. Aaron Goodyear. When the members and correspondents of the Society were engaged in journeys and voyages, they never failed to register for its information at least some of the more extraordinary facts which came under their notice. And a number of its papers are, in consequence of this, narratives of such observations, interesting in the highest degree at once to philosophers and to mere popular curiosity.

Yet about the beginning of the 18th century the members of the Society were themselves dissatisfied with the progress and success of their efforts. They began to feel that their first ardour of inquiry had become cool. They complained, that "the discouraging neglect of the great, the impetuous contradiction of the ignorant, and the reproaches of the unreasonable, had unhappily thwarted them in their design to perpetuate a succession of useful inventions." Nor was it to have been expected that the fears for religion and liberty in the latter part of the reign of Charles the Second and his successor, the civil

civil and political contentions to which they gave birth, and the direction of so much of the learned ability of the time, to exercise in the field of the popish controversy, should not have proved inauspicious to the advancement of philosophical studies. The reign of William, harassed with wars, with disputes relative to the rights of the reigning sovereign and to the royal succession, with factions in church and state, introduced no new fortunate æra for the advancement of philosophy and the arts. Even in the beginning of the 18th century, and during the course of some years, the same dissensions and wars continued to produce the same effects upon the fortune of science. The wits presumed to throw ridicule upon science and erudition, which they were too idle to cultivate. The reign of Anne was for England perhaps the golden age of wit and elegant literature; and men showed themselves sufficiently disposed to prefer the light and shining, to that which was only solid and unostentatious.

Yet, while Newton, the pride of the Royal Society, distinguished himself among its active members, or presided at its meetings, it could not but continue to be regarded as the grand focus of physico-mathematical discovery and knowledge. His own communications in optics, astronomy, and general physics, were invaluable. His system of the universe was adopted as one which was infallibly true, and which it was for the honour of the nation, by all possible means, to illustrate and maintain. Hence, from the commencement till nearly the middle of the 18th century, the labours of the Society were principally employed in pursuing the analogies of the Newtonian system throughout all that diversity of phænomena by which its certainty was to be tried. Such were almost all the experiments, observations, and theories, of the illustrious, the indefatigable Halley. Desagulier, Facio, Keill, and Maclaurin, successfully laboured in the same field. Every other branch of those sciences which the Society studied especially to improve, continued also to be more or less advanced by the labours of its members.

It was at length evident that the field which its founders had, with noble ambition, marked out for themselves was too vast. The Society of Arts arose by perhaps an unconscious derivation from the Royal Society. To the latter was still left the province of the sublimer and abstruser sciences. The latter, with admirable national enthusiasm, undertook the task of applying, and encouraging the application of, scientific truth to the improvement of the common arts.

For a time, about the middle of the 18th century, when the Newtonian system was fully established, and no new path

path of noble discovery had been opened, the exertions of these philosophers might comparatively languish. The discoveries of Dr. Stephen Hales concerning the diversities of airs, about that time, however, renewed the truths which had been first explained by Hooke. Discoveries in natural history and chemistry were continually more and more multiplied. Franklin at length communicated to the Society the grand truth of the identity of lightning with electricity, and a new theory combining all the electrical phænomena which had been as yet observed. Priestley, following Hales, Hooke, and Boyle, in experiments upon air, discovered all the varieties of aëriform substance. Cavendish, Kirwan, and others, examined airs in their relations to the calces of metals, &c. Sir Joseph Banks has done high honour to the Society by the advancement which it has, under his auspices, made in botanical discovery, and in the culture of the other branches of natural history. At the present time, its labours are, in all the branches of physical and mathematical science, most zealously continued. Amid so many rival institutions the Royal Society of London holds still the first place. Its memoirs, now with great regularity annually published, fully vindicate its claim to the highest estimation of the public. We have reviewed its history with a conscious pride that we are of the same country and language with a succession of philosophers who have discovered, or collected and arranged, the better part of the physical knowledge peculiar to modern times. He who would aspire to the praise of a philosopher would do well to study the volumes of its Transactions with peculiar care.

XXXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

THIS learned body had its first meeting after the summer recess on the 5th inst. (November), which was occupied by the reading of the Croonian lecture by Everard Home, Esq.

The meetings on the 12th and 19th were taken up in reading the Bakerian lecture on the theory of light and colours, by Dr. Young, professor of natural philosophy at the Royal Institution.

In this lecture the author offers no new experiments, a sufficient number for his purpose having been made and recorded by preceding philosophers, on which he rests four hypotheses: The first is, that a *luminiferous æther*, of an extreme degree of rarity, pervades the universe; the second, that, when bodies

bodies become luminous, *undulations* are excited in this æther—The third, that colours are owing to *vibrations* in this æther acting on the retina—and the fourth, that all matter attracts this æther; and that, when bodies are luminous, it acts in a more dense, though not in a more elastic state upon them.

In the meeting on the 26th was read an analysis of a mineral from North America, which contains a metal different from any hitherto known: by Charles Hatchet, Esq.

This is an acidifiable metal, but differs in some respects from those acidifiable metals already known. A vivid orange-coloured precipitate can be thrown down from one of its solutions, which, as it remains unchanged after being exposed to air and light, will be a most valuable acquisition to artists. By another process a green precipitate is obtained.

It is proposed to give this metal the name of *Columbium*. The specimen from part of which this analysis was made is in the British Musæum; but Mr. Hatchet is in hopes of being able to obtain a fresh supply from America.

Part II. of the Philosophical Transactions for 1801, just published, contains the following articles:

12. A Historical and Anatomical Description of a doubtful amphibious Animal of Germany, called by Laurenti *Proteus Anguinus*. By Charles Schreibers, M. D. of Vienna.—13. Observations tending to investigate the Nature of the Sun, in order to find the Causes or Symptoms of its variable Emission of Light and Heat; with Remarks on the Use that may possibly be drawn from Solar Observatons. By William Herschel, LL. D. F. R. S.—14. Observations on the Structure, and Mode of Growth, of the grinding Teeth of the Wild Boar, and Animal incognitum. By Everard Home, Esq. F. R. S.—15. Accounts of some Experiments on the Ascent of the Sap in Trees. In a Letter from Thomas Andrew Knight, Esq. to the Right Hon. Sir Joseph Banks, Bart. K. B. P. R. S.—16. Additional Observations tending to investigate the Symptoms of the variable Emission of the Light and Heat of the Sun; with Trials to set aside darkening Glasses, by transmitting the Solar Rays through Liquids; and a few Remarks to remove Objections that might be made against some of the Arguments contained in the former Paper. By William Herschel, LL. D. F. R. S.—17. On an improved Reflecting Circle. By Joseph de Mendoza Rios, Esq. F. R. S.—18 Observations and Experiments upon Dr. James's Powder; with a Method of preparing, in the humid Way, a similar Substance. By Richard Chenevix, Esq. F. R. S. M. R. I. A.—19. Case of a young Gentleman, who recovered his Sight when

when ſeven Years of Age, after having been deprived of it by Cataracts before he was a Year old; with Remarks. By Mr. James Ware, Surgeon.—20. An Account of ſome Galvanic Combinations, formed by the Arrangement of ſingle metallic Plates and Fluids, analogous to the new Galvanic Apparatus of Mr. Volta. By Mr. Humphry Davy, Lecturer on Chemiſtry in the Royal Inſtitution.—21. A Continuation of the Experiments and Obſervations on the Light which is ſpontaneouſly emitted from various Bodies; with ſome Experiments and Obſervations on Solar Light, when imbibed by Canton's Phoſphorus. By Nathaniel Hulme, M. D. F. R. S. and A. S.—22. Experiments on the chemical Production and Agency of Electricity. By William Hyde Wollaſton, M. D. F. R. S.—23. Farther Obſervations on the Effects which take Place from the Deſtruction of the Membrana Tympani of the Ear; with an Account of an Operation for the Removal of a particular Species of Deafneſs. By Mr. Aſtley Cooper.

THE SOCIETY OF ARTS AND SCIENCES AT UTRECHT.

This ſociety has propoſed the following

Prize Queſtions:

1ſt, What is the real nature of the electrical matter? Is it compound, and of what principles is it compoſed? What are the chemical changes which it experiences by its union with other bodies, and which it produces in theſe bodies?

The beſt anſwer, to be tranſmitted before the 1ſt of October 1803, will be entitled to a medal of the value of thirty ducats.

2d, The following, which has not yet been anſwered, is again propoſed:—What is the real nature of the dyſentery? Is it always the ſame? What are the different cauſes of this diſeaſe? What are its ſymptoms, and their conſequences? What is the beſt treatment, and method of cure? and, in particular, how far is the uſe of opium proper as a remedy? The anſwer to be ſent in before the 1ſt of October 1803.

3d, A medal, of twenty ducats value, will be adjudged to the author of the beſt paper on natural hiſtory, and a ſilver medal to the next beſt.

The papers to be ſent in before the 1ſt of October 1802.

4th, Why are the diſeaſes now prevalent in Holland not ſo ſimple as formerly? Is it owing to bilious, viſcous, or other cauſes? By what means can it be aſcertained with moſt certainty, in the commencement of theſe diſeaſes, which of the above cauſes predominate? What is the beſt method of cure?

The

The papers to be tranſmitted before the ſame period.

5th, By what means can duelling be prevented in a country where an idea prevails, that infamy attaches to a perſon who on receiving certain injuries does not call forth the offender, or who when called forth does not accept the challenge? And in ſuch caſes, what meaſures are to be purſued?

The papers to be ſent in as before.

The anſwers to theſe queſtions, written either in the German, Dutch, Engliſh, French, or Latin languages, muſt be inſcribed with a certain device, and tranſmitted, free of poſtage, to Dr. Luchtmans, at Utrecht, ſecretary to the ſociety.

FRENCH NATIONAL INSTITUTE.

Notice of the labours of the Claſs of the Mathematical and Phyſical Sciences during the laſt three months of the year 9.

Mathematics.

C. Delambre read the following account of the mathematical labours:

A memoir on the equilibrium of arches, by C. Boſſut, was read. As the ſtones which compoſe an arch mutually counterbalance each other, they remain ſuſpended without any ſupport from below, all their effort being directed towards the abutments, which ſupport the arch, as if it formed one continued body.

To enſure the duration of an arch, it is not ſufficient that all the ſtones ſhould be in equilibrium; the abutments on which they reſt muſt oppoſe a ſufficient reſiſtance to the efforts the arch makes to overturn or cruſh them.

A reſearch into the means proper for preventing their being overturned conſtitutes what is called the problem of the puſh of arches, which engaged the attention of ſeveral geometricians of the laſt century; but they entirely neglected that problem which has for its object the means of preventing cruſhing.

To try the reſiſtance of which different ſtones are ſuſceptible, Soufflet invented a machine, which was afterwards improved by C. Rondelet.

The antients had no very certain or geometrical principles for inſuring the ſolidity of their edifices; at leaſt, no trace of them is ſeen in the work of Vitruvius, nor is any thing found in that work on the figure of the ſtones or on that of the wood; which induces C. Boſſut to think that their architects, attending merely to decoration and external form, left to the builders what related to the conſtruction and ſolidity,

 in

in which, unfortunately, they have had too many imitators among the moderns.

La Hire was the first, at least in France, who determined, by the theory of the wedge, the proportion according to which the weight of the arch stones ought to be augmented in a semi-circular arch.

In 1704, Parent determined by points the figure of the extrados of an arch the intrados of which is a semi-circle. He determined also the push of such an arch on the abutments.

James and John Bernoulli, Huygens, and Leibnitz, having resolved the problem of the catenarian curve, it was soon perceived that this curve was that of the equilibrium for an arch composed of arched stones infinitely small and equally heavy. David Gregory showed this fact, and it is deduced still more directly from a method given by James Bernoulli. A second method of the same author, corrected by C. Bossut, conducts to the same conclusion.

La Hire in 1712 gave a method, founded on some experiments, which, on account of its great facility, was adopted by practical workmen, who gave themselves no trouble to inquire whether it was applicable to all cases. Not content with following it in semi-circular arches, they applied it even to domes, though the equation of the equilibrium in this case is of the third degree, and not of the second, as in the preceding case.

Couplet has treated of the push of semi-circular arches, and of the thickness of their abutments; first, by considering the arch stones as infinitely smooth, and experiencing no resistance from friction; and then he endeavours to correct what may be defective in this supposition: but he does not go further than Parent and La Hire.

Bouguer has treated of domes. He has shown that a variety of curves may be employed in them, and of these he points out those most advantageous. But he has not calculated the push, nor has he examined the law of the forces which must act on the arch stones when the generating curve is subjected to given conditions; a subject fertile in curious and useful problems.

In 1770 C. Bossut undertook to treat of the question in its whole extent, both in regard to semi-circular arches and domes. He examined every thing that relates to the form and push of arches. His memoirs were printed in the Transactions of the Academy of Sciences for the years 1774 and 1776.

Mascheroni,

Mafcheroni, who was deputed by the Cifalpine republic to affift in fixing the unity of the new meafures, and of whom the fciences were deprived laft year, made in 1785 new refearches, and gave fome ingenious propofitions refpecting the equilibrium of domes with circular elliptical and polygonal bafes.

After many new reflections and feveral experiments, which may be of the greateft ufe in practice, C. Boffut has refumed his two memoirs, which he has thrown into one, fimplifying his calculations in feveral places. He has made a great many additions both in theory and practice, fo that the whole now forms a work which may be confidered as new.

Comet.

C. Meffier read a note on the comet which he difcovered on the 12th of July laft, about half after 11 in the evening. It had a very faint light: in 41 minutes time it had 24′ 40″ direct motion in right afcenfion, and 6′ 38″ in northern declination decreafing. The fame comet was feen the fame day, and almoft at the fame inftant, by Mechain and Bouvard; and the latter obferved it on the meridian at 11 h. 57′ 49″ true time. Its right afcenfion was 111° 15′, and its northern declination 69° 30′.

It has fince been known that it was obferved the fame day at Marfeilles by C. Pons, who had even feen it the day before. But the clouds did not then permit him to afcertain, by regular obfervations, whether it was a comet or only a nebulus. Mechain has calculated its elements.

Obfervation of the Summer Solftice of the Year 9 (1801).

Duc-la-Chapelle, affociate, has communicated to us the refult of the obfervations he made at Montauban, to determine the folftitial altitude of the fun and the obliquity of the ecliptic.

By the mean of nine days obfervations he found the apparent obliquity to be 23° 28′ 9″, fuppofing the femi-diameter of the fun 15′ 48″, and the latitude of his obfervatory 44° 0′ 52″.

Thefe obfervations were made with Lacaille's fextant; and it would perhaps be proper to take from the tables of that aftronomer the diameter of the fun and the refractions which he determined with the fame inftrument, becaufe thefe tables contain alfo the errors of that fextant. By thefe means the obfervations of Duc-la-Chapelle would agree with ours. By a mean of eighteen days obfervations, made with Borda's circle, I found the obliquity to be 23° 28′ 8″2. Mechain

 and

and Le François found the seconds to be only from 6 to 7. It is exceedingly difficult to avoid these small differences. Uniting my observations for three years, I find, by taking a mean, that the apparent obliquity at the summer solstice of the year 8 (1800) was 23° 28′ 6″4, or 23° 27′ 58″ for the mean obliquity. The observations of Bradley, Mayer, Lacaille, and Le Gentil, gave nearly 23° 28′ 18″ for 1750; it would thence result that the secular diminution is only 40″ instead of the 50″ which are generally supposed. A part of the difference may arise from the observations, and particularly the old observations, which perhaps ought to be examined with the more correct elements which are employed at present.

Remarks on the Fifty Thousand Stars, the Observations of which have been published by Lalande.

Lalande has announced, in the preface to his *Histoire Celeste*, that there are in the heavens a great many vacant spaces, many changing stars, and many red stars. He has now given further details respecting these objects in a memoir accompanied with tables. By vacant spaces Lalande here understands spaces in which no stars are seen of the ninth magnitude, which are the smallest that can be perceived with an achromatic telescope of sixty-seven millimetres aperture (about $2\frac{1}{2}$ inches) for illuminating the object-glass that the threads may be seen. It is beyond all doubt, that, by excluding all foreign light, and employing the strongest magnifying glasses, these vacant spaces, properly so called, would be found to be considerably diminished in number; and perhaps there is not in the whole heavens a single place to which a telescope could be pointed without seeing a great number of stars, but below the ninth magnitude, and consequently too faint to be of any use in astronomy.

Lalande gives a catalogue of all these vacant spaces; that is to say, his table contains the right ascension and declination of the centre of each of these spaces.

The changing stars are contained in a second table, and are in number thirty-one. There are only twelve the periods of which are known, but there are several others which decrease in magnitude so as to disappear at intervals. By attentive observation, the time which elapses between two consecutive disappearances might be determined; and this kind of observation Lalande recommends to the curiosity of those who, being possessed of only moderate instruments, may, however, be desirous of being useful to astronomy.

The third table contains thirty-three stars of a red colour.

About

About the year 1756 Mayer had remarked this tint in the nineteenth of *pisces*, which he denotes in the regifter of his obfervations by the epithet of *rubicunda*, as we have feen by the copy fent to us by M. Lichtemberg of all the obfervations made by Mayer on the day on which he obferved the planet of Herfchel. Michell and Bailli fuppofe that the colour of the ftars may depend on the different intenfity of their fire, or the degree of their inflammation; and that the red colour may indicate a fire decreafing. Under this idea it might be of importance to examine the changes of colour which the ftars experience. However, thefe variations, if they exift, are doubtlefs exceedingly flow; for the different fhades at prefent remarked between Antares, Arcturus, Aldebaran, Syrius, and the Lyre, exifted in the time of Ptolemy.

Extract from a Memoir on the Degree of Magnetifm acquired by Bars of Steel of different Thicknefs, and on fome Refults in regard to the Magnetic Needle, by C. Coulomb.

Almoft all the magnetic phænomena can be fubjected to calculation if we fuppofe in fteel two magnetic fluids, in each of which the moleculæ repel each other in the inverfe ratio of the fquares of the diftances, and attract in the fame ratio the particles of the other fluid. This law was proved by Coulomb in the Memoirs of the Academy for 1786, from experiments which appear decifive.

When fteel is in its natural ftate, and not magnetized, the two fluids are neutralized by each other; that is to fay, they keep each other mutually in equilibrium, and no longer exercife any action, fince one of the fluids attracts a magnetic molecula with the fame force as the other repels it.

When the fluids are feparated they tend to unite and to neutralize each other; and they would indeed unite were there not a coercive force which oppofes this union. This force may be either the adhefion of the moleculæ of the fluid to the fteel, or the friction which they experience in paffing from one point to another.

The author has proved (*Mémoires de l'Academie* 1787, p. 491) that whether the two fluids were feparated and carried each to an extremity of the bar, or whether they were only difplaced in each molecula of the fteel without the power of paffing from one molecula to another, calculation gave the fame refult. He has proved at the fame time that the latter fuppofition is the only one which can agree with experience.

According to thefe fuppofitions, a bar of fteel may acquire every degree of magnetifm, provided that there is no point in it where the refult of the action of all the moleculæ of the fluid of the bar which act on the magnetic fluid of that point, is greater than the cohesion which prevents it from being displaced.

But among all the degrees of magnetism there is one which may be called the point of saturation, or the *maximum* of magnetism; it is that where the result of the forces on each molecula is precisely equal to the adhesion of that molecula to the steel. It may be readily conceived that when it attains to this state the bar has the highest degree of magnetism which it can acquire; it is also that degree which ought to be given to bars destined for making needles to the mariner's compass.

To know whether a needle be magnetized to saturation, practice seems to furnish but one method, which is, to magnetize it with bars of different strength. If a bar, for example, be successively magnetized with bars the force of which is as 1 to 2, and if the bar the first time has the same directive force as when magnetized with the strongest magnets, it appears that we may thence conclude, with a great deal of probability, that the bar is magnetized to saturation. Theory furnishes us with a more direct method, which is, to find by calculation what may be the directing force of a bar magnetized to saturation, and to examine whether the result of this calculation be agreeable to experience. It was in this manner that Coulomb proved, in the third volume of the Memoirs of the Institute, p. 176, that if two bars of steel, all the sides of which are homologous, that is to say, the corresponding sides of which are in the same ratio, be compared with each other, these bars, suspended horizontally as the needles of a compass, will have the momenta of their directing forces as the cube of one of their dimensions. This result perfectly agrees with what has been found by experiment; which affords a new confirmation of the theory.

In the present memoir the author endeavours to determine the magnetic state of several bars united in succession one above the other, or, what amounts to the same thing, to determine the force of different bars in regard to their thickness.

But it may be easily conceived that, if the theory adopted is agreeable to nature, when two bars of great breadth, and very thin, are magnetized to saturation, each point magnetized, in the two bars united, will experience, on the part of these two bars, an action almost double to that which the same point would experience if each bar magnetized to saturation were insulated. Thus, by uniting the two bars, each of them ought to retain only a little more than half the magnetism which it had when insulated, since the adhesion is the

the same for two bars as for one. This, indeed, the author proved by taking two bars of a white temper of 80 millimetres (3,14960 inches) in length, a third of a millimetre (0·01312 inch) in thickness, and forty millimetres (1,57480 inch) in breadth. Experience showed that the ratio of the momentum of the directing force of one insulated bar to that of the same bar united, is as 185 to 100, or nearly, as may be seen, the ratio of 2 to 1.

But it may be readily conceived that, in proportion as bars have more thickness and less breadth, as the fluid condenses itself in the edges and angles to which it is impelled in one direction without experiencing reaction, it must then happen that this ratio will be diminished. In this respect, experience gave a result of great importance to practice. It is a ratio almost constant; so that, if a bar a millimetre (0·03937 inch) in thickness be joined to another of the same thickness; if these two bars be magnetized to saturation separately, and if they be then magnetized together united, the ratio of the momentum of their directing forces in these two cases will be very nearly as 165 to 100.

This ratio takes place also, as theory indicates, and as is confirmed by experience, whether the bars be hardened of a white temper, or whether they be tempered to the same degree of spring, provided they are compared in the same state of temper and hardness.

It must however be remarked, that either by the method by which we magnetize, or some other property of the magnetic fluid which theory has not yet appreciated, when a bar very long compared with its other dimensions is magnetized, the whole fluid proceeds towards its extremities in an extent so much narrower as the breadth and thickness of the bar are less, and as the degree of temper is greater. Between these two parts there is an interval which gives scarcely any sign of magnetism; whence it happens, that by joining two very long bars the preceding law cannot have place, because the action of the magnetism of the two bars united only causes the magnetic fluid to flow back into the parts which gave no sign of magnetism when each needle was insulated.

It is only with needles then which give signs of magnetism throughout their whole length that we ought to find an agreement between experience and theory. It is always found, for example, that if the directive force of an insulated bar of two hundred millimetres (7,87400 inches) in length, whatever may be its other dimensions, is to the directive force of the same bar, when united with one equal to it, as 165 to 100; all shorter bars will give the same result, and all bundles

dles formed of ſuch bars will give, in regard to a bundle double in thickneſs, the ſame reſult.

Theory indicates alſo, that when a bar is compared with itſelf in different ſtates, whatever may be its dimenſions, if it be magnetized to ſaturation, its directive force ought to be augmented with the coercive force which keeps the magnetic fluids ſeparated. But as this coercive force is conſtant for equal ſtates of the ſteel, it thence reſults, that whatever may be the dimenſions of a bar it will give a conſtant ratio when its directive force is compared in two different ſtates. Here, alſo, experience is agreeable to theory. Bars made from the ſame piece of Engliſh ſteel-plate, whatever their number and dimenſions, when hardened at a cherry red, gave directive forces greater than when brought down from a higher to that temper in the ratio of 24 to 10, which announces that the coercive forces are in the ſame ratio.

The ſame bars brought to a blue temper gave, in regard to thoſe brought down to a white temper, the ratio of 164 to 100; but in this there is a little variety, becauſe it is almoſt impoſſible to enſure the ſame degree of tempering.

In ſteel of inferior quality there is ſtill more advantage in giving it a greater degree of temper. The author found bars of German ſteel the coercive force of which, when they were tempered at a cherry red, was to the ſame force, when brought from a higher to the ſame temper, as 34 to 10; but, on the other hand, this ſteel brought to a white temper has a directive force much leſs than ſteel of the Engliſh plates.

C. Coulomb has read to the claſs only this firſt part of his memoir.

The ſecond part is deſtined to determine the curve of the magnetic denſities and its extent in bars of a white temper, in thoſe brought back to the ſtate of ſpring, and in thoſe brought down to white temper. The object of this reſearch is very intereſting, either for conſtructing artificial magnets, or for fixing the ſtate and dimenſions of needles moſt advantageous for thoſe employed in the mariner's compaſs. The author found, for example, that when the bars are very long in regard to their other dimenſions, it is of advantage, after they are tempered almoſt to a white, to bring them back to blue: for the excellent cylindric wires of ſteel uſed in commerce, when magnetized with the ſtrongeſt magnets, have the ſame directive force whether they are tempered to a white or are brought back to the ſtate of blue ſpring when their length is thirty-three times their diameter; but if they are much longer than thirty-three times their diameter there is a great advantage in bringing them to the ſtate of ſpring; and on the

the other hand, when their length is much less than thirty-three times their diameter, there is a very great advantage in tempering them white. The causes of these varieties will be detailed in the second part of this memoir; and it will there be seen, says the author, that they only confirm theory, which he has always compared with experience, in the different memoirs published in the volumes of the Academy of Sciences and those of the Institute.

Physics.

C. Cuvier, secretary, read the following notice:

On the Gas obtained from the Reduction of the Oxide of Zinc, and the Nature of Charcoal.

The aëriform fluid obtained by reducing the oxide of zinc by charcoal was stated by C. Guyton to be a gaseous oxide of carbon, that is to say, carbonic acid with excess of base. Continuing to reason on this hypothesis, Guyton thought that the substance found there in excess, being very much divided and weakly retained, ought to be very much disposed to exercise its affinities, and that it could operate reductions cold and in the wet way. Trials, however, made with solutions of lead, mercury, and silver, did not answer his expectation. The gas experienced no alteration in these liquors; but when put in contact with oxygenated muriatic acid there was a spontaneous combustion, and the gas, converted into common carbonic acid by the accession of the oxygen of the muriatic acid, became capable of precipitating lime water.

Berthollet had a peculiar opinion in regard to the manner in which this reduction of the oxide of zinc takes place, and which he explained by the composition of common charcoal, which contains a large quantity of hydrogen.

This doctrine he has endeavoured to establish by a very extensive dissertation on the nature of charcoal, and the different products thence obtained.

For this purpose he resumed all the labours of his predecessors on charcoal, and subjected them to a new examination. He has shown that Lavoisier was not altogether exact in his analysis of the carbonic acid, because at that period he was not well acquainted with the specific gravity of oxygen gas, and because he did not pay attention to the water, of which the carbonic acid always contains a great deal. He brings forward the proofs which C. Monge gave of this last-mentioned fact, and has established, that common charcoal contains a large quantity of hydrogen and a little oxygen. He has shown that this charcoal distilled in a pneumato-chemical apparatus gives carbonated hydrogen gas only as long

long as its oxygen is not exhausted, because a little of the latter substance is necessary to produce that gas, which consequently is a ternary combination. The residuum then contains only carbon and a less quantity of hydrogen than simple heat can deprive it of; and the diamond, according to Berthollet, differs from calcined charcoal only by its not containing hydrogen, and not, as Guyton thought, because the charcoal already contains oxygen.

In regard to the different combinations which contain hydrogen and carbon, Berthollet divides them into two classes: the ternary, which contain oxygen, also such as the gas extricated from sugar, that obtained from metallic oxides with charcoal, and that which carbonate of barytes produces with the same substance, &c.; and the binary, which contain no oxygen, as the gas known under the name of *olefying*, that extracted from alcohol and oil, and, in all probability, that arising from the decomposition of water by charcoal. His method to determine whether gas contains oxygen or not, is simple and ingenious. It consists in general in burning the gas, and observing whether the oxygen it has consumed during its combustion has been sufficient to produce the results. In the contrary case, the gas burnt must have already contained a greater or less quantity of it.

Berthollet has examined also the composition of the carbonic acid, not by a direct method, but by admitting as true the proportion which best corresponded to all the experiments which his researches required. 100 cubic inches of this acid contain, according to Berthollet, 84 cubic inches or 43 grains of oxygen, 16 grains of carbon, and from 9 to 10 grains of water.

In regard to the question which gave rise to all this labour, Berthollet considers the gas resulting from the reduction of zinc, as a ternary compound of carbon, oxygen and hydrogen, in the proportion of two parts of hydrogen, seven parts of carbon, and thirty-two of oxygen.

While the French chemists were discussing this question with so much activity, it engaged the attention of foreigners also. Mr. Cruickshank gave to the gas obtained the same name as Guyton, viz. *oxide of carbon*. He however saw water formed in all his experiments except one, where, by the nature of the substances employed, no more ought to have been produced than just as much as the carbonic acid produced at the same time could absorb: thus the results of the English chemist were, at bottom, the same as the last made by Berthollet, though he employed the denominations of Guyton.

XXXIII. *Intel-*

XXXIII. *Intelligence and Miscellaneous Articles.* November 1801.

GALVANISM.

J. TOURDES, professor in the School of Medicine at Strasburgh, has lately communicated the following notice on this subject to professor Volta:—" The confidence and friendship of which you have so often given me proofs during my residence in Italy, embolden me to communicate to you the result of an experiment which appears to me to resolve one of the most disputed points of physiology, that of the vitality of the blood. This liquid deprived of the serum, lymph, &c. reduced to the fibrous part, and subjected to your galvanic or rather electric apparatus, (for the identity of the galvanic and electric fluids have been established in an incontestable manner by your late researches) at the temperature of about 30 degrees of Reaumur, exhibited a trembling oscillation and palpitation analogous to those experienced by flesh of animals newly killed; a *double motion of contraction and dilatation*, sensible to the eye by means of a magnifying glass; a characteristic mark of the vital force peculiar to the muscles, cellular tissue," &c.

Since our last publication some interesting experiments have been performed with the circuit of Volta that have given results which, though analogous to facts before known, were equally new and interesting, and serve to prove that we are as yet only entering the threshold of the inquiry respecting its powers, and the purposes to which it it may hereafter be applicable.

The experiments alluded to were performed by the Askesian Society on the 4th instant, being their second meeting for the present session.

The battery consisted of two troughs, each containing 60 plates of silver, and an equal number of zinc; in all 120 pairs of metallic plates, each $2\frac{1}{4}$ inches square, or containing $5\frac{1}{16}$ square inches of surface.

By means of this battery the Society deflagrated gold with great facility. The gold in thin leaves was connected with one end of the battery, and, on being brought into contact with the exterior plate of the other end to complete the circuit, instantly inflamed. The combustion only went on at those points in which the gold leaf and the plate were in contact, and ceased entirely unless the gold was kept advancing against the plate, to supply, by fresh portions brought into contact,

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the place of those burnt away. It is singular that no oxide or residue of any kind could be discovered; and it is probable that the gold was volatilized by the same heat that occasioned its deflagration: this point, however, is to be further investigated, when a larger portion of gold will be exposed to the action of the Voltaic circuit, to determine with certainty whether the whole, or any part of it, is converted into vapour.

In our number for September, we mentioned the combustion of phosphorus by the Voltaic trough, which was effected by putting a small piece on the point of an iron wire, and deflagrating the wire by bringing it in contact in the circuit. By similar means the Society exploded gunpowder. A small portion of it was disposed in the folds of some gold leaf, which was then deflagrated in the manner already described, and inflamed the powder.

CHEMISTRY.

A new fact was mentioned at a late meeting of the British Mineralogical Society, by Mr. Sandman, one of the members, and an excellent chemist. Some months since, to one part of butter of antimony he added five parts of water: instead of an instant precipitation taking place, the mixture assumed the form of a thickish mass, of such a consistency, that it would not even pour from the vessel. After four months, the mixture began to clear itself, and threw down a quantity of very white small crystals, some of which were laid before the Society. Their form could not be accurately determined, but seemed to approach the cubic. Whether they be a crystallized oxide, or merely a muriate of antimony, has not yet been ascertained; but the question is to be investigated, and the experiment will be repeated on a large scale.

The ingenious Mr. Davy, of the Royal Institution of Great Britain, has been occupied for some time in a series of experiments upon tanning, which will soon be laid before the public. He has tried a number of woods and barks, besides those that have been hitherto thought exclusively adapted to the purpose, and has met with several that contain the tanning principle in great quantity. His experiments will lead to a great saving in the expense of even oak bark, which it has been usual to have picked and separated from one part of the rind, which is found to be, notwithstanding, as rich in tan as any other part of the bark. One circumstance remarked by this chemist is curious: Acorns do not seem to contain in their natural state any portion of tan; but after being baked in a heat of about 212° F. they are found to contain a considerable quantity. If they be overbaked, they again lose that quality.

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The ſame chemiſt is engaged in analyſing the ink of the cuttle-fiſh. The experiments are not finiſhed, but from the progreſs he has made it appears to be carbon held in ſome ſtate of ſolution.

METEOROLOGY.

A phænomenon was ſeen to paſs Batan Rouge, on the Miſſiſippi, in the latitude of 31° north, on the night of the 5th of April 1800, of which the following is the beſt deſcription we have been able to obtain *:—It was firſt ſeen in the ſouth-weſt, and moved ſo rapidly, paſſing over the heads of the ſpectators, as to diſappear in the north-eaſt in about a quarter of a minute. Its apparent ſize was that of a large houſe ſeventy or eighty feet long, and of a form nearly reſembling the annexed figure (Plate V. fig. 3.)

It appeared to be about 200 yards above the ſurface of the earth, wholly luminous, but not emitting ſparks; of a colour reſembling the ſun near the horizon in a cold froſty evening, and which may be called a crimſon red. When paſſing right over the heads of the ſpectators, the light on the ſurface of the earth was little ſhort of the effect of ſun-beams, though at the ſame time, looking another way, the ſtars were viſible; which appears to be a confirmation of the opinion formed of its moderate elevation.

In paſſing, a conſiderable degree of heat was felt by thoſe who ſaw it, but no electric ſenſation. Immediately after it diſappeared in the north-eaſt, a violent ruſhing noiſe was heard, as if the phænomenon was bearing down the foreſt before it, and in a few ſeconds after a tremendous craſh, ſimilar to that of the largeſt piece of ordnance, cauſing at the ſame time a very ſenſible earthquake.

Search being afterwards made in the place where the burning body fell, every vegetable body was found burnt or greatly ſcorched, and a conſiderable portion of the ſurface of the earth broken up.

ASTRONOMY.

Profeſſor Bode, of Berlin, has received two letters from M. Piazzi of Palermo, in which that indefatigable obſerver of the heavens declares that he is now convinced, as profeſſor Bode is, that the ſtar diſcovered on the 13th of January laſt is a planet, though owing to the faintneſs of its light he at firſt thought otherwiſe. He adds alſo, that it will not be viſible till November †. The German aſtronomers propoſe to

* Communicated in a letter from W. Dunbar, Eſq. (one of the gentlemen employed in running the line of demarcation between the United States of America and the Spaniſh ſettlements) to Mr. Swift, of Fenchurch-ſtreet.

† In England the general belief is that this ſuppoſed planet will turn out to be a comet.

call

call this new planet Juno, by analogy to the names of the other planets; but M. Piazzi wishes that it should be called *Ceres Fernandia*, in allusion to Sicily, which is consecrated to Ceres, and to the name of the monarch who at present reigns over that country.

STATISTICS.

The French minister of the interior is now employed in collecting materials for a statistic description of France. An account of fifteen departments has already been received, some of which the minister has published. The names of the departments, respecting which memoirs have been received, are, Allier, Upper Alps, Aude, Cher, La Drôme, Ille-et-Vilaine, Mont-Blanc, Orne, Ourthe, Upper Saône, Two Sevres, Tarn, Var, Vosges, and La Vendée. These memoirs, written by the prefects of the different departments, contain interesting facts, and curious details, respecting the different districts of France, and will form the first collection of positive information respecting the state of that extensive country.

EARTHQUAKE.

The following are the particulars of an earthquake which took place at Bologna, as inscribed in the registers of the observatory, by C. Ciccolini:

This morning (October 8) at 8 hours 52 minutes 53 seconds, the air being calm, the sky overcast, and Reaumur's thermometer at 13 0°, I felt three strong shocks of an earthquake. The direction of the first was from north-east to south-west: the undulation gradually decreased, when a second and a third shock were felt in the same direction as the first. These three shocks were in general taken to be one; but I was able to distinguish them exactly, as they continued half a minute; one of the clocks in the observatory having stopped, I had the exact time of the earthquake. Some chimneys were thrown down in the city, one of which had almost beat in the ceiling of an apartment where the celebrated mathematician Soladini lodges. The remembrance of the earthquakes which afflicted the city of Bologna almost for a whole year, in 1779 and 780, occasioned a general consternation. It must, however, be observed, that the state of the atmosphere was very different from what it was at that period, the sun being then pale, the sky continually obscured by clouds of a lead colour, and the horizon darkened by very thick fogs. Lightning frequently fell, and igneous meteors appeared under different forms; and there were above 80 auroræ boreales. Nothing of the kind took place during the present season. The earthquake was felt with violence at Catene and Ceuta.

XXXIV. *On the chemical Knowledge which the Philosophers of the 16th and 17th Centuries had of the different Gases* *.

MODERN chemistry has been of the utmost service to the science of nature in general, by making us better acquainted with the finer substances of light, caloric, and, in particular, of the gases: but very imperfect would be our knowledge of nature, very poor would be our harvest of new truths, and very insignificant would be our progress in our researches respecting nature, if we had not reason, from the rapid advance which philosophy has made in modern times, to flatter ourselves with the hopes of a more accurate knowledge of these finer substances. We should, however, be guilty of a very great error, if we ascribed the whole merit of this more accurate knowledge to the 18th century; for, though most of the philosophers of the two preceding centuries either overlooked these kinds of gas altogether, or considered them as common air corrupted and deprived of its elasticity, yet some of them were more clear-sighted, and penetrated deeper into these secrets of nature.

If we pass over the remotest traces which occur here and there in the works of the antient writers, J. Baptist van Helmont seems to stand at the head of those more acute-sighted philosophers; for, though it cannot be denied that he disfigured his valuable discoveries by numerous fictions, and concealed them under new and commonly barbarous names, which were for the most part improper; yet he first informed physicians and naturalists, and proved clearly by observations and experiments, that, besides the air which we continually breathe, there are other fluids, which, though they approach very near to the air in subtlety, transparency, and particularly in elasticity, yet differ from it very much; and, as they differ also from vapours, he has distinguished them by the particular name of *gas* †. He endeavoured to establish the difference between them by this circumstance—that as air nourishes flame, these fluids extinguish it ‡, and even the vital flame §. He was acquainted also with the air of the cele-

* From *Göttingisches Journal der Naturwissenschaften*, by J. F. Gmelin, vol. i. p. 4.

† Ortus Medicinæ; Amsterdam 1648. 4to. p. 73.

‡ Ibid. p. 163.

§ Ibid. p. 47. 87. 95. 110. 163.

brated *Grotto del Cane*, near Naples *, as well as with the exhalations in mines, againſt the pernicious effects of which cautions had been given long before his time by Andrew Libavius † and George Agricola ‡; and he knew alſo that they killed animals which had purpoſely or accidentally been expoſed to them, as well as imprudent perſons, and particularly miners. He was in particular acquainted with that gas which ſome, becauſe it inflames when it comes in contact with a burning body, call inflammable gas, and which others, becauſe they conſider it as an eſſential component part of water, name hydrogen gas; and he knew alſo that it had been obſerved before him in mines by Libavius: he convinced himſelf that even eructations were of the ſame nature, and that it formed a principal component part of ſmoke: he likewiſe conſidered flame to be only inflamed ſmoke §. His knowledge, however, was not confined to this gas alone, for under the name of *gas ſylveſtre* he was acquainted with nitrous gas, produced by the action of aquafortis when ſilver is diſſolved in it ||; and he knew alſo, that when it came in contact with the atmoſphere it formed fiery red vapours.

He was well acquainted alſo with the muriatic acid gas; for what elſe could he underſtand by that gas into which, to uſe his own expreſſions ¶, ſal-ammoniac and aquafortis were converted when mixed together and made luke-warm? He knew likewiſe the ſulphurous acid gas; for he ſought in it, as a component part of the vapour of burning ſulphur, the reaſon why that vapour extinguiſhes a lighted candle or taper when expoſed to it.

He had become acquainted alſo, different ways, with that gas which ſhows itſelf in air in which various bodies, and particularly coals, have been burnt **; and on that account he had given it the name of coal gas; though he had ſeen abundance of it in various places, and accompanying other phænomena, both in living human bodies, as for example in belching ††, and on a larger ſcale, as in wine and beer cellars, and during the efferveſcence of different ſubſtances when acids were poured upon them ‡‡, in the *Grotto del Cane* and

* Ortus Medicinæ, p. 110. 163. 615.
† Commentat. Metallic. Francof. ad Mœn. 1597. 4to. p. 297.
‡ De Natura eorum quæ effluunt ex terra.
§ Commentat. Metallic. ut ſupra, p. 421. 414. 84.
|| De Aquis Stygiis, ut ſupra, p. 615. 424.
¶ Ortus Medicinæ, p. 423.
** Ibid. p. 110. 106. 110. 405, 406—437: Tumulus peſtis, p. 55.
†† Ibid. p. 116. 421. 431.
‡‡ Ibid. p. 424; and De Febribus, c. ix. p. 43.

other subterranean places *; and particularly in acid waters, from which it rises in bubbles, and they are indebted to it, he says, for all their healing qualities †. He remarks also of this gas ‡, that, as it extinguishes the flame of a lamp or taper, it extinguishes also the flame of life. He proved, by a very simple experiment §, that the volume as well as the goodness of air is lessened by bodies burning in it; for, having placed a burning taper under a glass in such a manner that it stood three inches above the water in the vessel, he saw the water rise, assume the place of the decreasing air, and at length extinguish the taper.

He had observed also that an air could be produced from nitre, which he called *flame gas*, and which was disengaged on coal being added to it ||; and he thence conjectured the presence of vital air in that salt. He entertained also the opinion, which modern chemistry has supported by so many conclusive proofs, that all these air-like fluids ¶ are indebted for their form to the effects of fire, or, as we commonly say at present, to caloric.

It cannot, however, be denied, that Van Helmont considered as different gases those which differ only by accidental corruption, as the carbonic acid gas, according as it is drawn from this or that body, and in this or that manner; and that he confounded others, or did not make a proper distinction between them. Yet before Priestley, most of the philosophers who paid attention to this subject, made no other difference between these kinds of gas, than that some of them inflame when brought into contact with a burning body, while others instantly extinguish a flame.

Van Helmont suspected also, as appears clearly from his own words, the great similarity which, in regard to air, is found between flame and animal life. This was more clearly perceived by Thomas Willis ** and Francis Sylvius de le Boë ††, a man whom few have equalled in genius or eloquence; for the latter says, that, as flame requires more air than the quietly glowing fire of coal, in the like manner

* Ortus Medicinæ.
† De Lithiasi, c. iv. p. 34.
‡ Ortus Medicinæ, p. 163.
§ Ibid. p. 84.
|| Ibid. p. 423.
¶ Ibid. p. 72.
** Affectionum quæ dicuntur hystericæ et hypochondriacæ Pathologia spasmodica, contra Responsionem epistolarem Nath. Highmori, cui accesserunt Exercitationes medico-physicæ duæ, &c.; Lugd. Bat. 1671. 12mo. p. 80—102.
†† Disputat. de Respiratione, § 69—73.

breathing animals which breathe through real lungs have need of more air than those which merely evaporate, that is to say, than such was have no evident and real lungs; and as, in general, a stronger fire requires a stronger draught of air than a weaker, in the like manner a more active life requires quicker breathing, and a more sluggish a slower: as air acts differently on flame according as it is more or less clear, warm, or dry, in the like manner it acts upon breathing and the vital flame: and, in the last place, as flame burns more briskly according as it has free access to the air on all sides, and is weaker when the air is withheld, and is at length totally extinguished, breathing is exposed to the same effects under the like circumstances.

This Sylvius proved nothing by experiments, but he made it probable, according to his manner, that sourish particles of nitre * floated about in the common atmosphere; and approached so near to the prevailing principles of the present day †, that he supposed fire (or caloric) was continually diffused through the air.

The celebrated Spanish mineralogist Alphonso Barba was acquainted with those pernicious damps which arise so often in mines, and which, though they resemble air in other respects, have however an offensive smell, extinguish lights, and deprive men, birds, and even snakes, of their life ‡.

Boyle, to whom natural philosophy in general is under so many obligations, ascribed that increase of weight which metals acquire by calcination §, as he had frequently observed, rather to the fire than to particles from the atmosphere, and was therefore very remote from that opinion which prevails in the present century, and which is supported by so many experiments; but he was at the same time acquainted with the carbonic acid gas as it rises from coral, when it effervesces with vinegar, from bread, cherries, grapes, pears, apricots, plums, gooseberries, peas, &c. when they ferment ||, and its highly pernicious effects on animals; as also with the inflammable, as it partly occurs in abundance in many mines, for example, those of Hungary ¶, partly as it arises by a solution of iron in diluted sulphuric acid or muriatic acid **,

* Praxis Medicinæ, lib. i. c. 21. § 111.

† Ibid. lib. iii. c. 11. § 47.

‡ Bergbüchlein, Frankf. 1726. part i. c. 2. p. 5—8.

§ Detect. penetrabilit. vitr. p. 296, 297.

|| Second continuation of Physico-mechanical Experiments, art. ii. exp. 11. art. iii. exp. 1. 7. 10, 11. art. v. exp. 1, 2. 13.

¶ Examinat. of Antiperistasis, Oper. b. ii. p. 366.

** Nova Experimenta Physico-mechanica de Vi Aëris elastica, p. 152—154: New Experiments touching the Relation betwixt Flame and Air, Oper. b. iii. p. 255, 256.

and

and its property of inflaming when brought into contact with flame.

The latter as a highly pernicious kind of gas, abundant in coal-pits, is mentioned by Martin Lifter, Jeffop, and P. R. Moflyn; and a fire which took place in a coal-pit by the inflammation of fuch vapour is mentioned by Hodgfon *; a like vapour was found by Beaumont in other fubterranean holes †; by T. Shirley ‡ above a fpring, and Wolfftriegel and Vollgnad § in a fpring. Pope, in the firft volume of the Philofophical Tranfactions, gave a very lively picture of that corrupted kind of gas in a hole near the lake of Agnano; and L. A. Portius ‖ and Leonhard à Capua ¶, that of the *Grotto del Cane* and other caverns in the neighbourhood of Naples; E. Hagendorn ** and F. Hoffmann, the dangerous quality of air in which coals have been burnt ††; and T. Birch, that of air which has been corrupted by the breath of animals ‡‡. S. Ledelius mentions the death of a perfon which took place in a cellar filled with wine in a ftate of fermentation §§. This gas, as it rifes both from fermenting liquids as well as from lixivious falts and earths, when they effervefce with acids, and in which, even at his time, Bernoulli fought for the caufe of this effervefcing ‖‖, was perfectly well known to fir Chriftopher Wren ¶¶. He relates a method by which fuch fluids can be collected and preferved in veffels; and he knew that the above gas is abforbed by water, and he diftinguifhes it very clearly from nitrous gas. Even the nitrous gas of the moderns feems to have been known to Huygens and Papin, for they relate *** that they obtained fuch a fluid from the mixture of fpirit of wine and fpirit of nitre under the receiver of an air-pump.

* Philofophical Tranfactions, vol. x. 1675, no. 117. p. 391, &c. Ibid. no. 119. p. 450. Ibid. vol. xii. 1677. no. 136. p. 895. Ibid. vol. xi. 1676. no. 134.

† Hooke's Philofophical Collections, 1679. 4to. no. 1.

‡ Philofophical Tranfactions, vol. ii. 1667. no. 25. p. 482.

§ Mifcellan. Acad. Cæf. Natur. Curiof 4to. dec. i. Lipf. 1670. obfervat. xxxiii. an. 4 & 5. Francof. & Lipf. 1676. obfervat. clxxi.

‖ Differtation. Variar. Venet. 1683. ii.

¶ Lezioni intorno alla Natura delle Mofete; Napol. 1683. 4to.

** Obfervat. et Hiftoriar. Medico-practicar. rarior. Centur. tres; Rudolft. & Goerliz. 1698. 8vo.

†† Bedenken von dem todlichen Dampfe der Holzkohlen; Halle 1716. 8vo.

‡‡ Hiftory of the Royal Society of London for the Improvement of Natural Knowledge; London, 4to. vol. ii. 1668.

§§ Ephemerid. Acad. Cæfar. Nat. Curiof. dec. iii. an. 2. obf. 45.

‖‖ Differtatio de Effervef. et Fermentatione; Bafil. 1690. 4to.

¶¶ Philofophical Tranfactions, vol. x. 1675. p. 446. *** Ibid.

At the ſame period, F. Slare and T. Willis aſcribed * the dark red colour of the blood to the air; and J. Mayow †, another Engliſhman, with whom another Oxford phyſician, Henry Mund ‡, and alſo Willis §, L. M. Barbieri, and J. B. Giovanni ||, concurred, made the whole uſe of breathing to conſiſt in this—that the lungs of animals inhale from the atmoſphere nitrous particles, which are diffuſed over the animal ſpirits and communicate warmth to the blood. J. N. Pechlin ¶, alſo, attributes the faculty of ſome divers being able to remain longer under water to a greater quantity of nitre. From all this it appears that the phyſicians of that period had a kind of idea of vital air, and its influence on the animal œconomy.

Stephen Hales, who made further progreſs by his numerous experiments in diſcovering the ſecrets of nature, placed beyond all doubt, by a long ſeries of experiments, the elaſticity of theſe fluids as they are expelled from bodies by heat, fermentation, corruption, and efferveſcence, a power which was before obſerved by Newton **, and at the ſame time ſhowed ſeveral and ingenious methods †† how they could be obtained, preſerved, meaſured, weighed, and even handled. He remarked the inflammability of thoſe which the inflammable bodies of every kingdom of nature yield by expoſure to a ſtrong heat, the properties of others which ariſe from efferveſcing mixtures, and which ſuddenly extinguiſh flame. He obſerved likewiſe, exceedingly well, the great difference of the nitrous gas which he obtained when he poured upon antimony *aqua regia*, or ſpirit of nitre; or the latter diluted with water [aquafortis] upon iron filings or quickſilver, and its property of forming red clouds with common air as ſoon as it comes in contact with it; and, by repeated experiments, that it abſorbed a great portion of air; and alſo, that the oftener the experiment was repeated, the gas always abſorbed the leſs air; that ſeveral of theſe gaſes were abſorbed by water; how much the beſt air is corrupted by the breathing of even the ſoundeſt men, ſo that at laſt it is totally

* Philoſophical Tranſactions for 1682, no. 204. Ibid. p. 92—94.

† Tractatus quinque Medico-phyſici; Oxon. 1699. 8vo. no. 1,

‡ Βιοχρηστολογια; Oxon. 1680. 8vo.

§ Philoſoph. Tranſact. ut ſupra.

|| Spiritus Nitro Aërii Operationes in Microcoſmum; Bonon. 1681. 12mo. Diſſertation, ſur la Fermentation ſur le Nitre et ſur l'Air; Toulouſe 1685. 12mo.

¶ De Aëris et Alimenti Defectu; Kilon. 1676. 8vo.

** Optics; London 1701. 4to.

†† Statical Eſſays; London, 8vo. third edit. 1783. vol. i. c. 6. and vol. ii. p. 315. 317.

unfit

unfit for respiration. He knew also the ammoniacal as well as the muriatic acid gas, and the sulphuric acid gas, and had learned, from his own experience, that the latter can be as strongly compressed as common air. He knew also that metals increase in weight by calcination, and again decrease on being revived; for he found that red lead in the preparation had increased in weight about a twentieth part, and by a strong heat gave a great deal of air: his own experience had taught him also that phosphorus, sulphur, and a tallow candle, absorb some of the air in which they burn, as animals absorb some of the air in which they breathe; though he ascribed the pernicious change which the air thereby experiences, not to the loss it sustains, but rather to the corrupt evaporation with which it is filled. He had observed, though less perfectly, that phosphorus after combustion increased in weight by imbibing something from the atmosphere. He had discovered that an aëriform substance was contained in acidulous waters, and that air was continually absorbed by plants in a healthy state *.

In this difficult doctrine, however, he left a great deal to be explained by his followers; for he did not define and was not acquainted with the difference of many of the gases, and some of them escaped his notice altogether. The further illustration of them was reserved for the modern chemists. Thus, in particular, the following authors have very much contributed to enlarge our knowledge of the carbonic acid:—Dr. Black in Medical and Philosophical Commentaries, by a Society in Edinburgh, vol. ii.; Dr. Macbride in Experimental Essays on Medical and Philosophical Subjects, London 1764. 8vo; T. Henry in his Experiments and Observations, London 1773. 8vo.; T. And. Emmer in *Dissertatio de Aëre fixo seu Acido aëreo*, Edin. 1784; and the Dutch naturalist D. De Smedt in *Dissertatio de Aëre fixo*, Ultraj. 1773. 8vo. Among the Germans, N. J. Jacquin in *Examen Chemicum Doctrinæ Meyerianæ de Acido pingui, et Blackianæ de Aëre fixo respectu Calcis*, Vindob. 1769. 8vo; and the late J. J. Well, in his Defence of Dr. Black's Doctrine respecting fixed Air, against the Objections made by Wiegleb, Vienna 1771. 8vo.; and on The Causes of the heating of unslaked Lime, Vienna 1772. 8vo. Among the Italians, F. F. Fontana in his Physical Researches respecting fixed Air, Florence 1774. 8vo. In Swisserland, Sol. Schinz in *De Aëre, ejus Speciebus, præcipue de Aëre fixo Lapidis calcarei*, Turic. 1778. 4to.; and in Sweden, T. Bergman in

* Statical Essays passim.

De Acido aërio Opuscula Physica et Chemica, vol. i. p. 1, &c. The last-mentioned chemist showed also, in his treatise *De Analysi Aquarum* in his *Opuscula*, vol. i. p. 68, as did in England, Browgrigg in the Philosophical Transactions, vol. lxiv. part 2. no. 39; and, in France, Venel in *Mémoires presentés à l'Academie des Sciences à Paris par divers Savans*, vol. ii. that it is to this acid that acid waters are principally indebted for their properties; so that several chemical authors, such as Bergman *De Aquis medicatis frigidis arte parandis*, in his *Opuscula*, vol. i. art. 6. p. 186; Venel in the work above quoted; Rouelle in Roux's *Journal de Medecine, Chirurgie*, &c. for May 1774; Duchanoy *Essai d'imiter les Eaux Minerales*, Paris 1780. 8vo.; Laugier *Mineralogie nouvelle, ou l'Art de faire les Eaux Minerales*, Sens 1786. 8vo.; Meyer, apothecary to the court at Stettin, in the Transactions of the Friendly Society of the Searchers into Nature of Berlin, vol. iv. 1781. p. 313; and the late professor Köstlin in his Method of imitating the Water of Acid Springs by Means of fixed Air, Stuttgard 1780. 8vo., have described the process by which they can be exceedingly well imitated.

With the like care and ingenuity the following writers have examined the nature of the inflammable kinds of gas:—Volta *Lettere sull' Aria inflammabile delle Paludi*, Milan 1777; Senebier *Recherches analytiques sur la Nature de l'Air inflammable*, Geneva 1784. 8vo.; Minkelers *Memoire sur l'Air inflammable de differentes Substances*, Louvain 1784. 8vo.; Moscheni *Esame intorno alla Natura e Proprieta dell' Aria inflammabile paludosa*, Lucca 1788; Lassone in *Mémoires de l'Academie des Sciences à Paris* for the year 1766; Kirwan, Philosophical Transactions, vol. lxxvi. 1786. part i. Gengembre in *Memoires present s à l'Academie des Sciences par divers Savans Etrangers*, vol. x.; Raymond *Annales de Chimie*, vol. x. 1791. p. 19; Nieuwland, Deiman, Trooftwyk, and Bondt, *Recherches Physico-chymiques*, Amsterd. 8vo. 1792. no. 1. The three last, together with Lauremburg, examined the different kinds of inflammable gas. See Crell's Chemical Annals 1795. vol. ii. p. 195. 310. 430; and 1796. vol. ii. p. 110 and 222.

Respecting the common nitrous gas, we have the following works:—Fontana *Recherches physiques sur la Nature de l'Air nitreux et de l'Air dephogistique*, Paris 1776. 8vo.; count Morozzo *Lettre sur la Composition du Gas mephitique et du Gas nitreux*, Turin 1784. 8vo.; respecting Priestley's so called dephlogisticated nitrous air, Bochaute in *Memoires de l'Academie des Sciences à Bruxelles* 1783; and Deiman, Bondt, Nieuwland, and Trooftwyk, *Recherches Physico-chymiques*,

miques, no. 2. 1793; respecting azote, professor Schmidt in Gren's Journal of Natural Philosophy, vol. i. no. 3; and Wiegleb in Crell's Chemical Annals, 1796, vol. ii. p. 467; respecting the inflammable muriatic acid gas, Westrumb in Crell's Annals 1790, vol. i. p. 3. 100.

But this new field in the province of natural knowledge has been cultivated in a more extensive manner by the following authors:—In Italy, Barbarigo in his *Saggi Fisici*, Padova 1779. 8vo.; F. Fontana in *Memorie di Matematica e Fisica della Societa Italiana*, Verona, 4to. vol. i. 1782; Bucci *Osservazioni circa il Flogisto e le differente Specie d'Aria secondo le moderne Scoperte*, Pavia 1783. 8vo.; and Volta in *Proposizioni ed Esperienze di Aèrologia*, Como 1776. In France, by Berthollet in *Observations sur l'Air*, Paris 1776. 12mo.; Bucquet in *Mémoires présentés à l'Académie des Sciences à Paris par divers Savans*, vol. vii.; Corrinus *Dissert. sistens Historiam Aëris factitii*, Argentor. 1776. 4to.; De la Metherie *Essai analytique sur l'Air pur et les différentes Espèces d'Air*, Paris 1785. 8vo.; Rouland *Tableau historique des Propriétés et des Phénomènes de l'Air, consideré dans ses differens Etats et sous ses divers Rapports*, Paris 1784. 8vo.; Sigaud de la Fond *Essai sur différentes Espèces d'Air, qu'on designe sous le Nom d'Air fixe*, Paris 1778. 8vo.; and Thouvenel *Mémoire Chimique et Medicinal sur la Nature, les Usages, et les Effets de l'Air, et des Airs, des Alimens, et des Medicamens, relativement à l'Economie Animale, Ouvrage couronné par l'Académie de Toulouse* 1778, Paris, 4to. In England, the anonymous author of a Treatise on the various Kinds of permanently elastic Fluids or Gases, 1777. 8vo; P. Plunket *Dissertat. de Aëre Mephitico*, Edin. 1779; Cavallo on the Nature and Properties of Air; Cavendish in the Philosophical Transactions, vol. lvi; and Higgins's Experiments and Observations relating to acetous Acid, fixable Air, dense inflammable Air, &c. London 1786. 8vo. In the Netherlands, P. Von Trooftwyk and Deiman, *Antw. op de Vraage: Welke zyn de waarlyck onderscheidene Soorten der luchtgelykende Vloeistoffen*, &c. Haarlem 1786. 8vo. And in Germany, Achard in *Nouveaux Mémoires de l'Acad. des Sciences à Berlin*, 1781; Herbert *De Aëre Fluidisque ad Aëris Genus pertinentibus*, Vienn. 1780. 8vo.; Leonhardi *Program. Aërologiæ physico-chemicæ recentioris primæ Lineæ*, Lipf. 1781; and Weber *Ueber gemeine und durch Auflösung aus Körpern entwickelte Luft*, Landshut 1785. 8vo.

But among the philosophers who have endeavoured to explain and illustrate the nature of these gases by numerous experiments, made with great care, and accurately described, none

none have diſtinguiſhed themſelves more than Scheele and Dr. Prieſtley. The former a native of Germany, but ſettled in Sweden, invented very ſimple proceſſes, yet well calculated to anſwer the propoſed end, for examining theſe gaſes, by which means he diſcovered new kinds, and proved the exiſtence of thoſe already known in a much clearer method than had been done before in his *Abhandlung von der Luft und dem Feuer nebſt einem Vorbericht von T. Bergman*, Upſal and Leipſic 1777. 8vo.; and in *Kongl. Svenſk. Vetenſkap. Academ. Handling*, 1774. p. 84. The latter, being furniſhed with a more extenſive apparatus, placed in a clearer point of view, not only the nature and differences of the gaſes before diſcovered, but diſcovered new ones alſo. An account of his experiments may be ſeen in Experiments and Obſervations on the different Kinds of Air, London, 8vo. vol. i. 1774, vol. ii. 1775, vol. iii. 1777; Experiments and Obſervations relating to various Branches of Natural Philoſophy, with a Continuation of Obſervations on Air, 8vo. vol. i. London 1779, vol. ii. Birmingham 1782, vol. iii. 1790; Directions for impregnating Water with fixed Air, &c. London 1772, 8vo.; Philoſophical Tranſactions of the American Society 1796.

XXXV. *An Account of ſome Galvanic Combinations, formed by the Arrangement of ſingle Metallic Plates and Fluids, analogous to the new Galvanic Apparatus of Mr.* VOLTA. *By Mr.* HUMPHRY DAVY, *Lecturer on Chemiſtry in the Royal Inſtitution**.

I. ALL the galvanic combinations analogous to the new apparatus of Mr. Volta, which have been heretofore deſcribed by experimentaliſts, conſiſt (as far as my knowledge extends) of ſeries containing at leaſt two metallic ſubſtances, or one metal and charcoal, and a ſtratum of fluid. And it has been generally ſuppoſed that their agencies are, in ſome meaſure, connected with the different powers of the metals to conduct electricity. But I have found that an accumulation of galvanic influence, exactly ſimilar to the accumulation in the common pile, may be produced by the arrangement of ſingle metallic plates, or arcs, with different ſtrata of fluids.

The train of reaſoning which led to the diſcovery of this fact, was produced by the obſervation of ſome phænomena relating to the connection of chemical changes with the evolution of galvanic power.

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

It

It appeared, in ſeveral experiments, that ſeries of double metallic plates, incapable of acting as galvanic combinations, when arranged in the proper order, with portions of water, were readily made to produce galvanic effects, by being alternated with acids, or other fluids capable of oxidating one only of the metals of the ſeries. Thus, double plates, compoſed of ſilver and gold, (metals which have been ſuppoſed to differ very little in their powers of conducting electricity,) produced galvanic action, when placed in contact, in the common order, with cloths moiſtened in diluted nitric acid. And copper and ſilver acted powerfully with nitrate of mercury.

Theſe facts induced me to ſuppoſe, that the alternation of two metallic bodies with fluids, was eſſential to the production of accumulated galvanic influence, only ſo far as it furniſhed two conducting ſurfaces of different degrees of oxidability; and that this production would take place, if ſingle metallic plates could be connected together by different fluids in ſuch a manner, that one of their ſurfaces only ſhould undergo oxidation, the arrangement being regular.

On this ſuppoſition, I made a number of experiments on different arrangements of ſingle metals and fluids; and, after many various proceſſes, I was enabled to aſcertain, that many of theſe arrangements could be made active, not only when oxidations, but likewiſe when other chemical changes were going on in ſome of their parts.

In deſcribing the different galvanic combinations formed by ſingle metallic plates and fluids, I ſhall divide them into three claſſes, following, in the arrangement, the order of time with regard to diſcovery.

II. The firſt and moſt feeble claſs is compoſed, whenever ſingle metallic plates, or arcs, are arranged in ſuch a manner, that two of their ſurfaces, or ends oppoſite to each other, are in contact with different fluids, one capable, and the other incapable, of oxidating the metal. In this caſe, if the ſeries are numerous, and in regular alternation, galvanic influence will be accumulated, analogous, in all its effects, to the influence of the common pile.

Tin, zinc, and ſome other eaſily oxidable metals, act moſt powerfully in this claſs of combinations.

If pieces of poliſhed tin, about an inch ſquare and $\frac{1}{20}$ of an inch thick, be connected with woollen cloths of the ſame ſize, (moiſtened, ſome in water, and ſome in diluted nitrous acid,) in the following order—tin, acid, water, and ſo on, till twenty ſeries are put together—a feeble galvanic battery will be formed, capable of acting weakly on the organs of ſenſe,

ſenſe, and of ſlowly producing the common appearances in water; the wire from the oxidating ſurface of the plates evolving hydrogen, and the wire from the non-oxidating ſurface (when of ſilver) depoſiting oxide.

In all caſes, when the batteries of the firſt claſs are erected perpendicularly, the cloth moiſtened in acid muſt be placed under the cloth moiſtened in water; and, in this arrangement, as the acid is ſpecifically heavier than water, little or no mixture of the fluids will take place.

When zinc is employed, on account of its rapid oxidation in water containing atmoſpheric air, three cloths ſhould be uſed; the firſt moiſtened in weak ſolution of ſulphuret of potaſh, (which is poſſeſſed of no power of action upon zinc, and which prevents it from acting upon the water;) the ſecond moiſtened in a ſolution of ſulphate of potaſh, of greater ſpecific gravity than the ſolution of ſulphuret; and the third wetted in an oxidating fluid ſpecifically heavier than either of the ſolutions. In this caſe, if the order be as follows—zinc, oxidating ſolution, ſolution of ſulphate of potaſh, ſolution of ſulphuret of potaſh—very little mixture of the fluids, or chemical action between them, will take place; and an alternation of twelve ſeries of this kind forms a battery capable of producing ſenſible effects.

III. The ſecond claſs of galvanic combinations with ſingle plates is formed, when plates, or arcs, compoſed of a metallic ſubſtance capable of acting upon ſulphurated hydrogen, or upon ſulphurets diſſolved in water, are formed into ſeries, with portions of a ſolution of ſulphuret of potaſh and water, in ſuch a manner, that one ſide of every plate, or arc, is in contact with water, whilſt the oppoſite ſide is acted on by the ſolution of ſulphuret. Under theſe circumſtances, when the alternation is regular, and the number of ſeries ſufficiently great, galvanic power is evolved; and water, placed in the circuit with ſilver wires, is acted on; oxide being depoſited on the wire connected with the ſide of the plate undergoing chemical alteration, whilſt hydrogen is evolved from the ſide in contact with water.

Silver, copper, and lead, are each capable of forming this combination. Plates made from either of thoſe metals may be arranged with cloths, (moiſtened, ſome in water, and others in ſolution of ſulphuret of potaſh,) in the following order—metal, cloth moiſtened in ſulphuret of potaſh, cloth moiſtened in water, and ſo on.

Eight ſeries will produce ſenſible effects; and the wire from the top of the pile produces oxide.

Copper

Copper is more active, in this class of batteries, than silver; and silver more active than lead.

IV. The third and most powerful class of galvanic batteries, constructed with fluids and single metals, is formed when metallic substances oxidable in acids, and capable of acting on solutions of sulphurets, are connected, as plates, with oxidating fluids and solutions of sulphuret of potash, in such a manner, that the opposite sides of every plate may be undergoing different chemical changes; the mode of alternation being regular.

The same metals that act in the second class may be used in the third class; and the order of their powers is similar. The pile may be erected in the same manner as the pile with zinc in the first class; the cloths moistened in acid being separated from those moistened in solution of sulphuret by a third cloth soaked in solution of sulphate of potash.

Three plates of copper or silver, arranged in this manner, in the just order, produce sensible effects; and twelve or thirteen series are capable of giving weak shocks, and of rapidly producing gas and oxide in water; the wire connected with the oxidating end of the apparatus evolving hydrogen; and the wire attached to the end acting on the sulphuret, depositing oxide when composed of silver, and generating oxygen when of gold.

V. In all the single metallic piles constructed with cloths, the action is very transient: the decomposition of the acids, and of the sulphurets, is generally completed in a few minutes; and, in consequence, the galvanic influence ceases to be evolved. The arrangement of all the different series may, however, (by means of an apparatus constructed after the ideas of count Rumford,) be made in such a manner as to give considerable permanency to their effects. This apparatus is a box, covered with cement incapable of conducting electricity, and composed of three pieces of mahogany, each containing grooves capable of receiving the edges of the different plates proper for composing the series. One half of these plates must be composed of horn or glass, and the other half of metallic substances; and the conductors of electricity, and the non-conductors, must be alternately cemented into the grooves, so as to form water-tight cells.

When the apparatus is used, these cells are filled, in the galvanic order, with different solutions, according to the class of the combination; and connected in pairs with each other, by slips of moistened cloth, carried over the non-conducting plates.

A combination of fifty copperplates, arranged in this manner,

ner, with weak solutions of nitrous acid, or nitrate of ammoniac, and sulphuret of potash, gives pretty strong shocks, rapidly evolves gas from water, and affects the condensing electrometer.

It does not lose its power of action for many hours; and, when this power is lost, it may be restored by the addition of small quantities of concentrated solutions of the proper chemical agents to the fluids in the different cells.

From two experiments made on copper and silver, it would appear, that the single metallic batteries act equally well, when the metals made use of are slightly alloyed, and when they are in a state of purity.

XXXVI. *Experiments on the chemical Production and Agency of Electricity.* By William Hyde Wollaston, M. D. F. R. S.*

Notwithstanding the power of Mr. Volta's electric pile is now known to be proportional to the disposition of one of the metals to be oxidated by the fluid interposed, a doubt has been entertained by many persons, whether this power arises from the chemical action of the fluid on the metal; or, on the contrary, whether the oxidation itself may not be occasioned by electricity, set in motion by the contact of metals that have different conducting powers.

That the oxidation of the metal is the primary cause of the electric phænomena observed, is, I think, to be inferred from the following experiments, which exhibit the galvanic process reduced to its most simple state.

Exper. 1. If a piece of zinc and a piece of silver have each one extremity immersed in the same vessel, containing sulphuric or muriatic acid diluted with a large quantity of water, the zinc is dissolved, and yields hydrogen gas, by decomposition of the water: the silver, not being acted upon, has no power of decomposing water; but, whenever the zinc and silver are made to touch, or any metallic communication is made between them, hydrogen gas is also formed at the surface of the silver.

Any other metal beside zinc, which by assistance of the acid employed is capable of decomposing water, will succeed equally, if the other wire consists of a metal on which the acid has no effect.

Exper. 2. If zinc, iron, or copper, are employed with gold,

* From the same.

in

in dilute nitric acid, nitrous gas is formed, in the same manner, and under the same circumstances, as the hydrogen gas in the former experiment.

Exper. 3. Experiments analogous to the former, and equally simple, may also be made with many metallic solutions. If, for instance, the solution contains copper, it will be precipitated by a piece of iron, and appear on its surface. Upon silver merely immersed in the same solution, no such effect is produced; but as soon as the two metals are brought into contact, the silver receives a coating of copper.

In the explanation of these experiments, it is necessary to advert to a point established by means of the electric pile.

We know that when water is placed in a circuit of conductors of electricity, between the two extremities of a pile, if the power is sufficient to oxidate one of the wires of communication, the wire connected with the opposite extremity affords hydrogen gas.

Since the extrication of hydrogen, in this instance, is seen to depend on electricity, it is probable that, in other instances, electricity may be also requisite for its conversion into gas. It would appear, therefore, that in the solution of a metal, electricity is evolved during the action of the acid upon it; and that the formation of hydrogen gas, even in that case, depends on a transition of electricity between the fluid and the metal.

We see, moreover, in the first experiment, that the zinc, without contact of any other metal, has the power of decomposing water; and we can have no reason to suppose that the contact of the silver produces any new power, but that it serves merely as a conductor of electricity, and thereby occasions the formation of hydrogen gas.

In the third experiment also, the iron by itself has the power of precipitating copper by means, I presume, of electricity evolved during its solution; and here likewise the silver, by conducting that electricity, acquires the power of precipitating the copper in its metallic state.

The explanation here given receives additional confirmation from comparative experiments which I have made with common electricity; for it will be seen that the same transfer of chemical power, and the same apparent reversion of the usual order of chemical affinities in the precipitation of copper by silver, may be effected by a common electrical machine.

The machine with which the following experiments were conducted, consists of a cylinder 7 inches in diameter, with a conductor on each side 16 inches long and $3\frac{1}{2}$ inches diameter,

meter, each furnished with a sliding electrometer to regulate the strength of the spark received from them.

Exper. 4. Having a wire of fine silver, $\frac{1}{120}$ of an inch in diameter, I coated the middle of it, for 2 or 3 inches, with sealing-wax, and, by cutting through in the middle of the wax, exposed a section of the wire. The two coated extremities of the wire, thus divided, were immersed in a solution of sulphate of copper placed in an electric circuit between the two conductors; and sparks, taken at $\frac{1}{10}$ of an inch distance, were passed by means of them through the solution. After 100 turns of the machine, the wire which communicated with (what is called) the negative conductor had a precipitate formed on its surface, which, upon being burnished, was evidently copper; but the opposite wire had no such coating.

Upon reversing the direction of the current of electricity, the order of the phænomena was of course reversed; the copper being shortly redissolved by assistance of the oxidating power of positive electricity, and a similar precipitate formed on the opposite wire.

Exper. 5. A similar experiment made with gold wires $\frac{1}{100}$ of an inch diameter, in a solution of corrosive sublimate, had the same success.

The chemical agency, therefore, of common electricity is thus proved to be the same with the power excited by chemical means; but, since a difference has been observed in the comparative facility with which the pile of Volta decomposes water, and produces other effects of oxidation and deoxidation of bodies exposed to its action, I have been at some pains to remove this difficulty, and can at least produce a very close imitation of the galvanic phænomena by common electricity.

It has been thought necessary to employ powerful machines and large Leyden jars for the decomposition of water; but, when I considered that the decomposition must depend on duly proportioning the strength of the charge of electricity to the quantity of water, and that the quantity exposed to its action at the surface of communication depends on the extent of that surface, I hoped that, by reducing the surface of communication, the decomposition of water might be effected by smaller machines, and with less powerful excitation, than have hitherto been used for that purpose; and in this hope I have not been disappointed.

Exper. 6. Having procured a small wire of fine gold, and given it as fine a point as I could, I inserted it into a capillary

lary glafs tube; and, after heating the tube fo as to make it adhere to the point, and cover it in every part, I gradually ground it down, till, with a pocket lens, I could difcern that the point of the gold was expofed.

The fuccefs of this method exceeding my expectations, I coated feveral wires in the fame manner, and found, that when fparks from the conductors before mentioned were made to pafs through water, by means of a point fo guarded, a fpark paffing to the diftance of $\frac{1}{5}$ of an inch would decompofe water, when the point expofed did not exceed $\frac{1}{700}$ of an inch in diameter. With another point, which I eftimated at $\frac{1}{1500}$, a fucceffion of fparks, $\frac{1}{20}$ of an inch in length, afforded a current of fmall bubbles of air.

I have fince found that the fame apparatus will decompofe water with a wire $\frac{1}{10}$ of an inch diameter, coated in the manner before defcribed, if the fpark from the prime conductor paffes to the diftance of $\frac{1}{10}$ of an inch of air.

Exper. 7. In order to try how far the ftrength of the electric fpark might be reduced by proportional diminution of the extremity of the wire, I paffed a folution of gold in *aqua regia* through a capillary tube, and, by heating the tube, expelled the acid. There remained a thin film of gold lining the inner furface of the tube, which, by melting the tube, was converted into a very fine thread of gold, through the fubftance of the glafs.

When the extremity of this thread was made the medium of communication through water, I found that the mere current of electricity would occafion a ftream of very fmall bubbles to rife from the extremity of the gold, although the wire, by which it communicated with the pofitive or negative conductor, was placed in abfolute contact with them. Hence it appears that decompofition of water may take place by common electricity as well as by the electric pile, although no difcernible fparks are produced.

The appearance of two currents of air may alfo be imitated by occafioning the electricity to pafs by fine points of communication on both fides of the water; but, in fact, the refemblance is not complete; for, in every way in which I have tried it, I obferved that each wire gave both oxygen and hydrogen gas, inftead of their being formed feparately, as by the electric pile.

I am inclined to attribute the difference in this refpect to the greater intenfity with which it is neceffary to employ common electricity; for, that pofitive and negative electricity, fo excited, have each the fame chemical power as they are

 obferved

obferved to have in the electric pile, may be afcertained by other means.

In the precipitation of copper by filver, an inftance of deoxidation (or phlogiftication) by negative electricity has been mentioned: the oxidating power of pofitive electricity may be alfo proved by its effect on vegetable blue colours.

Exper. 8. Having coloured a card with a ftrong infufion of litmus, I paffed a current of electric fparks along it, by means of two fine gold points, touching it at the diftance of an inch from each other. The effect, as in other cafes, depending on the fmallnefs of the quantity of water, was moft difcernible when the card was nearly dry. In this ftate, a very few turns of the machine were fufficient to occafion a rednefs at the pofitive wire, very manifeft to the naked eye. The negative wire, being afterwards placed on the fame fpot, foon reftored it to its original blue colour.

By Mr. Volta's apparatus, the fame effects are produced in much lefs time.

Befide the fimilarity which has thus been traced between the effects of electricity excited by the common machine and thofe obferved from the electric pile, I think it appears alfo probable that they originate from the fame fource.

With regard to the latter, its power is now known to depend on oxidation; fo alfo does the excitement in the former appear very much to depend on the fame procefs; for,

Exper. 9. I have found, that by ufing an amalgam of filver or of platina, which are not liable to be oxidated, I could obtain no electricity. An amalgam of tin, on the contrary, affords a good degree of excitement. Zinc acts ftill better; but the beft amalgam is made with both tin and zinc, a mixture which is more eafily oxidated than either metal feparately.

Exper. 10. But, as a further trial whether oxidation affifts in the production of electricity, I mounted a fmall cylinder, with its cufhion and conductor, in a veffel fo contrived that I could at pleafure change the contained air.

After trying the degree of excitement in common air, I fubftituted carbonic gas, and found that the excitement was immediately deftroyed, but that it returned upon readmiffion of atmofpheric air.

In conformity to this hypothefis, we find that the metal oxidated is, in each cafe, in a fimilar ftate of electricity; for the cufhion of the machine, by oxidation of the amalgam adhering to it, becomes negative; and, in the fame manner, zinc oxidated by the accumulated power of an electric pile, or fimply by action of an acid, is alfo negative.

This

This similarity in the means by which both electricity and galvanism appear to be excited, in addition to the resemblance that has been traced between their effects, shows that they are both essentially the same, and confirms an opinion that has already been advanced by others, that all the differences discoverable in the effects of the latter, may be owing to its being less intense, but produced in much larger quantity.

XXXVII. *On the Presensation which Animals have of Changes in the Weather.* By Dr. F. A. A. Meyer, *at Göttingen* *.

I SHOULD only repeat what has been already long known, if I should attempt to prove that, by the great enlargement of our knowledge respecting the natural history of foreign animals, by means of various learned travellers, we are now enabled to explain, to the satisfaction of the judicious naturalist, many phænomena which occur among indigenous animals. The sensations which take place in animal bodies before a change of weather, which animals express by various external appearances, and which, without taking part in the dispute whether animals are endowed or not with souls, may be called a *presensation*, seem however to require considerable explanation. I am well aware that it will be impossible for me to explain things which have not as yet been perfectly explained by men of the greatest knowledge; but I am convinced that every thing which can contribute to the illustration of an obscure subject deserves to be submitted to a proof, and that it is of use to communicate even single observations on points concerning which systems cannot be formed till after the expiration of centuries. Considering the subject in this point of view, I present the following.

Presensation may be admitted under three heads:—I. The presensation which animals have of dry fair weather. II. The presensation which animals have of rainy weather. III. The presensation which animals have of stormy weather.

What regards the two first classes of the presensation of animals is taken from the Göttingen Pocket Almanac for the year 1779, the editor of which, as is well known, was at that period counsellor Lichtenberg. In that work I found the most authentic observations of the latest writers collected together.

* From *Magazin für das Neueste aus der Physik*, vol. vii.

together. The observations respecting the third class I collected myself.

First, then, respecting the presensation which animals have of fair dry weather.

Clear, dry weather generally follows after wet weather, when the atmosphere has been freed from the vapours collected in it by their falling to the earth in rain. Clouds as well as rain are the means by which the air frees itself from the electric vapours that are continually arising; and if these again fall down, it appears very natural that animals, which live chiefly in the open air, should express, by various external movements, the ease with which they breathe, and perform all the vital functions. From this principle it seems not difficult to explain the following observations:

The fluttering of bats in the evening, beetles flying about on the highways, and the sporting of gnats towards sunset, require no explanation. I shall only remark, what is already well known to every observer, that this presensation is highly useful to bats as well as to insects. Every shower of rain would render it impossible for them to fly, as their wings are not secured by any oily matter against moisture: they would therefore be rendered much heavier by rain, and unfit for flying, and they could not be so easily placed again in folds, which, considering the structure of these animals, is absolutely necessary, as when they have remained dry.

The same principle seems to be applicable to the high flight of larks and swallows, which perhaps hasten to the upper regions of the atmosphere because they are freer from vapours and more suited to them, and because the lower regions, being more loaded with vapours, afford them less pleasure than those above. The insects also which they pursue for food take then, perhaps, a higher flight.

The croaking of the green water-frog in ponds I cannot sufficiently explain; but it seems to express the pleasure arising from the greater quantity of insects then flying about, and which they can catch with more ease and convenience. But clear dry weather is not so agreeable to frogs as the return of warm weather. If they make a noise in the time of cold rain, warm dry weather will follow. But if the dry weather proceeds from raw winds, and if warmth and rain succeed, their noise may foretel rain; and therefore Linnæus's rule *prædicit pluviam* will lose nothing of its truth. He seems so much the more to be right, as more raw than warm dry days take place in the climate of Sweden. I have, to my great inconvenience, experienced the truth of his assertion,

tion, on journeys which I was under the neceſſity of continuing for ſeveral days.

That the weather-fiſh * (*cobitis foſſilis*) leaves the water quite pure during dry weather, and the green frog † fits at the top of the glaſs, may proceed from the lighter or heavier ſtate of the atmoſphere, particularly as the latter is remarkably fond of cleanlineſs and moderately pure air.

The aſſembling of ravens in the fields, and the ſinging of the wood-pigeon, may be eaſily accounted for from the above principles.

I have never ſeen birds in good weather dreſs their feathers with oil from their fat glands, in order to ſecure them from rain; but I have obſerved many do ſo when the atmoſphere was overcaſt, and when there was an appearance of rain. I ſhould therefore include this circumſtance in the following claſs, did not experience admit alſo of another explanation, viz. that the birds, from the atmoſphere becoming lighter, hope for the ſpeedy arrival of dry weather, and therefore anoint themſelves, and ſecure their feathers from moiſture, that they may be able to fly higher than uſual with the leſs impediment. If the laſt explanation ought not to be altogether rejected, as I do not think it can, we may admit of this obſervation; eſpecially as all the experience of men worthy of belief allows of no reaſoning to be brought againſt it.

The expreſſion of animals which ſhow a preſenſation of rainy weather may be explained partly from the increaſing weight of the atmoſphere, partly from their manner of living, and partly from the want of moiſture which is neceſſary to their exiſtence.

The reſtleſſneſs of domeſtic cattle may proceed from many cauſes. It is known that the atmoſphere in ſummer, before rain falls, is generally heavier, on account of the electric vapour that ariſes. The inſects which infeſt cattle, and which mark this heavineſs, become then more numerous, and, getting into the ſtalls where the cattle are kept, torment them and make them reſtleſs. The aſcending vapour has alſo perhaps ſome influence on the ſkins of theſe animals, which ceaſes when the earth does not ſuffer ſo much vapour to eſcape as before; or the air, too ſtrongly charged with electricity, excites in them an unpleaſant ſenſation. It in-

* So called becauſe kept in Germany to foretel changes of the weather. When the weather is fine they continue quiet, but before a ſtorm or rain are very reſtleſs.—EDIT.

† This animal, though very common in many parts of Europe, is not found in England.—EDIT.

 deed

deed appears strange to explain the same phænomenon from two perfectly opposite causes, a want and an excess of electricity; but we know cases of the like kind in medicine, such, for example, as that where the cramp and sleep produces atonia. People who have wounds or old ulcers feel, on a change of weather, a contraction and burning in those parts; and why should not such affections take place in animals?

All these grounds taken together will be sufficient, in my opinion, to explain why horses and asses rub themselves, shake their heads, and snuff the air by turning up their noses; why asses bray much, and jump about; why cattle scrape up the earth, and stamp with their feet; and why swine, though not hungry, eat greedily and dig up the earth a great deal with their snouts. The restlessness, running about, scraping with the feet, and eating grass, among dogs; and moles continually throwing up the earth, can all be deduced from the same; as well as the cats dressing themselves.

I have remarked that cocks crow on every change of weather, besides at the usual time. They as well as pigeons hasten to their places of shelter in order to be secured against the rain, the approach of which they must be sensible of, by the continually increasing weight of the atmosphere.

The cause of fowls, pigeons, quails, and other birds, washing themselves, appears to me to be a certain heat or itching, which they wish by these means to remove.

Swallows, in all probability, take a low flight on the approach of rainy weather, because the electric atmosphere is too heavy for them, and because they have not sufficient strength to mount above it. But cranes, as being stronger birds, employ all their strength to rise above it, and therefore fly so high.

I have remarked in ravens, that their croaking, unless when they smell carrion, proceeds from fear. They observe perhaps, by the atmosphere still becoming heavier, that a storm highly disagreeable to them will soon take place, and therefore they croak, and attach themselves to trees; and when they are startled by any thing uncommon, they take a high flight, making a loud cry. They easily discover their persecutors among men, and always cry with a loud noise as long as they think they are pursued by them.

That jackdaws, on the approach of rainy weather, flap their wings, and pick their feathers with their bills, may be explained partly by an unpleasant sensation before rain, and partly from the state of the atmosphere.

To the before-mentioned itching or burning sensation I refer also the bathing and plunging of water-fowl. That the birds of the foreſt ſhould haſten to their neſts is very natural, as from the ſtate of the atmoſphere they muſt apprehend rain.

The crying of peacocks, except at pairing time, appears to be a phænomenon analogous to the crowing of cocks. I have often remarked it on a change of weather, and often even on a change of wind.

That ſtorks and cranes place their bills under their wings, is a phænomenon remarked alſo among domeſtic fowls when they fly to their rooſts to ſecure themſelves againſt rain. Their pecking their breaſts ſeems to ſignify an itching ſenſation in that part of their bodies.

The croaking of the male green or tree frog ſeems to denote an unpleaſant ſenſation, for in fine weather I never heard them ſend forth the ſmalleſt cry. But the appearance of toads implies a pleaſant ſenſation, as theſe animals are ſo fond of living in dirt.

Ants labour with great diligence, and bees haſten home, and do not fly far from their hives, becauſe they follow their inſtinct. The former endeavour to complete their habitations and to ſecure themſelves better againſt rain, and perhaps to lay up proviſion for the rainy ſeaſon. The latter haſten home to their hives, and fly no more abroad, becauſe the wet would impede them in their flight and labour.

Gnats (*conops*) come into houſes to ſecure themſelves from rain, which would impede their flight, and there they attach themſelves to the legs to procure that nouriſhment which is denied them without.

The increaſed biting of fleas I cannot explain, as the natural hiſtory of theſe inſects is as yet too obſcure.

Earth-worms creep from their holes through inſtinct, as they can move themſelves forward only upon earth that is ſlippery.

A preſenſation of ſtorms I have obſerved only among the perfect of the *mammalia*, and as yet but among two, viz. man and the dog. Both theſe ſeem to have a ſenſe of the increaſed electricity of the atmoſphere. It appears in general, that the more imperfect animals remark only the approach of dry weather; the more perfect, the approach of rain; and the moſt perfect, the approach of ſtorms. All animals, perhaps, with their external ſenſes, and all plants by their organs, are ſenſible of the variations of the weather: but plants are not here my object, and it is not neceſſary to prove the influence of the weather on them, as it is ſufficiently apparent to every obſerver. Here I allude only to the external expreſſion of internal ſenſations, as may be ſeen by the adduced instances; else one might consider the shutting and expanding of many plants as foreboding variations of the weather.

The dog, on the approach of rainy weather, expresses signs of uneasiness; scratches himself, because the fleas then bite him with more violence; digs up the earth with his feet, runs round, and eats grass: he is accustomed, however, to do the latter when he is very hot, perhaps to cool himself, and in general a storm follows soon after. Before a storm he evaporates more strongly, so that his smell becomes intolerable; he creeps in a dejected manner to his master, and lies quiet. The cat also seems to have this in common with the dog, that she creeps to her master also on the approach of a storm. But all these phænomena require a further explanation.

The most perfect of all animals, man, is on the approach of storms only subject to certain unpleasant sensations; but these must teach him, in the most striking manner, that his spiritual part, even though it disengages itself so much from oppressive cares, is irremediably connected here below with a sluggish body, which frequently exercises tyrannic sway over the soul. Men in a sound state of health are subjected, on the approach of stormy weather, to a heaviness of body and mind, a want of capacity to perform their usual occupations, a yawning and relaxation, which are highly disagreeable. These are often accompanied also with a sensation of heat. All these phænomena appear in some more and in others less, and in some do not take place at all: but the last case happens very rarely. Sick persons, or those whose juices are corrupted, experience, besides the above, an itching heat in those parts of their bodies which are covered; and many who have old wounds, ulcers, and the like, have in these uncommon sensations. Many of these may be ascribed to perspiration checked by the great heat; though, as Weikard, a philosophic physician, asserts, the want of electric matter in the body may have some share in them also.

When stormy weather happens in winter, these sensations, as well as the before-mentioned presensation of animals, do not take place; at least no one has ever observed them. This, in all probability, arises from the influence of the season.

I must conclude this essay with requesting, that what I have here said may be considered, as it really is, an hypothetic explanation of well known facts.

XXXVIII. *Ac-*

XXXVIII. *Account of a Journey from the Fortreſs of Severnaja, at the northern Foot of Mount Caucaſus, to Choy in Perſia; with a Deſcription of Teflis, the Capital of Georgia.* By P. F. Engelman *.

WE ſet out on the 14th of January, and croſſed the Terek at Moſdok, accompanied by the Perſian ambaſſador † with his attendants, and having in our ſuite two officers and ſix Coſſacs of the Caucaſian legion, and an interpreter for the Perſian, Turkiſh, and Georgian or Gruſinian language. The caravan conſiſted of twenty-three horſes for carrying our baggage, and twenty-ſeven riding-horſes. The whole company were on horſeback, becauſe it is not poſſible to travel through the mountains in any other manner. By way of eſcort through the narrow defiles to the borders of Georgia, we were furniſhed with a guard of fifty regular Coſſacs and ſixty chaſſeurs.

On the 15th we croſſed the Leſſer Kabarda, which is ruled by ſeveral princes ſubject to the Ruſſian government, and next day arrived at Wladi-kaukas, a fortreſs lately conſtructed at the bottom of the higher Caucaſian ridge, in order to protect our communication with Georgia; from which we proceeded on the 17th to aſcend the mountains.

Caucaſus, which the Perſians, Indians, and Arabians call *Kukas*, extends from the Black to the Caſpian ſea. This ridge of mountains we had to croſs in its whole breadth to the capital of Georgia, from which the chain proceeds to Anatolia and along the Euphrates. Its greateſt length is from the mouth of the river Kuba to Derbent. A chain of ſnow mountains runs along the middle of this ridge from the Black to the Caſpian ſea. Two rivers flow from theſe ſnow mountains in this neighbourhood; one of which is the well-known Terek, that throws itſelf into the Caſpian ſea below the Ruſſian fortreſs of Kiſlar; the other, called Aragui, runs towards the ſouth, and falls into the Kur near the town of Zeheta, at the diſtance of about fifteen werſts from Teflis. Along the Kur and the Terek a commodious road was conſtructed, about two years ago, in order to open a free communication with Georgia; and this route in the ſummer-time is paſſable for carriages; but in the winter it is not poſſible to travel in that

* From *Neue Nordiſche Beyträge*, by profeſſor Pallas, vol. vi.

† The author accompanied, as conductor of the caravan, prince Tamara, who was ſent to Schiras and Iſpahan on a political miſſion, but who, on account of ſome circumſtances which took place, did not proceed thither.

manner

manner through the ſnow mountains, as the road between the mountains is covered with ſnow, and the travellers are obliged to paſs over ridges and eminences which are practicable only for perſons on horſeback or on foot. Along this road the high rocky mountains riſe in pyramidal forms, and like two walls of rock on each ſide; the valley, which forms a paſſage between them, where broadeſt may be about 300 yards. The people who inhabit the country in the neighbourhood of this road, in the mountains, and ſometimes on the river Terek, where the ſituation of the mountains will admit of it, are called Oſſi or Oſſetines. The former name was known in antient times, and is ſtill uſed among the people themſelves, but by the Ruſſians they are called Oſſetinzy. Their language has no affinity to any of the Aſiatic dialects but the Perſian. Thoſe who live to the north are ſtill pagans, and worſhip dogs, cats, and various other animals which have been ſtruck by lightning. Their habitations are all like fortreſſes, and ſurrounded by antique round towers, &c.

On the 18th we arrived at the ſnow mountains, after being attacked at a narrow paſs on our way by the above-mentioned Oſſetines. The pretended cauſe was, that they demanded toll from us; but, as it had been ſettled by all the princes reſiding among the mountains, that every thing belonging to Ruſſia ſhould be ſuffered to paſs free, our commander would not comply with their demand, nor pay them a ſingle farthing. When the Oſſetines found that they could obtain nothing, they began to roll down on us aſtoniſhing large maſſes of rock; and they diſcharged alſo their fire-arms, but without doing us any hurt. Our ambaſſador, however, was ſo incenſed, that he commanded 60 of the chaſſeurs and 150 of the Coſſacs, who formed our eſcort, to march againſt them; but they could not reach them but by a circuitous route of about forty werſts. When the Oſſetines ſaw that this party were going round to attack them in the rear, they ſolicited forgiveneſs, and ſuffered us to paſs unmoleſted.

On the 19th we continued to aſcend the ſnow mountains, where we were much aſtoniſhed at the ſudden change of the climate. Hitherto the weather had been temperate, and we had enjoyed the moſt beautiful views, highly picturesque on account of the different colours of the rocks, which in many places were ornamented with a ſort of cryſtallized figures. In the month of March, Caucaſus muſt be a real paradiſe; for the cypreſs-like juniper trees, the cheſtnut, and other beautiful trees, are every where found mixed in the moſt agreeable manner. Travelling, however, through the ſnow mountains was not ſo pleaſant. Where we croſſed them they conſiſted of of about five ridges running parallel to each other, the middlemoſt or higheſt of which roſe above the clouds. Each ridge is from a half to three quarters of a German mile in breadth. When we reached the ſummit of the firſt mountain we experienced a moſt violent cold, and the ſnow lay on the ground above an arſchine and a half in depth. As this mountain was not very ſteep, we paſſed it without much difficulty. When we reached the bottom we had to croſs the river Terek; but no bridge was neceſſary, for the river is continually frozen, and the ice is covered by ſnow accumulated for many years, and which never diſſolves in ſummer however hot the weather may be; and if it were not for the prodigious noiſe which the water makes, no one would ſuſpect that ſo rapid a ſtream flows under it.

We then paſſed the ſecond ridge, which however ſeemed to be far more dangerous, as it is remarkably ſteep. When we had aſcended half way the road became ſo narrow, that between the perpendicular rock on the one ſide, and the precipice, more than 400 fathoms in depth, at the bottom of which is the bed of the river, only two or three feet were left for us to paſs. While in this ſituation a violent wind aroſe, as is commonly the caſe, and drove down a prodigious quantity of ſnow, which, as misfortune would have it, fell upon our ambaſſador, his ſteward, and a riding boy, and hurried them all together, with their horſes, to the bottom. Luckily there happened to be in this place a projection of rock, on which they all three fell; but ſo covered with ſnow that for ſome minutes they could not be ſeen, until they had forced their way from the ſnow with their heads. We immediately let down to them, by means of ropes, a few Coſſacs and Oſſetines, who are very expert on ſuch occaſions. Theſe people, having deſcended in this manner at leaſt 140 fathoms, helped the ambaſſador and the other two perſons from the ſnow, and, having made them faſt to ropes, they were drawn up without any farther accident. Amidſt fear and terror we proceeded with the caravan through this dangerous place, dreading every moment that a new fall of ſnow rolling down from the mountain would hurry us to the bottom of the precipice. By the protection of Providence, however, we were preſerved, and arrived in ſafety at the top of the mountain. Our ambaſſador was not hurt by his fall, but greatly frightened. Two of the horſes, contrary to all expectation, found means to clamber up the ſteep ſide of the mountain; and though they often fell among the ſnow ſo that nothing of them was to be ſeen except the head, they made their way through it; but the third was too weak; and while exerting the

the laſt remains of his ſtrength by a ſudden ſpring, he fell back and rolled down the precipice, ſo that we ſaw no more of him.

We now deſcended the mountain, the road being neither very ſteep nor long, till we arrived at the next ridge, which is, as it were, placed upon the former. This third and higheſt ſnow mountain was ſo ſteep that we almoſt deſpaired of being able to paſs it. We found it now impoſſible to ride; for, though the road was here and there two feet in breadth, we were frequently obliged, for the diſtance of half a werſt and more, to clamber over the rocks ſometimes up to the middle in ſnow. When we had at length, with great labour, pain, and fatigue, got to the ſummit, we obſerved the clouds rolling under us, and could no longer ſee any thing of the ſurrounding country. We now travelled above an hour, involved in a ſort of thick fog, expoſed to the wind and ſnow, and to a more penetrating cold than we ever remembered to have experienced at Peterſburgh.

When we began to deſcend from this high ridge, and had got about half way down, three of the mules laden with the ſervice of ſilver plate deſtined as a preſent to Perſia, fell into a ſmall fiſſure near the road, which at firſt, as we ſaw their heads above the ſnow, did not appear to be of much conſequence. Theſe animals, however, exerted their whole ſtrength to extricate themſelves, and two of them were fortunate enough to ſucceed; but the third, which had approached too near to the declivity of the mountain, by its exertions fell deeper into the ſnow, and, getting ſtill nearer and nearer to the edge of the precipice, rolled down the ſide of the mountain to the depth of about 400 fathoms from the road. The boxes containing the plate broke looſe by the violence of the fall, one rolling down on one ſide and the other on the other; and one of them, being daſhed againſt the projecting point of a rock, burſt in two; but the other fell unhurt on a ſmall eminence which was at the diſtance of two hundred fathoms from us. In that ſituation we were obliged to leave it, as it was impoſſible to approach it on account of the ſteepneſs of the mountain. A Gruſinian (Georgian) prince, whom the czar ſent to meet us on the frontiers, promiſed, as ſoon as the weather would permit, and the new ſnow ſhould be melted, to take proper care that the Oſſetines in the next village ſhould bring up the box which remained whole, and alſo collect the ſcattered plate which had fallen from the box that had burſt, and which could not ſink into the old hardened ſnow, as it is ſo ſtrong even in ſummer as to bear the weight of a horſe. On our return, indeed, one of the boxes with

with the plate was brought to us at Teflis, and all the plate which had fallen from the broken one was again found, except ſix gilt ſilver diſhes, with one large and two dozen of ſmall ſilver ſpoons; of the gold deſſert ſervice, ſeven knives and ſome ſmall diſhes were recovered; and, of the gilt, thirteen knives and ſome forks *.

Having again proceeded on our journey, we at length, about one o'clock in the morning, after great pain and difficulty, on account of the ſnow, which was continually rolling down from the mountain, arrived without further accident at a village at the bottom of the ridge diſtant about eighteen werſts, which had coſt us eighteen hours labour from the village where we had ſpent the preceding night. This village was one of the pooreſt and moſt wretched in all Georgia; but to us it was a welcome place of refreſhment, where we remained during our twentieth reſting day, and, after three days journey more, arrived in ſafety on the 23d of January at Teflis, the capital of Georgia. This place we reached at midnight; and though it was ſo dark that we could not ſee a yard before us, we concluded from the dirt and narrowneſs of the ſtreets, and from the diſagreeable effluvia which aſſailed us from every corner, that, when day-light appeared, we ſhould find ourſelves neither in a Berlin nor a Dreſden: and this indeed was the caſe, as will appear from the following deſcription.

But we ſhall here firſt give a ſhort view of the province of Tuoletti, which we traverſed in our way from the ſnow mountains to Teflis. At about the diſtance of twenty-five werſts from the ſnow mountains the country begins to be exceedingly pleaſant, as the mountains gradually become lower, and are covered with wood to the tops. The valley along the river, which forms the principal paſſage from Ruſſia to Georgia, ſtill grows wider the nearer it approaches the capital; ſo that, where the Aragui diſcharges itſelf into the Kur, which runs through Teflis, about twenty werſts from that city, the breadth of the valley is from five to ſeven werſts. In other places its breadth varies from a half to a whole werſt, and is every where interſperſed with the moſt beautiful groves of beeches, cheſtnut, apple, and pear-trees. Between theſe fruit-trees vines ſhoot up, and the bruſh-wood is of that kind generally ſeen in Europe. Through this woody vale the Aragui flows in different branches, and the route in winter and the end of ſummer goes through the woods. No proſpect more beautiful can be conceived than

* As theſe articles were deſtined as a preſent to an eaſtern prince who ate rice with his hand, it does not appear that they would have been of much uſe to him.

that

that of the mountains on each side of this valley. At about the half of their height lie in general the villages, which, according to the Asiatic mode, are built with flat roofs. Where any of the princes reside, the village is surrounded with a high wall, strengthened by old round turrets. In this manner, two or three villages often lie one above the other, each having its gardens and fields laid out on the declivity of the mountain; so that it appears wonderful how it is possible to cultivate them. Rivulets every where fall, in the form of cascades, from the rocky brows of the mountain; and this district, in an amphitheatrical form, extends above sixty wersts * with the most agreeable variety; so that a more beautiful landscape can hardly be found either in Swisserland or Italy.

When compared with this beautiful neighbourhood, Teflis, the capital, makes a very poor appearance. It is about eight wersts in circumference, and is divided into two parts by the river Kur, which flows through it. The principal part of the town lies on the declivity of a high mountain, which terminates at the river with rocky banks of from eight to eleven fathoms in height. On the land side it is inclosed by a wall, built in the old form, eight fathoms in height, and at the distance of every fifteen fathoms stands a large round tower which supplies the place of a bastion. On this side, the city has three gates; and towards the mountain, which rises almost perpendicularly behind the wall, it is protected by an old castle, which is exceedingly strong, and which exhibits the true old method of building. This castle is situated at the elevation of at least two hundred fathoms above the level of the city. The other part of the city is connected with the former by means of a wooden bridge, which is only twenty-five fathoms in length; as the river Kur, though in other places from a hundred to two hundred and eighty fathoms in breadth, is here confined on both sides by high rocky banks, so that it never can become broader; and its real breadth under the bridge amounts only to twenty fathoms. This part of the city is fortified in the same manner as the former, and contains the czar's palace, a church, and a few houses belonging to the citizens. In the whole city there are seventeen churches, Georgian and Armenian, with one catholic church and three mosques. The number of the houses in proportion to the extent of the ground is very great, as the breadth of each house in front is only from four to twelve fathoms. They are all irregularly built; some of them have neither windows nor chimneys, as the flat roof

* A werst is about 4200 English feet.

has in the middle a ſort of cupola with a round aperture in it, which ſerves both to admit light and to afford a paſſage to the ſmoke. The fire is made in the middle of the floor, and round it are the ſleeping places: people even of the middle claſs reſide in houſes of this kind.

The city is interſected by an aſtoniſhing number of ſtreets, ſhort and crooked, and not above two fathoms in breadth; one principal ſtreet only which is capable of admitting a carriage paſſes through the whole city. In my opinion there is not a dirtier place in the world than Teflis; for when the ſnow falls in winter it lies ſometimes on the flat roofs of the houſes to the depth of two or three feet, and is thrown down into the ſtreets by the inhabitants by means of ſhovels, leſt it ſhould penetrate into the houſes by its melting, which generally takes place very ſoon. As the ſtreets are not paved, this ſnow occaſions ſo much dirt, that to perſons on foot they are almoſt impaſſable. Beſides, carrion of every kind, ſuch as dead cattle, ſwine, dogs, cats, and the like, is thrown out into the ſtreets, where it is ſuffered to become putrid. Even cloſe to the czar's palace I obſerved the remains of a dead cow, which emitted an intolerable ſtench, and would have long continued a nuiſance had it not been devoured by the dogs. In winter the ſtench is not ſo offenſive as in ſummer during the great heats, when ſtrangers are almoſt ſuffocated by it; and on this account it is very ſurpriſing that peſtilential diſeaſes are not as prevalent in Teflis as at Conſtantinople. Strangers immediately on their arrival are ſenſible of the impurity of the air, and in conſequence of it are continually ſick. The healthieſt time here is the autumn, becauſe the ſtrong winds which blow then from the mountains purify the air; but they would produce a much better effect if the city were regularly built, for at preſent the ſtreets are too crooked and narrow to afford it a free paſſage.

The czar's palace ſtands on the rocky bank of the river; the lower part of it is built of ſtone, the upper is of wood and ſurrounded by a gallery; but in other reſpects it has a very mean appearance. In the interior part it exhibits nothing remarkable; all its ornaments conſiſt merely of rich and beautiful carpeting. The area before it is often ſo covered with dirt that it is impoſſible to approach the palace. The magnificence of the court is, in my opinion, much inferior to that which we obſerved at the court of the Perſian chan Achmet Mehemet, at Eriwan; who, however, is ſubject and tributary to the czar of Georgia. To deſcribe the etiquette of the court would be too tedious and ridiculous; I ſhall therefore only obſerve, that prince Heraclius is a prudent,

dent, intelligent, and courageous man, whose talents are displayed in his physiognomy. He is of a middle stature, and still fresh and vigorous though seventy years old. He has a great many children, the eldest of whom is forty-five years of age. The czar is exceedingly polite, and on days of festivity invites all the Russian officers to his table. I had twice this honour. We were served in the European manner; the dishes however, for the most part, were cooked according to the Asiatic taste, but excellent of their kind. On such occasions the czar sits at table as the Europeans do, but in common he follows the Asiatic custom.

The czar's consort and the princesses often appear publicly on festivals; but some years ago they were more reserved, being seldom seen. The children are all stout and handsome.

Of the Tartars subject to the czar of Georgia, and who are distinguished by the name of *Kasach*, I obtained at Teflis the following information:—They reside on the rivers Nachatyr, Tebete, Kur, Achystaf, Pambek, and Algete. On the first lie seven villages; on the second, thirteen; on the third, five; on the fourth, fifteen; on the fifth, eighteen; and, on the sixth, two. Some of these Tartars are of the sect of Omar; others are of that of Ali, and do not practise circumcision. They acknowledge, besides the Supreme Being, five saints. David, who had a hundred wives, and therefore Mahomet allowed his followers to have the same number; Dschebrail, who, though invisible, excites the people to offer up their prayers to God; Asrail, who by God's command sets the soul free from the body; Michael, who weighs the sins of the dead, and sends them either to hell or to paradise; and, in the last place, Israfil, who, when God shall make the water like the earth, will sound the trumpet to call the dead to judgment. Mahomet they do not account a saint, but their lawgiver, who has commanded them to pray five times in the twenty-four hours, and to implore God to forgive their sins; to purify themselves before prayer by washing; to undertake a pilgrimage to his grave when possible; not to steal; not to work on Friday, but to fast; to observe the festivals, &c.

They divide the year into twelve months, and assign to the months an unequal number of days, which vary every year except for the month Ramasan, which always consists of thirty, and in which their principal festivals fall. Their week has seven days, and begins with Friday, which they call Dschuta: the rest are, Dschuta-Ertessi, Basar, Basar-Ertessi, Chas, Tscharschembe, Perschembe. They have three grand festivals. The first is in the month Ramasan; the second is called Kurban, and the third Naurus-Bairam.

Their

Their great fasts in Ramasan are observed so strictly, that they spend whole days in prayer, and taste no food till the evening.

At their marriages no other ceremonies are usual than, at the time of the betrothing, the male and female friends assemble at the house of the bride, and sing from morning till night, dance, and make merry, till the wedding day; after which they spend three more days in a similar manner. At the birth of a child, the midwife, after it has been washed, besprinkles it with salt water, and gives it any name she thinks proper. A woman, after delivery, is accounted unclean for forty days; during which she must neither sleep with her husband, pray in the church, nor perform any domestic office. Marriage is forbidden only between the first degrees of consanguinity, such as that of brother and sister; in all other degrees it is permitted. A man may keep as many wives as he chooses: the first must always be a maid, but the rest may be widows. If a married woman is guilty of adultery, the husband has a right to put her to death, as well as the adulterer. If a husband, however, is unfaithful, the wife has no resource. Men are accounted fit for marriage from the age of fifteen to fifty, and women from that of twelve to forty. If a man loses all affection for his wife, he may give her a letter of divorce and send her away; but a woman cannot separate from her husband without his consent.

They bury their dead in the following manner:—The body is washed, and a *mullah*, or priest, is sent for to accompany it to the grave; the mullah then recites some prayers, and the body being deposited in the grave the relations cover it with earth. The relations do not return to the house of the deceased for three days, but at the expiration of that period they prepare an entertainment in honour of his memory; and this festival is repeated on the seventh, and again on the fortieth day.

During nine months of the year these people remain stationary in their villages; but after the first of July they retire with their cattle to the river Tebete among the mountains, where they lead a wandering life till the first of October. To the flesh of deer, swine, and horses, they have an aversion. Their language is a somewhat corrupted dialect of the Tartaric.

A kind of borax, brought from Akalzi and Wan, is sold at Teflis, for the use of the goldsmiths, under the name of *bora* *. At Oldi, near the former of these places, about three days journey from Teflis, and at Bajasid, near the latter, there are said to be springs, the water of which is greenish.

* This is the impure mineral lixivious salt described by Model under the name of *sal Persicum.*

It is collected in ſtone ciſterns, where it is ſuffered to remain three or four months with ſhavings of leather, blood, and urine, till it becomes putrid; after which it is clarified, and boiled in copper kettles. A concrete ſalt remains, which is the *bora*.

After a reſidence of almoſt three weeks at Teflis, during which time we put our equipage again into order, and purchaſed freſh cattle, we ſet out on the 12th of February, the weather being then as mild as it is at Peterſburgh in the ſpring. In general, ice is ſeldom ſeen in the neighbourhood of Teflis; and the weather in winter is, for the moſt part, rainy. Some opinion reſpecting the climate of this country may be formed from the fruit-trees, ſuch as the pomegranate- and fig-tree, which grow here in the open air in all the gardens, and yet never ſuffer from the froſt.

We had to repaſs the mountains, but the road now was not ſo dangerous. On the 17th we arrived at the bottom of that height called the Bambakſche ridge. During theſe five days we had an agreeable journey through a beautiful diſtrict every where interſected by ſmall ſtreams, and covered with fruit-trees of every kind. The inhabitants of this agreeable diſtrict are, however, much to be pitied, as they are continually expoſed to the depredations of the Leſghinians. In the courſe of one day's journey we ſaw thirteen villages which theſe plunderers had deſtroyed. To ſuch ſcenes of devaſtation the Georgians are often expoſed, as the Leſghinians reſide in the mountains between Kachet and Dageſtan, and are exceedingly numerous and warlike. The pacha of Achalziche every year takes from two to three thouſand of theſe people into his ſervice, but gives them little pay, as they receive permiſſion from him to indemnify themſelves in their march by what they can plunder. To ſtrengthen this corps he unites with them ſome Turkiſh troops. Some time ago, when three companies of our chaſſeurs, under the command of major Sengenberg, defeated the Leſghinians, of the two thouſand killed, one-half almoſt were Turks. On another occaſion, when four hundred remained on the field of battle, twenty Turks and two of their ſtandards were brought into Teflis. Since that period the Leſghinians have been afraid of our chaſſeurs; but the two battalions which we maintained in Georgia were not ſufficient to defend it from theſe robbers *.

On

* Since that period Georgia has been annexed to the Ruſſian empire. The reaſons which produced this event will be found in the following extract of a letter from Peterſburgh, dated October 30, 1801:

" The manifeſto by which his Imperial Majeſty declares that Georgia, called

On the 18th our road lay over the cold snow mountains from Bamback. We were still eighty wersts from the first Persian town Eriwan, and had ascended the ridge, which extends sixty wersts; when all of a sudden, after having travelled five wersts, we were assailed by a violent wind, which accumulated the snow in heaps before us; so that we were obliged to return, to avoid being buried alive: a catastrophe which whole caravans sometimes experience among these mountains, as they must travel here sixty wersts without finding any village or place of shelter, and consequently can hope for no assistance when overtaken by a snow storm.

On the 19th of February we pursued another route, in order to avoid, in some measure, the snow mountains. This route however was full 150 wersts longer, and exceedingly

called by the Russians Grusinia, is united to the crown, gives an account of the motives that determined this measure. The sovereigns of Russia have always considered it as a duty to protect that state: the withdrawing of the Russian troops from it had occasioned every kind of disorder, and civil war had often burst forth. The barbarous tribes by whom it is surrounded, threatened it on every side; and without the Russians it was impossible to repress the banditti of Mount Caucasus. When the emperor Alexander ascended the throne, he found Georgia united to the crown, in consequence of a manifesto dated January 18, 1801. However, as the Russians had produced tranquillity in it, the emperor was desirous of trying to restore to it its independence; but this country had again to dread the vengeance of those who had been excluded from the sovereignty—it was threatened with a new civil war. The prince by whom it was governed could not secure it either internally or externally, and to all these circumstances was added the wish of the Georgians themselves. 'Your hopes this time,' said the emperor to them, 'shall not be deceived. It is not to increase my power; it is not for interest; it is not to extend the boundaries of an empire already so extensive, that we have taken upon us the government of Grusinia: our dignity alone, our honour and humanity, impose on us the sacred obligation of listening to the voice of those who suffer; and to establish in Grusinia a just government, which may ensure the safety of persons and property, and the protecting force of the law.'

"These are the intentions of our august monarch. The produce of the taxes of Georgia will not go out of the country; it will be consecrated to the support of government, to the reparation of towns and villages ruined by intestine war, and to the relief of the inhabitants. Each will be maintained in his rights, and in the exercise of his religion. The children of the princes will retain their estates, and may live where they please. Lieutenant-general Knorring, a native of Georgia, will be charged with the government; and in order that he may carry into effect the instructions of the emperor, he will associate with him other persons born in the country. All the Georgians will take an oath of fidelity to their new sovereign, and the ecclesiastics and pastors will be the first to give this example of submission.

"His majesty concludes with expressing an ardent wish that peace and justice may prevail in the country; that an end may be put to acts of violence; and that agriculture, industry, commerce, and manufactures, may revive, under the protection of the new laws, equal to all."—Edit.

 dangerous,

dangerous, as we were obliged to pass very near the Turkish borders, which are generally infested by the Lesghinians.

On the 23d we arrived at the village of Talin, which belongs to Armenia the Less. Scarcely had we got to our lodging when intelligence was received that 2000 Lesghinians had crossed the river behind us, and attacked a village which we passed a little time before. Had we fallen in with these people on the road, our silver plate must have been lost.

On the 24th we came to the celebrated Armenian monastery of Weds, or Edsch-mijazin (seven churches), which is called also the Monastery of Ararat, where the Armenian patriarch, the chief of the ecclesiastical establishment of that country, has his residence. Here we were received with great hospitality, lodged in an elegant apartment, and waited on with great attention. Next day I made a drawing of the monastery, together with Mount Ararat and the neighbouring country; which pleased the patriarch so much when shown to him by our commanding officer, that he requested me to make a copy of it for him, and to encourage me to do so he gave me a considerable present.

This monastery is said to have been built upwards of 1300 years ago, in the reign of an Armenian king named Tartat: and beyond all doubt it is one of the richest in the world, as it receives a great income from the Armenians scattered throughout Turkey, Persia, Georgia, and even Europe; above 800 peasants also belong to it, and a large tract of land, on which 300 head of cattle, 100 horses, and 30 camels, are kept for the purposes of agriculture. When the patriarch dies, a new one is chosen from their synod; and an account of this choice is then transmitted to the Turkish sultan, the czar of Georgia, and the chan of Ispahan. The number of the monks amounts to 120, who are lodged in the monastery, where they enjoy every convenience. Travellers are shown here a fragment of Noah's ark, which a monk is said to have obtained from an angel. This monk had resolved to ascend to the summit of Mount Ararat, where the ark rested; but he was prevented by the Deity, who sent an angel to convey him back; and the angel, to console him for this disappointment, brought him this fragment, respecting which many other wonderful stories are told.

On the 26th we continued our journey to Eriwan, which is only fifteen wersts distant from the monastery. This city lies in a fruitful valley on the river Sangintschey, at the bottom of the snow mountains. It is fortified by a double wall, furnished with a great many round towers at the distance of fifteen fathoms from each other, and each mounted with

 four

four cannon: mortars alſo are planted on the edge of the ditch by which the city is ſurrounded. The city has only one gate, and would be a place of ſtrength if its ſituation were more advantageous. One-half of it is ſurrounded by mountains within cannon ſhot; but on the ſouth ſide it is protected by inſuperable rocks, which extend, almoſt perpendicularly, to the banks of the river Sangin. The houſes are in the ſame ſtyle of architecture as thoſe of Teflis.

The chan, Achmet Mehemet, was about ten years of age, and ſucceeded his brother, who had been aſſaſſinated in the ſtreets about a year before. The diſtrict belonging to Eriwan forms a great part of Armenia the Leſs: it is exceedingly fertile, and brings to the chan a conſiderable income; from which, however, he muſt pay a tribute to the czar of Georgia.

On the 27th our colonel paid a viſit to the chan, all of us accompanying him in great parade on horſeback. When we arrived at the court of the palace we all diſmounted, the colonel excepted, who rode up to the palace, where he was received by one of the chan's vaſſals, who conducted him into the hall of audience, to which all the officers followed him. Chairs covered with red cloth were handed to us; after which we were preſented with coffee, tea, and ſweetmeats; and then with the *kallian*, a kind of machine uſed for ſmoking tobacco through water. The audience laſted about half an hour, during which the chan ſpoke very little, and of courſe we had no opportunity for much converſation. When the audience was ended, we returned to our lodgings in the chan's ſummer palace. The ſtreets through which we paſſed were ſo full of people, that it was with difficulty we could get along.

On the 1ſt of March we continued our journey towards Nachtſchiwan, the capital of Armenia; but when we arrived at Derwiſchlär, ſixty-nine werſts from Eriwan, we were obliged, on account of the danger of the road, to return to Eriwan, as the Curds along the river Aras were in a ſtate of inſurrection, and had already committed many acts of robbery. Theſe Curds are a people nearly in a ſavage ſtate, who lead a roving life among the mountains. Their habitations conſiſt of a kind of half tent made of a ſort of hair cloth, and which has a reſemblance to a piquet tent. Such tents are inhabited by whole families. They live chiefly on vegetables, and their dreſs is Turkiſh.

On the 4th we arrived again at Eriwan, where we ſtaid till the 18th, when we ſet out, under the eſcort of 150 men, for Nachtſchiwan, which we reached on the 20th of March.

XXXIX. *Account of the Process employed in making Cheshire Cheese**.

Preparation of the Rennet.

WHEN the maw skin comes from the butcher, the chyly matter is taken out, and the skin cleaned from slime and every apparent impurity, by wiping or gently washing. The skin is then filled nearly full of salt, and, placing a layer of salt upon the bottom of a mug, the skin is laid flat upon it: this mug is large enough to hold three skins in a course: each course of skins should be covered with salt; and when a sufficient number of skins are thus placed in the mug, that mug should be filled up with salt, and, with a dish or slate over it, be put in a cool place, till the approach of the cheese-making season the following year. The skins are then all taken out and laid for the brine to drain from them; and being spread upon a table, they are powdered on each side with fine salt, and rolled smooth with a paste roller, which presses in the salt. After that, a thin splint of wood is stuck across each of them to keep them extended while they are hung to dry. Take all the maw skins provided for the season, pickled and dried as before; put them into an open vessel or vessels, and for each skin pour in three pints of pure spring water. Let them stand twenty-four hours, then take out the skins, put them into other vessels; add for each one pint of spring water, let them stand for twenty-four hours as before. On taking the skins out the second time, gently stroke them down with the hand into the infusion. The skins are then done with. Mix those two infusions together, pass the liquor through a fine linen sieve, and add to the whole a quantity of salt, rather more than is sufficient to saturate the water; that is to say, until a portion of the salt remains undissolved at the bottom of the vessel. The next day, and also the summer through, the scum as it rises is to be clearly taken off; and as the liquor should not be suffered to remain without a portion of undissolved salt at the bottom, it will be necessary to add frequently fresh salt, as that which was dissolved will gradually form itself into crystals, and be taken off with the rising scum. Somewhat less than a wine half-pint of this preparation will be generally sufficient for 60 lbs. of cheese. Whenever any of this liquid is taken out for use, the whole should be well stirred up.

* From the Agricultural Report of that County.

Colouring

Colouring for the Cheshire Cheese.

The colouring for cheese is, or at least should be, Spanish annotta; but as soon as colouring became general in this country, a colour of an adulterated kind was exposed for sale in almost every shop: the weight of a guinea and a half of real Spanish annotta is sufficient for a cheese of 60 lbs. weight. If a considerable part of the cream of the night's milk be taken for butter, more colouring will be requisite. The leaner the cheese is, the more colouring it requires. The manner of using annotta is, to tie up in a linen rag the quantity deemed sufficient, and put it into half a pint of warm water over night. This infusion is put into the tub of milk in the morning with the rennet infusion; dipping the rag into the milk, and rubbing it against the palm of the hand as long as any colour comes out.

Setting the Cheese together.

It is, we believe, generally admitted, that not only the quantity, but the quality of the curd, as to texture, viz. toughness or otherwise, depends in a great measure upon the length of time the cheese is in coming; and that the time, again, depends on the quantity and strength of the coagulum used, the state of the atmosphere, and the heat of the milk when put together. In this stage of the art, where a degree of accurate certainty seems to be required, there is no other guide but the hand and the external feelings: the thermometer of a Cheshire dairy-woman is constantly at her fingers' ends: accordingly the heat of the milk when set, is endeavoured to be regulated by the supposed warmth of the room and the heat of the external air; having reference also to the quantity and strength of the steep, so that the milk may be the proper length of time in sufficiently coagulating, which is generally thought to be about an hour and a half. The evening's milk of suppose twenty cows having stood all the night in the cooler and brass pans, the cheesemaker in summer, about six o'clock in the morning, carefully skims the cream from the whole of it, observing first to take off all the froth and bubbles, which may amount to about a pint; this, not being thought proper to be put into the cheese, goes to the cream-mug to be churned for butter; and the rest of the cream is put into a brass pan. While the dairy-woman is thus employed, the servants are milking the cows, having previously lighted a fire under the furnace, which is half full of water. As soon as the night's milk is skimmed, it is all carried to the cheese-tub, except about three-fourths of a brass pan full, viz. three or four gallons, which is immediately placed in the furnace of hot water in the pan, and is made fcalding hot; then half of the milk thus heated in the pan is poured alfo into the cheefe-tub, and the other half is poured to the cream, which, as before obferved, is fkimmed into another brafs pan. By this means all the cream is liquefied and diffolved fo as apparently to form one homogeneous or uniform fluid, and in that ftate it is poured into the cheefe-tub; but before this is done feveral bowls or veffels full of new milk will generally have been poured into the cheefe-tub, or perhaps the whole morning's milk. Care is taken to fkim off all the air-bubbles which may have formed in pouring the new milk into the cheefe-tub.

The rennet and colouring being put into the tub, the whole is well ftirred together, a wooden cover is put over the tub, and over that is thrown a clean linen cloth. The ufual time of coming is an hour and a half, during which time it is to be frequently examined: if the cream rifes to the furface before the coming takes place, as it often does, the whole muft be ftirred together, fo as to mix again the milk and cream, and this as often as it rifes, until the coagulation commences. A few fmart ftrokes on different fides of the tub with the cheefe-ladder, &c. will forward the coagulation, if it is found too long in forming. If the dairy-woman fuppofes the milk, &c. to be accidentally put together cooler than fhe intended, or that its coolnefs is the caufe of its not coming, hot water or hot milk may be poured into it, or hot water in a brafs pan may be partially immerged therein: but this muft be done before it is at all coagulated; for, after that takes place, though but imperfectly, it muft not be tampered with fo as to break the forming curd, for a confiderable part of the cream would thereby be directed into the whey, and the quantity of curd much leffened. Before the coagulation takes place, an additional quantity of rennet may alfo be put in, if thought neceffary; but this will, after coagulation, be added with little effect, as no means can be ufed to mix it with the whole mafs without difturbing the forming curd. If the cheefe has been fet together hotter than it was meant to be, the oppofite means, under the fame precautions, may be recurred to. But the more general practice is, to fuffer the procefs to proceed hot as it is, until the firft quantity of whey is taken off; a part of which, being fet to cool, is returned into the tub to cool the curd. When the cheefe happens to come much fooner than a proper time, owing to too great a degree of heat in the milk in fetting or putting together, or too great a ftrength of fteep, there is lefs curd, and it is confiderably ſiderably tougher than when the milk has been ſet cooler together than uſual, or when too little ſteep has been uſed. In the latter caſe, the curd is exceedingly tender: and, when that ſo happens, a part of the whey is taken out of the cheeſe-tub, and heated as much as may be thought ſufficient to give to the curd, when mixed with it, a proper degree of toughneſs. In an hour and a half, as mentioned before, if all goes on well, the coagulation will be formed. This point is determined by gently preſſing the ſurface of the milk with the back of the hand.

Breaking down the Curd, Gathering, &c.

If the milk has been ſet together very warm, the curd, as before obſerved, will be firm: in this caſe, the uſual mode is to take a common caſe knife, and make inciſions acroſs it to the full depth of the knife's blade, at the diſtance of about one inch; and again croſsways in the ſame manner, the inciſions interſecting each other at right angles. The whey riſing through theſe inciſions is of a fine pale-green colour. The cheeſe-maker and two aſſiſtants then proceed to break the curd; this is performed by their repeatedly putting their hands down into the tub, the cheeſe-maker with the ſkimming-diſh in one hand breaking every part of it as they catch it, raiſing the curd from the bottom, and ſtill breaking it. This part of the buſineſs is continued till the whole is broken uniformly ſmall: it generally takes up about forty minutes, and the curd is then left covered over with a cloth for about half an hour to ſubſide. If the milk has been ſet cool together, the curd, as before mentioned, will be much more tender, the whey will not be ſo green, but rather of a milky appearance. The cheeſe-maker in this caſe, inſtead of the knife, has recourſe to the ſkimming-diſh, the edge of which ſhe holds perpendicular to the ſurface of the whey in the tub, and dips it gently an inch or two into the curd, turning it over, until the whole ſurface is thus turned. The breaking then proceeds as before; but a cautious and gentle mode of doing it is more neceſſary than in the former caſe. Rather more time, of courſe, is neceſſary for breaking down a cold than a hot cheeſe; but when ſufficiently broken, it is covered over, and left to ſubſide as before. After ſtanding about half an hour, as much whey is taken out of the tub into the braſs pans as conveniently may be, without taking any of the curd with it. The bottom of the tub is now ſet rather a-tilt, the curd is collected to the upper ſide of it, a board is introduced of a ſemicircular form to fit looſely one-half of the tub's bottom. This board is placed on the curd, and a 60 lb. weight upon

upon it, to press out the whey, which draining to the lower side of the tilted tub, is laded out into brass pans. Such parts of the curd as are pressed from under the board, are cut off with a knife, placed under the weighted board, and again pressed. This is repeated again and again, the whey being constantly laded out as it drains from the curd. The whole mass of curd is then turned upside down, put on the other side of the tub, again pressed, pared, and pressed as before.

The board and weight being removed, the curd is cut into several pieces of about eight or nine inches square, piled upon each other, and pressed with the board and weight, repeating the cutting and piling, as long as any whey drains from it. It scarcely need be added, that the more gently the whole of the business in the tub is performed, the more perfect will be the separation of curd and whey.

The next thing is to break the curd in a brass pan. After being pressed in the tub as long as any considerable quantity of whey drains from it, the curd is cut into nearly three equal portions; one of which is taken into a brass pan, and is there, by two women, broken exceedingly fine. As soon as it is coarsely broken, a large handful of salt is added, which in the subsequent breaking is well mixed with the curd; that portion of curd, being sufficiently broken, is put into a cheese-vat, which is placed to receive it on a cheese-ladder over the cheese-tub: the vat is generally furnished with a coarse cheese-cloth. The second and third portions of the curd are treated in the same manner, and emptied into the vat; sometimes five or six times the quantity of salt is added to the middle portion of the curd; others salt all alike. The breaking takes up more or less time as the cheese was set together hotter or colder; half an hour is perhaps the longest time.

Thrusting or Hand-pressing the Cheese into the Vat.

The curd, when put into the cheese-vat in its broken state, is heaped above the vat in a conical form, to prevent it from crumbling down; the four corners of the cheese-cloth are turned over it, and three women, placing their hands against the conical part, gently but forcibly press it in nearly a horizontal direction, constantly shifting their hands when any portion of the curd is starting from the mass, and turning down and folding up the cloth as occasion requires. As soon as the curd adheres together so as to admit it, a small square board, with a corner of the cloth under it, is put on the top of the conical part of the curd, with a 60 lb. weight upon the board. Several iron skewers are at this time stuck in the

the cone, and alſo through holes in the ſides of the vat. Several uſe a wooden lever to preſs down the cheeſe.

The employment of the women is now drawing out and putting in the ſkewers; thruſting and keeping together the portions of the curd, that the power of the weight or lever diſplaces. This operation is continued until the whey, which at firſt ran from it freely, begins to be diſcharged by drops; the weight and ſkewers are then removed, and one woman takes up the corners of the cloth, while the others break the curd half way to the bottom of the vat, as ſmall as they can.

Some people uſe a wooden or tin hoop, nine inches broad, inſtead of holding up the corners of the cloth during this breaking. After the upper half of the cheeſe is thus broken, a weight or other power is again applied to it, and is ſkewered and thruſted as before: at firſt the whey again runs freely, and the operation is continued as long as thoſe means will preſs out a drop of whey. Two of the women then take the four corners of the cloth, the ſkewers, &c. being removed; and the other woman lays hold of the vat which is drawn from the cheeſe; and after rinſing it in warm whey, and putting another clean cloth over the upper part of the cheeſe, it is returned inverted into the vat again, and, being placed on the ladder over the tub, is broken half way through, as before; thruſting, weighting, and ſkewering, &c. is repeated, and continued from two to four hours, or as long as a drop of whey can be extracted from the cheeſe.

Putting the Cheeſe into the Preſs.

When no more whey can be extracted by the aforeſaid means, the cheeſe is again turned into the vat and rinſed, as before, with warm whey. The cloth now made uſe of is larger and finer than the former, and is ſo laid that on one ſide it ſhall be level with the ſide of the vat, and on the other wrap over the whole ſurface of the cheeſe, and the edges put within the vat; thus perfectly incloſing the whole cheeſe in the cloth. In this ſtage of the buſineſs, the cheeſe is ſtill higher than the edge of the vat; and to preſerve it in due form recourſe is had to a tin binder or hoop, about three inches broad, which is put round the cheeſe on the outſide of the cloth, and the lower edge of the binder preſſed down within the vat, ſo low as that the upper edge of it may be level with the ſurface of the cheeſe. The cheeſe is then carried to the preſs; and a ſmooth ſtrong board being placed over it, the preſs is gently let down upon it; the uſual power of which preſs is about 14 or 1500 lb. weight.

As ſoon as the cheeſe is put into the preſs, it is well ſkewered; the ſkewers are of a ſtrong iron wire, 18 or 20 inches long, ſharp at the points, and turned with a bow at the other end. The vat and tin binder have holes to receive the ſkewers, eſpecially the binder, which holes are ſeldom more than one inch from each other. As the preſs ſtands cloſe to a wall, only one ſide of the cheeſe can be ſkewered at a time; therefore as many ſkewers are ſtuck in different directions as conveniently may, leaving as many holes unſkewered as are ſkewered, to give an opportunity of changing the holes. The buſineſs of ſkewering continues till the next morning at ſix o'clock, and in that time the oftener they are ſhifted the better; every ſecond time of ſhifting them the cheeſe is turned half way round in the preſs, to come at the other ſide of it. In half an hour from the time the cheeſe is firſt put into the preſs, it is taken out again and turned in the vat into another clean cloth. At this time the edges of the cheeſe are pared off, if they have become ſharp under the preſs; but as the vats are now uſually made with the angles rounded, the paring is rendered unneceſſary, the vat being wiped dry before the cheeſe is returned to it.

When the cheeſe is thus the firſt time taken out of the preſs, it is the cuſtom of ſome places to put it into warm, and in others into hot whey, where it ſtands an hour or more. It is then taken out, wiped dry, and after it has ſtood till cool, it is returned back to the preſs. This is done with a view to hardening its coat, that it may ſtand the better. At ſix o'clock in the evening the cheeſe is again turned in the vat into another clean cloth. At this and the former turning, ſome dairy-women prick the upper ſurface of the cheeſe all over, an inch or two deep, before it is replaced under the preſs, with a view of preventing bliſters. At ſix o'clock in the morning it is again turned in the vat, with a clean cloth as before. The ſkewers are now laid aſide. When the next day's cheeſe is ready for the preſs, the former one is again turned in the vat with a clean cloth, and put under another preſs. At ſix o'clock in the evening, and at ſix the morning following, it is again turned in the vat, uſing at theſe two laſt turnings two cloths finer than thoſe before uſed, in order that as little impreſſion as poſſible from the cloth may remain on the coat of the cheeſe.

Salting the Cheeſe.

Four or five days after the cheeſe has been under the preſs, a freſh fine cloth is put under it, which ſerves only as a lining to the vat, and is not turned over the upper ſurface

of

of the cheese, as has been hitherto the case. It is then taken and placed nearly midside deep in brine in a salting tub; the upper surface of the cheese being covered all over with salt. It stands generally about three days in the salting tub; is turned daily, and each turning well salted, the cloth being twice changed in the time. The cheese is then taken out of the vat, and in lieu of which a wooden girth or hoop is made use of, equal in breadth nearly to the thickness of the cheese: in this it is placed on the salting-benches, where it stands about eight days, being well salted all over, and turned each day. The cheese is then washed in luke-warm water; and after being dried with a cloth, it is placed on the drying-benches, where it remains about seven days: it is then washed in warm water, as before, with a brush, and wiped dry with a cloth: after it has stood about two hours from this washing, it is smeared all over with about two ounces of sweet whey butter, and then placed in the warmest part of the cheese-room.

Cheese-Room.

During the first seven days it is rubbed every day well over, and generally smeared with sweet whey butter. Afterwards a circular space is left unrubbed of four or five inches diameter, in the centre of each side the cheese, which, as long as it is afterwards kept, is, or should be, turned daily, and rubbed three times a week in summer and twice in winter. Scraping the rind should be rendered unnecessary by frequent cleanings. In a warm room the coat will be easily prevented from rising. These cheese-rooms are commonly placed over the cow-houses, and this is done with a view to obtain that moderate and necessary degree of temperature so essential to the ripening of cheese, to which the heat arising from the cattle underneath is supposed very much to contribute. The most desirable covering for a cheese-room, as contributing to that temperature so much desired, is thatch, for reasons that must be obvious. Before the cheese is brought into the rooms, the floors are mostly well littered with what the farmers here call *sniddle*, though wheat straw is frequently made use of for this purpose, but the knots of it are apt to leave an impression on the cheese. The aftermeath of grass well dried seems to be a good substitute for sniddle.

XL. *On*

XL. *On the Antiquity and Invention of Fire Engines.* By *Professor* BECKMANN.

AN examination of the invention of forcing pumps I ſhall leave to thoſe who may be diſpoſed to write a hiſtory of hydraulics; I ſhall however obſerve, that, on the teſtimony of Vitruvius *, they are generally aſcribed to Cteſibius; that on this account they are uſually called *machinæ Cteſibicæ*; and that Cteſibius lived at Alexandria about the time of Ptolemy Philadelphus and Ptolemy Euergetes I.; conſequently, about two hundred years before the Chriſtian æra. My object here is to lay before the reader what information I have been able to collect reſpecting the queſtion, At what time did theſe machines begin to be employed for extinguiſhing fires?

To render them fit for this purpoſe, it was neceſſary that ſome alteration ſhould be made in their conſtruction, as at firſt they were employed only in the raiſing of water. To render them fit for extinguiſhing fires, it was neceſſary that the water ſhould be forced through the upper aperture to as great a height as poſſible; whereas, for the former purpoſe, it is enough if it flows from the upper aperture in ſufficient quantity into the reſervoir, from which it is to be conveyed to the place of its deſtination. In this caſe, the apparatus required for a machine to be uſed as a fire engine would be a defect; as the power which communicates to the water its velocity might be employed with more advantage to raiſe a larger quantity.

In my opinion, it is highly probable that Cteſibius had an idea of converting his forcing pump into a fire engine; for his ſcholar Hero of Alexandria ſpeaks expreſsly of this application, and deſcribes the method of conſtructing a kind of engine with two cylinders †; but it is doubtful whether this

* Lib. x. c. 12. p. 347. compare alſo lib. ix. c. 9. p. 321.

† In that work entitled Πνευματικα, or *Spiritalia*. It may be found with a Latin tranſlation in the *Veterum Mathematicorum Opera*, Pariſiis, 1693. folio. In p. 180, the following paſſage occurs: Ὁι δε σιφωνες, ὁις χρωνται εις τοις εμπρησμοις, κατασκευαζονται ὁυτως. Siphones, quibus utuntur ad incendias, hoc modo conſtruuntur. The annexed drawing repreſents an engine with two cylinders. Reſpecting the other editions of this work ſee *Fabricii Bibliotheca Græca*, vol. ii. p. 593, where the Latin tranſlation which I have in my poſſeſſion is omitted. It is entitled *Heronis Alexandrini Spiritalium Liber. A Federico Commandino Urbinate ex Græco nuper in Latinum converſus*. Pariſiis apud Aeg. Gorbinum, 1583. 4to. This edition ends with the words *et magis continuata converſio fiat*, which in the Paris edition ſtand in p. 230. It wants alſo the end of the Paris edition from the words, *Quomodo animal dividatur et bibat*, to *et tunc oblato poculo*

this application of the forcing pump ſoon became common, and whether the antient Romans were in poſſeſſion of this uſeful machine. All that I know on the ſubject is as follows:

Pliny the younger informs the emperor Trajan in one of his letters, that the city of Nicomedia in Bithynia had been almoſt entirely deſtroyed by fire; and that the devaſtation had been greater becauſe a violent ſtorm had taken place during the conflagration, and becauſe there was no apparatus in the city for extinguiſhing fires *. The *ſipho* here mentioned by Pliny ſeems, beyond doubt, to have been the fire engine of Cteſibius; though among the other articles we are to underſtand water conduits, and pipes employed for conveying water into the city. I muſt however confeſs that the word *ſipho* ſignifies alſo pipes of this kind: and particularly in a paſſage of Strabo †, where he ſpeaks of the ſubterranean conduits at Rome, and ſays, that almoſt all the houſes had ciſterns, *ſiphones*, or water pipes, and running ſtreams. But Pliny mentions at the ſame time water buckets, which are to be conſidered as an appendage abſolutely neceſſary to fire engines. It is alſo hardly poſſible to believe that a city ſituated as Nicomedia was, cloſe to a bay ‡, ſhould be in want of water.

But I can produce, and I believe I am the firſt to do ſo, for I do not find that it has ever been mentioned by any other writer, a very ſtrong proof from a cotemporary author. Apollodorus the architect, who was employed by the emperor Trajan in conſtructing the celebrated bridge on the Danube, and in completing ſome extenſive works at Rome; and who,

poculo bibet. More information reſpecting Hero may be found in *Heronis Cteſibii Belopœeka, hoc eſt Teliſactiva, Bernardino Baldo Urbinate Guaſtallæ Abbate Illuſtratore et Interprete. Item Heronis Vita eodem Auctore.* Auguſtæ Vindel. 1616. 4to. See alſo *Heronis Alex. Vita, et Scripta et quædam Inventa, Præſide R. A. Wagnero, Auctore J. A. Schmidt.* Helmſtadii 1714. 4to.

* *Epiſt.* 42. lib. x. Incendium latius ſparſum; primum violentia venti, deinde inertia hominum, quod ſatis conſtat, otioſos et immobiles tanti mali ſpectatores perſtitiſſe, et alioqui nullus uſquam in publico ſipho, nulla hama, nullum denique inſtrumentum ad incendia compeſcenda, et hæc quidem ut jam præcepi, parabantur.

† Lib. v. edit. Almel. p. 360. Τοσουτον δ'εϛι το εισαγωγιμον ὑδωρ δια των ὑδραγωγειων, ὡϛε ποταμους δια της πολεως και των ὑπονομων ρειν, ἁπασαν δε οικιαν σχεδον δεξαμενας και σιφωνας, και κρουνους εχειν αφθονους. Tantum aquæ per aquæductus in urbem derivatur, ut flumina per urbem et cloacam labantur, et quælibet propemodum domus ciſternas, tubulos ac canales habeat copioſos. Compare Lipſius *de Magnitudine Romana*, Antwerp 1598. fol. lib. iii. c. 11. p. 181.

‡ *Plinius* v. cap. ult. Eſt in intimo ſinu (Aſtaceno) Nicomedia Bithyniæ præclara.

as we are told by Dio Caſſius, was put to death by Trajan's ſucceſſor, Adrian, on account of a ſneering anſwer which he gave to that prince, tells us, in the remains of his work on warlike machines, what methods are to be employed when the upper part of an edifice is on fire, and the machine called *ſipho* is not at hand. In this caſe, ſays he, leather bags filled with water muſt be faſtened to long pipes in ſuch a manner that, by preſſing the bags, the water may be forced through the pipes and thrown on the place which is on fire *. The *ſipho*, therefore, was a machine by means of which water could be thrown to a height on places on fire, and which could not be reached in any other manner.

That in the fourth century, at leaſt, a fire engine properly ſo called was underſtood by the term *ſipho*, is fully proved by Heſychius †; as alſo by Iſidore, who lived in the beginning of the ſeventh century ‡. The latter remarks, that ſuch engines were employed in the eaſt for extinguiſhing fires; and thence there is almoſt reaſon to conclude that they were not then uſed in the weſt.

It now remains to determine the queſtion, at what time ſuch apparatus for extinguiſhing fires began to be employed at Rome. From the regulations for guarding againſt accidents by fire, and in regard to extinguiſhing fires, which occur in the Roman code §, one might be induced to ſuppoſe that this capital muſt have been furniſhed with thoſe neceſſary machines, of the want of which, in a provincial city,

* *Poliorcetica*, p. 32. in Veterum Mathematic. Oper. *Κἂν που ακρωτηριον καιηται δυσεπιβατον, μη η δε οργανον ὁ καλειται σιφων, καλαμοι παλιν τετρημμενοι ὥσπερ ὁι των ιξευτων, ἁρμοζονται, οπου δει φερειν αυτους ὑδωρ, ασκοι τε πληρεις πιεζομενοι εκθλιβωσι δι' αυτων επι τον καιομενον τοπον.* Si forte extremitas aliqua, ad quam difficilis ſit aſcenſus, ardeat, nec præſto ſit inſtrumentum illud quod vocatur ſipho, arundines rurſus perforatæ, cujuſmodi ſunt aucupum, aptantur in iis locis quo aquam ferre eas oportet, et utres aqua pleni preſſi aquam ejaculantur in locum qui igni conſumitur.

† *Σιφων οργανον τι εις προεσιν ὑδατων εν τοις εμπρησμοις.* Inſtrumentum ad ejaculandas aquas adverſus incendia.

‡ *Origines* xx. 6. Siphon vas appellatum quod aquis ſufflando fundat; utuntur enim hoc Orientales. Nam ubi ſenſerint domum ardere, currunt cum ſiphonis plenis aquis et extinguunt incendia, ſed et cum aras expreſſis ad ſuperiora aquis emundant. In many places hand engines are uſed in the like manner for waſhing the windows of the upper ſtories.

§ See *Digeſt.* i. tit. 15. where it is recommended to all perſons to keep water by them in their houſes. Ut aquam unuſquiſque in cœnaculo (the upper ſtories?) habeat, jubetur admonere. Alſo *Digeſt.* xlvii. tit. 9. many things relating to this ſubject may be found in *L. A. Hambergeri Opuſcula*, Jenæ et Lipſ. 1740. 8vo. p. 12. in the diſſertation *de Incendiis*. The following diſſertations, which I have not ſeen, contain alſo information reſpecting the police regulations of the Romans in regard to fires. G. C. Marquarti *de cura Romanorum circa Incendia*, Lipſ. 1689. 4to. E. Ottonis *Diſſert. de Officio Præfecti Vigilum circa Incendia*, Ultrajecti, 1733.

Pliny

Pliny complains, and which he had himſelf ſupplied. This conjecture, however, I cannot confirm; and antient as well as modern examples prove that the police regulations eſtabliſhed in ſmall towns are not always to be found in capitals. Antioch and ſeveral cities had ſtreet-lanterns, which were wanting in the proud Rome. But what excites ſome doubt is, that theſe fire engines are never mentioned in the Roman laws, nor in the accounts of the fires which frequently happened at Rome. When a misfortune of this kind happens at preſent, it is always mentioned whether a ſufficient number of engines were preſent, and what effect they produced; and Pliny, indeed, does not omit them in the ſhort account he gives of the fire at Nicomedia.

One paſſage only in Ulpian is commonly produced as a proof that, in his time, there were fire engines at Rome. Where he enumerates thoſe things that belong to a houſe when ſold, he mentions, beſides other articles, the *ſiphones* * employed for extinguiſhing fires. But if this term here means a fire engine, the paſſage appears to prove too much; for, in that caſe, we muſt believe that each houſe then was furniſhed with an engine of its own: if ſo, theſe engines muſt have been ſmall portable ones, ſuch as are common in many houſes at preſent; and the paſſage therefore cannot be adduced as a proof of public engines, ſuch as Pliny regrets the want of at Nicomedia. But it is more probable that Ulpian only alludes to thoſe *ſiphones* which according to Strabo's account were to be found in every houſe at Rome; that is, pipes which conveyed water into them for domeſtic purpoſes. From the total want of fire engines, or the imperfect manner in which they were conſtructed, what Seneca ſays muſt have been very true, that the height of the houſes at Rome rendered it impoſſible to extinguiſh the fires which happened in that city †. That the houſes there were exceedingly high, and the ſtreets, bridges, and highways, very nar-

* *Digeſt.* xxxiii. 7. 18. Acetum quoque, quod extinguendi incendii cauſa paratur, item centones, ſiphones, perticæ quoque et ſcalæ, et formiones et ſpongias et hamas et ſcopas contineri, plerique et Pegaſus aiunt. Alexander ab Alexandro, whoſe authority however is not deciſive, underſtands here engines. *Dier. Genial.* v. 24. p. 342. Siphones vero, fiſtulas follibus junctas ſeu machinas hydraulicas, quibus agitatis, at ſuperiora ædium exhauriunt aquam, qui etiam organa pneumatica dicti. Theſe *fiſtulæ follibus junctæ*, which no commentator has explained, are thoſe tubes which were before propoſed by Apollodorus.

† *Controverſ.* ix. lib. 2. p. 153. Ædes, quas in tantum exſtruxere, ut domus ad uſum et munimentum paratæ, ſint nunc periculo, non præſidio; tanta altitudo ædificiorum eſt, tantæque viarum anguſtiæ, ut neque adverſus ignem præſidium, neque ex ruinis ullum ullam in partem effugium ſit.

row, is well known. Archenholz * and others are of opinion, that the houses were raised to such a height on account of the great heat in that warm climate; but the principal cause was that assigned by Vitruvius †, and which still produces the same effect. For want of room on the earth, the buildings were extended towards the heavens; so that at last the greatest height of an edifice was fixed by a law at 70, and afterwards at 60 feet. In Hamburgh at present, where ground is valuable, and still becoming more so, most of the houses are 60 feet, and some 70 feet in height; and how much this height, notwithstanding the perfection to which fire engines have been brought, renders the extinguishing of fires difficult, and often impossible, is shown by the melancholy instance of Gera, where the houses are built lower. With Neubert's engine, which was tried at Hamburgh in 1769, eight fire-men raised $11\frac{1}{2}$ cubic feet of water to the height of about 62 or 63 feet. The best English engines can throw water to the height only of from 32 to 33 feet ‡.

In the East, such engines were employed not only to extinguish fires, but to excite conflagrations. The Greek fire, invented in the year 678, by Callinicus, an architect of Heliopolis, called afterwards Balbec, the use of which was continued in the East till the year 1291 at least §, and which certainly was liquid ||, was employed in many ways, but chiefly on board ships, being thrown, by means of large fire engines, into the ships of the enemy. Sometimes this fire was kindled in certain vessels which might be called *fire ships*, and which were introduced among the hostile fleet ¶; sometimes it was thrown upon the enemy from jars and other vessels with machines of various kinds **; and sometimes the soldiers

* England und Italien, Lipf. 1785. 8vo. ii. p. 216.

† Lib. ii. c. 8. p. 68. In ea majestate urbis et civium infinita frequentia, innumerabiles habitationes opus fuit explicare. Ergo cum recipere non posset area plana tantam multitudinem ad habitandum in urbe, ad auxilium altitudinis ædificiorum res ipsa coegit devenire.

‡ This is a mistake of the author. Some English engines can throw water to a much greater height.—EDIT.

§ Hanovii Disquisitiones; Gedani 1750. 4to. p 65.

|| *Annæ Comnenæ Alexiad.* lib. xvi p. 385. Πυρ ὑγρον: the same appellation occurs in Theophanes Chronographia.

¶ Theophanes, p. 294 and 352.

** Such a machine is called by Joinville in *Histoire de S. Louis IX.* Paris 1668. fol. p. 39. ung engin, qu'ils appelloient la Parriere, par lequel enging ils nous gettoient le feu Gregois à planté. Does not the following passage also in *Leonis Tactica*, Lugd. Bat. 1612. 4to. p. 347. allude to the Greek fire? though Meursius thinks that the author speaks of unslaked lime, which, however, could not produce the described effect: Χυτρας και αλλ' ἰυς ασβεστου πληρες, ὡν ῥιπτομενων και συντριβομενων, ὁ του ασβεστου ατμος soldiers squirted it from hand engines, or, as appears, by blowing it through tubes: but neither these hand engines nor such tubes could have been the machines employed for projecting it from the fore part of ships against those of the enemy. They were constructed of copper and iron; the extremity of them had the appearance sometimes of the open mouth and jaws of a lion or other animals; they were painted, and even gilded; and it appears that the fire could be thrown from them to a great distance.

These machines are called expressly by various authors *spouting engines*. Thus Johannes Cameniata, speaking of the taking of his native city Thessalonica by the Saracens in the year 904, says that the enemy threw fire into the wooden works of the besieged, which was blown into them by means of tubes, and thrown from other vessels *. This passage, which I do not find quoted in any of the works that speak of the Greek fire, proves, at the same time, that at the beginning of the tenth century the Greeks were not the only possessors of the art of preparing this fire, the forerunner of our gunpowder. The emperor Leo, who about this time wrote his Art of War, recommends the use of such tubes on the fore part of ships, and to be placed under the shelter of a proper covering †: he mentions the squirting of the Greek fire in two more passages ‡. In a word, people may have easily conceived the idea of discharging the Greek fire by means of a forcing pump, because the use of this machine for extinguishing fires was known there long before the invention of Callinicus.

At what period the cities in Germany began to be furnished with fire engines I have not been able to determine.

ατμος συμπνιγει και σκοτιζει τους πολεμιους, και μεγα εμποδιον γινεται. Ollas calce viva plenas alii injiciunt, quibus confractis, calcis vivæ pulvis dissipatus suffocat et strangulat hostes, et magno ad præliandum impedimento est.

* In Leonis Allatii *Συμμικτα*. Coloniæ Agripp. 1653. 8vo. p. 389. *Πυρ τε δια των σιφωνων τω αερι φυσησαντες, και τινα αλλα σκευη* . . . . ignem siphonibus efflantes, et alia vasa, flammarum plena, in murum conjicientes.

† Cap. xix. § 6. p. 322. *Εχετω δε παντως τον σιφωνα κατα την πρωραν εμπροσθεν χαλκω ημφιεσμενον, ως εθος, δι' ου το εσκευασμενον πυρ κατα των εναντιων ακοντισει* (according to the reading in *Fabricii Bibliotheca Græca*, vi. p. 373.): In prora siphonem aere obtectum de more habeas ad ignem in hostes ejaculandum, et celse supra siphonem pseudopatium ex asseribus confectum et asseribus circumtectum, in quo viri ad bellandum instructi sint.

‡ *Pag.* 344. Interdum frontem classis directam instrues, ut ubi usus ferat in hostium proras irruat, et siphonibus ignem ejicientibus naves illorum incendat. *Δια του πυρος των σιφωνων.* *P.* 346. Multæ molitiones excogitatæ sunt; cujus generis sunt, ignis cum tonitru et fumo ignito per siphones emissus, et incendens naves. *Οιον τοτε εσκευασμενον πυρ μετα βροντης και καπνου πυρωδους δια των σιφωνων πεμπομενον.*

In my opinion they had regulations respecting fires before they had engines; and the former do not seem to be older than the beginning or middle of the sixteenth century. The oldest for the city of Franckfort on the Mayn, with which I am acquainted, are of the year 1460*. The first general order in Saxony respecting fire, is that of duke George in the year 1521 †. The first for the city of Dresden, and which was to serve for the whole country, is of the year 1529 ‡. In many cities, the first ordinances published by government for the purpose of preventing danger from fire, may be found perhaps among the regulations in regard to building, which appear to be somewhat older than the particular ones respecting fires. At Augsburg an express regulation in regard to building was drawn up and published so early as 1447 §. On turning over the old chronicles it is observed that great fires began to be less frequent in the sixteenth century; a circumstance which beyond all doubt must be ascribed to the improvements in building ||, the regulations made by government for the guarding against fires, and the introduction of apparatus for extinguishing them. But by the invention of fire engines every thing in this respect has been so much changed, as to render it necessary to revise and entirely alter the old regulations made in regard to fires; and it is not impossible that the first mention of public fire engines will occur in the new regulations respecting fires made in the sixteenth and seventeenth centuries.

Mr. Von Stetten has remarked, that the first mention of fire engines at Augsburg occurs in the building accounts of the city for the year 1518. They are called there *instruments for fires, water syringes useful at fires* ¶; and these names seem to announce that the machine was then in its infancy. At that time they were constructed by Anthony Blatner, a goldsmith of Friedberg, but who in the above year was a citizen of Augsburg. From the observation added, that the wheels and poles (levers) were made by a wheel-wright, and by the largeness of the sum put down in the

* A passage from it is quoted in Orth's *Anmerkungen uber die erneuerte Reformation der Stadt Frankfurt* 1751. 4to. iii. 404.

† It is intitled Begreyff der Fewer Ordenunge, four pages folio. It is wanting in the *Codex Augusteus*, but its contents may be found in Canzler und Meisner für ältere Litteratur und neuere Lectüre, Lips. 1785. 8vo. iii. 3. p. 97.

‡ Wecken's Beschreibung und Vorstellung der Residenz Dresden, p. 482.

§ Von Stetten Kunstgeschichte der Stadt Augsburg, i. p. 87.

|| Thus, in the year 1466 straw thatch was forbidden at Franckfort; and in the year 1474 roofs constructed of shingles. *Lersner*, ii. p. 22.

¶ Kunstgeschichte der Stadt Augsburg, ii. p. 112.

the account, there is reafon to conclude that thefe were not hand engines, but large compound machines. In that refpectable dictionary *Maalers Teutfchfprauch*, printed at Zurich in 1561. 8vo., I find *feuerhaken* (fire hooks), and *feuerleitern* (fire ladders); but no machines for extinguifhing fires, nor any where a fire engine.

In the year 1655 the jefuit Cafpar Schott was ftruck with admiration on feeing at Nuremberg an engine, made there by John Hautfch. It ftood on a fledge ten feet in length and four in breadth. The veffel that contained the water was eight feet in length, four in height, and two in breadth. It was put in motion by twenty-eight men, and forced a ftream of water an inch in diameter to the height of eighty feet, and confequently above the houfes. The whole machine was drawn by two horfes. Hautfch is faid to have caufed an engraving to be made of this machine, which he circulated, with an offer of conftructing fimilar ones at a moderate price, and teaching the ufe of them. He refufed to fhow Schott the internal conftruction of his machine; but the latter readily conceived it. From what Schott fays, it evidently appears that the cylinders did not ftand perpendicular, but lay in a horizontal direction in the box; fo that the piftons moved horizontally, and not vertically as at prefent. Vertical cylinders, therefore, feem to be among the improvements fince made to this engine. Schott adds, that this invention was not new, it had been known in other cities; and he himfelf forty years before, confequently in 1617, had feen one, but much fmaller, in his native city *. Schott, as is well known, was born at Konigfhofen, not far from Würzburg, in 1608. George Hautfch, the fon of the former artift, conftructed fuch engines alfo; and probably made fome improvement in them; for Wagenfeil † and others afcribe to him the invention.

The firft regulations at Paris refpecting fires, as far as is known, relate to fuppreffing thofe incendiaries who, under the name of *boutefeux*, occafioned great devaftation in the capital as well as in the provinces ‡. Paris feems to have obtained the firft fire engines in 1699: in that year, at leaft, the king gave to Dumourier Duperrier an exclufive privilege

* *Magia Univerfalis*, p. iii. lib. 6. p. 510; and in *Pafchii Invent. Nov.-antiq.* Lipf. 1700. 4to. p. 668. The Magia Univerfalis was printed in 1657. See alfo Doppelmayr, p. 301, who fays that the water was raifed to the height of a hundred feet.

† De Civitate Noribergenfi, p. 153. Marpergers Geöfnetes Manufakturhaus. Hamburg 1707. 12mo. p. 210. Doppelmayr, p. 303.

‡ Continuation du Traite de la Police par Delamare, Paris 1738. fol. p. 137.

to construct these machines, which were called *pompes portatives*; and he engaged for a certain sum to keep in repair the seventeen engines which had been purchased for the use of the city, and to provide and pay the necessary workmen. In the year 1722 the number was increased to thirty, which were distributed in several quarters of the city; and the contractors received annually at that time 20,000 livres. The city, however, besides the thirty royal engines, had others which belonged to the *hotel de ville*, with which the sieur Duperrier had nothing to do *.

In the middle of the sixteenth century these machines were certainly still very imperfect. They had neither an air chamber nor buckets, and required a great many men to work them. They consisted only of a sucking pump and forcing pump united, which forced the water out in spurts, and with continual interruptions. Such machines in moving the lever experience a stoppage, during which no water is forced out; and, as the pipe is fixed, it cannot convey the water into a house on fire, though it may reach a fire at no great distance where there are doors and windows to afford it a passage. At the same time, the workmen are liable to be crushed by the fall of the houses, and on that account must keep from them. Hautsch, however, had given to his engine a flexible pipe, which could be turned to any side as might be necessary; but not an air-chamber, which Schott certainly would have described. Even in Belidor's time, the fire engines in France were of no other kind. In the year 1760 these improved machines were used only in England; at least professor Busch † concludes so from this circumstance, that Ferguson considers Newsham's engine, which threw the water out in a continued stream, as a new invention.

It is not with certainty known who first conceived the idea of constructing an engine with an air-chamber, in which the included air, by compressing the water, forces it out in a continued stream. According to a conjecture of Perrault, it would appear that Vitruvius seems to speak of a similar construction; but Perrault himself acknowledges that this obscure passage ‡ can be explained in a different manner. The air-chamber in its action has however some resemblance to Hero's fountain, in which the air, being compressed by the water, forces the water out §.

* Continuation du Traité de la Police, &c. p. 157.

† Versuch einer Mathematik zum Nutzen und Vergnügen, Hamburg 1791. 8vo. p. 396.

‡ Lib. x. c. 12.

§ Spiritalia, xxxvi. p. 85.

I can find no older engine with an air-chamber than that described by Perrault, and of which he has given a figure. He says it was preserved in the king's library at Paris; that it was employed for throwing the water to a great height during fires; that it had only one cylinder, and yet threw out a continued jet of water. He neither mentions the period of the invention, nor the name of the inventor; and I can only add that his book was printed in 1684 *. The principle however seems to have been pointed out before by Mariotte, who by some is, on this account, considered as the inventor; but he does not appear to have entertained any idea of a fire engine, at least he makes no mention of it †.

It is certain that the air-chamber, at least in Germany, came into common use after it had been applied to these engines by Leupold, who constructed and sold a great many of them. In the year 1720 he published an account of them in a small work consisting of a few sheets in quarto: but at first he kept the construction a secret; and those engines which he sold consisted of a strong copper box, entirely shut and strongly soldered. It weighed only sixteen pounds; occupied very little space; had but one cylinder; and one man by means of it could throw the water, without interruption, to the height of from twenty to thirty feet ‡. About the year 1725, Du Fay saw one of these machines of Leupold at Strasburgh. He conceived, as he says, its construction, and made it known in the Transactions of the Academy of Sciences for 1725. It is very singular that Du Fay makes no mention of Mariotte, nor of the engine preserved in the king's library at Paris. Leupold however, so early as the year 1724, gave a figure and description of his engine in his *Theatrum Machin. Hydraulicarum*, vol. i. p. 120. tab. 45. fig. 2., which was not known to the French author.

An useful improvement of this engine consists in the hose added to the engine, which can be lengthened or shortened, and to which the fire-pipe is applied, so that the person who directs it can approach the fire with much less danger. This invention, as is well known, belongs to two Dutchmen,

* I possess only the second enlarged edition of his *Architecture de Vitruve*, large folio, where the account is to be found, p. 318. Whether it be in the first edition of 1673, I do not know. Daniel Bernoulli has given a figure and description of it in his *Hydrodynamica*, Argentorati 1738. 4to. p. 172. fig. 51.

† The passage may be found in *Traité du Mouvement des Eaux*, p. 4. dis. i.; and in *Oeuvres de Mariotte*, Leide 1717. 2 vol. 4to. i. p. 445. fig. 89.

‡ An extract from his work may be seen in *Breslauer Samlungen*, versuch vi. p. 2035, and versuch vii. p. 374.

both named John van der Heide *, who were inspectors of the apparatus for extinguishing fires at Amsterdam. The first public experiments made with them took place in the year 1672, and were attended with so much success, that at a fire next year the old engines were used for the last, and the engines with moveable hose for the first, time. In the year 1677 the inventor obtained an exclusive privilege to construct engines according to their principle for twenty-five years. In the year 1682 a sufficient number of these engines were distributed in the different quarters of the city, and the old engines were entirely laid aside. In the year 1695 there were in Amsterdam more than sixty of these engines, the six of which nearest the place where a fire broke out, were required to be present to give assistance. In a few years they were common in all the towns of the Netherlands.

All these circumstances have been related by the inventor himself in a particular work, which, on account of the excellent copper-plates in it, has become exceedingly scarce †. The first seven of these represent some dreadful conflagrations, at which the old engines were used without producing much effect. Among them is the conflagration of the stadt-house of Amsterdam in the year 1652. The twelve following plates are representations of fires which were extinguished by means of the new engines, and exhibit at the same time the various ways in which they could be employed with advantage. According to an annexed account, it appears that the city of Amsterdam lost by ten fires, at which the old apparatus was used, 1,024,130 florins; but during the ten following years, when the new engines were introduced, only 18,355 were lost by forty fires; so that the annual saving by this invention was 98 per cent.

The internal construction of these engines has no where been represented, nor do I remember to have read a passage in any author which proves that they were furnished with an air-chamber, though in the privilege they are called

* In the privilege, however, they are called *Jan ende Nicolaas van der Heyden.*

† Beschryving der nieuwlijks uitgevonden en geoctrojeerde Slang-Brand-Spuiten, en haare wijze van Brand-Blussen, tegenwoordig binnen Amsterdam in gebruik zijnde.—Door der zelver Inventeur Jan van der Heide en Jan van der Heide de jonge, generaale Brandmeesters der Stad Amsterdam. Amsterd. 1690. To this subject belongs also the following work, a copy of which is in our library: Beschryving der Brand-orders, en wijze van Brandblussen, tegenwoordig binnen Amsterdam in gebruik zijnde, door Jan van der Heide, en Jan van der Heide de Jonge, generaale Brandmeesters der zelver Steede. Amsterd. 1695; one sheet and a half 4to. with four plates, and a description of them. One of these descriptions is inserted in the Leipsic Fire Regulations for the year 1759. 4to.

Spouting

*ſpouting engines**. The deſcription even of the leather pipe, or hoſe, is very ſhort and defective; probably with a view to render it difficult to be imitated: it is only ſaid that it was made in a peculiar manner of leather; ſo that it was not only thick, but could withſtand the force of the water †.

The conveyer or bringer was invented alſo in Holland about the ſame time. This name is given at preſent to a box which has on one ſide a ſucking pump, and on the other a forcing pump. The former ſerves to raiſe the water from a ſtream, well, or other reſervoir, by means of a ſtiff hoſe, the extremity of which is furniſhed with a metal ſtrainer to prevent dirt from entering it, and which is kept ſuſpended above the mud by means of a piece of cork. The forcing pump forces the water thus drawn up, through another hoſe, into the engine, and thus renders unneceſſary the labour of ſupplying it with water by means of buckets.

At firſt, indeed, this machine was very ſimple. It conſiſted of a leather hoſe ſcrewed upon the engine, the end of which widened into a kind of bag ſupported near the reſervoir, and kept open by means of a frame; and the labourers poured water into it from buckets. The Van der Heides, however, had for this purpoſe employed a pump which they called a *ſnake pump*, and by which they could raiſe the water upwards of a thouſand feet. But how it was conſtructed I no where find mentioned: from the figure, I ſhould almoſt conjecture that they uſed only a cylinder with a lever. They ſometimes alſo placed a portable pump in the water, and thus raiſed water into the annexed hoſe, which conveyed it to the engine. Every leather pipe employed for conducting water they called a *water ſnake*; and theſe were not made, as the hoſe of the fire engine, of leather, but of ſail-cloth: they however ſay, that for this purpoſe the ſail cloth required a peculiar preparation ‡, which conſiſted in making it water proof by applying to it a certain kind of cement. The hoſe alſo through which the water is conveyed muſt be ſtiffened, and diſtended by metal rings; otherwiſe the external air on the firſt ſtroke of the pump would compreſs the hoſe ſo that it could admit

* Slang-Brand-Spuyten, beſtaande in een geduirig ſtralende Brand-Spuit, met een buygelijke buys daar aan, om haar gedaante een Slang genaamd, die men kan verlangen naar eyſch ende welgevallen.

† Page 6. Slang, of Spuit-Slangen van leer gemaakt, dat op een byzondere wyze bereid en zaamen gevoegt word om dicht, beſtendig en tegen't geweld van't water beſtand te zyn. In the French explanation of the plates theſe new engines are called *pompes à boyaux*.

‡ Page 5. Water-Slang, zynde een lange en buiggelyke buis, van zeker ſoort van doek, hier toe byzonderlyk bereid, gemaakt.

into it no water. It is here ſeen that the uſe of ſail-cloth hoſe is not ſo modern an invention as ſome have ſuppoſed. That our apparatus for conveying water to the fire engine is much more ingenious and convenient muſt be allowed; but I would ſtrongly recommend that in all cities there ſhould be pumps or running wells of water, to the ſpout of which the one end of hoſe might be fixed, while the other is ſcrewed upon the engine. The Van der Heide reckoned it an advantage in their invention, that this apparatus rendered it unneceſſary to have leathern buckets, which are expenſive; or at leaſt leſſened their number, as well as that of the workmen.

From this information, the truth of which cannot be doubted, we may readily believe that engines with hoſe were not invented in the year 1697 by Gottfried Fuchs, fire director at Copenhagen, as was however publicly aſſerted in 1717; with this addition, that the invention was ſoon after employed in Holland and at Hamburgh *. Fuchs ſeems only to have introduced into Denmark the invention of the above Dutchmen at the time of the great fire on the 19th of April 1689, when the opera houſe at Amalienburg, with more than 130 perſons, as well as the beautiful palace of Amalienburg, were deſtroyed. We are at leaſt told in hiſtory†, that this event gave riſe to an improvement in the apparatus for extinguiſhing fires, and to a new ordinance of the 23d of July of that year reſpecting fires, as well as to the introduction of fire engines with hoſe; which however had been employed at Amſterdam more than a dozen of years before.

Hoſe or pipes of this kind for conveying water were not, however, unknown to the antients. The architect Apollodorus, at leaſt, ſays, in the paſſage already quoted, that to convey water to elevated places expoſed to fiery darts, inſtead of pipes, the bowels of an ox might be employed, having at the one end a bag filled with water, which being compreſſed, the water will be forced through the inteſtines to the place of its deſtination ‡. Theſe were conveyers of the ſimpleſt conſtruction.

Among

* Breſlauer Samlung 1717. 1 verſuch, p. 108. Paſchii Inventa nov.-antiqua, p. 668. W. G. Heſſe Abhandl. zu Verbeſſerung der Feuerſprützen. Gotha 1778. 8vo. p. 7.

† Algemeine Welthiſtorie, th. 33. p. 631.

‡ *Poliorcet*, p. 32. *Κατα δε τα προκειμενα τοις πυρϐολοις μερη, αντι σωληνων, βοων εντερα παραφεροντα ὑδωρ εις ὑψος. Τουτων ασκοι πληρεις ὑδατος παρατιθενται και θλιϐομενοι αναφερουσι.* In partibus autem quæ expoſitæ ſunt telis incendiariis, pro tubis, boum inteſtina habere oportet, quæ aquam in ſublime deferant.

Among the neweſt methods of improving the hoſe of fire engines, is that of weaving them without a ſeam. In the year 1720 ſome of this kind were made at Leipſic, of hemp, by one Beck, a lace-weaver, as we are told by Leupold in his before-mentioned account of fire engines, printed in that year. After this they were made alſo by Erk, linen-weaver at Weimar; and at a latter period they were made of linen at Dreſden, and alſo in Sileſia *. At preſent, Gegner and Ehrliholzer have a manufactory at Bethnal Green †, where they make water-tight hoſe without ſeams. Aſſeſſor Mögling has cauſed ſome of the ſame kind to be manufactured on his eſtate near Stuttgard on a loom invented by himſelf, and which are now uſed on many of the towns of Wurtemberg ‡. I ſhall here take occaſion to mention a kind of loom on which Braun cauſed ſhirts without ſeams to be wove, ſimilar to thoſe ſometimes brought from India as curioſities, and of which he gave a particular deſcription, with an engraving §.

I ſhall in the laſt place obſerve, that notwithſtanding the belief of the Turks in predeſtination, the uſe of fire engines has been lately introduced at Conſtantinople by Ibrahim Effendi ||.

XLI. *Deſcription of a new Electrometer.* By HENRY LAWSON, *Eſq.*

SIR, *To Mr. Tilloch.*

As the following deſcription of an inſtrument, which anſwers its purpoſe very well, may prove acceptable to the public, I ſend it to you for inſertion in the Philoſophical Magazine.

Some time ago it ſtruck me that ſome additions to Brooks's electrometer might be made ſo as to fit it for a good diſcharging electrometer to meaſure the repulſion between two balls

deferant. Ante hæc inteſtina utres aqua pleni collocantur, qui preſſi aquam ſurſum emittunt.

* Leipziger Intelligenzblatt 1775. p 345; and 1767. p. 69; Teutſcher Merkur 1783.

† The Environs of London, by Daniel Lyſons; Lond. 1792—1796.

‡ W. G. Rappolt über die Stärke rund gewebter Seile, Tubingen 1795. 8vo. Phyſikal ökonom. Bibliothek xix. p. 258.

§ Veſtitus Sacerdotum Hebræorum; Amſtel. 1701. 4to. i. p. 273. A great deal of uſeful information reſpecting improvements in the apparatus for extinguiſhing fires may be found in Aug. Niemann *Ueberſicht der Sicherungsmittel gegen Feuersgefahren*; Hamburg und Kiel 1796. 8vo. See *Phyſikal ökonom. Bibliothek* xix. p. 412.

|| Buſchings Erdbeſchreibung. ii. p. 673.

(of

(of a certain ſize) in grains, and alſo effect the diſcharge of a battery at the ſame time. The inſtrument known by the name of Cuthbertſon's diſcharging electrometer was at that time the beſt, and indeed the only inſtrument for diſcharging batteries or jars by its own action, then made; but I think this will be found in the eſſentials, and in the theory and uſe, a more perfect inſtrument.

On the baſe S (fig. 1. Plate VI.) is fixed the glaſs pillar G, ſupporting the hollow braſs ball B. T, is a light graduated braſs tube divided (from the weight W towards the ball B) into thirty parts, repreſenting grains. W, is a ſliding weight. L, a light braſs ball ſcrewed to the end of the tube T, on the other end of which tube adjuſts the heavy counter balance-ball C, the tube T and its two balls being ſuſpended at their common centre of gravity by a ſilk line in the centre of the ball B, the mechaniſm of which is ſhown in fig. 2. The braſs ball F is ſtationary, and of the ſame ſize as the ball L, and is fixed by a braſs arm to the ball B. The adjuſting ball A is alſo of the ſame ſize, and adjuſts cloſe to the ball L, or at any lower ſtation between that and the ring *r*. The braſs tube to which the ball A is fixed is divided into inches, halves, and quarters: (a more minute diviſion is unneceſſary and improper.) The diviſions begin, or the 0 line is marked, on the ſaid tube at the ring *r*, when the three balls A L F are cloſe together. The ring *r* ſerves as an index, as the diviſions paſs in ſucceſſion into the glaſs tube P on lowering the ball A. The hook H is ſcrewed into the baſe of P. The quadrant, or Henley's electrometer, Q, is ſupported on a long braſs ſtem to keep it out of the atmoſphere of the lower part of the inſtrument. Fig. 2. ſhows the internal conſtruction of the ball B, fig. 1. In the firſt place, the ball ſcrews in half, horizontally. The light tube T paſſes through the ball, and is ſuſpended nearly in the centre of it by ſome ſilk twiſt *s*, which ſmall ſilk twiſt is fixed into the eye of the adjuſting wire *a*, part of which wire is filed ſquare and goes through the ſquare hole *b*. The nut *n* ſcrews on *a*, and ſerves to adjuſt the light tube T vertically. The light plates P P are of copper, and move freely on the wire *w w* ſomewhat like a hinge, and reſt on the copper wires C C, ſerving to make the direct communication between the inſide and out of the battery or jar. N *N* are notches ſerving to let the tube T deſcend when the diſcharge is made. Into the tube Z the glaſs pillar is ground. Note, that at the bottom of the notch N is a piece of braſs filed with a Y, and ſo placed as to keep the centres of the balls L and F, fig. 1, under each other when they come cloſe together.

When

When the inftrument is adjufted, which is done by placing the weight W, fig. 1, at o on the line of grains, and then fcrewing or unfcrewing the counterbalance ball C, till the tube T rifes flowly into its horizontal pofition; then fet the ball A at the diftance from the ball L that you choofe, and the weight W placed at the divifion or number of grains that you wifh the repulfive power of the electricity to arrive at before the difcharge is made: this being done, connect the battery or jar with the ball B by means of the wire *y*, the end of which goes into B at the hole X, and fhould ftand at right angles to B, the ball of *y* refting on the battery: then connect the outfide of the battery or jar with the hook H; as the battery charges, the electrometer Q continues to rife; and when it is fo highly charged that the repulfive power between the balls L and F is equal to the number of grains at which the weight W was placed, the ball L will defcend, and deliver the charge of the battery to the ball A. The fubftance or thing through which the fhock is intended to be paffed, muft form part of the communication between the hook H and the outfide of the battery or jar.

It is evident, from confidering the inftrument, that it is attended with thefe excellences; that, the whole being compact, the electric atmofphere muft be fo alfo; that it has but one *infulation* to depend upon; that the equilibrium of the divided arm and ball may always be adjufted, confequently the weight ufed known to a certainty; and the hook for communicating with the outfide of the battery being placed on the foot of the inftrument, the communication may be made by wires *lying* on the table; which are by far the beft mode of conducting fhocks, as chains, by the imperfect connection of their links (efpecially if long), very often, and without being fufpected, hinder the fuccefs of many very well concerted experiments.

XLII. *Defcription of an improved Chemical Apparatus for preferving feparate the Gafeous Products evolved in many Proceffes; by which alfo Abforption is completely prevented, and Liquids may be ftrongly impregnated with the different Gafes.* By W. H. Pepys *jun. Efq.**

THE chemift, in his various refearches into the compofition and qualities of the numerous fubftances prefented by Nature, had long to regret the want of an apparatus capable

* Communicated by the Author.

of

of preserving all the gaseous as well as liquid products evolved from them during his experiments. The apparatus invented by Woulfe, and which will immortalize his name, was the first, and indeed may be said to be the only one yet made known, calculated at all to answer so desirable an end: but it had defects; and though some of them have been removed by subsequent improvements, still it has its imperfections. Every chemist will readily perceive that I have in my eye the inconvenience experienced on changes of temperature: if absorption take place between the retort and receiver, a portion of liquid from the first Woulfe's bottle rushes into the receiver; or, if the tube of Welter is used, an undetermined quantity of atmospheric air finds admission to supply the vacuum thus formed.

To obviate these inconveniences, in a train of experiments, in which I was engaged in the year 1798, on the oxymuriatic acid and its compounds, after thinking of different contrivances, it struck me that I should be able to accomplish what I had in view, by procuring an adaptor, mounted with a glass valve (similar to that used in the improved Nooth's apparatus), and, at the end furnished with the valve, ground into a tubulated receiver; and then, by means of a bent tube, connecting the upper opening of the adaptor with a series of Woulfe's bottles. By means of this apparatus, which was executed agreeably to my instructions by Mr. Blades, I was enabled to form the purest oxymuriates without the smallest risque of any accident from absorption.

By another modification of the apparatus, in which the woulfes were dispensed with, I obtained some of the finest crystals of oxymuriate of potash I have ever seen, without the smallest portion of gas escaping—a circumstance of no small moment; oxymuriatic gas, as every one knows, being extremely noxious. In this arrangement, two additional adaptors were joined to the one ground into the receiver and furnished with the valve as already mentioned; and the uppermost adaptor was furnished with a tube leading into a pneumatic apparatus; or, when the redundant gas was not to be preserved, into a chimney. By employing the apparatus in this form, every bubble of gas has to pass through the three adaptors, and experiences a considerable agitation along with the liquor employed in them; the gas forcing the liquid from the lowest into the second, and from that into the uppermost adaptor,—till, a portion of it being taken up by the fluid, and the lower orifices of the upper adaptors being at times left quite free, the liquid, following the common law of gravity, returns to occupy again the lower station:

tion. When there is a quick and plentiful supply of gas, the whole of the liquid is kept in continual motion, offering thereby a proportionally large surface for impregnation, when that is the object in view; as when it is wished, for instance, to impregnate water very highly with ammoniacal gas procured from the sulphate or muriate of ammonia by lime. The quantity of liquid employed at one time in the adaptors should be about sufficient to fill the lowest one full, and the middle one about a third part.

The apparatus of which I have been speaking, which is less expensive than ground woulfes, not so easily injured, and more readily replaced when any part is fractured, I have found so extremely useful, and applicable to so many purposes, that I persuade myself such a description of it as will enable any one to get it constructed, will be acceptable to many. United with a series of woulfes, it forms one of the completest apparatuses that has yet been devised for a great variety of chemical experiments; for it is not proposed to supersede the woulfes: indeed, when gaseous products are intended to be passed through a variety of liquids, they will, in many cases, be found indispensably requisite *. I shall take the present opportunity, now that we are speaking of Woulfe's apparatus, of mentioning a few circumstances which, having found useful myself in constructing a series of woulfes, may prove equally so to others.

Lavoisier frequently regretted the want of a perfect Woulfe's apparatus, the usual method of manufacturing which is by grinding the tubes to fit the bottles after the tubes have been bent: from this circumstance, it generally happens that the bottles have no equal standing, but are obliged to be accommodated to the bend of the tubes and the fitting of the necks by means of wedges and supports put under the bottles, which continually exposes the tubes to the risque of being broken during the adjustment of the parts. To remedy this inconvenience, when I want tubes, I cause them to be ground to fit the woulfes while straight (See A, fig. 4. Plate VII.); I then bend them into two angles of about 130° or 140°, just above the ground stopper parts (See B, fig. 4.): then taking the two bottles to which the tube has been ground, and placing

* In other cases, however, even where gases are to pass through different liquids, they could be dispensed with, if, as Mr. Chenevix suggested when I showed him the apparatus, a set of adaptors were provided, each furnished with a valve, like the lowest adaptor, instead of the bent tube now attached to them. The gas evolved might by this means be made to pass through any number of adaptors, which would save all the trouble attending glass tubes. The valves, however, would considerably increase the expense.

 them

them on a true plane, and heating the tube, as exactly as I can in the centre between these angles, I there form a third angle (C, fig. 4.) while the glass is soft and yielding *, making it to accommodate itself to the ground necks, in which it is left without being moved for about a quarter of an hour to cool thoroughly. If moved too soon, the tube is apt to alter its form. In this manner, tubes may be bent, one at a time, for a range of any extent; and when arranged afterwards on any plane surface, will always be found to fit.

I shall here mention a kind of tubes which I have found extremely convenient on many occasions: I mean tubes made of elastic gum or caoutchouc. They are composed of a web or tube of wove cat-gut, coated outside and inside with elastic-gum varnish. They stand the action of most of the gases. Ether, alcohol, or oils, however, would destroy them. The accommodation they yield is very great; as the woulfes may be agitated or moved in any direction, and may be placed on the most uneven surfaces. These tubes are fitted into corks previously impregnated with wax or tallow; and when they are to be made use of, I first put a ring of elastic-gum (a neck of a gum bottle) over each neck of the woulfe, but so as to stand a little higher than the glass, forming a kind of wall round the cork and tube, to receive melted wax, or lute, to secure them in their places.

Description of the improved Apparatus.

Fig. 1. (Plate VII.) A, the retort joined to a tubulated receiver B. C, the adaptor ground into the neck of the tubulated receiver, and furnished with a glass valve made in the same manner as those used in the improved Nooth's apparatus. D, woulfes joined to the adaptor to receive the unabsorbed gas: and E, a bent tube to carry the gas that may still pass unabsorbed either into a pneumatic apparatus or into a chimney.

Fig. 2. the valve on a larger scale, and inserted in the neck of the adaptor. It consists of an internal tube of small calibre but pretty stout in substance, and ground into an exterior tube closed at the upper end, but perforated with several small holes to allow the gas to pass. After the internal tube is ground to fit the external, a portion of it is cut out (as at *a*)

* If the tubes are not very thick, the heat from a flare lamp of tallow and cotton-wick is sufficient for the purpose. When the tubes are thick I prefer a charcoal fire made in a black lead crucible, across which the tubes may be easily heated to the required degree. Whichever of these methods be followed, the heat should not be applied too suddenly; nor should the tubes be blown upon, or exposed to a low temperature, to cool them hastily, as by such treatment they are apt to fly.

sufficient

ſufficient to receive a ſmall hemiſphere of glaſs, and to allow the hemiſphere to riſe a little in its ſmall chamber, but not to turn over in it. The upper piece of the internal tube is then thruſt home to the place in which it is to remain, and the glaſs hemiſphere introduced, with its plane incumbent on the upper end of the lower piece of the internal tube, which is ground perfectly flat, as is alſo the plane of the hemiſphere. From the conſtruction, it is obvious that, by the upward preſſure of any gas, the glaſs hemiſphere may be raiſed ſo as to allow the aëriform fluid to paſs into the adaptor, but that there can be no return of any thing into the receiver even when a partial vacuum takes place in it; and the greater the exceſs of the expanſive force or preſſure exerted at any time in the adaptor over that maintained in the retort and receiver, the cloſer does the valve become.

Fig. 3. two adaptors, A and B, ground to fit into each other, and alſo to fit into C of fig. 1. Any number may be fitted to each other in the ſame manner. By this apparatus liquids may be highly impregnated with the gaſeous products evolved during diſtillations. The bent tube T is for the purpoſe of conveying the ſuperabundant gas to a chimney or to a pneumatic apparatus.

Fig. 4. improvement in the mode of fitting the glaſs tubes in a Woulfe's apparatus. This has already been ſufficiently explained.

XLIII. *Life of* JOSEPH TOALDO, *Profeſſor of Aſtronomy and Meteorology at Padua.*

JOSEPH TOALDO, whoſe merit as a philoſopher and meteorologiſt is well known in all the civilized countries of Europe, was born on the 11th of July 1719 at San Lorenzo di Pianezza, a ſmall village near Maroſtica, at the foot of the Alps, in the valleys of Vicenza. The firſt part of his education he received from various eccleſiaſtics, who gave him an early taſte for the ſciences, and accuſtomed him to diligence and application. In the year 1733 he was ſent to the ſeminary of Padua, where he ſtudied Latin, rhetoric, philoſophy, and theology, but in particular the mathematics. Here he took the degree of doctor of theology, and was deſtined for the place of a teacher in the ſame inſtitution in which he had been inſtructed. About this period he began to diſtinguiſh himſelf as a writer. The firſt literary labour he engaged in was a new edition of the works of Galileo, to which he added various fragments never before pub-

 liſhed

liſhed, together with annotations and a preface. This edition contained alſo the celebrated dialogues reſpecting the ſyſtem of the world. As one of the maſters of the above ſeminary, he taught grammar, rhetoric, philoſophy, and mathematics; and here he firſt introduced the infiniteſimal calculus according to the principles of his preceptor Suzzi, one of the moſt celebrated analyſts of Italy. His ſervices to this eſtabliſhment were ſoon rewarded by the archbiſhop with the benefice of Montegalda, which he enjoyed for fourteen years. Though this place engaged a great deal of his attention, it allowed him ſufficient leiſure for continuing his mathematical ſtudies, which, on account of the different occupations of his academical office, he had not been able to purſue with that attention which he wiſhed. He however exchanged this benefice for another more convenient, after he had been invited by the ſenate of Venice, in the year 1762, to take upon him the vacant profeſſorſhip of aſtronomy and meteorology in the univerſity of Padua.

In this inſtitution he found it neceſſary to make a great many changes in order to render his inſtruction uſeful. The firſt thing to be done, according to the ſyſtem which he propoſed, was to erect an obſervatory. The curators of the univerſity complied with the requeſt which he made on this ſubject, and intruſted to him the care of drawing up the plan and ſuperintending the conſtruction of the building. On this account he undertook a tour through Italy for the purpoſe of inſpecting the principal obſervatories, that he might be better able to deviſe a plan for that of Padua. The foundation of this obſervatory was laid in the year 1767, and in 1774 it was completed. He now procured an excellent quadrant from England, which he employed for making obſervations in conjunction with his nephew and aſſiſtant Chiminello. To ſupply the want of good elementary books he publiſhed a ſhort view of plane and ſpherical trigonometry, with tables, under the title of *Tavole Trigonometriche, con una Introduzione, que contiene un Compendio di Trigonometria piana e ſpherica, applicata alla Pratica, con molte altre Tavole*; Padua 1769. 4to.; which was afterwards reprinted and introduced into many of the Italian ſeminaries.

This work was ſoon followed by another on the influence of the heavenly bodies on the weather and atmoſphere; which greatly contributed to extend his fame as a philoſopher, and which contained the reſult of a long ſeries of meteorological obſervations made by him. The original title is, *Della vera Influenza degli Aſtri nelle Stagioni e Mutazioni di Tempo, Saggio meteorologico fondato ſopra lunghe Obſervazioni*; Padua

Padua 1770. 4to. This work, which made the author known throughout all Europe, was tranſlated into different languages, and procured for him admiſſion into various learned ſocieties. About the ſame time he publiſhed ſome eſſays in favour of electrical conductors, many of which, in conſequence of his recommendation, were erected in the Venetian territories: alſo a chronological view of uncommon changes in the weather, with tables of the ſtate of the barometer, and flux and reflux of the ſea. In the year 1773 he began his meteorological journal, which he continued till his death. In the year 1774 his celebrity was ſtill further increaſed by his anſwering a prize queſtion, propoſed by the Academical Society of Montpellier, on meteorology as applied to agriculture; which was crowned by the above ſociety, and printed in Rozier's *Journal de Phyſique.* It was tranſlated in almoſt every country of Europe, and, together with the author's preceding work, excited general attention to a ſtudy which before had been much neglected: this paper contributed alſo, in a conſiderable degree, to the eſtabliſhment of the Meteorological Society of Manheim. After this period the author continued with great zeal to exert himſelf in diffuſing meteorological knowledge, and in improving meteorological inſtruments; and in 1776 he publiſhed, in the Economical Journal of Venice, an extract from De Luc's Treatiſe on the Barometer and Thermometer, by which the conſtruction of theſe inſtruments was much facilitated. Next year (1777) he tranſlated Lalande's Aſtronomical Tables and his *Abregé de l'Aſtronomie,* as he did afterwards his *Aſtronomie des Dames*; and he erected a marble buſt of that eminent aſtronomer in the obſervatory. After this time his attention was excluſively directed to aſtronomy and meteorology, and he in particular endeavoured to confirm more and more his doctrine reſpecting the influence of the moon on the different changes of the weather. He publiſhed alſo an hiſtorical view of the ſervice rendered by the Venetian ſchools to aſtronomy, geography, and navigation. In the year 1783 he obtained, in conjunction with his nephew Chiminello, from the academy of Manheim, the prize for the beſt treatiſe on the conſtruction of a comparative hygrometer; and in 1784 he publiſhed a ſmall work on finding the longitude, *De Methodo Longitudinum, et Obſervatione Tranſitûs Lunæ per Meridianum*; which was much approved, particularly in England. From this period he wrote nothing except his journal till the year 1787, when he publiſhed a ſmall work in two ſheets, entitled *Confronto delle Ragioni coi principali Prodotti della Campagna,* which appeared at Venice. His Tables of Vitality, *Tavole di Vitalita*, a ſmall but laborious work, was printed next year at Padua and Venice.

In the year 1788 he undertook a third tour * to Rome, Naples, Trieſte, and Tuſcany; during which he examined the place where Hannibal croſſed the Alps, and wrote a paper on this ſubject, which was inſerted in the fourth volume of the Tranſactions of the Academy of Padua, of which he had been a member from the time of its eſtabliſhment. Next year he introduced the French clocks at Padua, and publiſhed a treatiſe on gnomonics, and reſpecting *ſchediaſmata aſtronomica*, the eclipſe of the ſun, and the paſſage of Mercury.

Beſides theſe works, the public were indebted to him for various ſhort eſſays, publiſhed ſometimes in his own journals and printed ſometimes ſeparately: ſuch as a ſhort treatiſe on chronology; an eſſay on the extraordinary winter, together with a chronological view of the weather in general; ſeveral reſearches reſpecting the continued drought of the winter of the year 1779; on fogs, and the influence of fiery meteors; a prognoſtication of the weather from the flight of birds; conſiderations on a new cycle, and the ſtate of the planets; general rules for foretelling the periods of rain and wind in the Adriatic gulph, from an obſervation of the heavens. Other papers by him are to be found in many of the public Journals, and in the Tranſactions of learned ſocieties. The Journal of Modena contains his defence of Leibnitz againſt De Luc, and obſervations on the falling of the mercury in the barometer during rainy weather; that of Piſa, a treatiſe on the influence of the moon, in anſwer to the objections of Friſi. Various meteorological and philoſophical papers written by him may be found likewiſe in the Venetian, Vicentine, and Milaneſe journals, in ſome of the French journals, and in the Tranſactions of the Society of Manheim. The Philoſophical Tranſactions, thoſe of the Inſtitute of Bologna, and of the Academy of Berlin, contain three of his papers, viz. *De Æſtu reciproco Maris Adriatici*; *De Calore Lunari*; and *De Vi Lunæ in Atmoſphæram, ex Obſervationibus barometricis*. Some of his aſtronomical obſervations were publiſhed in the Memoirs of the Academy of Sciences at Paris. The Tranſactions of the Academy of Padua, however, contain the greateſt number of papers written by this diligent philoſopher: ſuch as, a deſcription of an *aurora borealis*, with a chronological account of the appearance of this phæ-

* Beſides the tour above mentioned for inſpecting the Italian obſervatories, he had undertaken a ſecond about the year 1780, through Lombardy, Piedmont, and to the coaſt of Genoa.

nomenon

nomenon since the time of the Romans; a determination of the longitude and latitude of the observatory of Padua; a treatise on the physical properties of the atmosphere in different places, from thermometrical and barometrical observations, and from the phænomena of vegetables; observations on thunder; researches respecting the heat in several parts of Italy, from the latitude of 41 to 47°; description of a beautiful fire-ball seen on the 11th of September 1784 at Padua; and a great many astronomical observations made in conjunction with Chiminello. He left behind him several works in manuscript, among which were, observations on the travels of Marco Polo, and on the real epoch of the celebrated Chinese wall.

Amidst such occupations Toaldo spent his days in tranquillity, notwithstanding the unhappy ferment which prevailed in his native country; and might perhaps have been still alive had not his end been hastened by chagrin. A fruitless attempt made in favour of a young man who had been deprived of his office, gave him so much uneasiness, that he first lost his natural vivacity, and soon after felt himself indisposed; on the 7th of November 1797 he was attacked by a nervous affection, and died on the 11th, in the 79th year of his age.

Toaldo was of small stature, had a friendly aspect, and in general an appearance that inspired confidence. His conduct was amiable, his deportment easy, and his conversation, which was animated, displayed great knowledge and an extensive acquaintance with various branches of science: simple in his manners, open and sincere, he abandoned himself only to the softer passions, and seemed to have no other ambition than that of being useful. He was steady in his friendship, always ready to do good offices in the most disinterested manner, and indulgent towards every one. To the talents of a literary man he added the virtues of the citizen, and therefore was universally respected, especially by those who enjoyed his intimate acquaintance.

XLIV. *Account of the Discovery of Coal at the Cape of Good Hope**.

IN most families at Cape Town a slave is kept expressly for collecting fire-wood. He goes out in the morning, ascends the steep mountains of the peninsula, where waggons cannot approach, and returns at night with two small bundles of

* From *Barrow's Travels*.

faggots (the produce of six or eight hours hard labour) swinging at the two ends of a bamboo carried across the shoulder.

Some families have two and even three slaves, whose sole employment consists in climbing the mountains in search of fuel. The expense of a few faggots, whether thus collected or purchased by the load, for preparing victuals only, as the kitchen alone has any fire-place, amounts, in a moderate family, to forty or fifty pounds a year.

The addition to the inhabitants of five thousand troops, and a large fleet stationed at the Cape, increased the demand for fuel to such a degree, that serious apprehensions were entertained of some deficiency shortly happening in the supply of this necessary article. Under this idea, the attention of the English was directed towards finding out a substitute for wood. The appearance of all the mountains in southern Africa being particularly favourable to the supposition that fossil coal might be found in the bowels of most of those inferior hills connected with and interposed between them and the sea, his excellency the earl of Macartney, well knowing how valuable an acquisition such a discovery would prove to the colony, directed a search to be made.

Boring-rods were prepared, and men from the regiments who had laboured in the collieries of England were selected to make the experiment. Wynberg, a tongue of land projecting from the Table Mountain, was the spot fixed on, and the rods were put down there through hard clay, pipe clay, iron-stone, and sand-stone, in successive strata, to the depth of twenty-three feet. The operation of boring was then discontinued, by a discovery then made of actual coal coming out, as miners express it, along the banks of a deep rivulet flowing out of the Tygerberg, a hill that terminates the isthmus to the eastward. The stratum of coaly matter appeared to lie nearly horizontal. Immediately above it was pipe clay and white sand-stone, and it rested on a bed of indurated clay. It ran from ten inches to two feet in thickness; different in its nature in different parts; in some places were dug out large ligneous blocks, in which the traces of the bark, knots, and grain, were distinctly visible; and in the very middle of these were imbedded pieces of iron pyrites, running through them in crooked veins or lying in irregular lumps. Other parts of the stratum consisted of laminated coal of the nature of turf, such as by naturalists would be called *lithanthrax*, and pieces occurred that seemed to differ in nothing from that species known in England by the name of Bovey coal. The ligneous part burned with a clear flame without much smell, and left a residuum of light white ashes

like

like thofe of dried wood. The more compact earthy and ftoney parts burned lefs clear, gave out a fulphureous fmell, and left behind a flaty caulk, that foon contracted on the furface a deep brown ochraceous cruft. The borer being put down in feveral places in hopes of meeting with the main bed of coal, the general refult was as follows:

In the bed of the rivulet:	Feet.
Coal - - - - -	2
Blue foapy rock - - - -	5
White foapy rock - - - -	22
Gray fand-ftone with clay - - -	21
Sand-ftone of chocolate brown - -	14
Blueifh foapy clay - - -	31
Striated fand, red and white, containing clay -	33
	128

Here the operation was difcontinued.

XLV. *Notices respecting New Books.*

BABYLONIAN INSCRIPTIONS.

THOUGH a great many travellers fpeak of the ruins of the antient Babylon, and defcribe the bricks of which thefe ruins are compofed, very few of them have noticed the fingular infcriptions or characters impreffed on them. Pietro della Valle, a moft circumftantial and accurate traveller, even carried fome Babylonian bricks to Italy, one of which he prefented to Kircher, then refiding at Rome, to be preferved in his mufæum, ftill extant in that city; but neither he himfelf, nor that learned jefuit, who was fo much engaged in refearches refpecting Egyptian, Chinefe, and other kinds of oriental literature, ever made mention of Babylonian infcriptions.

The firft by whom they were noticed feems to have been father Emanuel, a Carmelite friar, who, having refided fome time at Bagdad, in his manufcript account, fpeaks of characters impreffed on the antient bricks ftill remaining among the ruins of Babylon, which, d'Anville fays, would fupply the literati, who are defirous of penetrating into the remoteft antiquity, with entirely new matter for meditation and ftudy.

After him Niebuhr, in his Travels to Arabia, mentions infcriptions on the Babylonian bricks ftill extant, but without telling whether they contained characters unknown, or fimilar to any already difcovered; only, when fpeaking of the bricks on which they are inscribed, he says, that he saw inscriptions of the same kind on other bricks at Bagdad and in Persia.

But the most circumstantial, and the most recent notice respecting them, is that of M. Beauchamp, correspondent of the Royal Academy of Sciences at Paris, who, by residing several years at Bagdad, had more leisure to examine and describe the ruins of Babylon; which he did in his account inserted in the *Journal des Savans* for 1790.

In this account, the author, speaking of the remains of Babylon, says,—" On the side of the river are those immense ruins which have served, and still serve, for the building of Helle, an Arabian city containing ten or twelve thousand inhabitants. Here are found those large and thick bricks, imprinted with unknown characters, specimens of which I have presented to the abbé Barthelemy." What kind of characters, however, these were, neither Beauchamp nor that celebrated antiquary, who wrote with so much ability on the Phœnician, Palmyrenian, and other inscriptions, thought proper to determine; and it is only by his successor, M. Millin, that we have lately been made acquainted with the existence of some Babylonian bricks at Paris, containing inscriptions, which were sent by Beauchamp from Babylon, and of which drawings or copies were transmitted to M. Herder, at Weimar, and to professor Munter, at Copenhagen.

In the mean time, the Honourable East India Company, being always desirous to lend their assistance to those who may be employed in the elucidation of oriental antiquities, and being informed that, near the town of Hillah, on the river Euphrates, there exist the remains of a very large and magnificent city, supposed to be Babylon; and that the bricks of which those ruins are composed, are remarkable for containing, on an indented scroll or label, apparently a distich, in characters totally different from any now made use of in the East, directed the governor of Bombay to give orders to their resident at Bassorah, to procure from thence ten or a dozen of the bricks, and to transmit them, carefully packed up, as early as possible to Bombay, that they might be thence forwarded to them in one of their ships sailing for England.

In consequence we were gratified, at the commencement of the present year and century, at London, with the first view of inscriptions, which, on comparing them with the Persepolitan characters as given by Le Bruyn, Chardin, Niebuhr, and other travellers, appeared to be of the same origin, being only more complex, and connected by long lines, forming whole and half squares, stars, triangles, &c.; so so that they prove to be a different combination, though formed of nearly the same elements and nail-headed strokes.

It is well known that, for more than a century past, about which time the Persepolitan inscriptions were first discovered by European travellers, opinions have been much divided respecting these characters. Some have believed them to be talismans, and others the characters of the Guebres, or antient inhabitants of Persia; others held them for mere hieroglyphics, and others for alphabetic characters, like ours. Kæmpfer supposed them to express whole ideas, like the Chinese characters, but that they had been appropriated solely for the palace of Istakhar.

After that period, however, some of a similar kind were found also in Egypt; but as neither the Egyptian hieroglyphics, nor the characters observed on the mummies, had the least resemblance to them, they served only to prove the connexion, which we know from history that Persepolis once had with Egypt. Raspe, on finding some others on a cylinder of loadstone, persuaded himself that they were the same with the Chinese characters; and, consequently, that the Chinese writing had been formerly known and cultivated on this side of the Ganges.

The above account of the discovery of these inscriptions is taken from Dr. Hager's preface to his late work entitled *Dissertation on the newly discovered Babylonian Inscriptions**, in which the learned author endeavours to prove that the Persepolitan characters were derived from the Babylonian.

"By the Babylonian bricks here exhibited," says the learned author, "the whole difficulty in regard to their origin is removed; as it is evident that Babylon, in point of cultivation, was much earlier than Persepolis, and that the Chaldeans were a celebrated people when the name of the Persians was scarcely known.

"To confirm this opinion, and by it to prove that the Persepolitan characters were derived from the Babylonian, I have thought it necessary to begin this work by a brief examination of the antiquity, extent, and sciences of the Babylonians; and through scantiness of original monuments, to prove, by astronomy, architecture, and languages, their well-founded claim to antiquity. At the same time I have endeavoured to show that not only the Persians, but also the Indians, were disciples of the Chaldeans; and that the Egyptians themselves, who pretend to be the instructors of all nations, probably derived their pyramids and obelisks from Babylon. Proceeding then to the Babylonian inscriptions, I have

* To be had at Richardsons, and Cuthell and Martin's.

have shown their similarity to that celebrated alphabet which the Indians call divine or celestial (*deva-nagari*), because they believe that it was communicated by the Deity himself in a voice from heaven; and I have tried to prove that they were not derived from heaven, but from our earth, and from the borders of the Euphrates. I have confirmed my assertion by means of the Tibetan characters, those acknowledged descendants of the Indian ones, and thus endeavoured to invalidate the opinion of that great antiquity and boasted originality of the Bramins."

In a future Number we shall give a further extract from this curious work: in the mean time we present our readers with a specimen of the inscriptions. (See Plate VIII.)

MEDICINE.

Professor Schumacher, of Copenhagen, has published the first volume of his Medico-chirurgical Observations. As the author attends an hospital, he had occasion to make curious remarks worthy of confidence. His zeal induced him to substitute in his practice the use of indigenous for that of foreign plants. He has employed the *gratiola* instead of ipecacuanha. In such cases when this plant does not produce its effect, he recommends assisting it with the addition of a little rhubarb. He has discovered that, in wounds and ulcers attended with inflammation, the *cortex hippocastani* or *cortex salicis* produces the same effect as *cortex peruvianus*, which is far dearer. The *carex arenaria*, also, may be substituted for sarsaparilla. It results from the author's experiments, that the *fabæ pichurim* are an efficacious remedy for the whites; especially when the disease is inveterate, and arises from weakness.

Dr. Tode, of Copenhagen, continues also, with success, his Medico-chirurgical Dictionary. In the first number of the fifth volume the author informs the faculty that a fœtus of 199 days is susceptible of being brought into the world alive, but without all the symptoms of perfect conformation, and without being in a state to prolong its existence.

MEDICAL JURISPRUDENCE.

Citizen Bellock, a French physician, has lately published a work entitled *Legal and Judicial Medicine*. The object of this work is to inquire how far medical knowledge may be usefully employed in legal investigations. The author has treated the subject with much ability, and with all that cautious *pyrrhonism* so necessary in an inquiry of such importance. Among the points he discusses we shall notice the following:

following:—1. Are there criteria by which we may discover whether a person has hanged himself, or been hanged by others?—2. Can it be ascertained whether a human body found dead in water has been drowned, or whether the person had been thrown in after death?

He does not omit to notice the two famous trials of the last century, in which much light was thrown on these two questions. The one related to the unfortunate Calas condemned at Toulouse, and solemnly justified at Paris: the other to young Sirvan, found drowned in a well a league from his father's house.

BOTANY.

Dr. Schrader, one of the most celebrated German botanists, publishes at Göttingen a botanical journal containing an account of the most important discoveries in regard to the science of vegetables. This journal, written in Latin and in German, appears every three months. Each number or volume consists of about 450 pages duodecimo, illustrated with three plates, each of which exhibits several figures. The author divides each number into four parts. The first is destined for the papers and memoirs addressed to him; in the second he gives an extract from new works; the third contains the most important discoveries and observations made in the science of botany; and the fourth, every thing most interesting in the correspondence which the author maintains with the literati in Europe.

Though every part of botany is cultivated with the most brilliant success in the north of Europe, the study of the cryptogamia class of plants, that is to say, those the sexual organs of which are difficult to be discovered, seems to engage, in a particular manner, the botanists of those countries. To them we are much indebted for the light which has been thrown on this department of botany. The labours of Hedwig, Hoffman, Schrader, Humboldt, Persoon, Flügge, Stromeyer, &c. have dissipated the obscurity with which the nature and origin of mushrooms were surrounded. This journal, therefore, of which six numbers have already appeared, as it contains all the new discoveries and observations in regard to botany, cannot fail to be of utility to those who are fond of this science.

A very valuable work on this subject is now publishing in Paris. It is an account of new and rare plants collected by C. Cells, with fine plates. The history of these plants is drawn up with great ability by the celebrated botanist Ventenat,

tenat, of the National Inftitute. Four numbers of this work have already appeared :- the fifth is about to be publifhed.

MINERALOGY.

A very valuable treatife on this fubject has juft been publifhed by C. Haüy, member of the National Inftitute, and keeper of the mineralogical collections of the School of Mines; 4 vols. 8vo., and a quarto volume of plates. A fine edition of the fame work, confifting of the fame number of volumes, in 4to., was publifhed at the fame time.

XLVI. *Proceedings of Learned and Economical Societies.*

ROYAL SOCIETY OF LONDON.

AT the anniverfary meeting on the 30th of November the amount of receipts and expenditures was declared, and the names of the new and the deceafed members were read over by the fecretary.

The prefident, the Right Hon. Sir Jofeph Banks, K. B. in the name of the Society, prefented Sir Godfrey Copley's gold medal to Mr. Aftley Cooper for his paper on the effects which take place from the deftruction of the membrana tympani, and on an operation for the removal of a particular fpecies of deafnefs.

Afterwards the Society proceeded to the choice of the council and officers for the enfuing year.

The members, as ufual, on that day dined together at the Crown and Anchor tavern, in the Strand.

Dec. 10. was read a paper by Dr. Mafkelyne, aftronomer royal, drawn up from the notices publifhed by Piazzi, of his difcovery of a new planet rather larger than our earth, and revolving in an orbit between Jupiter and Mars; to which he propofes giving the name *Ceres Ferdinandia*. All doubts refpecting this difcovery will be refolved at its next appearance in March 1802.

On the 17th was read a paper, by Everard Home, Efq. giving an account of one of the moft extraordinary fubjects of comparative anatomy that has ever appeared—a defcription of the *Platypus anatinus*, an amphibious animal, of which a male and a female were brought from New South Wales. They are there found on the banks of one of the lakes, in which they fwim not on the furface, but occafionally come up to refpire. It has four fhort legs furnifhed with claws,

claws, and the mouth is terminated by a bill like a duck. The parts of generation are most uncommon, and, from the female not having a uterus, Mr. Home is led to suspect it is oviparous.

THE JABLONOWSKY SOCIETY OF THE SCIENCES AT LEIPSIC.

This Society has proposed for 1802 the following

Prize Questions:

History.—An account of the rights and condition of the dignity of earl in the northern kingdoms.

Mathematics.—An accurate view of the progress and improvement of the mechanical sciences during the eighteenth century.

Economy.—On the influence of the atmosphere on the fertility of the soil, according to the latest experiments and researches. How far may the nature, situation, and culture of the soil contribute to render this influence active and powerful?

The facts adduced by the candidates in their papers must be confirmed by the testimony of respectable authors; but the papers themselves, according to a condition established by the founder, must be written in Latin or in French. The prizes consist of gold medallions of the value of 24 ducats each.

The society invites all the friends and promoters of science to answer the above questions. The answers must be transmitted, with a sealed note containing the name and residence of the author, to Charles Frederick Hindenburg, professor of philosophy at Leipsic. The period for receiving the answers has not yet been announced.

ACADEMY OF THE USEFUL SCIENCES AT ERFURT.

In the sitting of May 2, M. Resch laid before the society several specimens of leather tanned, according to his process, by means of one-fourth bark and three-fourths turf (black peat-moss), which were found exceedingly good: on account, however, of some doubts excited in regard to the activity of turf, he means to cause his experiments to be repeated.

M. Homeyer, of Zimmer near Alfeld, transmitted to the society some twigs grown together, in order to show how mankind may have fallen upon the invention of grafting.

Two papers were read, one on the *asplenium* and some kinds of fern related to it, by M. Bernhardi, of Erfurt; and experiments on the preparation of cinnabar in the wet way, by M. Bucholz, of Erfurt.

In

In the sitting of June 2, professor Frank read a paper on the question lately proposed by Messrs. Anton and Kreschman—whether the origin of Bards and the Druids can be justly ascribed to the old Germans? The author is of the opinion of those who answer this question in the negative.

FRENCH NATIONAL INSTITUTE.

Account of the labours of the French National Institute during the last three months of the year 9.

[Continued from p. 188.]

Observations made during three Years on the metallic Chains suspended from the Dome of the Pantheon to ascertain any horizontal or vertical Change that might take place in the Building. By C. Prony.

The late minister of the interior, Benezech, having appointed a commission to resume the labours of those before established for examining the Pantheon, and pointing out the means that might be necessary for its preservation, C. Prony, member of this commission, read an interesting memoir on this subject, which was heard with great attention. Five metallic chains, composed of links each 3·937 inches in length, are suspended below the cupola of the Pantheon. Four of these chains are parallel to the vertical surfaces of the planes of the pillars; the fifth is attached to the centre of the exterior dome, and hangs down near to the ground. The centres of five horizontal circular plates are made to correspond very exactly to the points where the weights with which each chain is loaded terminate. These plates are raised on vertical graduated stalks, which rise and fall freely in sockets, and which can be fixed to any height by means of screws. Concentric circles are traced out on each plate, and meteorological instruments placed in different parts of the edifice indicate the differences of temperature.

Chains suspended in this manner can sink down only in consequence of three causes. 1st, The dilatation of the metal, which would augment their volume and the length of their mass: 2d, Elongation, occasioned by an extension which a certain weight might give to that length where the system is interrupted by links: 3. The partial or total sinking of the edifice.

The effects which changes of temperature might produce on a metallic system, interrupted by links, were examined with as much accuracy as it is possible to attain to in researches of this nature, with an instrument invented by that eminent artist Ramsden. Lavoisier and Laplace repeated theſe experiments at the houſe of C. Prony, and ſhowed that plates of different metals are lengthened by a given and conſtant quantity, from the temperature of ice to that of boiling water *.

The ſecond cauſe of elongation is eſtimated by a method deſcribed with great accuracy in Prony's memoir.

The third was that of the greateſt importance to be known to the commiſſion; for ſome architects entertained fears that the edifice was ſinking. This movement, according to the hypotheſes of ſome, by cauſing the pillars, which are ſaid to be too weak to ſuſtain the maſs, to give way, would have cruſhed them, and the dome would have ſunk vertically on itſelf.

The poſſibility of ſuch a diſaſtrous effect ſuggeſted to the commiſſion a deſire to aſcertain how far this was probable. Prony has detailed, in his memoir, a great number of precautions which he took to eſtimate this danger. After a long and accurate inveſtigation, he was convinced that no alteration has taken place in the ſurfaces of the walls of the foundation, and particularly in the vertical direction of the pillars; that the parallelifm of the circular cornices of the cupola is ſtill preſerved; that the three domes are in no manner affected; and that an examination of the pillar which has ſunk moſt, proves it to be ſound. The bliſtering or ſcaling off of the compoſition which ſurrounds the pillars was occaſioned therefore only by the ſinking of the latter, and the ſinking aroſe from an error in the conſtruction. But can this ſinking make any progreſs? The attention of the commiſſion will no doubt be directed to this object; and the inſtrument invented by C. Prony is calculated to aſcertain it.

New Galvanic Experiments.

It appears that the identity of galvaniſm and electricity is perfectly eſtabliſhed by recent facts. C. Biot has found that the galvanic fluid in the different kinds of apparatus which produce it, follows exactly the ſame progreſs as electricity would under the like circumſtances.

Attraction and repulſion, indeed, being the ſame in galvaniſm as in electricity, the action which the moleculæ exerciſe upon each other is the ſame in the two fluids; the property which points poſſeſs of tranſmitting electricity, and which ſurfaces have to retain it, ought alſo to take place in galvaniſm; and, as the ſmall plates may be conſidered as more analogous to points than the large ones, they ought to

* Theſe elongations are eſtimated at an 864th for ſteel, and a 900th for caſt iron: the dilatation of forged and hammered iron differs very little from that of ſteel.

give

give in the ſame inſtant a leſs maſs of fluid, but poſſeſſing a great velocity, while the large plates will give a greater maſs with a leſs velocity. Shocks which depend on the velocity of the fluid will not be increaſed with the ſize of the plates; while metallic combuſtions which depend on its maſs and its continued preſence, ought with this ſize to be doubly augmented. This is a ſimple explanation of the experiments of C. Hachette, Fourcroy, Vauquelin, and Thenard.

C. Biot has conſidered galvaniſm alſo in regard to its chemical nature; and finding that what takes place in water brought into communication with the pile has been ſufficiently examined, he was deſirous of ſeeing what takes place in the pile itſelf, and what its action is on the ſurrounding bodies.

As it is well known that the zinc of the pile ſoon becomes oxidated when it acts, it was neceſſary to diſcover whence the oxygen aroſe. For this purpoſe C. Biot, in company with C. F. Cuvier, made experiments, which proved that the atmoſpheric air is decompoſed, and gives up its oxygen to the pile; that this oxygen increaſes the galvanic effects, but that it is not abſolutely neceſſary to them; that theſe effects take place even *in vacuo*, provided the pile is well moiſtened with water; which proves that the water is decompoſed.

Guyton found that the abſorption of the oxygen of the air by the pile is ſo complete, that it may be employed as an eudiometric mean to meaſure the quantity of oxygen contained in any given kind of air.

Biot next employed this oxidation of the pile as a means of confirming his theory reſpecting the progreſs of the fluid. In the caſe when the fluid poſſeſſes a greater velocity the oxidation ought to be accelerated; ſmall plates therefore become ſooner oxidated than large ones; a pile the two ends of which communicate by wires is alſo oxidated much ſooner, as might be expected, than that where there is no ſuch communication.

The ſame author has diſcovered that the galvanic fluid is tranſmitted with difficulty through water, but that it eaſily glides over its ſurface; an effect produced alſo, according to Prieſtley's experiments, by weak electricity poſſeſſing great velocity.

C. Ganthenet, who is not a member of the Inſtitute, has communicated ſome intereſting experiments on the ſame ſubject, in which he ſhows, in particular, that it is poſſible to make effective galvanic piles without any metallic ſubſtance whatever. Theſe experiments ſerve to confirm thoſe before made by Pfaff and Humboldt when they tried galvaniſm only on animals.

While

While these researches were making in France, the English made some of a peculiar and decisive kind. They have been able to imitate the most singular effects of galvanism by common electricity, by making the conductors much thinner and longer.

CHEMICAL ARTS.

New Method of bleaching Linen.

This method, invented by Seguin, consists in employing one part of sulphuric acid in five hundred parts of water.

New Method of bleaching the Pulp of which Paper is made.

The art of bleaching raw cloth almost immediately, by means of the oxygenated muriatic acid, is the invention of Berthollet. Chaptal made a happy application of it to restore the whiteness of books and prints that have been stained; but it remained to employ it for previously bleaching the pulp of which paper is made. This was attempted by Loysel some years ago, in order to render it difficult to imitate assignats, on account of the perfection of their manufacture. This paper money, in a political view, may be considered several ways; but as an object of art, it is certain that it has occasioned a multitude of ingenious inventions, which will long remain useful after assignats shall have been forgotten. The different processes are now gradually made public, since they are no longer secrets of state. The public are already in full possession of the stereotype art; and that of the fabrication of paper, no less interesting, is now laid open also. It is attended with the double advantage of beauty and œconomy.

By means of this method, all rags may produce a pulp exceedingly white; but they are not all proper for making paper strong, and difficult to be broken, though thin,—a quality essential to bank paper, and others of the same kind. New white or raw rags, cordage, and other articles, the hemp and flax of which have not been much worn, ought to be chosen for this purpose. Old rags do well enough for thick paper, or that which does not require much tenacity.

When the rags have been picked and cut into small morsels, they are put into vats, where they undergo a fermentation, which changes the nature of their colouring matter, by rendering it, as it were, saponaceous, and susceptible of being carried off by water when washed, and afterwards beat. The heat, during the fermentation, must not be suffered to exceed 100° or 112° of Fahrenheit's thermometer, which generally takes place in fifteen days.

 The

The moment of subjecting the rags to the action of the acid is not a matter of indifference. If they are bleached entire, as the interior of each thread does not experience any action, this would afterwards alter the whiteness of the paste; and if the workman should wait till they were absolutely reduced to pulp, the pulp would form a body with which it would be difficult to mix the acid intimately. The matter then must be taken at a mean state between that of rags and that of pulp completely triturated. For this purpose the rags are made to pass under a first cylinder to separate the fibres; they are then subjected to bleaching, and then converted into pulp by means of another cylinder.

If the rags are raw, two baths of oxygenated muriatic acid or bleaching liquor, and one of sulphuric acid, are employed. If they are white, one bath of the liquor and one of sulphuric acid will be sufficient.

The muriatic acid is prepared either with common muriatic acid and oxide of manganese, or by means of sulphuric acid with oxide of manganese and muriate of soda (common salt). The last method is the most economical; 25 parts of oxide of manganese, 70 of common salt, and 120 of sulphuric acid, of 31 degrees of density (by the areometer), will be sufficient to charge a receiver of 270 gallons. The strength of this liquor is appreciated by means of a solution of indigo, which is prepared by dissolving one part in weight of indigo in seven parts of sulphuric acid concentrated to 66 degrees, and which is then diluted in 992 parts of water, according to the method of Descrozilles. When one measure (in volume) of the bleaching liquor annihilates the colour of nine of this solution of indigo, the bath proper for bleaching 110 pounds avoirdupoise of rags, after being subjected to the first cylinder, is composed of 24 gallons of such bleaching liquor diluted in 120 gallons of pure water.

The expense of these different operations will be moderate, when one bath of bleaching liquor and one of sulphuric acid are employed, which is generally the case, and the paper is exceedingly beautiful. There is no doubt therefore that the price of paper will be much diminished, when the method here explained is employed and brought into general practice.

On the Fixity which Antimony acquires by its Alloy with Tin.

When attempts were made to restore to commerce that prodigious quantity of metallic matters which had been employed in making bells, it was found that the first operation necessary was to separate the copper and tin, which formed their principal constituent parts. The copper was easily obtained by calcining a part of the bell metal, and then throwing it upon another part of the same metal in a state of fusion; but the scoriæ produced, during this reduction, were lost.

More intelligent chemists, and particularly Anfry, found means to reduce the scoriæ, and to restore to commerce a large quantity of tin, and a still larger of copper. The tin, however, obtained in the last result has peculiar qualities; its fracture is different from that of common tin, it is harder, and more easily broken.

C. Sage has found means to imitate this tin, extracted from bell-metal, by mixing with tin a little antimony; and it results from his experiments, that the latter metal alloyed with tin, in the proportion of a sixth part, unites with it in so intimate a manner that it is almost impossible to separate it. C. Sage has no doubt then that the tin of bell-metal contains a certain quantity of it; but C. Sage has ascertained, by several experiments, that it does not prevent it from being employed in making tin plate, and even for tinning vessels.

MINERALOGY.

On the presumed Effects of Galvanism in the Mineral Kingdom.

C. Guyton has given a description of a kind of ore of antimony lately discovered in the province of Gallicia, specimens of which had been transmitted to him by Don Antonio Angulo, inspector general of the mines of Andalusia. He soon found, by analysis, that the metal in this ore is in the state of oxide; but the structure of the mineral, its interior texture, its well characterized striæ, plainly announced that it was the passage from sulphuret to oxide, without an alteration of form. It was therefore of importance to discover how this change could have been effected.

C. Guyton tried all the simple or compound substances which might be presumed to exist in the bowels of the earth, in order to imitate, in the laboratory, this labour of nature. The sulphuret of antimony, both natural and artificial, resisted all his numerous attempts. He supposed, very justly, that the decomposition of water ought to act a principal part in this operation, and even that it presented the only means of accounting for this alteration, since it ought at the same time to furnish oxygen, and to take away the sulphur by means of the hydrogen; but it was necessary to discover what could have determined this decomposition. C. Guyton considers it as the slow and progressive result of affinities put

 into

into action by the galvanic fluid. A comparison of what takes place in experiments where metals are spontaneously oxidated, by the interposition of water, appears to him to give great probability to this explanation, which he strengthens by several examples of similar transformations, particularly that of the pyrites of Berczof, which passes to the state of oxide, retaining, in three directions, the striæ of the primitive sulphuret. He quotes also numerous observations, by which C. Haüy has proved the influence of subterranean electricity on minerals. He entertains no doubt that this new idea will extend the field of mineralogy, by exposing to us nature in continual labour, acting at the same time on masses and on intimate moleculæ, by spontaneous attractions, and independent of all percussion; so that this subtle fluid, according to Guyton, will assume the first rank among those substances which have been dignified with the pompous name of *mineralizers*.

Analysis of a phosphated Lead Ore.

This ore, of a yellowish green colour, generally in small brilliant grains, and in a quartzy arenaceous reddish matrix, is found in pretty thick continued beds at Breitemberg, near Erlembach, in the department of the Lower Rhine. C. Sage has found, by an analysis, that it contains, in 100 parts, 37 of lead, 33 of quartz, and 30 of phosphoric acid. The lead contains no silver.

Emeralds found in France.

This discovery was made by Lelievre. Travelling near Limoges, he observed, in the stones employed for repairing the roads, a hard greenish substance, which he judged to be a beryl or an emerald. His conjecture was confirmed by the chemical analysis which Vauquelin made of specimens sent by him to that chemist, and by Haüy's examination of their mechanical structure. These emeralds will not be of much use as ornaments; but they may be employed by chemists to extract from them that kind of earth called *glucine*, which Vauquelin discovered in the emerald and the beryl, and which these emeralds of Limoges readily furnish, as C. Lelievre found several pounds of them, and was even able, by combining the fragments, to recompose a prism of more than four inches in diameter, and nearly the same in height.

METEOROLOGY.

On the Action which the Lunar Period of Nineteen Years exercises on the Variations of the Atmosphere.

As a decided influence on the variations of the atmosphere has

has always been afcribed to the moon; feveral methods have been tried to determine the nature of this influence. Some have thought that it was exercifed by means of the phafes; others have fuppofed that it depended on the refpective pofition of the moon and the fun; and as this pofition recurs nearly the fame at the end of nineteen years, the latter have concluded that meteors muft alfo return almoft the fame in the courfe of that time. C. Lamarck, who has fuppofed another influence of the moon in regard to its declination, and who has endeavoured to eftablifh his difcovery by feveral memoirs, has alfo endeavoured to afcertain what truth there may be in this periodical return of the variations of the atmofphere at the end of nineteen years; and he has found, by comparing meteorological obfervations, that this return is far from being fo correct as is generally believed. Aftronomers alfo know well, that the cycle of nineteen years is not exact within an hour and a half; an error which amounts to a whole day in the courfe of 308 years.

ZOOLOGY.

On the Difference which exifts between the Crocodiles of the old and the new Continent.

A precife determination of the large fpecies of animals is more important than might be imagined, as we have inftances that, for want of fuch determinations, travellers have placed in America feveral animals of the old continent which never exifted there, fuch as the lion, tiger, panther, and fome others. Thefe they confounded with other animals of a different kind, though nearly related to them. The fame error has occurred in regard to crocodiles, even on the part of naturalifts. In order to rectify fuch miftakes, C. Cuvier has fhown, that moft authors have been entirely unacquainted with the difference which exifts between the crocodiles of the old and the new world, or have badly underftood them, and explained them ftill worfe.

He has proved, by an accurate defcription, that thefe animals form two fpecies, the diftinguifhing characters of which are as follow:

1ft. The *crocodile* has an oblong muzzle, the upper jaw of which has indentations on each fide to afford a paffage to the fourth tooth in the lower; its hind feet are entirely webbed.

2d. The *caiman* has an obtufe muzzle; its upper jaw receives the fourth tooth of the lower in a particular cavity which conceals it; the hind feet are only half webbed.

The firft of thefe fpecies belongs to the old world, the fecond to the new. The name of the latter originates from

India, where it denotes the common crocodile, and from which it muſt have been introduced into America by the Spaniards or the Dutch.

The author has not comprehended in his reſearches the *long beaked crocodile*, or the *gavial*, which forms, by general acknowledgment, a peculiar ſpecies.

On two new Kinds of oviparous Quadrupeds.

Theſe two ſpecies, deſcribed by Lacepede, are intereſting not only on account of their novelty; they are intereſting alſo as they exhibit a number of toes not before obſerved but in the claſs of reptiles.

The firſt, which Lacepede calls *monodactylos*, has indeed only one toe on each foot. Theſe feet are ſo ſhort, and the body and tail ſo long, that the animal has a great reſemblance to a ſnake; it is covered with ſcales diſpoſed in tranſverſal bands.

The other ſpecies, called *tetradactylos*, has feet as ſhort, and a body as long, as the preceding; but each foot is furniſhed with four toes, and the body is marked on each ſide with a longitudinal furrow.

Theſe two ſpecies will form hereafter two new genera in Lacepede's genera of *lizards*.

On a new Kind of Inſects called Atractoceros.

This inſect was brought from the kingdom Owara, in Africa, by Paliſot-Beauvois. The name given to it by that traveller ſignifies the *ſpindle horn*, and indeed ſuch is the form of its antennæ. It is diſtinguiſhed from other coleoptera by its wings being much longer than the cover, and not folding under them, and by having five articulations in all the tarſi. This laſt character brings it near to the *ſtaphilini*, while the preceding gives it an analogy to the *necydales*. The form of its antennulæ is very ſingular. C. Beauvois thinks that this inſect lives in the woods.

ANIMAL PHYSIOLOGY.

On the Quantity of Air neceſſary for the Reſpiration of a certain Number of Individuals in a Space where it is not renewed.

Experiments made with machines proper for navigating under water, made laſt year at Havre, and lately by the Engliſh engineer Folkeſtone, made Guyton conceive the idea of comparing, on that ſubject, the conſequences of the chemical theory of reſpiration, the reſults of obſervations made under the diving-bell, and the attempts made for the ſame

purpoſe

purpoſe by Drebell and Halley. After having calculated, hour by hour, the conſumption of the reſpirable part of the air, and the formation of carbonic acid gas, he indicates the chemical means for ameliorating what remains of the former, and of abſorbing the latter; and, in the laſt place, of preventing the danger ariſing from accumulated animal emanations, by the momentaneous opening a bottle of oxygenated muriatic acid, which appears to him very proper to be ſubſtituted in the place of the liquor mentioned by Boyle as having ſerved to cool the air in a veſſel rowed under water.

PHILOSOPHY OF VEGETABLES.

On the Influence which the Air and other aëriform Fluids have on Germination.

This new labour of Sennebier is a ſupplement to his large work on the phyſiology of vegetables. The experiments were made by placing the ſeeds on moiſt flannel, under bells filled with different kinds of gas, the influence of which it was intended to try. All theſe ſeeds refuſed to germinate in azote, in carbonic acid, and in pure hydrogen gas. The preſence of oxygen was in general neceſſary, and its employment is in part to abſorb the carbon of the ſeeds, to form of it carbonic acid. What is ſingular is, that pure oxygen is not the gas moſt favourable to germination; it accelerates germination, but it weakens it. The action of oxygen muſt be meliorated by the preſence of a ſubſtance inactive by itſelf; and we here find that wonderful fact preſented to us by ſo many other circumſtances of the organic economy, viz. that the mixture moſt favourable to germination is that of which the atmoſphere is formed, that is, about three-fourths of azote to a fourth of oxygen; and germination does not take place at all if there is not an eighth of oxygen at leaſt in the atmoſphere.

But the manner in which oxygen is introduced into this atmoſphere is not a matter of indifference; it muſt be poured into it ſuddenly. Were it made to enter only little by little, the firſt portions would ſcarcely be ſufficient to abſorb the carbon of the ſeeds, and no more would remain to vivify them.

Seeds may be made to germinate alſo, by mixing oxygen with carbonic acid, or with hydrogen. In the latter caſe, the carbon which iſſues from the ſeeds unites itſelf intimately with the hydrogen. Too much carbonic acid in air does more hurt to ſeeds than too much azote, and too much azote more than too much hydrogen. Various vapours may alſo alter the air, ſo as to annihilate germination: of this kind are those of sulphuric ether, of camphor, of oil of turpentine, of vinegar, of ammonia, &c.

This necessity of oxygen in nature is liable, however, to some exceptions. Some seeds have such force, that they decompose the water to separate from it oxygen, if they cannot dispense with it entirely: of this kind are peas, which germinate in water deprived of air, in every sort of gas, and even in oil, provided they have been before steeped in water.

AGRICULTURE.

Progress of the Amelioration of Wool.

Tessier and Huzard gave an account to the class of the sale of the wool and sheep of the flock of Rambouillet in the year 9, as well as of the progress of the amelioration of wool in France during that period. The most interesting experiment mentioned by them was, that of leaving the wool on some sheep for two years: by these means it acquired double length, gave a double weight, without any inconvenience to the animals, and was rendered exceedingly proper for the manufacture of woollen stuffs, so that it was employed in making kerseymeers, which were presented to the class, and which are equal to the best English stuffs of that kind.

PHILOMATIC SOCIETY OF PARIS.

C. Sylvestre read a note on the long wool of the sheep of Rambouillet. It was long believed that sheep lose their wool every year; and this assertion, void of foundation, has been advanced in works of great respectability. The members of the Council of Agriculture, charged with the inspection of the national sheep establishment at Rambouillet, were desirous of verifying this fact; and for that purpose suffered the sheep to remain two or three years without being shorn. By these means they obtained very long wool of uniform fineness, and which was equal in weight to what would have been produced by two or three shearings.

C. Vauquelin communicated a note on the nature of the earth eaten by the inhabitants of New Caledonia. Humboldt, in one of his letters to C. Fourcroy, speaks of an earth eaten by the Otomaquas when they experience a scarcity of provisions*. C. Labillardiere has confirmed, by an observation made in a part of the world very distant from that inhabited by the Otomaquas, a fact equally singular—where the inhabitants of New Caledonia, when urged by hunger, ate large quantities of a greenish steatites, tender and friable. It may now be readily conceived how the horrid custom of eating

* For this letter see Philosophical Magazine, vol. X.

prisoners

priſoners of war might be introduced among a ſavage people reduced to ſuch a ſtate of famine as to be obliged to ſuſpend their hunger by diſtending their ſtomachs and inteſtines with an earthy ſubſtance which has no other alimentary quality than that of being friable.

Vauquelin, being deſirous of aſcertaining the nature of this earth, and whether it contained any thing nutritive, analyſed, by the known means, ſpecimens of it tranſmitted to him by C. Labillardiere.

This earth is ſoft to the touch, and conſiſts of ſmall fibres eaſy to be divided: it becomes red in the fire, and loſes $\frac{1}{100}$ of its weight. It contains 37 parts of pure magneſia, 36 of ſilex, 17 of oxide, 3 or 4 of water, and 2 or 3 of lime and copper.

It contains therefore no nutritive part, and can be conſidered only as a burthen—as a mechanical mean for ſuſpending the pain occaſioned by hunger.

The ſame chemiſt read a notice on blue oxide of iron. This ſubſtance, ſent to the Council of Mines by baron De Molt, is of a bright blue colour: it preſents itſelf under the form of ſmall inſulated maſſes in the cavities or fiſſures of quartz and hard greeniſh ſteatites. It is friable, but a little unctuous to the touch. It loſes its colour by the flame of the blow-pipe, and then fuſes into a greeniſh white glaſs.

It loſes its colour neither by acids nor by weak alkalies; a circumſtance which diſtinguiſhes it from *lapis lazuli* and pruſſiate of iron. It communicates to the muriatic acid in which it has been put to digeſt, a ſaffron yellow colour, and loſes a little of its own colour; but it cannot be entirely deprived of its colour without diſſolving it at the ſame time: nothing then remains but the ſame quantity of ſilex, which ſeems to ſerve it as matrix.

In examining the muriatic acid which ſerved for this operation, it is ſeen that it has diſſolved alumine, lime, and oxide of iron; but neither manganeſe, ſulphurated hydrogen, nor phoſphoric acid, ſubſtances to which ſome might be inclined to aſcribe the blue colour of this oxide of iron, are to be diſcovered in it. The cauſe of the very remarkable colour of this oxide, a colour which it has hitherto been impoſſible to communicate to iron by any chemical means, ſtill remains, therefore, to be determined. It appears, however, that the iron in this oxide is carried to a degree of oxygenation near to a maximum.

Vauquelin read alſo a note on the ſaline ſubſtance called *muriciate of Salzbourg*. This matter, to which Haüy has given the name of *gypſiferous muriate of ſoda*, was alſo ſent to to the Council of Mines by baron De Molt. Vauquelin found in it, as Klaproth had done, an union of sulphate of lime with muriate of soda; which gives to the former the property of crystallizing in the cubical form: but he remarked likewise, that 100 grains of this substance, broken into small fragments, and exposed to the most violent heat for half an hour, lost none of their weight: they only became somewhat opake.

It is very singular to see a crystallized salt entirely deprived of its water of crystallization, though composed of two other salts which generally contain a pretty large quantity of it.

C. Gillet read a note on the emeralds lately discovered in France by C. Lelievre. Of the substances thought to be foreign to the soil of France, the emerald is not the only one which has been discovered within a few years in that country. C. Gillet mentioned the following mineral substances found there a little before the revolution: arragonite, anatase, koupholite, stilbite, dypire, minilite silex, phosphated lead, native antimony, carburated iron (plumbago).

Since that period, notwithstanding the small number of mineralogical tours undertaken, the following have been discovered: dolomie, porphyroidal rock with a calcareous base, sulphated strontian, aventurine quartz, anthracite, ferruginous scheelin (wolfram), oxidated titanium, oxidated antimony, chromated iron, oxidated uranium, arseniated lead, &c.

MEDICAL SOCIETY OF BOURDEAUX.

This Society has offered a prize of 300 francs for the best account of the doctrine of Hippocrates, or of the Hippocratic school of medicine.

The answers to this question will be received till the 30th of July 1802.

COLLEGE OF PHARMACY AT PARIS.

On the 10th of November the College held a public sitting for the distribution of prizes, the minister for the interior being president.

Bouillon-Lagrange, professor of chemistry, after proclaiming the names of those who had obtained prizes, gave an account of the labours of the Free Society of Pharmacy at Paris, and announced the prize question founded by the præfect of the department of the Seine, which is to be determined in the beginning of the year 12. It is as follows:

To determine, by exact experiments, what happens to those salts most frequently employed, and particularly the sulphates of soda and magnesia, the tartrites of potash and soda, hyper-oxygenated muriate of mercury, and tartrites of potash

potash and antimony, when mixed with the usual beverages, such as ptisans, apozems, decoctions, soups, whey, the juice of herbs, and potions.

The following memoirs were then read:—Observations on the internal temperature of vegetables compared with that of the atmosphere, by C. Solomé; a memoir on medical wines, by Parmentier; a memoir on phosphoric ether, by Boudet; observations on the formation of a new French pharmacopœia, by Champseru; observations on the juice and syrup of gooseberries, by Bourriat; a discourse on the utility of pharmacy, by Delunel; an observation on the existence of phosphorus in sugar, by Boullay.

SOCIETY OF AGRICULTURE OF THE DEPARTMENT OF THE SEINE.

This Society has offered a prize of 500 francs for the best answer to the following question:

1st, How does manure act in general? 2d, What are the different kinds of manure suited to the different kinds of soil, and the different nature of roots and plants? 3d, What are their relative qualities in these different cases? 4th, What are the different ways of preparing these kinds of manure? 5th, What are the best methods of applying them?

The answers to these questions must be supported by facts. The prize will be decreed in the month of September 1802. The memoirs must be addressed, post paid, to the secretary of the society before the 20th of July the same year.

The Society propose to open a subscription for the purpose of procuring from Spain 4000 of the fine-woolled Spanish sheep, which, according to the treaty of Bale, still remain at the disposal of the French government.

THE ROYAL ACADEMY OF MEDICAL PRACTICE AT BARCELONA.

A prize of 375 rials has been offered by this Academy for the best essay on the advantages of the cow-pock inoculation; with this express condition, that the facts adduced must be supported by actual observations made by the author.

The prize will not be adjudged till the expiration of four years.

XLVII. *Intelligence and Miscellaneous Articles.* *December* 1801.

GALVANISM.

On this subject we have witnessed some extremely curious and interesting results since our last Number. Our present notice

notice of them, however, ſhall be brief, as the projected experiments have not all been gone through with thoſe variations of circumſtances which are intended to accompany them. Suffice it therefore to ſtate at this time,

That, in concert with ſeveral gentlemen, zealous promoters of ſcience, and whoſe names ſhall be ſtated hereafter, a pile was conſtructed conſiſting of thirty-ſix pairs of plates of ſilver and zinc, with diſcs of flannel moiſtened with a ſolution of muriate of ammonia interpoſed between the pairs; that each plate was ten inches in diameter, or contained 78·54 ſquare inches; and that, conſequently, the whole ſurface of ſilver in the pile, reckoning only one ſide of each plate, was 2827·44 ſquare inches, and that of zinc the ſame.

With this pile, at a meeting of a committee on the 19th inſt. (December), gold, ſilver, copper, tin, lead, and zinc, were deflagrated with aſtoniſhing facility.

Gold burnt with a very vivid white light, inclining a little to blue. This experiment afforded an opportunity of deciding a point left undetermined in one recorded in our laſt Number, by which gold was deflagrated without any reſiduum being perceived. On the preſent occaſion, there was left on the upper plate a copious oxide of a deep brown colour, inclining a little to purple. In the former experiment, the end-plate of the trough to which the gold was applied was vertical; the oxide could be but little in quantity, and, being nearly of the colour of the mahogany on which it fell, accounts ſufficiently for its not having been perceived.

Silver gave a vivid green-coloured flame, extremely brilliant. The colour was ſomewhat like that of a pale emerald, and the light more intenſe than that from the gold. Gave an oxide inclining to black.

Copper preſented phænomena ſimilar to thoſe obſerved in deflagrating gold.

Lead gave a light of a dilute blueiſh purple, very vivid.

Tin, a light ſimilar to that of the gold; but it burnt with leſs energy,—perhaps owing to the leaves being thicker.

Zinc gave a blueiſh white flame, fringed at the moment of contact with red. It was more difficult to be deflagrated than any of the former, but the plates at the ſame time were much thicker.

The oxides of theſe laſt four metals were not examined.

Some water having been poured upon the upper plate, ſo as to form a kind of little ſtanding pool, ſeveral of the metals were preſented to the plate through this water, and were deflagrated, giving the ſame coloured flame as when preſented to the bare plate. A vapour was ſometimes perceivable in-

ſtantly

ſtantly after the deflagration, ſuppoſed to be a portion of the water converted into ſteam by the intenſe heat.

One circumſtance is extremely worthy of remark. The ſhocks from this pile, though in a ſtate to produce ſuch powerful effects on the metals, could be taken with very little inconvenience.

Dr. Van Marum and profeſſor Pfaff, of Keil, have lately been engaged in a ſeries of experiments with the Voltaïc pile, by means of which they are ſtated to have charged an electrical battery of 140 ſquare feet of ſurface by mere contact with the pile. The account that has reached us does not ſtate whether the battery received as full a charge as could have been communicated by an electrical machine, or only ſuch a one as to become ſenſible to an electrometer. If the latter, the quantity of ſurface named tends to miſlead, for a ſingle jar might have given the ſame indication; but if 140 ſquare feet received a full charge, the experiment will prove a moſt intereſting one. The ſame account ſtates, that, "by the *electricity* of this apparatus, (namely, the Voltaïc pile,) they fuſed 12 inches of iron wire: even platina wire was fuſed by it." If we underſtand the language here uſed, it means that the wire was fuſed, not through the medium of an electrical battery charged by means of the pile, though the introduction of the word *electricity* may ſeem, at firſt view, to convey that idea, but by being brought directly into the Voltaïc circuit. If we are right in this opinion, it was, properly ſpeaking, a deflagration and not a mere fuſion of the wire that took place; *i. e.* the metal was wholly, or in a great meaſure, oxided, inſtead of being altered in form only, as is the caſe in ſimple fuſion: if ſo, it was an experiment of the ſame nature as thoſe related above, in which ſeveral ſquare inches of gold and ſilver leaf, &c. were deflagrated. The detail of Dr. Van Marum and profeſſor Pfaff's experiments, which theſe gentlemen have promiſed to publiſh in a ſhort time, will, however, ſoon clear up the ambiguities of the ſhort account that has reached us.

FULMINATING MERCURY.

The effects of an accidental exploſion of a quantity of the fulminating mercury of Mr. Howard, which was experienced at Dr. Pearſon's laboratory, while at lecture, a few days ago, ſeem to be ſufficiently remarkable to be worthy of being recorded, and may poſſibly throw ſome light on the agency of this powder, and indicate ſome uſeful applications in practice.

The powder juſt collected from the filtering paper, on which it lodged after the liquid from which it was precipitated

tated had paffed through, was put on paper, and, for expedition, laid in four loofe parcels, (each of which, when dry, would have weighed about thirty grains,) on a tile one inch and a half thick and eight inches fquare, which had been previoufly heated to a degree a little above what could be borne by the hand. The tile was placed on a table, and left without further notice, to allow the powder to become dry.

At the end of about half an hour the whole of it exploded, making a report equal at leaft to that of the difcharge of a 32 pounder, as declared by a naval furgeon then prefent. The tile was fhivered into a number of pieces, and broken quite through the middle; two panes of a window, at the diftance of about eight inches from the tile, were alfo fhivered; one of which was oppofite, and the other immediately above it; but none of the adjoining fquares of glafs were broken. On another fide, at the diftance of about two feet from the tile, ftood a frame containing bottles of fpecimens of chemical preparations, not one of which was broken; nor did any of the pupils fuffer, who were fitting on a form only three feet from the powder which exploded.—The whole of the room was filled with thick white fumes; and the windows were covered with quickfilver, depofited from the exploded powder.

1. The above accident fhows that the mercurial preparation cannot be employed for throwing projectiles like gunpowder, the mode of agency of the explofion being on quite different principles, as is evident from what is above related; particularly in the fracturing of the thick tile, and the very little diftance to which the concuffion of the air was propagated.

2. It appears that the fulminating mercury is calculated for fhivering rocks or large ftones by its explofion.

3. From the comparatively low temperature at which the powder exploded, it may be employed for certain deftructive purpofes in time of war, by being confined in veffels that are liable to be heated.

4. From the almoft infinite velocity with which a train of this powder may be fired, it is calculated for fetting on fire things which cannot be effected by a train of gunpowder *.

ANTIQUITY OF THE EARTH.

In a former Number we mentioned the French having found in Egypt, on fome of the antient monuments, different figures of the zodiac, which, fuppofing them to have been originally formed when the heavens had a correfponding pofi- tion,

* Communicated by Mrs. R. Lowry, a pupil of Dr. Pearfon.—For a further account of this powder, we beg to refer our readers to Mr. Howard's paper, given in a preceding volume.

tion, tended to give to our earth a much higher origin than our received chronology attributes to it. According to ſome philoſophers, we muſt go back from the preſent period 15,000 years; and even then we ſhall only arrive at the time in which the figures of the zodiac were contrived: but the world may have undergone many previous revolutions.

With all the ſemblance of ſagacity and truth which accompanies this mode of argument, nothing can be more unphiloſophical than the inference. Is it not an admitted fact, that one of the firſt efforts of aſtronomers, after their ſcience had attained a certain degree of perfection, was to draw from it ſuch modes of arrangement and calculation as might be uſeful in adjuſting the *current* time; and that the period from which they *ſuppoſed* their account to ſet out was in a great meaſure *arbitrary*, depending on the multiplication of cycles into each other, counting backward from any poſition of the heavenly bodies in the then current age, to the period in which theſe bodies (according to their tables) muſt have ſtood in a poſition fitting the purpoſe they had in view?

Does not the evidence adduced in different papers in the Aſiatic Reſearches amount almoſt to a demonſtration that, excepting a period which accords very well with the Scripture accounts, the whole Bramin chronology of *myriads* of years is merely a clumſy aſtronomical fiction, which at firſt was not even intended to deceive?

We may alſo ſtate here another well known fact,—that it was long a favourite opinion with aſtrologers, that all the planets muſt have been in conjunction * in the firſt ſcruple of the ſign Aries when the world was made; becauſe then, according to them, was the proper time to begin a year! Others might have been for a different arrangement, *i. e.* a differently conſtructed zodiac; but whichever had the greateſt number of ſuffrages at any period, can never prove any thing as to the fact in queſtion.

But, not to dwell on a ſubject ſo unprofitable, or on the early propenſity of different nations to *forge* for themſelves an high antiquity, is it not known to every one that the Julian period had an arbitrary commencement? If the hiſtory of this fact had been loſt, and correſponding zodiacs had been found, would it have been correct to have inferred that they were made 6500 years ago?

LITERATURE.

Lord Elgin, our ambaſſador at Conſtantinople, having ſent from that city to Athens ſeveral artiſts, who cauſed dili-

* Let the planets be all without any latitude in ſuch a conjunction—when did it take place?

gent

gent ſearch to be made in the Temple of Theſeus, we learn that they were ſo fortunate as to find there almoſt all the bas-reliefs which formerly ornamented the Temple of Minerva. Theſe monuments, a great many inſcriptions, and all the urns capable of being tranſported, have been ſent by the Porte to England.

A M. de Hammer alſo has made valuable diſcoveries at Paphos, and has found at Roſetta a complete copy of the original of the Arabian Nights Entertainments. Indeed, ſeveral copies of this work, in Arabic, have been found in Egypt, as well by Mr. Hammer as by Mr. Clarke of Jeſus College, Cambridge, and by the members of the French Inſtitute; but Mr. Hammer's attainments are infinitely more extenſive, and excluſively embrace ſome objects of the higheſt importance to the literary world.

Mr. Hammer is a German gentleman of rank, who was ſent by the Emperor of Germany, for the purpoſe of exploring the literary treaſures of Perſia, Arabia, Egypt, &c. During the eventful, and we may ſay miraculous, campaign of Sir S. Smyth in Syria, Mr. Hammer, then on his travels, became known to him, and profited by the extenſive influence and character of our countryman to proſecute his reſearches. He has by theſe means, ſeconded by extenſive talents, and a perfect knowledge of the Arabic and other languages, obtained more intereſting matter than even the French Inſtitute was able to collect. Amongſt other curioſities, he has brought to this country an Arabian manuſcript of great antiquity, furniſhing a complete key to the hieroglyphics, by which he has been enabled to tranſlate various inſcriptions on the pyramids, &c. and thus eſtabliſh the objects for which thoſe ſtupendous maſſes were erected.

Le Moniteur (the French official journal) of the 18th December, announces that the Chevalier de Coral, the Spaniſh ambaſſador at Conſtantinople, received intelligence at Hermanſtadt, on his journey, that a learned Spaniard, now travelling at the king's expenſe, has diſcovered, in Morocco, a complete manuſcript copy of Titus Livius, written in Arabic.

DEATHS.

Lately, at Chateauneuf, in the department of Saone and Loire, in the houſe of his brother-in-law, C. Drée, the celebrated Dolomieu, member of the National Inſtitute, and well known by his important labours in regard to mineralogy.

On the 19th of November laſt, at Nice, Joſeph de Beauchamp, the celebrated aſtronomer, who reſided many years at Bagdad and other places in the Eaſt.

XLVIII. *Remarks upon ſeveral Experiments made to prove the Converſion of Iron into Steel by means of the Diamond. By* DAVID MUSHET, *Eſq. of the Calder Iron Works* *.

IN the Philoſophical Magazine for November 1799 are inſerted ſeveral experiments performed by me to prove whether the experiment of the French chemiſts at the Polytechnic School relative to the formation of ſteel by means of the diamond was ſufficiently concluſive. Reſting upon the reſult of theſe experiments, no inference could be drawn to validate the accuracy of the Pariſian experiment. In every caſe, fuſed metallic maſſes were obtained, when neither diamond nor carbonaceous matter was uſed, which, from deſcription, reſembled every way the button obtained by fuſion in contact with the diamond. The reſults produced in my experiments, when hammered, and put to the uſual teſts of heating, and plunging into water, exhibited the uſual ſtate of ſteel, cloſed partially in the grain like ſoft ſteel, and reſiſted, in a conſiderable degree, the application of the file. This was proof beyond that afforded by the French chemiſts. The unuſual teſts of poliſhing upon the lapidary's wheel, dropping the nitrous acid, and exhibiting the fracture of the metallic button, are not ſatisfactory proof to the artiſt that ſteel was here produced.

Some time after the communication of the above experiments, my countryman Sir George M'Kenzie read a paper before the Royal Society of Edinburgh; in the former part of which he exhibited reſults to prove that the experiment of the French chemiſts relative to the diamond forming ſteel was quite concluſive; and, in the latter part of his paper, he detailed a variety of experiments to invalidate a concluſion which I had drawn, of carbon diſſolved in caloric penetrating cloſe veſſels, and by its union with iron forming ſteel. I ſhall in this paper take the liberty of making a few remarks upon theſe different experiments, as they occur in the order of Sir George's arrangement.

I think it proper here to premiſe, that my not ſubſcribing implicitly to the concluſions drawn by the Pariſian chemiſts and Sir George M'Kenzie relative to the diamond and iron, proceeds not from faſtidious ſcepticiſm as to the carbonaceous matter of the diamond; I only wiſh that the fact of its form-

* Communicated by the Author.

ing steel by its union with iron should be established by experiments more perfect and more rigorous than any hitherto performed, before a conclusion so important be drawn and rested upon.

The remarks made in the first paragraph of this paper apply equally to the results of Sir George as described by him, and that of the Polytechnic School. So far as the description of parties, and the tests used by each, go, the results obtained in the experiments communicated by me seem to prove, with equal certainty, the presence of steel; and, as a further proof of this, I shall adduce from the baronet's own experiments an identity of results obtained in two of his own operations, wherein it appears difficult to distinguish betwixt the nature of the products. I allude to that obtained when the diamond was inclosed in a cylinder of iron, and to another experiment, wherein, following the process of Clouet, he fused a mixture of iron, carbonate of lime, and pounded crucible *. In the treatment of these products respectively, no fixed or determinate feature of steel is applicable to the one more than the other. If, therefore, in the course of subsequent communications, I prove, by the most unexceptionable experiments, that the decomposition of the carbonic acid in lime never afforded one particle of carbon to the iron fused in contact with it, which iron possessed similar properties with that obtained along with the diamond, so far as the baronet's observation and tests went, will not the experiment of the diamond forming steel appear still more equivocal, and require more demonstrative proofs of its combination with iron than any yet adduced?

Upon the baronet's second experiment, wherein he exposed a portion of iron under circumstances similar to those of the diamond experiment, and obtained no fusion, I shall shortly remark: That the uncertain and unequal degree of heat obtained in a smith's forge, particularly if single blasted, will easily account for a few globules being fused in the one case, and none in the other. Blisters were found in both, and it does not appear by any comparative trials of quality that these blisters were essentially different, or produced by different causes. To prove that an irregularity of heat might produce the difference noticed, namely, the fusion of a few globules of the iron, a reference might be made to others of Sir George's own experiments, where in one he fused iron filings, but could not reduce fragments of horse-nails!

The major part of Sir George's experiments which follow those

* See Nicholson's Chemical Journal, No. xl. pages 105 and 107.

thoſe relative to the diamond, divide themſelves into two parts: thoſe wherein heat enough was employed, and in which, conſequently, the iron was found reduced; and *vice verſa.* In every caſe, had the baronet poſſeſſed powers of furnace ſufficient, he would have obtained a fuſed reſult. In thoſe experiments where fuſion was obtained, the quantities were ſo ſmall that they afforded but a ſcanty portion for ſubſequent teſts.

So far, however, we are agreed, that malleable iron is fuſible either alone in a crucible, or in contact with earths of various kinds. It is relative to the nature of the products thus obtained that we differ: a fair ſtatement of the facts, as they relate to the experiments of Sir George, the French chemiſts, and thoſe communicated by me, will beſt explain the points at iſſue.

C. Clouet firſt announced two new proceſſes for the fuſion of iron, and for the converſion of it, in the one caſe into caſt ſteel equal in quality to the Engliſh marks of Hindſman and Marſhall; and in the other, the converſion of it into a kind of caſt iron by means of glaſs. The former experiment, being deemed of national importance, was formally ſubjected to the ableſt chemiſts in France for ſatisfactory documents as to the correctneſs of C. Clouet's pretenſions. This was undertaken and executed by citizens Guyton, d'Arcet, and Vauquelin; who, after repeated experiments in the fabrication and ſubſequent forging of the ſteel, pronounced it, in conjunction with their moſt eminent artiſts, poſſeſſed of all the "external characters and intrinſic qualities of the Engliſh caſt ſteel of the manufactories of Huntzman and Marſchall; that it may be uſed for the ſame purpoſes, and be introduced in competition with it in commerce, without fear of any diſtinction being made to its prejudice."

Shortly after theſe experiments were announced in this country, I reſolved to repeat them along with others, not only with a view to prove the facts eſtabliſhed by the French chemiſts, but more particularly with a view to explain ſome curious reſults in the manufacture of crude iron, the ſource of which I had not before an opportunity to explore, and which would not admit of a ſatisfactory explanation upon Clouet's ſuppoſition of the decompoſition of the carbonic acid and the union of its carbon with iron.

The progreſs and detail of theſe experiments ſhall hereafter be given; ſuffice it at preſent to ſay, that, in following the proceſs laid down by C. Clouet, with clay, carbonate of lime, and iron, a reſult ſimilar to his was obtained: but I found alſo, that when lime, carefully deprived of its carbonic acid,

 was

was introduced as a substitute for the carbonate, the fusion and the quality of the button produced were in every respect the same! The metal, subjected to a variety of tests, exhibited the various phænomena mentioned in a former part of this paper. Convinced that, from whatever cause this appearance of steel was derived, the carbonic acid was entirely neutral, I next ventured to subject iron to fusion *per se*; and the result answered the novelty of my expectations. Beautiful buttons were obtained, of various magnitudes, possessed of similar properties with those obtained with the carbonate and clay, and with the de-acidified lime and clay. The quality of these unalloyed buttons varied, and exhibited, in a greater or less degree, the properties ascribed to the others. From frequent repetitions of the same experiment with various sized crucibles, I found the iron less altered in a crucible whose capacity was little more than the contents introduced, than when a larger crucible was used to the same weight of iron.

Thus circumstanced, and unable to account for this apparent formation of steel in any other way, I concluded that such was the affinity of iron for carbon, that when protected from common air, and exposed to a high temperature, it decomposed the ignited gas of the furnace, seized its carbon, and constituted steel; and that the degree of saturation depended upon the area presented by the interior surface of the crucible to any given quantity of iron.

I had advanced thus far when I communicated the general result of these experiments in the Philosophical Magazine for November 1799.

Soon after this, Sir George M'Kenzie read the paper formerly alluded to. In it he informs us that, when he produced heat enough, malleable iron was fusible *per se*; but he alleges that in no case was the quality of the metal altered.

In the present state of my information upon the subject at issue, I shall cheerfully give up the conclusion I had formerly drawn, of steel being formed by carbon dissolved in caloric. I have stated the progress of my mind so far as it was biassed by the experiments formerly communicated; and, far from wishing to confound by any arguments advanced in favour of the deduction I formerly made, I regret the facility with which the mind is naturally inclined to indulge in favour of received opinions.

The long received opinion, that pure malleable iron was infusible, and the unqualified approbation passed upon a modification of iron exactly similar in quality, by the ablest chemists in Paris, led me into a false appreciation of the quality of of the products obtained in my own manipulations. A ſeries of experiments, which I ſhall communicate through the medium of this publication, will, I hope, carry with them the moſt evident demonſtration, that in attributing to their reſults the merits of caſt ſteel, or ſteel of any quality, the French chemiſts have deceived themſelves, and led into error many whoſe confidence in their labours was entire.

I cannot, however, ſubſcribe with the ſame facility to the concluſion drawn by Sir George, that the fuſion of malleable iron does not in the leaſt alter its quality: but I ſhall at preſent wave adducing any experiments to prove the contrary; they ſhall, however, be given hereafter in their proper place. The baronet's experiments afford one inſtance, however, which he has overlooked in forming the opinions on which his aſſertions are founded. He fuſed two parcels of iron filings, the one alone, the other mixed with oxide. From theſe were obtained two buttons of iron exactly ſimilar in quality, which quality, from the eulogium of the artiſt, we may juſtly infer was improved by the fuſion.

The queſtion which moſt forcibly occurs here is this: What revived the oxide in contact with the iron? What affinity, in this caſe, was exerted to diſengage the oxygen and revive the metallic particles? The preſence of carbonaceous matter muſt be inferred. This again we muſt ſuppoſe united to the iron, involving this conſequence, that good malleable iron contains a portion of charcoal capable of reviving at leaſt 1-10th part of its weight of oxide of iron. If this mixture is neceſſary to conſtitute good malleable iron, and a ſubſtance be added, which, to a phyſical certainty, muſt deprive it of this neceſſary conſtituent part, will the fuſed aggregate ſtill be of the ſame quality as a portion of the ſame iron fuſed alone, or an original portion compared with it from the bar *?

I ſhall conclude this paper with the following obſervation, founded upon dear-bought experience, and which I hope neither Sir George nor his artiſt will conceive as in the leaſt degree perſonal.

In ſubmitting metallic reſults to undergo the neceſſary teſts by a workman, the greateſt poſſible degree of caution is neceſſary not to impreſs him with the nature of the wiſhed-for reſult. A ſingle experiment often is the foundation of a theory; and workmen, from the laudable motive of pleaſing

* I hope h reafter to ſhow that all malleable iron contains a portion of charcoal; that this is eſſentially neceſſary to its proper exiſtence: and that depriving it of this carbonaceous mixture debaſes its quality by a total annihilation of its properties.

a ſuperior,

a superior, as well as of exciting a disposition to a more valuable remuneration in subsequent trials, soon discover the bias of the operator's mind, and artfully strengthen that predisposing partiality which sets mature investigation aside. Thus frequently may a disinterested person be the innocent cause of much error, and the occasion of many tiresome and laborious experiments.

XLIX. *The same Methods of Reasoning common to Mathematics and to Natural History and other Branches of Physics. By Mr.* R. HERON.

SIR, *To Mr. Tilloch.*

YOUR valuable Publication supplies much agreeable evidence of the constant advancement of physical science in all its branches. Yet one cannot help observing, that there exist, between the votaries of the studies of pure and mixed mathematics, and the students of chemistry and natural history, a mutual jealousy and misunderstanding, which are, I fear, sometimes not a little inauspicious to the general progress of philosophy. You shall rarely see mathematics and natural history flourish at the same time, and in the same school of science. The mathematician looks with contempt on the naturalist; and that contempt the naturalist seldom fails abundantly to repay. There is even supposed to be an incompatibility between these different studies, so great, that he who excels in the one must not hope to succeed in the other.

But these, Sir, are merely groundless prejudices. It is not to propose a paradox—when I affirm, that " the truths of mathematics, and the reasonings by which these are discovered, have the most exact analogy with those of natural history, and indeed of every other branch of science that can be named." I am induced to send you the following short explanation of this proposition, not in the idea of supplying in it a new discovery for the use of the scientific and the learned, whose views are truly profound and enlarged; but, as believing it of great importance, that this principle should be rendered familiar to the minds of those who are now entering upon philosophical study; and as hoping that some of those who are mere bigots of mathematics, and of those who are, on the other hand, mere bigots of natural history, may be brought mutually into good humour by it.

I. Consider the nature of those reasonings by which mathematical

thematical truths are diſcovered. Number and figure, points and lines in all their poſſible combinations, magnitudes or quantities, are the ſubjects of mathematical inveſtigation. Of theſe, we bring not into the world, by reminiſcence from a former ſtate, or by innate inſpiration, any original knowledge prior to experience by the ſenſes. So ſimple, however, and primary are their ideas, that they may be conſidered as nearly coeval with the firſt opening of the mind to perception. The line, the point indeed, which we perceive by ſenſe, poſſeſs not each that perfection in its kind which mathematical ſcience aſcribes to it. Yet from ſenſe are our primary notions of the mathematical line and point nevertheleſs derived. We obtain the notions of animal and vegetable forms and colours more perfectly beautiful than any which are actually to be ſeen, ſolely by abſtracting and again combining in idea the beauties which are in nature diſtributed among different animals and vegetables. We form in imagination the character of a truly wiſe and good man by a ſimilar abſtraction and combination of qualities, which in real life are never to be ſeen together. We exalt our minds to conceive the exiſtence of the all perfect Ruler of the univerſe, alſo by the abſtraction of the beſt qualities which we diſcover in his creatures, and by combining them in our thoughts in the higheſt perfection in which we are able to imagine their coexiſtence. And it is juſt in the ſame manner that, by abſtraction from the point and line, which can be materially repreſented to our eyes, we attain to the conception of a mathematical point without length, breadth, or thickneſs; and of a mathematical line, which of theſe qualities has none attributed to it but length.

Thus far, then, the fundamental notions in mathematical ſcience are gained preciſely in the ſame manner in which we arrive at the knowledge of generic, ſpecific, or claſſific characters in natural hiſtory; by obſerving individuals in material nature, and by abſtracting thoſe qualities which are common to numbers; leaving out thoſe which ſerve but to particularize individuals, and making the qualities abſtracted and combined anew, to our minds, the common repreſentatives of all.

Numbers are generated from points; all other definite magnitudes from lines; and a line itſelf is alſo produced from a point. But we have no reaſon to believe, that, if material nature did not ſuggeſt to our conception the repetitions of unity, by preſenting more individual objects than one to our notice, we ſhould ever have formed the idea of two. Nor is it certain that we ſhould have been able, in imagination, to distinguish lines into the species of curved and straight, unless we had seen imperfect examples of both in the extremities or outlines of material bodies. Thus, not only our simplest mathematical ideas of unity and of extension are abstractions from material existence, just as exactly as are the genera, species, &c. in natural history; but our notions of the combining of unities and extensions, by multiplication and addition, so as to produce new varieties of them, are certainly in their origin produced by a farther observation of material nature, and without this, most probably, could not arise. The mathematician who pursues, in arithmetic, unity through all its combinations, or who combines, in extension, lines into all the imaginable diversities of curved and rectilinear figures, employs, in these acts, a mental process perfectly analogous to that of the poet who feigns, *ad libitum*, characters of deities, men, and dæmons, by new combinations of those moral, intellectual, and bodily qualities which he has had occasion to behold in real life. This mathematician does, in fact, a thing precisely similar to that which is done by the common mason or carpenter, who frames, of stone or wood, many new figures of building, furniture, and utensils, not exactly similar perhaps to any that he may have seen in simple unviolated nature. The chemist acts in the same manner, in every trial of the unknown combinations of any chemical substance of which he desires to investigate the character. And it is exactly thus, too, that the zoologist, to elucidate the natural history of any animal, places it in every diversity of circumstances in which he can imagine that a new quality may be displayed by it.

Compare the simpler operations in arithmetic with those of logic. Multiplication is only an abbreviation of addition: addition, in every act of it, simply compares several different individuals, and thus ascertains that they belong to the same species: every act, whether of addition or of multiplication, is precisely that elliptical syllogism which is named an enthymeme. In every act, whether of the one or of the other, there is, in truth, an analysis of particulars, and a general induction from them. I say, for instance, in addition, 2 and 2 are 4. This, as a syllogism, affirms, that all numbers containing equal units are equal: 4 expresses a certain combination of unities: 2 and 2 make together exactly the same combination of unities; 2 and 2 are therefore equal to 4. Let any one attend vigilantly to what passes in his mind in this act of addition; and he will find it to be clearly what is here stated. Again, this act of addition shows, just like the observation of a rose-tree only in leaf, and of a rose-tree in full

full flower in natural history, that 2 and 2 are, as well as 4, but combinations of the same species, genus, order, or class of unity. It is also an example of analysis and induction. You analyse when you consider, as one unavoidably does, the constituent units separately; you draw your general inference when you perceive the equalities. Any act in multiplication might, as to the reasoning of it, be in the same manner illustrated. Subtraction reasons exactly in the same manner as addition; but, instead of equalities, discovers dissimilitude, and of that dissimilitude gives a numerical definition. Division combines the reasonings of addition or multiplication with those of subtraction. The rule of three is, most obviously, a chain of abridged syllogisms; a process of regular analysis and induction; an ascertaining of the proper species, &c. to which certain individuals belong. Pursue the reasonings which respect numbers through every other variety of the accustomed operations; and you shall find, still, nothing but the same acts of ratiocination constantly repeated. A syllogistic comparison of ideas; to discover equality or difference; a process of analysis and induction; a reference of individuals to their respective species, of species to their genera, &c. &c.; are still the only acts of intellect which are performed: and still the mind is conscious of no ideas but what have their origin in the observation of material existences, and in abstraction from these.

Thus it is in that part of mathematics which abstracts quantity from extension, and concerns itself only about the relations and properties of number considered exclusively of figure.—Examine, on the other hand, any of the propositions and demonstrations of Euclid, or any other mathematician who investigates the qualities of figures in the antient form of mathematical reasoning. Does he demonstrate a theorem? This is only to establish, by analysis and induction, a general fact before unknown. Is he to solve a problem? This is to explain the secret causes of a general truth before known, but unaccounted for; to invent a rule, and prove it to be founded on a due knowledge of the relations of things; or to adopt a rule from others, and to show by what means it is that compliance with this rule perfectly accomplishes the end proposed. A theorem and a problem differ from one another merely as two different modes for suggesting the same question or questions of the same species. In both, the mind analyses and abstracts just as in the prosecution of any discovery in natural history. The train of demonstration in the works of all the antient mathematicians who treat of figure, is a series of enthymemes or abbreviated syllogisms. When I go about to demonstrate,

demonstrate, that the three angles of any triangle are equal to two right angles; what else is this, but to show that the three angles of a triangle are one species of the subdivisions into which the magnitude of two right angles may be marked out? Can a single case of mathematical inquiry be pointed out, in which the reasoning shall not appear to have the most perfect analogy with that which is used in every other branch of science, and, indeed, in the whole business of common life? Impossible.

But the mathematician represents all magnitudes by combinations of lines incapable of exhibiting solid contents, or even, with accuracy, the diversities of surface; and in his demonstrations employs the letters of the alphabet to denominate those lines. He does. But the abstractions of the fancy of the poet are thus represented, and but in part, by the imagery of the sculptor and the painter. The naturalist thus employs delineations of those objects which he cannot keep constantly under his actual inspection. The letters of the alphabet, and other signs not in any other way significant, are employed in our reasonings in every other branch of knowledge, just as well as in mathematics. If a few technical terms be peculiar to this branch of the science of the magnitudes of figure; are not certain sets of technical terms, in almost every other instance. much more numerous than those of mathematics, peculiar respectively to every different science, and to every different branch of art?

It may still, however, be alleged, that there is a peculiarity in algebra, that branch of mathematical reasoning in which the moderns chiefly delight. But, no: the reasoning is, even in algebra, still the same. The signs only, and the abbreviations, are somewhat different. Algebra refines even upon the abstractions of arithmetic, and enables the mind to proceed through a long train of reasoning respecting numerical quantities, magnitudes, or distinct values of any sort, without the intervention of common language, or even of the common signs of number. But the object is still to arrange individuals under their species, species under their genera, genera under abstractions still more comprehensive, &c. The reasoning is still by an analytical collection of particular facts, and a conclusion to a general truth from their agreement. The series of abbreviated syllogisms may be just as clearly traced in demonstrations by algebraic symbols, as in those in which more tedious forms of words are employed. Algebra is but the brachygraphy of mathematics; and short-hand writing implies no peculiarity of reasoning, but merely a voluntary and conventional connection of certain signs with certain primary objects, whether ſounds, words written in alphabet, or diſtinct exiſtences in nature. In the higher parts of algebra, and indeed throughout all mathematics, are many abbreviations nearly ſimilar to that in which arithmetic ſubſtitutes multiplication for addition. But has not every branch of knowledge, in like manner, its abbreviations, by means of which its reaſonings are brought continually nearer to the quickneſs and certainty of intuition?

II. All that ſcience does is, not to create new exiſtences, to bring into the knowledge of man ſomething that was never before known to mind, but merely to obſerve individuals, and to form, by abſtraction, general notions by which human thought may become capable of a much greater number of individual conceptions than it could by other means poſſibly comprehend. In ſhort, all human ſcience is thus reduced to arrangement convenient to memory, and ſerving merely, as it were, to enlarge the range of the mind's eye. Now, mathematics is not a ſolitary exception: all its diſcoveries are merely diſcoveries of exiſtence and arrangement. A mathematical point is the *genus generaliſſimum* of the ſcience. Points repeated with relation merely to numerical quantity; and, on the other hand, points repeated ſo as to form extenſion; are the two great genera included immediately under that genus generaliſſimum. If we purſue lineally extended quantity into ſubdiviſions continually lower, we ſhall find next the three ſpecies of lineal extenſion by ſtraight lines, by curves, and by angles. Under the ſpecies of ſtraight lines are all the ſubordinate ſpecies of merely longitudinal extenſion, with their mutual relations and differences. The ſpecies of curves has under it all the inferior ſpecies of magnitudes or figures, which can be formed and bounded by curved lines alone, without the intervention of angles or ſtraight lines. The ſpecies of angles comprehends, as ſubordinate, all the different ſorts of ſingle angles which can be formed by the meeting of two ſtraight lines. Subordinate to the angle and the ſtraight line, jointly, are all thoſe figures complete on all ſides, which are formed by the combination of ſtraight lines and angles only. There is a fifth claſs of ſubordinate ſpecies referable to the angle, the ſtraight line, and the curve, jointly, which includes all the ſubordinate ſpecies of figures, all thoſe modes of extenſion, in which angles, ſtraight lines, and curves are combined. It were eaſy to purſue theſe ſubdiviſions continually lower, even to every conceivable diverſity, in the modes of extenſion. It might be ſhown that number is merely arrangement into genera and ſpecies, &c. with the ſame

same ease with which this is evinced of extended magnitudes. But I will not multiply examples of what is so obvious.

When time is made the subject of mathematical calculation; it is then considered either as numerical quantity, or as lineally extended magnitude, or as a combination of the one with the other.

Distances are obviously subjects of mathematical calculation, solely because they are modes of lineal extension.

Nothing else can be brought under the mere forms of mathematics without a fiction, that it is either numerical or lineally extended quantity.

All the truths of mathematics, antient and modern, pure and mixed, might be more happily than in any other mode of exposition unfolded in a systematic arrangement, like that under which Linnæus has disposed the history of the kingdoms of nature, or the table in which the French chemists contrived beautifully to state the truths and analogies in the Lavoisierian system of that science.

I do not at all communicate this, Sir, as any mighty discovery. I should only wish to convince some mathematicians that naturalists reason just as much as they, and precisely in the same way; that mathematics, like all other branches of human science, deals merely in the discernment of existence and in arrangement; that it is not, therefore, more remote than any other science from the common sense and common utilities of life; that it has no right to be esteemed the only science by which the reasoning powers are to be successfully invigorated; that its forms may be yet advantageously simplified; and, that all its general and specific truths might be very intelligibly exhibited in one system, and in one or two grand tables.

At my first leisure, Sir, I shall take the liberty of addressing you on a different subject; and shall communicate certain facts, from which the *electrical fluid* appears to me to be certainly an *oxide of caloric.*

I am, Sir, yours,

R. HERON.

London,
December 7th, 1801.

L. *Report*

L. *Report given in to the Class of the Mathematical and Physical Sciences of the French National Institute on the Experiments of Volta, made the 2d of December* 1800 *.

THE first galvanic phænomena consisted of muscular contractions excited by the contact of a metallic arc. Galvani and several other philosophers considered them as produced by a peculiar electricity inherent in animal parts. Volta first announced that the animal arc introduced into these experiments served only to receive and manifest the influence, but very little, or not at all, to produce it. The muscular irritation, which was at first supposed to be the important part of the phænomenon, was, according to him, only an effect of the electric action produced by the mutual contact of the metals of which the exciting arc was formed. This opinion, which found partisans as well as opponents, caused the experiments proper for supporting or combating it to be multiplied; and, as is always the case in the infancy of discoveries, there appeared, along with facts, a number of singular anomalies, which rendered it more difficult to trace out their connection, and which were then even inexplicable, because they arose from very delicate circumstances, the influence of which was not well known.

Such was the state of this branch of philosophy when the commission delivered in to you its first report: its object had been to determine with accuracy the conditions proper for calling forth and modifying the galvanic effects; it did not attempt to explain them, and confined itself to presenting them in that order which seemed most proper. At that period we were not acquainted with the researches by which C. Volta, pursuing the path he has opened, endeavoured to connect with his first discovery all the phænomena exhibited by galvanism. He has since made known a great many others, equally important, which he has connected together by a very ingenious theory; and if any thing still remains to be done to determine with exactness the laws of this singular action, and to subject it to rigorous calculation, the principal facts at least, which ought to serve as the basis, seem to be invariably fixed.

Your commission proposes at present to give an account of these fundamental experiments, and the method in which

* The commission charged with this report consisted of Laplace, Coulomb, Hallé, Monge, Fourcroy, Vauquelin, Pelletan, Charles, Brisson, Sabathier, Guyton, and Biot.

Volta

Volta employed them for the eſtabliſhment of his theory. It is much indebted to that learned man for the readineſs with which he repeated them, ſeveral times, before the commiſſioners; who can thus vouch for their truth and correctneſs. The principal fact, that from which all the reſt may be deduced, is as follows:

If two different metals, inſulated, and having only their natural quantity of electricity, are brought into contact, when removed from that contact they are found in different ſtates of electricity: one is poſitive, and the other negative.

This difference, very ſmall at each contact, being accumulated in an electric condenſer, becomes ſufficiently ſtrong to cauſe the electrometer ſenſibly to diverge. The action is not exerciſed at a diſtance, but only on the contact of different metals; it ſubſiſts as long as the contact continues, but its intenſity is not always the ſame. It will here be ſufficient to take as examples copper and zinc. In their mutual contact the copper becomes negative, and the zinc becomes poſitive.

Having proved the development of metallic electricity independently of any moiſt conductor, C. Volta introduces theſe conductors.

If a metallic plate be formed of two pieces, one of zinc and the other of copper, ſoldered end to end; if the extremity of the zinc be held between the fingers; and if the other, which is of copper, be made to touch the upper plate of the condenſer, which is alſo of copper, the latter will be charged negatively. This is evident from the preceding experiment.

On the other hand, if the copper extremity be held between the fingers, and if the other extremity, which is of zinc, be made to touch the upper plate of the condenſer, which is of copper, when the contact is deſtroyed, and the upper plate is removed, it has not acquired electricity, though the lower plate communicates with the common reſervoir.

But if paper moiſtened with pure water, or any other moiſt conductor, be placed between the upper plate and the zinc extremity, the condenſer becomes charged with poſitive electricity. It becomes charged alſo, but negatively, when the upper plate, covered with a moiſt conductor, is touched by the copper extremity, holding the zinc extremity between the fingers. Theſe facts are inconteſtable; they have been verified by the commiſſion.

The manner in which C. Volta explains them, and refers them to the preceding, is as follows:

Metals, ſays he, and perhaps all the bodies in nature, exerciſe, exercife, as has been faid, a reciprocal action on their refpective electricities at the moment of contact. When the metallic plate is held by its copper extremity, a part of its electric fluid paffes into the oppofite plate, which is of zinc; but if this zinc is in immediate contact with the condenfer, which is alfo of copper, the latter tends to difcharge itfelf of its fluid with equal force, and the zinc cannot tranfmit any to it: after the contact, therefore, it muft be found in its natural ftate. On the other hand, if a piece of moift paper be placed between the zinc of the plate and the copper plate of the condenfer, the moving power of the electricity, which exifts only at the time of contact, is deftroyed between thefe two metals: the water, which feems to enjoy this property in a very weak degree in regard to metallic fubftances, checks only a very little the tranfmiffion of the fluid from the zinc to the condenfer, and the latter may be charged pofitively.

In the laft place, when the condenfer is touched with the extremity of the plate which is of copper, the moift paper interpofed, the proper action of which is exceedingly weak, does not prevent the metallic plate from conveying a part of its metallic electricity into the plate of zinc: then, by deftroying the contact, the condenfer is found charged negatively.

From this theory it is eafy to explain the Voltaïc pile. To do it with more fimplicity, we fhall fuppofe that it is formed on an infulator, and we fhall reprefent by unity the excefs of electricity which a piece of zinc ought to have over a piece of copper immediately touched by it *.

If the pile is compofed only of two pieces, a lower one of copper and the fuperior of zinc, the electric ftate of the former will be reprefented by $-\frac{1}{2}$, and that of the fecond by $+\frac{1}{2}$.

If a third piece be added, which muft be of copper, to produce a difplacement of the fluid, the lower piece of zinc muft be feparated by a piece of moift pafteboard; it muft then acquire the fame electric ftate as the latter; at leaft, it will be fo if we neglect the action proper to the water, which appears very fmall, and perhaps alfo the very weak refiftance

* The quantities of electricity accumulated in a body beyond its natural ftate are, *cæteris paribus*, proportional to the repulfive force with which the moleculæ of the fluid tend to feparate from each other, or to repel a new molecula which one may attempt to add to them. This repulfive force, which in free bodies is balanced by the refiftance of the air, conftitutes what we call the *tenfion* of the fluid: a tenfion which is not proportioned to the divergency of the ftraws in Volta's electrometer, nor to that of the balls in Sauffure's, and which cannot be accurately meafured by means of the electric balance.

which

which that liquid, as an imperfect conductor of electricity, can oppose to the communication. The apparatus being insulated, the excess of the upper part can be acquired only at the expense of the piece of copper which is below: the respective states of these pieces will be no longer the same as in the preceding experiment, and will become,

In the lower piece, which is of copper, $-\frac{2}{3}$; in the second, which touches it, and which is of zinc, $-\frac{2}{3}+1$, or $\frac{1}{3}$.

The third, which is of copper, and which is separated from the preceding by a piece of moist card, will have the same quantity of electricity, that is to say, $+\frac{1}{3}$; and the sum of the quantities of electricity lost by the first piece and acquired by the other two, will be still equal to zero, as in the case of the two pieces.

If we add a fourth piece, which will be of zinc, it must have an unit more than that of the copper over which it is immediately placed: as this excess can be acquired only at the expense of the inferior pieces, since the pile is insulated, we shall have:

For the lower piece, which is of copper, -1: for the second piece, which touches it, and which is of zinc, 0; that is to say, it will be in its natural state;

For the third piece, which is of copper, and which is separated from the former by a piece of moist card, 0; it will be also in its natural state;

Lastly, for the upper piece, which is of zinc, and which is in contact with the preceding, -1.

Pursuing the same reasoning, the electric state of each piece of the pile will be found by supposing it insulated, and formed of any number of elements; the quantities of electricity will increase for each of them, from the base to the summit of the column, according to an arithmetical progression, the sum of which will be equal to zero.

For the greater simplicity, if we suppose that the number of the elements is even, it will be easy to ascertain, by a very simple calculation,

That the lower piece which is of copper, and the upper piece which is of zinc, must be equally electrified, one more and the other less; and the case will be the same in regard to the pieces taken at an equal distance from the extremities of the pile.

The electricity, before it passes from positive to negative, will become null; and there will always be two pieces, one of zinc and the other of copper, which will be in the natural state. They will be in the middle of the pile: this is what has been seen in the case of four pieces, for example.

 Let

Let us now ſuppoſe that the communication is eſtabliſhed between the lower part of the pile and the common reſervoir: it is evident that in this caſe the lower piece of copper, which is negatively electrified, will tend to reſume from the ground what it has loſt; but its electric ſtate cannot be changed without that of the upper pieces varying, ſince the electric difference of the one from the other muſt always be the ſame in the ſtate of equilibrium. It is neceſſary, then, that all the negative quantities of the lower half of the pile ſhould be neutralized at the expenſe of the common reſervoir; and then it will reſult:

That the lower piece, which is of copper, will have the ſame degree of electricity as the ground, which ſhall be repreſented by 0;

The ſecond piece, which is of zinc, and which immediately touches the preceding, will have + 1;

The third, which is of copper, and which is ſeparated from the lower one of zinc by a piece of moiſt paſteboard, will have, like it, + 1;

The fourth, which is of zinc, and which touches the preceding, will have + 2;

And the quantities of the electricity of the different elements will increaſe in this manner according to an arithmetical progreſſion.

If the ſummit of the pile be then touched with one hand, and its baſe with the other, theſe exceſſes of electricity will be diſcharged through the organs into the common reſervoir; and will give a ſhock the more ſenſible, as, this loſs being repaired at the expenſe of the ground, there muſt thence reſult an electric current, the rapidity of which, greater in the interior of the pile than in the organs, which are imperfect conductors, permits the interior part of the pile to reſume a degree of tenſion approaching that which it had in the ſtate of equilibrium.

The communication being ſtill eſtabliſhed with the common reſervoir, if the ſummit of the pile be brought into contact with the upper plate of a condenſer, the lower one of which touches the ground, the electricity, which at that extremity was in a very weak degree of tenſion, will paſs into the condenſer, where the tenſion may be conſidered as null; but the pile being inſulated only at the expenſe of the common reſervoir, the new quantities of electricity, recovered by the upper plate, will paſs into the condenſer like the preceding, and will be there, at length, accumulated in ſuch a manner, that, by ſeparating the collecting plate, very ſenſible electrometric ſigns, and even ſparks, may be extracted.

In regard to the limits of this accumulation, it is obvious that it depends on the thicknefs of the fmall ftratum of gum which feparates the two plates of the condenfer; for, as in confequence of this thicknefs the electricity accumulated in the collecting plate cannot act but at a diftance on that of the lower plate, it is always more confiderable than that which forms an equilibrium with it in the latter; and hence there refults in the collecting plate a fmall tenfion, which has here for its limit the tenfion exifting in the upper part of the pile.

As the electricity of the column is accumulated in the condenfer, it will be accumulated in like manner in the interior of a Leyden flafk, the exterior of which communicates with the common refervoir; and fince, in proportion as the pile difcharges itfelf, it is recharged at the expenfe of the fame refervoir, the flafk will be equally charged whatever be its capacity; but its interior tenfion can never exceed that which takes place at the fummit of the pile: if the bottle be then removed it will give a fhock correfponding to that degree of tenfion; and this is what is confirmed by experience.

Such muft be the ftate of things, neglecting, as very fmall, the action proper to the water on the metals; fuppofing,

1ft, That the tranfmiffion of the fluid takes place from one pair to the other in the infulated pile through the pieces of moift paper by which they are feparated, even when there exifts no communication between the two extremities of the column:

2d, That the excefs of electricity which the zinc takes from the copper is conftant for thefe two metals, whether they are in their natural ftate or not.

C. Volta fupports the firft propofition by an experiment which we have already mentioned, and in which the condenfer is charged on bringing the collecting plate, covered with a piece of moift paper, into contact with the copper extremity of a metallic plate, the other or zinc extremity of which is held between the fingers.

In regard to the fecond fuppofition, it is more fimple than might be imagined; but a feries of very nice experiments, which we had not opportunity of making, would be neceffary to afcertain how far it is agreeable to nature.

Hitherto, for the fake of precifion, we have fuppofed the pile to be compofed of copper and zinc: the fame theory, however, is applicable to any two metals whatever; and the effects of the different kinds of apparatus which they will ferve to form, will depend on the differences of the electricity which

which would be eſtabliſhed between them at the moment of contact.

What we have ſaid extends alſo to all other bodies between which there may exiſt an analogous action: thus, though this action may appear in general very weak between liquid and metallic ſubſtances, there are however ſome, ſuch as the alkaline ſulphurets, the action of which with the metals becomes very ſenſible: the Engliſh chemiſts, therefore, have been able to ſupply by theſe ſulphurets one of the metallic elements of the pile; and before them profeſſor Pfaff, of Kiel, employed them for this purpoſe in his experiments.

In this reſpect, C. Volta has diſcovered between the metallic ſubſtances a very remarkable relation, which renders it impoſſible to conſtruct a pile with theſe ſubſtances alone. We ſhall here explain them according to his account, but we have had no opportunity of confirming them.

If the metals be arranged in the following order—ſilver, copper, iron, tin, lead, zinc,—each of them will become poſitive by the contact of that which precedes it, and negative with that which follows it: the electricity will paſs then from the ſilver to the copper, from the copper to the iron, from the iron to the tin; and ſo on.

Now the property in queſtion conſiſts in this, that the moving force of the ſilver to the zinc is equal to the moving forces of the metals comprehended between them in the ſeries: hence it follows that, by placing them in contact in this order, or in any other at pleaſure, the extreme metals will be always in the ſame ſtate as if they immediately touched each other; and conſequently, ſuppoſing any number of elements whatever thus diſpoſed, and of which the extremities are for example ſilver and zinc, we ſhall have the ſame reſult as if theſe elements were formed of theſe two metals; that is to ſay, there will be no effect, or it will be that which would have been produced by one element.

It has hitherto appeared that the preceding property extends to all ſolid bodies, but it does not ſubſiſt between them and the liquids: hence it happens that a pile may be conſtructed by the medium of the latter. From this reſults the diviſion which Volta makes of conductors into two claſſes; the firſt comprehending ſolid bodies, and the ſecond liquids.

It has not yet been poſſible to conſtruct a pile but by a proper mixture of theſe two claſſes: it cannot be done with the firſt alone, and we are not yet ſufficiently acquainted with the mutual action of the bodies which compoſe the ſecond, to determine whether the caſe is the ſame in regard to them.

We have ſuppoſed that the moiſt paſteboards placed between the elements of the pile are moiſtened with pure water. If inſtead of water a ſaline ſolution be employed, the ſhock will become incomparably ſtronger; but the tenſion indicated by the electrometer does not appear to increaſe in the ſame ratio. C. Volta has proved to us this fact by the help of his apparatus formed of cups, by pouring ſucceſſively into them pure water and acidulous water.

He concludes from this experiment that acids and ſaline ſolutions favour the action of the pile, chiefly becauſe they increaſe the conducting property of the water with which the paſteboard is moiſtened. In regard to the oxidation, he conſiders it as an effect which eſtabliſhes a more intimate connection between the elements of the pile, and thus contributes to render its action more energetic.

Such nearly is the ſubſtance of the theory of C. Volta reſpecting that electricity called *galvanic.* His object has been to reduce all the phænomena of it to one, the exiſtence of which is now fully confirmed; it is the development of metallic electricity by the mutual contact of metals. It ſeems to be proved by theſe experiments, that the peculiar fluid to which muſcular contractions and the phænomena of the pile were for ſome time aſcribed, is nothing elſe than the common electric fluid put in motion by a cauſe reſpecting the nature of which we are ignorant, though we ſee its effects.

Such is the fate of the ſciences, that the moſt brilliant diſcoveries only open a more extenſive field for new reſearches. After having aſcertained and eſtimated, as we may ſay, by approximation the mutual action of the metallic elements, it remains to determine it in a rigorous manner, to diſcover whether it is conſtant for the ſame metals, or whether it varies with the quantities of electricity they contain, and with their temperature. We muſt aſcertain with the ſame preciſion the peculiar action which the liquids exerciſe on each other and on the metals. It will be then that we can eſtabliſh our calculation on exact data; that we can diſcover the real law followed by the diſtribution and motion of the electricity in Volta's apparatus, and complete the explanation of all the phænomena it exhibits. But theſe nice reſearches require inſtruments more correct than any yet invented by philoſophers to meaſure the force of the electric matter.

In a word, it remains to examine the chemical effects of the current of electricity, its action on the animal economy, and its relation with the electricity of minerals and fiſhes; reſearches which, from the facts already known, cannot fail to be of very great importance.

When

When a ſcience, already far advanced, has made an important ſtep, new connections are formed between the branches of which it is compoſed: we are always fond of looking backwards to meaſure the field it has paſſed over, and to ſee how the human mind has advanced in it. If we thus go back to the birth of electricity, we ſhall find it, at the commencement of the laſt century, confined merely to the phænomena of attraction and repulſion alone. Dufay firſt aſcertained the conſtant laws to which they are ſubjected, and explained their apparent ſingularities. His diſcovery of two kinds of electricity, reſinous and vitreous, laid a foundation for the baſis of the ſcience; and Franklin, by preſenting it under a new point of view, made it the foundation of his theory, to which all the phænomena, even that of the Leyden flaſk, were naturally referred. Æpinus completed the proofs of this theory, brought it to perfection by applying to it calculation, and by the help of analyſis attained to thoſe phænomena which C. Volta has ſo happily employed in the condenſer and electrophorus. The rigorous law of electric attraction and repulſion, ſtill wanting, was eſtabliſhed by exact experiments, and, connecting itſelf with that of magnetiſm, was found to be the ſame in regard to celeſtial attractions. It is well known that C. Coulomb is the author of this diſcovery.

At length appeared the galvanic phænomena, ſo ſingular in their progreſs and ſo different in appearance from every thing known before. At firſt, a peculiar fluid was created to explain them; but C. Volta, by a ſeries of ingenious experiments conducted with ſagacity, propoſes to refer to one cauſe the development of metallic electricity; to employ them for the conſtruction of an apparatus which will allow their force to be augmented at pleaſure; and connects them, by his reſults, with important phænomena of chemiſtry and the animal economy.

In conſequence of a requeſt made by one of your members, and which you referred to a commiſſion, we propoſe that the gold medal of the Inſtitute be awarded to C. Volta, as a teſtimony of the ſatisfaction of the claſs for the noble diſcoveries with which he has enriched the theory of electricity, and as a mark of its gratitude for having communicated them to it.

LI. *Thoughts on the ſuppoſed Variations in the Axis and Poles of the Earth. By Profeſſor* BODE, *of Berlin**.

IT is highly worthy of remark, that, notwithſtanding the harmony which prevails in the courſe of the planets round the ſun, and in their rotation, as well as in regard to the parallelifm of their axes, great variations are found in the inclination and poſition of the latter, and in the periods of the rotation of theſe bodies.

This depends on the different influence which the ſun has on a planet during its rotation, or the variation of its aſtronomical ſeaſons, and on the inclination of its axis in regard to its orbit; for the ſun, in his courſe from the one tropic to the other, paſſes over an arch equal to twice the complement of this inclination; and therefore the leſs the inclination, he muſt advance nearer to the poles, and employ much leſs time in proceeding from the equator towards either tropic.

Now, as it is known from obſervations that there are ſome planets the axes of which have a much greater or much leſs inclination than that of our earth, the great variations which muſt naturally thence reſult in regard to the beneficent influence of the ſun's light and heat, will give a quite different nature to their fields, productions, and inhabitants.

The inclination of the axis of our earth, which is 66½ degrees, ſeems to be well adapted for promoting its fertility and rendering it habitable, as the ſun employs ſix months in moving from the equator towards either pole, and again returning; by which means the effect of his rays in producing that heat and light ſo indiſpenſably neceſſary to the animal, vegetable, and mineral kingdom, increaſes and decreaſes in a much ſlower and gradual manner. The two temperate zones occupy the greater part of the earth's ſurface, and theſe remain habitable as far as poſſible towards either pole.

The direction and inclination of the axes of the different planets are as little proportioned to their different diſtances from the ſun, as their periods of rotation, ſize, denſity, maſs, and ſphericity; as is well known by obſervations which have been made. Now, as there muſt be ſufficient grounds for this arrangement, it may be aſcribed, in my opinion, to the matters of different ſpecific gravity of which the earth and the other planets are compoſed, and to their mixture; in conſequence of which, immediately after their formation, their

* From *Der Geſellſchaft Naturforſchender Freunde zu Berlin Neue Schriften*, vol. ii. 1799.

hemiſpheres

hemiſpheres on each ſide of their orbit, which always divides the body in the middle, were put in perfect equilibrium.

Our earth as well as every other planet is carried round the ſun by the radius vector, (the ſtraight line drawn from the planet to the ſun,) as the centripetal forces continually act according to that line, or in a perpendicular direction. But this line of attraction, during the revolution of the earth, on account of the continual paralleliſm of its axis, forms with the latter very different angles; and yet the duration and period of the rotation remain unaltered. This great difference in the angles has therefore no influence whatever in this reſpect; and it appears that there exiſts ſome other great power capable of preſerving the direction and inclination of the axis unchanged, and which, in my opinion, may be aſcribed alſo to the ſun.

Our earth during its annual courſe, revolving from weſt to eaſt, turns in ſucceſſion every part of its ſurface to the ſun, and at the ſame time revolves from eaſt to weſt on a line paſſing through its centre perpendicular to the plane of its orbit; or paſſing through the poles of the ecliptic, as a ſecond axis, for the purpoſe of conſtantly maintaining the paralleliſm of its proper axis of rotation. Now, if it can be admitted that the ſun produces this revolution of the earth in the plane of the ecliptic correſponding with the arch of its orbit, this, in my opinion, is a more natural explanation of the paralleliſm of the earth's axis than when it is conſidered merely as the conſequence of a certain eſtabliſhed poſition of it incapable of alteration; as this principle is applicable only to the action of mechanical powers on bodies that move and revolve in a ſtraight line; whereas the ſun is continually drawing our earth from the ſtraight line, and carrying it around it in a cruciform manner.

According to this theory, we may eaſily conceive how it is poſſible that a planet the axis of which, by the equilibrium of its heterogeneous parts eſtabliſhed at its creation, is put into a certain direction, ſubjected to certain laws, may, by the powerful influence of the ſun, be made to deviate from it ſo that its axis ſhall aſſume another poſition and inclination, or, in other words, change its poles.

Our earth, in conſequence of ſlow or ſudden variations of the laſt kind, would produce in the heavens the following phænomena:

1ſt, If the inclination of the earth's axis ſhould change, the zodiac would be ſtill the ſame, but the obliquity of the ecliptic would be greater or leſs; the equator would paſs through other ſtars; and, in this caſe, all countries lying

 under

under the ſame parallels of latitude would experience a like change in regard to the duration of the ſeaſons.

2d, If the axis of the earth, without changing its inclination, ſhould acquire a new direction towards ſome other quarter of the heavens, the two points ♈ and ♎, where the ecliptic interſects the equator, and all the other circles connected with it, would paſs through other points of the ecliptic. This would make no change in the duration of the ſeaſons, or the ſtate of the ſun at noon.

3d, If the inclination as well as poſition of the axis ſhould be changed, a reſult compounded of both the above caſes would take place.

4th, But if the earth ſhould be carried around the ſun in another plane, the reſult would be a new apparent courſe of the ſun. The ſeaſons would remain the ſame, or acquire ſome change in their duration, according as the axis ſhould be inclined as at preſent, or in a different manner towards the new orbit, and all the countries of the earth would participate in this change. Theſe four caſes, however, have no influence on the elevation of the pole of any place.

5th, On the other hand, ſhould the poles of the earth change their place, the obliquity of the ecliptic would alſo be changed, the equator would paſs through other countries, and the elevation of the pole of theſe countries would be changed. This change of the pole, however, can take place only in the direction of ſome meridian; and conſequently from north to ſouth, and from ſouth to north. By theſe means one half of the equator would be carried over northern and the other over ſouthern countries; and therefore all countries would not equally participate in the changes thence occaſioned in the duration of the ſeaſons; at leaſt, all countries lying under the ſame ſouthern or northern zones could not come together into the torrid zone, or into an uniform ſituation.

If the alteration of the pole ſhould take place in the direction of the meridian that paſſes through the ſolſtices, neither the ecliptic nor the different quarters of the earth would experience any change; the obliquity of the former only would be altered as well as the poſition of the equator in proportion to the above alteration. Should the alteration of the pole take place in the direction of any other meridian, a change would be produced in the different quarters of the globe, the obliquity of the terreſtrial equator, and the ſigns of the ecliptic. In the laſt place, ſhould the pole move along the meridians paſſing through either of the equinoctial points, the above phænomena would take place, and the points of the ecliptic,

ecliptic, 0° ♈ and ♎, would be transferred to ♋ and ♑, and north and ſouth to eaſt and weſt.

A queſtion now ariſes, whether any traces exiſt on the ſurface of our globe which ſeem to ſhow that the inclination of the earth's axis has ever changed either ſuddenly or progreſſively, or whether the pole has altered its place? On the firſt view, it would appear that this queſtion ought to be anſwered in the affirmative.

Many naturaliſts and geologiſts, in order that they might more eaſily explain the general deluge, and why marine productions are often found at a conſiderable depth under the earth's ſurface, or on the tops of high mountains; how the remains of auſtral plants, and of terreſtrial and aquatic animals, could have been conveyed to the northern regions, and buried there in the boſom of the earth; how the different ſtrata of mountains have been formed, and other phænomena of the like kind, have been able, as is well known, to contrive no better hypotheſis than to ſuppoſe that ſome derangement of the poles of the earth, or changes in the inclination of its axis, muſt have taken place in former times.

But, in my opinion, the determination of this queſtion belongs rather to aſtronomers than to geologiſts, becauſe by the former it may be conſidered under a more general point of view, viz. what connection exiſts between the poſition of the earth's axis and the ſun, and the attractive power in the ſolar ſyſtem; and what obſervations have been made in regard to the poſition and direction of their axes, from which, by analogical reaſoning, we may explain the relation between cauſes and effects when ſuch changes take place.

The ableſt aſtronomers and geometricians have ſhown that the ſmall annual receſſion of the equinoctial points to the weſt, of about 51 ſeconds, and the revolution of the earth's axis around the poles of the ecliptic, which thence follows in the courſe of 25,700 years, ariſe from the united action of the attractive power of the ſun and moon on the ſpheroidal form of the earth. As long, therefore, as this attraction of the ſun and moon acts in an uniform manner, it ſeems to be impoſſible that any continued or very perceptible changes in regard to the earth's axis can take place.

It is, however, found by obſervations, that in the courſe of many centuries an alteration, though very ſmall, has taken place in the inclination of the earth's axis. At preſent it is 23 minutes greater than it was in the time of Hipparchus, that is, above 2000 years ago.

In conſequence of this obſervation it has been apprehended that the ecliptic may, at ſome future period, coincide with the

the equator; the ſun, of courſe, be continually in the equator; and the axis of the earth acquire a perpendicular direction; by which means the cold of the polar regions and in the temperate zones will become greater, and conſequently the fitneſs of the earth for being inhabited, as well as its culture and fertility, muſt decreaſe.

But La Grange and La Place have freed us from any uneaſineſs on this head, as they have proved, by ingenious calculations and concluſions, that this ſmall change in the inclination of the earth's axis is an effect of the mutual attraction of the planets on the orbit of our earth; that ſince the time of Hipparchus it has contributed in an uniform manner to the decreaſe of the obliquity of the ecliptic, or the increaſe of the above inclination; that it does not always continue; but that, properly, the above ſuppoſed axis of the earth's rotation perpendicular to the ecliptic, in conſequence of this action of the planets, moves in very long periods around the poles of the orbit of the earth which lie in the neighbourhood of the poles of the ecliptic.

According to theſe obſervations, the variation in the obliquity of the ecliptic is merely a nutation of the earth's axis, which, ſince the time of Hipparchus, has occaſioned a very ſmall decreaſe in it, but will afterwards become ſtationary, and then produce an increaſe.

Schubert, of Peterſburgh, has found, by La Grange's formula, that the obliquity of the ecliptic in the period of 65,000 years always remains between 20° 43′ and 27° 45′. At preſent it is about 43 minutes leſs than the mean of the above two quantities, and will ſtill decreaſe, for 4900 years, to 22° 53′; after which it will again increaſe.

Hence may be deduced very important conſequences in regard to the durable ſtate of our earth, on which diſtant bodies, in the courſe of its annual revolution, exerciſe an action; but theſe bodies, on account of their ſituation and great diſtance, are able to produce only very ſmall and periodical nutations, by which the maintenance of the whole is promoted, and the earth preſerved from great and diſaſtrous changes. As long as the preſent conſtitution of the ſolar ſyſtem remains the ſame, theſe ſmall and harmleſs periodical nutations will take place.

But if the earth's axis and its poles are ſecured by ſuch powerful bonds from all ſudden or progreſſive changes, thoſe apparent changes and revolutions which we obſerve at and under the ſurface of our earth muſt be referred to other cauſes than a change in the earth's axis, or diſplacement of its poles; and this will be ſtill more evident when it is conſidered fidered how trifling fuch changes are in regard to the whole earth, and whether its centre of gravity could by thefe means fuffer any alteration.

A curfory view of geology will fhow how little we know of the interior parts of the earth, and how fmall a portion of it is fubject to our dominion and refearch. Of the 21,951,022 fquare leagues, which is the extent of the earth's furface, the lands that rife above the ocean comprehend only about one-third, or feven millions. Now if we fuppofe, which perhaps ought to be taken into account, that the height of the land above the ocean is 6000 feet, or 1 mile, (though few lands are fo high;) and that the depth of the ocean is the fame; this in a globe of one inch diameter would fcarcely amount to the fiftieth part of a line, or the fifth part of a moderate fized grain of fand; and this whole external cruft, of 6000 feet in thicknefs, contains only the 1100th part of the whole number of cubic miles, which form the folid content of the earth.

Now all thofe traces of revolutions in the earth which geologifts have been able to obferve, have been found either in the above-mentioned cruft, into which no one has ever penetrated above a fourth part of that depth, (for the greateft depth to which mines have been dug is not more than 1500 feet;) or in the bowels or at the top of moderate fized mountains, the height of which on a globe a foot in diameter would not amount to a grain of fand.

Can it be fuppofed that revolutions have taken place in this thin cruft of the earth capable of altering its centre of gravity, and at the fame time the place of its poles and its axis, fo much as fome have fuppofed, in order to account for various phænomena, fuch as that of the bones of the elephant, &c. being dug up in Germany, and even in Siberia; or if, by fome violent action of the powers of nature in the univerfe, mountains have been feveral times overturned, and fea and land changed their fituation; would thefe cataftrophes, which muft have occafioned great devaftation among the human race at the periods when they took place, have been able in any manner to change the pofition of the earth's axis and of its poles? By no means: they muft have been of much lefs confequence, in that refpect, than the devaftation which an infect would occafion in a globe a foot in diameter by gnawing the paper with which it is covered: and who will affert that the centre of gravity of fuch a globe would by thefe means be deranged?

A change in the pofition of the earth's axis, or of its poles, can be fuppofed to take place only when the whole mafs of which

which it consists is entirely inverted, and its homogeneous and heterogeneous parts mixed together in confusion. But in this case, the exterior crust of the earth which we inhabit would be wholly transformed; mountains, countries, and seas, would change their places, and be conveyed to the height or depth of a hundred miles perhaps, and more: and how is it possible that man, who can penetrate only to a very small depth in the earth, should be able to observe such remains of the old world?

Besides, the globe, which is of a spheroidal form, revolves round its less diameter, which is five miles shorter. Now, if the angle of the inclination of this axis should be suddenly or gradually changed, its rotary motion would not be deranged, but the obliquity of the ecliptic would be exposed to changes, of which, according to observations made at all periods, no traces have been found. But if the poles of the earth should leave their place, a new axis would be produced, the direction of its diurnal rotation would form another equator, and the spherical form would be changed. But the immense centrifugal force would be continually producing the most dreadful revolutions in the land and sea, and the remains of the antient population would be buried so deep in the earth as to escape the researches of succeeding generations.

The poles of our earth, at present, seem to occupy the most commodious positions; for as, on account of the cold, they are uninhabitable to beings of our species, whatever be the inclination of the earth's axis, the ocean has been assigned them as their place, and all the lands of the earth are situated around them, that, during the daily rotation and annual revolution of the earth round the sun, its surface may as much as possible be exposed to the beneficent effects of that luminary in his course from the equator towards the poles. If we should transfer the north pole, for example, to the middle of Asia, the south pole would fall in South America, and an immense tract of cultivated land, on account of the cold, would be converted into uninhabitable deserts. It is, however, highly probable, that the present poles, since the formation of the earth, as we are at present acquainted with its surface, consisting of sea and land, have always had the same inclination.

As far as certain astronomical observations go back, and the antiquity of these amounts to 4450 years, when a Chinese astronomer observed (exactly according to the precession of the equinoxes) the star α in the northern Dragon in the neighbourhood of the north pole, no phænomena have taken place

place in the heavens which give reason to suppose any considerable variation in the inclination of the earth's axis, or in the place of the poles. What Herodotus therefore says, respecting the legends of the Egyptian priests, is mere fables; for if the sun, in the course of 11000 years, actually changed the place of his rising and setting three times, so often must the place of the poles, and consequently the climate of Egypt, have been changed. But supposing this to have been the case, how could the inhabitants of so level a country preserve themselves from utter destruction during the repeated transformation of land into water, which must have been the consequence of these changes? By a mere increase or decrease in the present inclination of the earth's axis, which is $66\frac{1}{2}^{\circ}$, Germany, for example, could never be transferred into what is called the torrid zone. In the first case, if the inclination should be changed to 90°, the sun in summer would be removed about $23\frac{1}{2}^{\circ}$ further towards the south from that country. In the second case, if the inclination should decrease to 30°, the sun would go to the distance of 60° from the equator, and in summer pass twice over Germany in a perpendicular direction; but in winter he would be invisible for a month, and the cold there would be much more intense than at present. On this account, neither the austral plants nor elephants could ever thrive in such a climate.

If Germany was ever situated in the torrid zone, or near the equator, the north pole must have been situated in the Pacific ocean between Asia and America, and the south pole at the southern extremity of Africa, more than 3000 miles from the places which they occupy at present. The north-east part of Asia must in that case have been nearer the north pole than at present, and Siberia in the temperate zone.

But as the bones of the elephant have been found under the earth in the northern parts of Siberia as well as in Germany, both countries must have been situated in the torrid zone: but, in this case, the north pole must have been situated in California, and the south pole in Madagascar, above 4000 miles from their present place. If the poles were ever in that situation, it may be asked, If elephants existed then in Siberia and Germany, where were the forefathers of those which now exist in the East Indies and Africa? as these countries at that period must have been in the middle of the south temperate zone, or partly in the frigid zone.

Thus there are manifest contradictions in so readily supposing a displacement of the pole, and when the revolutions which such a change must have produced, in regard to the surface of the earth and sea, are not taken into consideration.

A natural

A natural conſequence follows. If our northern diſtricts of the earth were ever ſituated in the torrid zone, a part of the preſent torrid zone (for it is not poſſible that this ſhould have been the caſe with the whole of it) muſt have occupied the ſouth temperate zone, or the frigid zone. If elephants could exiſt in thoſe ſouthern diſtricts, at that time forming the temperate and frigid zone, or if Germany and Siberia were ſuddenly tranſported more than four thouſand miles towards the north pole, could theſe animals withſtand this general revolution of the ſurface of the earth and ocean? Or, during this cataſtrophe, were ſome of theſe ponderous animals tranſported over the land and ſea to their preſent place of abode, in order to propagate their breed?

Theſe queſtions cannot be anſwered; but are unneceſſary, if we are convinced, from the principles here explained, that ſuch diſplacements of the poles of the earth are not admiſſible.

But as the bones of elephants and the remains of auſtral plants are found under the earth in northern countries, this may be explained in the moſt natural way, by ſuppoſing that formerly a ſpecies of theſe large animals and auſtral plants exiſted in the temperate north latitudes; for it is certain that many petrified ſhells and plants, as well as the impreſſions of inſects, are found under the earth, the originals of which are unknown; or that the preſent temperate zone enjoyed in the antient world a warmer phyſical temperature.

Are there not ſome proofs, generally ſpeaking, that the maſs of the cold in the north increaſes, and that the ice is more and more accumulated in the neighbourhood of the north pole; that even our vegetables do not poſſeſs the ſame degree of perfection, and betray ſymptoms of degeneration, though the ſmalleſt diſplacement has never taken place in the poles of the earth, and no change in the inclination of the earth's axis that could have an influence on the climate? Is not the cold more widely diffuſed in the ſouthern hemiſphere than in the northern, though both are equally expoſed to the ſun, and though the ſun in ſummer is many thouſand miles nearer the former than the latter?

The phyſical climates of the earth however, and particularly the two temperate and two frigid zones, as they are far more expoſed to the varied influence of the ſun's rays than the torrid zone, appear to have been ſubjected to great variations at long intervals. But theſe depend much on local and temporary circumſtances, and are not determined merely by the different effects of the ſun's influence, in conſequence of his annual return. They are at the ſame time, owing to the chemical fermentation in the mixture of the component

component parts, occaſioned by the acceſſion of heat; to the ſolution and decompoſition of theſe ſubſtances ſo different in their nature, evaporated from the ſurface of the earth, whether land or water, which is filled with animal and vegetable bodies; and to theſe vapours being in part conveyed into the atmoſphere, and there converted into other matters, and being again ſent back to it in the form of fertilizing moiſture.

In the laſt place, all our obſervations have hitherto ſhown that without the globe of the earth there is no power capable of producing ſuch irregularities in its poles and its axis.

It was long believed indeed that comets were capable of producing ſuch revolutions. Who knows, ſaid ſome, whether one of theſe bodies may not have approached too near to our earth; and, being furniſhed with too great attractive power, may have deranged its axis or its pole, or dragged its train over us; and, ſetting every thing on fire, may have laid waſte ſea and land, altered the earth's centre of gravity, and occaſioned a general devaſtation?

But our preſent more accurate knowledge of the orbits and nature of theſe celeſtial bodies will never admit of any ſuppoſition of this kind. It is known from certain experience, and the conſequences thence deduced, that theſe maſſes have very little ſpecific gravity, and therefore muſt poſſeſs a very weak attractive power. According to every appearance, they ſeem to be compoſed of minute moleculæ mixed with the fine ethereal matter of light, and their trains are only highly ſubtile luminous and tranſparent ſubſtances; ſo that we have very little to apprehend from either of them.

No hiſtorian worthy of credit ever makes mention of the diſaſtrous effects of comets on our earth; and mankind are now too enlightened to be led into error by what the prejudice, ſuperſtition, and ignorance of former times have handed down to us on this ſubject.

Even in modern times comets have paſſed very near our earth, without producing any remarkable phænomena, either in regard to the atmoſphere or the common courſe of the weather; and much leſs were the poles or the axis of the earth any way affected by their approach; otherwiſe ſome traces of it would not have eſcaped the reſearches of the modern aſtronomers. On the other hand we know, by experience, that comets which have paſſed near to our earth, or the other planets, in their way to the ſun, have ſuffered ſome perturbation in their orbits by the attractive power of the latter.

I will not however aſſert that our earth, before it was completely formed and rendered habitable, and before every

part

part of it was brought into complete equilibrium and ſtability, may not have experienced ſome changes and variations, in conſequence of which its poles and its axis may have been in ſome meaſure deranged; but all the traces of ſuch changes hitherto diſcovered in the thin cruſt within the reach of our obſervation ſeem to prove that they were produced by partial changes in the ſituation of certain parts of the land or ſea, which were in no manner able to diſplace the earth's centre of gravity, or to derange its poles and its axis.

In the courſe of a thouſand years, revolutions of this kind may take place in regard to the earth's ſurface, in conſequence of its organization, on account of the many chemical mixtures and ſolutions which are capable of continually exciting the active powers of nature; but the equilibrium, and powerful law of mutual attraction, by which one world is connected with another, may free us from all apprehenſion in regard to any derangement of the poles or axis of the earth capable of being followed with diſaſtrous conſequences.

LII. *Obſervations on the Manner in which the Spider* (Aranea Diadema) *ſpins its Web. By* M. C. G. Lehmann *.

It muſt be allowed by thoſe who have any knowledge of entomology, that the natural hiſtory of ſpiders has been much neglected; for, notwithſtanding the great progreſs which has been made in that ſcience, we are as yet very little acquainted with the organiſation, mode of life, and propagation of the moſt common kinds of theſe animals; and many ſpecies make a figure in our ſyſtems and cabinets which differ from each other only by their age or their ſex. Theſe inſects, ſo remarkable on account of their induſtry and manner of life, are viewed with a ſort of contempt and averſion, which would be inexplicable did we not know the great power of thoſe impreſſions and prejudices which we receive in our youth. Theſe in general are communicated to us by people who are the leaſt qualified to give the mind that direction neceſſary for the purpoſes of life, and who in particular ſeem to have united to inſpire us with a dread of ſpiders. Hence the complaint of ſo many naturaliſts, that this averſion always deters them from obſerving and accurately examining theſe inſects; and thoſe who have undertaken to do ſo have

* From *Der Geſellſchaft Naturforſchender Freunde zu Berlin Neue Schriften*, vol. iii. 1801.

generally

generally been obliged to take a great deal of trouble to overcome this antipathy *.

But however much mankind may have beheld with aversion spiders and other vermin, they could not help admiring the ingenuity of the latter in spinning their webs, and the order and regularity displayed in these productions. As far back therefore as natural history can be traced, in the time of Aristotle, and in the fragments of his predecessor †, we find, if not accurate descriptions, at least some mention of the labours of this insect. In like manner, the moderns have thought the spider's web worthy of their attention; and it might consequently have been expected from the accuracy of our naturalists, that they would have given us more minute and completer descriptions. This ought to have been the case in particular with the web of the cross-spider, one of the most striking and most remarkable. I have several times read such descriptions, and studied them with pleasure, because agreeable to nature; but always found this general complaint, that it is still a secret in what manner the cross-spider extends its first thread, in order that, when it has been fastened, it may have a field for carrying on its work. It is well known that the cross-spider disposes its web in a more or less perpendicular direction in some open place. Thus, for example, we often find in a window four or five threads stretched from one side to the other; from these several radii proceed to a common centre; and around this centre, at a distance which may be estimated in general at a few lines, there are several concentric circles, each progressively larger; in the middle of which the spider places itself concealed, to wait until some insect becomes entangled in these threads, when it immediately rushes forth and seizes its prey. Thus far the explanation of the process which the spider employs is very easy, especially as it is known that this insect, wherever it goes, marks its way by a thread, which it leaves behind it; and that this thread, on account of its viscosity, can always be made fast to any object, when the spi-

* It is of importance to consider by what means that aversion commonly called natural, and which is merely the result of improper education, can be overcome. Rosel accustomed himself to view these insects first at a distance. He then considered their webs; and at last looked at the insects themselves through a microscope. Göze first viewed individual parts of spiders, such as the legs, head, &c. till he was at length able to look without any sentiment of aversion at the entire insect. Both these naturalists, by long habit, so far overcame this aversion, that they could handle and examine spiders with the same indifference as others can flies.

† Democritus. See Plin. Hist. Nat. lib. ii. cap. 24.

der only preſſes it a little, or moiſtens it with a drop of its viſcous liquid.

But theſe webs are commonly found in foreſts and woods, often on the high branches of different trees, not unfrequently ſuſpended between the roofs of different houſes ſtanding near each other, and even often extended over ſtreams and ponds of water, ſo that it is impoſſible to conceive how the ſpider could proceed from the one point to the other, in order to eſtabliſh her firſt thread, and to faſten to it the different radii. Some have endeavoured to account for this circumſtance by the moſt ſingular ſuppoſitions, as is commonly the caſe when philoſophers attempt to explain the phænomena of nature before they have examined them with ſufficient accuracy; but I ſhall not here trouble the reader with theſe hypotheſes, which contradict themſelves *. Accident, to which we are indebted for moſt of our diſcoveries, particularly in natural hiſtory, though we are unwilling to allow it any ſhare in our diſcoveries, gave me an opportunity of making an obſervation which decides the above queſtion. On a warm ſummer's evening I ſaw a croſs-ſpider *(aranea diadema)* let herſelf ſpeedily down from a branch of a tree by her uſual thread; and at the ſame time a multitude of ſimilar threads, which had their origin at the ſame place as the other, floated around; while the air, which was not perceptible to my ſenſes, carried them to a conſiderable diſtance, and even a conſiderable height. Though I approached with the utmoſt gentleneſs, the direction of the floating threads was immediately changed, and in a moment they had reached a neighbouring branch, to which they adhered. The ſpider ſoon obſerved that her floating threads had reached a fixed point, and immediately clambered up to them, having thus given me an opportunity of ſeeing the ſecret of the commencement of her ingenious labour. Guided by this trace, it was now eaſy for me to make my obſervations with greater accuracy; and the reſult of them is as follows. When a ſpider is deſirous of proceeding to a diſtant place, ſhe ſuddenly lets herſelf down by a pretty ſtrong thread, and at the ſame time draws one of her hind legs over the glandular parts which contain the materials of her web. By theſe means ſhe forces from them a great number of ſmall threads, and conſigns them to the air; which, on account of their ſpecific lightneſs, makes them float, ſo that they remain at that

* I ſhall only mention one.—Liſter in his *Hiſt. Animal. Angliæ*, which contains a great many important obſervations on ſpiders, explains this, p. 7, as an *ejaculatio* filorum, and endeavours to make it more evident, by comparing it to the manner in which the ſun emits his rays.

height at which the ſpider firſt throws them out. As ſhe then ſuddenly ſinks down in a retrograde direction, the firſt ones are extended to a conſiderable length.

But if the leaſt breath of air then takes place, they are thereby carried to a conſiderable diſtance, and ſtill become longer, as the inſect can then ſpin them out at pleaſure from her receptacles; ſo that they ſoon find ſome object to which, on account of their viſcoſity, they adhere. The ſpider then clambers up to them, ſtill keeping herſelf in connection with her firſt ſtation, as ſhe always carries with her the thread by which ſhe firſt let herſelf down, and faſtens the ſecond end of it when ſhe has reached a new point of reſt.

I have too little acquaintance with works on natural hiſtory to be able to determine whether this obſervation has before been made by others *; but by ſome queſtions in the Hanoverian Magazine, and by the private information of diſtinguiſhed naturaliſts, it appears that it is not generally known. But whether it be new, or may only ſerve to confirm what has been already remarked, I am happy that I have it in my power to point out to other ſearchers into nature how they may at all times be convinced of the truth of what I have here ſtated. If a perſon takes a ſpider from its web, and places it on the hand, it will endeavour to eſcape from this warm object as a place of danger, and let itſelf down in the manner above deſcribed; but if one of the threads that float about in the air be laid hold of by the other hand, the ſpider ſuffers herſelf to be drawn over by it; and this experiment can be repeated ſeveral times, till at length, ſeared by being ſo often deceived, ſhe throws herſelf looſe, and ſuffers herſelf to fall to the ground.

To theſe obſervations on the manner in which the ſpider begins her web, I ſhall add ſome others, which indeed comprehend but a ſmall part of the important diſcoveries that Quatremere Disjonval pretends to have made; and reſpecting which, ſince the attention of naturaliſts is called to this ſubject, I hope to obtain further information. The account that various kinds of ſpiders foretell changes of the weather ſeveral days before they take place, excited my whole attention; and I was the more deſirous of ſtudying the araneology of the above obſerver, as I was always of opinion that thoſe inſects which have organs peculiarly adapted for perceiving changes in the ſtate of the air † would be capable of

* Swammerdam, in his *Bib. Nat.* p. 24. has deſcribed a ſimilar obſervation; but his deſcription appears to me not very clear.

† See more on this ſubject in my *Commentatio de Senſibus externis Animalium exſ.* p. 37.

giving us the beſt and earlieſt information reſpecting changes in the weather. It was impoſſible for me at firſt to procure Disjonval's work; and my curioſity began to be leſſened when I heard that no perſon was able to comprehend it. For this reaſon I reſolved to make obſervations myſelf; and with this view I collected a great many of the *aranea diadema* and *aranea domeſtica*, as the latter are thoſe which Disjonval muſt have had the beſt opportunity of obſerving when in priſon. The latter ſhowed no alteration in their web or their conduct, but in regard to the former I ſoon had an opportunity of making very important obſervations. I have already mentioned what every body knows, that this ſpider commonly ſits in the middle of its web. I now found that ſhe often extended a thread from the centre of her web, that is the uſual place of her reſidence, to the neareſt corner; in which ſhe concealed herſelf, inſtead of being in the web: and I always remarked, that this change was connected with a change of the weather, as all my croſs-ſpiders made the ſame change together; and immediately after the weather became very raw and cold. It is indeed very natural that thoſe inſects, when they have a preſenſation of bad weather, ſhould endeavour to ſhelter themſelves from it. But my obſervations were too much interrupted by buſineſs and various circumſtances, and I was too ill provided with good inſtruments for comparing the ſtate of the weather and of the atmoſphere, ſuch as barometers, thermometers, hygrometers, &c. to be able to ſay any thing deciſive on the ſubject. What I have ſaid will however furniſh a hint to others who may be diſpoſed to carry theſe reſearches further.

The ſame opportunity enabled me to make another obſervation, of which I do not recollect ever to have heard or read, viz. that the croſs-ſpider regularly deſtroys its web every twenty-four hours, and in the place of the old ſubſtitutes a new one. This it always does in the night-time, but ſuſpends the renovation of it when a cloudy ſtate of the weather affords it no hope of catching any prey, and perhaps when any mechanical obſtacles ſtand in the way*. This obſervation

* That thoſe ſpiders which weave their web in the form of a wheel renew it daily, has been remarked by many obſervers as well as by myſelf. The cauſe may be eaſily conceived from what I have ſaid. For ſeveral years, during the ſummer, I reſided in a houſe ſituated in a garden; and oppoſite to the window, which was generally open, a large ſpider had extended her web. I took great care not to deſtroy it, becauſe it ſerved inſtead of one of thoſe gauze ſcreens which are commonly employed to keep out flies and gnats. The web in the day-time was frequently injured by accidents, but next morning I found it always repaired. One morning, having

observation has thrown great light upon many things relating to the natural history of spiders. It points out the principal method which ought to be pursued in making observations on the weather by means of these insects, as they construct no new web after destroying the old, when impeded by the unfavourable state of the weather. This also enables us to explain why we often observe floating about, during very hot days, prodigious multitudes of spiders webs, commonly known under the name of *gossamer*, and which has given occasion to so much dispute among philosophers; for it may be readily conceived, when we reflect how numerous spiders are, what a multitude of these webs must be let loose, when they all destroy their webs at the same time, in order to construct new ones. During cloudy weather none of them are to be seen, for at such periods none are let loose; and even if there are a few of them in the atmosphere, they acquire heaviness from the moisture they imbibe, of which they are very susceptible, and of course fall to the earth. I was enabled also to determine the dispute respecting the organs of vision in spiders. They undoubtedly have eyes, which are indispensably necessary for the functions they have to perform, and yet they do not seem to observe when a stick or other dangerous weapon is held close to these organs, but they instantly retreat when in the least touched. At present, however, this circumstance can be easily explained, for these insects are nocturnal animals. In the night-time they spin their webs, and during the morning and evening twilight catch the greatest number of insects. In the night they

having got up earlier than usual, I saw the spider at day-break destroy all the concentric threads proceeding from the centre outwards along the radii, breaking off with her feet the cross threads, which she cemented to the radii she had left untouched; and which, by these means, became progressively thicker. She did the same with the principal threads by which the web was made fast; and which, in consequence of the threads added to them, were rendered much stronger. The smallest flies or gnats which attempted to enter through the window were therefore caught fast in the net; but towards noon, or somewhat later, the flies became shyer, and no more were caught. The spider therefore about that time retired to a corner which she had prepared in the window, where she laid her eggs, and where she remained quiet till the next morning. This observation shows that the threads of the web lose their viscosity by the sun's rays and the heat of the weather; and becoming too dry and hard to be fit for catching those insects on which the spider feeds, she is under the necessity of renewing them. On the other hand, the principal threads, which serve only for strengthening the web, have no need of being renewed. Damp weather may perhaps render these webs as unfit for catching insects as the heat does, and therefore the spider thinks it unnecessary to renew her web as long as that temperature continues.

see as well as the owls to catch their prey; and in the day-time, like those birds, they are dazzled by the too great splendour of the light. But at that time they have no need of very acute sight, as they seldom leave their web; and, when they do, are conducted back to it by their delicate sense of touching. Besides, they seldom have occasion for it in the day-time, as their webs are then too visible to the insects which they catch; so that the latter can be better on their guard against them than they can during the twilight.

LIII. *Outlines of a View of Galvanism, chiefly extracted from a Course of Lectures on the Galvanic Phænomena, read at the Theatre of the Royal Institution by Mr.* DAVY *.

I. *Historical Introduction.*

§ 1. THE science relating to the peculiar action of different conductors of electricity on each other, has lately excited a considerable degree of attention in the philosophical world.

Owing its origin to the phænomenon discovered by Galvani †, the production of muscular contraction by the application of metals to the nerves and muscles of animals, it has derived its name from that philosopher.

Galvanism was at first limited in its application to organized bodies; but, in consequence of the labours and inventive genius of experimentalists, our contemporaries, it has gradually become connected with chemistry and general physics; it has afforded powerful instruments of investigation; and its operations have been traced throughout the whole of nature.

In giving an account of the progress of this science, in its relation to the powers of the human mind, it will be sufficient to notice such experiments only as have derived their origin from extensive theoretical views, and such discoveries as have led to accurate generalisations of phænomena already known.

Though the history of galvanism extends only through the period of the last nine years, yet we may notice in it four epochs, each of them distinguished by the development of facts,

* From the *Journals of the Royal Institution of Great Britain.*

† The first fact relating to the action of metals on the animal organs, was observed by Sulzer, who has described the sensation of taste produced by the contact of lead and silver with the tongue, in his *Théorie des Plaisirs*, published in 1767.

facts, variouſly intereſting from their novelty and the extent of their application.

§ 2. Conſidering the firſt epoch as formed by the publication of the fundamental galvanic fact, we may derive the ſecond from the diſcovery of the exiſtence of inorganic galvaniſm. Till the reſearches of Fabroni, Dr. Aſh, and Creve, had been made known, the galvanic influence was generally conſidered as exiſting only in living animal organs. But the diſcovery of the peculiar action of metals in contact with each other upon water, demonſtrated the production of it in arrangements compoſed wholly of dead matter, and laid the foundation for a new claſs of inveſtigations, which have intimately connected the galvanic phænomena with known phyſical effects.

§ 3. The third epoch in the hiſtory of the ſcience is, perhaps, the moſt brilliant and moſt important. It will long be celebrated on account of the diſcovery of the accumulation of the galvanic influence. Before this diſcovery was made, the world, in general, beheld nothing deeply intereſting in galvaniſm: it had no relations to the common wants of life, and the facts that compoſed it were ſo obſcure as to be difficultly comprehended, except by long attention. The galvanic battery of Volta not only gratified the paſſion for novelty by the curious effects it produced, but likewiſe awakened the love of inveſtigation, by diſtinctly exhibiting the analogy between galvaniſm and common electricity.

§ 4. The fourth and laſt epoch in galvaniſm may be conſidered as founded upon the knowledge of the general connexion between the excitement of galvanic electricity, and chemical changes; and it chiefly owes its exiſtence to the labours of Britiſh experimentaliſts *. The diſcovery of the chemical agencies of galvaniſm has led to reſearches which finally cannot fail to elucidate the philoſophy of the imponderable or ethereal fluids. The year that is juſt paſt will long be diſtinguiſhed in the hiſtory of ſcience; ſeldom has phyſical inveſtigation been purſued with greater ardour; and if new facts, by being ſometimes inſulated and incapable of application to eſtabliſhed theories, have perplexed the public mind, yet they have at the ſame time been uſeful to it, by producing a habit of rational and active ſcepticiſm, which cannot fail of becoming, at a future period, the parent of truth.

II. *Of the leaſt complicated Galvanic Arrangements,* i. e. *ſimple Circles.*

§ 1. The conductors of electricity, which, by their action

* Meſſrs. Nicholſon, Carliſle, Cruikſhank and Henry, Dr. Wollaſton, and Major Haldane.

on each other, are capable of producing galvanic effects, may be divided into two claffes *. The one clafs comprifes what may be called perfect conductors, oxidable metallic fubftances and charcoal: the other includes lefs perfect conductors, which are either oxidated fluids, or fubftances containing thefe fluids.

The fimpleft galvanic arrangements require for their formation at leaft two bodies of the fame clafs and one of a different clafs *. With regard to the form of their aggregation, they muft be fo difpofed, that the bodies of the one clafs may be in contact with each other, in one or more points, at the fame time that they are connected in other diftinct points with the body of the other clafs.

§ 2. The fimple galvanic circles may be divided into two general kinds.

The firft is formed by two different metallic fubftances, or one metallic fubftance and charcoal, and a peculiar fluid.

The fecond is compofed by two different fluids and one metallic fubftance.

Thus, if plates of zinc and of filver be made to touch in one point, and be connected together in other points by a portion of common water or of muriatic acid, a galvanic fimple circle is formed of the firft order.

Or if feparate portions of nitric acid and of water, moiftening pieces of cloth or bibulous paper, be brought in contact with each other on a fmall furface, at the fame time that other furfaces of them are connected with different parts of a plate of tin, a circle of the fecond kind is compofed †.

§ 3. All arrangements, however, of two conductors of one clafs with one of the other, are not capable of producing galvanic effects: and even the powers of acting circles are very different in degree. It appears from all the facts, that chemical changes taking place in fome of the parts of the circle are intimately connected with its agencies. For, though a momentary circulation of galvanic influence may poffibly be produced by the contact of three different bodies, yet it appears moft likely that the permanent excitation of it depends upon a certain exertion of their chemical affinities.

The moft powerful circles of the firft kind are thofe compofed of two folids of different degrees of oxidability, and of a fluid capable of oxidating at leaft one of the folids ‡. And, even in the feebleft circles, it appears that fome chemical action is uniformly exerted either by oxidating fluids or folutions of alkaline fulphurets.

Thus filver and gold do not appear to evolve galvanic influence

* Volta. † D. ‡ Ritter.

fluence when in contact with pure water, which is incapable of acting chemically upon either of the metals; though when they are connected with water, holding in ſolution nitric acid, or any other fluid decompoſable by ſilver, they form an active galvanic arrangement *.

And zinc and ſilver, which act very little with pure water, form a powerful combination with water holding in ſolution atmoſpheric air or acids †.

The following table of ſome circles of the firſt kind, in which the different ſubſtances are arranged according to the order of their known galvanic powers, will ſhow how intimately chemical agencies are related to the production of galvaniſm.

TABLE OF SOME GALVANIC CIRCLES,

Compoſed of two perfect Conductors and one imperfect Conductor.

More oxidable ſubſtances.	Leſs oxidable ſubſtances.	Oxidating fluids.
Zinc	With gold, charcoal, ſilver, copper, tin, iron, mercury.	Solutions of nitric acid in water, of muriatic acid and ſulphuric acid, &c. Water holding in ſolution oxygen, atmoſpheric air, &c.
Iron	. . . . gold, charcoal, ſilver, copper, tin.	
Tin	. . . . gold, ſilver, charcoal.	
Lead	. . . . gold, ſilver.	
Copper	. . . . gold, ſilver.	Solution of nitrate of ſilver and mercury. Nitric acid, acetous acid.
Silver	. . . . gold.	Nitric acid ‡.

The moſt active ſingle circles of the ſecond order are thoſe in which the two imperfect conductors are capable of exerting different chemical agencies on the perfect conductor, at the ſame time that they are poſſeſſed of power of action on each other. But even circles in which only one of the fluid parts is decompoſable by the ſolid, are poſſeſſed of power of action.

Thus copper, ſilver, or lead, acts very powerfully when connected in the proper order with ſolutions of alkaline ſulphurets and of nitrous acid, both of which fluids are poſſeſſed of diſtinct chemical agencies upon them §. And

* D. † Fabroni.

‡ Dry nitre, cauſtic potaſh, and ſoda, are conductors of galvaniſm when rendered fluid by a high degree of heat; but the order of their conducting powers has not been yet aſcertained.

§ D.

copper

copper or silver acts, though with less intensity, when water, or a fluid which they are incapable of decomposing, is substituted for one of the chemical agents.

The following table contains some powerful galvanic combinations of the second order, arranged according to the intensity of their action.

TABLE OF SOME GALVANIC CIRCLES,

Composed of two imperfect Conductors and one perfect Conductor.

Perfect Conductors.		Imperfect Conductors.	Imperfect Conductors.
	Copper Silver Lead Tin Iron Zinc	Solutions of alkaline sulphurets, capable of acting on the first three metals, but not on the last three.	Solutions of nitrous acid, oxygenated muriatic acid, &c. capable of acting on all the metals.

§ 4. Arrangements, exactly similar in their action to the common simple circles, may be formed by the combination of more than three conductors. For, that simple galvanic action may be evolved, nothing more is apparently requisite, than that the points of contact between the solid and fluid parts of the circle, *i. e.* the places where chemical affinities are liable to be exerted, be immediately preserved; at the same time that those parts are respectively homogeneal, or composed respectively of similar conductors. Thus zinc, silver, iron, gold, and water, arranged in a circle, in the order of their enumeration, produce action exactly analogous to zinc, gold, and water. And nitrous acid, water, solution of caustic potash, solution of sulphuret of potash, and silver, evolve galvanic influence in the same manner as nitrous acid, water, solution of sulphuret of potash, and silver.

It appears, however, that the length and surface of the conducting series, connecting the exciting parts of the circle, are in some measure related to the quantity of action it is capable of exhibiting. This law, indeed, applies only analogically to perfect conductors; though, with regard to imperfect conductors, it is demonstrated experimentally; as in all cases a diminution of power, in some unknown ratio, is produced by the increase of the length, or, what is apparently equivalent, the diminution of the surface of the chain which they compose.

The limits of surface, and of length, of imperfect conductors in in circles, connected with the maximum and minimum of their activity, have not been yet afcertained; and there is every reafon to believe that they will be different in different imperfect conductors. Thus, portions of fulphuric acid and of nitrous acid will probably form better conducting feries than fimilar portions of water; and a chain of water will perhaps act better than an equal chain of water mixed with alcohol.

From feveral experiments, it would appear that the powers of circles are particularly connected with the extenfion of the furfaces of contact between the perfect and imperfect conductors.

§ 5. All galvanic influence in fingle circles is manifefted either by its efficiency in affecting living animal organs, forming part of the arrangement; or by its power in modifying the chemical changes produced by the action of the perfect on the imperfect conductors.

A. When one fluid part of a powerful fingle circle is made to touch the tongue at the fame time that another fluid part is connected with fome irritable furface of the body, an acid tafte is perceived *, which becomes lefs diftinct in proportion as the contact is longer preferved.

If the eye be made part of the galvanic circuit, a tranfient flafh of light is produced at the moment the chain is completed †; and, when the bared cutis is employed, a faint painful fenfation uniformly denotes the circulation of the galvanic influence ‡.

But the moft diftinct mode of exhibiting, by animal excitement, the exiftence of fmall quantities of galvanic power, is by connecting one part of the circle with a nerve, and another part with a mufcle, in a limb juft feparated from the body of a living animal. In this cafe, whenever the connection of the arrangement is made or broken, mufcular contractions are produced §.

B. In galvanic fimple circles all primary chemical action taking place between the imperfect and perfect conductors is apparently increafed by their galvanic arrangement. Thus zinc, which oxidates when in contact with common water alone, oxidates much more rapidly when forming a galvanic circle with it by means of gold, or other difficultly oxidable metals ||. And tin appears to diffolve fafter in diluted nitric acid, when compofing a circle with it by the contact of gold, than when fingly immerfed in it ¶.

* Sulzer, Volta, Robifon, and other philofophers.
† Mr. Hunter. ‡ Humboldt. § Galvani.
|| Dr. Afh. ¶ Original experiment, D.

C. But

C. But increase of primary chemical action is not the only inorganic effect of galvanism; for it changes the nature of this action in a very peculiar manner. In the oxidating circle with common water, no perceptible quantity of hydrogen is evolved, as in common processes; but an alkaline substance appears to be formed at the point of contact of the least oxidable perfect conductor with the fluid. Thus, if zinc and silver be made to form a circle with distilled water, holding in solution air, for many weeks, a considerable oxidation of the zinc is perceived, without the perceptible evolution of gas; and the water, at its point of contact with the silver, becomes possessed of the power of tingeing green, red cabbage juice, and of rendering turbid, solution of muriate of magnesia *.

In the oxidating circles with acids, gas is not only evolved from the parts of the conductors undergoing chemical change, but likewise from other parts in which no primary action apparently exists. Thus, when zinc and silver form a circle with diluted muriatic acid, gas is not only given out at the points of contact of the acid with the zinc, but likewise at the place where it is connected with the silver †. And in the circle with tin, gold, and diluted nitric acid, nitrous gas is evolved from the gold as well as from the tin *.

D. Indeed in all the single galvanic circles, whenever an oxidating influence is exerted at one of the places of contact of the perfect and imperfect conductors, a deoxidating action appears to be produced at the other place. Thus, when iron, which oxidates rapidly when forming a circle with silver and common water, is arranged with zinc and common water, it remains perfectly unaltered, whilst the zinc is rapidly acted upon.

§ 6. The phænomena exhibited by the simple galvanic circles cannot well be theorized upon, except in the laws of their connection with the more complicated and more striking galvanic facts. And it is from the discovery made by Volta, of the accumulation of galvanism only, that the instruments of investigation are derived, by means of which the nature of this influence is known, and its most important agencies and relations ascertained.

III. *Of Compound Galvanic Circles, or Galvanic Batteries.*

§ 1. The instruments for accumulating galvanic power, or the compound galvanic circles, are composed of the same elements as the simple circles; but those elements are disposed in a different form of aggregation.

* Original experiment, D. † Dr. Wollaston.

To

To compoſe a galvanic battery, ſeries of the conductors capable of forming ſimple circles are required. And they muſt be arranged in ſuch a manner, that the conductors of the ſame claſs in every ſeries may be in contact with each other, in one or more points, at the ſame time that they are reſpectively connected with different conductors of the other claſs, and one of them belonging to the ſame ſeries, a regular alternation being obſerved.

Thus, the order of a compound galvanic circle is, conductor of the one claſs, conductor of the one claſs, conductor of the other claſs; conductor of the one claſs, and ſo on, in uniform arrangement.

§ 2. The compound galvanic circles, like the ſimple circles, may be divided into two general orders.

The firſt order is compoſed of ſeries, containing, at leaſt, two perfect conductors, and one imperfect conductor. The ſecond is formed by arrangements, conſiſting of not leſs than two imperfect conductors, and one perfect conductor *.

Thus, if plates of zinc, and of ſilver, and pieces of cloth, of the ſize of the plates, moiſtened in water or diluted muriatic acid, be arranged in the order of zinc, ſilver, cloth; zinc, ſilver, cloth; and ſo on, till twenty ſeries are perfectly connected, a galvanic battery is formed of the firſt claſs †.

And if plates of copper, and pieces of cloth, moiſtened, ſome in water and ſome in ſolution of ſulphuret of potaſh, be connected in a circle, in the order of copper, cloth moiſtened in water, cloth moiſtened in ſolution of ſulphuret of potaſh, and ſo on, a compound circle will be formed of the ſecond claſs ‡.

The moſt elegant mode, however, of arranging the metals of compound circles with fluids, is by means of veſſels compoſed of electrical nonconductors, ſuch as glaſſes, or boxes formed of baked wood.

Thus, plates of copper and of zinc, paired, and ſoldered together at their edges, may be cemented into a trough compoſed of baked wood, in regular alternation, and in ſuch a manner as to leave a number of water-tight cells correſponding to the number of ſeries. And this arrangement will become active when the cells are filled with water or ſaline ſolutions, and when the extreme metals are properly connected §.

Or, inſtead of the double plates, ſingle plates of copper and of horn, or glaſs, may be uſed in uniform alternation; when the cells muſt be filled with different fluid chemical agents in the regular order, ſo as to be connected together by

* D. † Volta. ‡ D. § Mr. Cruikſhank.

by pieces of moiftened cloth, paffing over the nonconducting plates *.

§ 3. The fubftances moft active in the fingle circles are likewife moft active in the compound circles. And in all cafes, the relative quantities of galvanic power, exhibited by equal numbers of different feries, are, in fome meafure, proportional to the intenfity of the peculiar primary chemical agencies, exerted by the different conductors compofing them, on each other †.

Thus, in the battery with zinc and filver, there is little or no action produced, when the connecting fluid is pure water, or water holding in folution hydrogen gas, which is incapable of acting on the metals †. The action is greater with water faturated with oxygen ‡ than with water faturated with atmofpheric air; and it is moft intenfe when folution of red fulphate of iron, or of nitric acid, is employed.

The tables of the fingle circles will indicate, with the neceffary change of arrangement, the relative powers of the feries forming compound circles.

§ 4. Provided thofe places of contact in the compound circle, in which the peculiar chemical changes are produced, remain permanent, the parts of the feries which do not immediately act chemically on each other, may be connected together in the fame manner as in fingle circles, by means of conductors of their own clafs, without any alteration of the nature of the agencies.

Thus, in the circle with copper, iron, and water, the copper and iron may be connected by means of a wire of brafs §. And even the continuity of the portions of water may be broken by moift mufcular fibre, and other imperfect conductors, without any other change in the effect than a diminution of its intenfity ||.

§ 5. The galvanic influence, when highly accumulated, produces very extraordinary chemical and phyfical effects, and, in many of its appearances, affumes the precife form of common electricity.

A. When in a powerful battery (one, for inftance, containing two hundred feries) the communication, after being broken, is again rendered complete by the contact of two perfect conductors, a flafh or fpark of light is perceived analogous to that produced by electricity §. This fpark, or flafh, when the battery is moft powerful, is capable of paffing through a confiderable ftratum of air ¶, and of inflaming mixtures of oxygen and hydrogen **. When the metallic

* Count Rumford. † D. ‡ Major Haldane. § Volta.
|| Pfaff, Ritter, and D. ¶ Mr. Cruikfhank. ** Mr. R. Boulton.

fubftances,

fubftances, by which it is tranfmitted, are of very fmall volume, it is poffeffed of the power of igniting them, and of making them enter into combuftion when in contact with oxygen *.

B. The galvanic influence, when highly concentrated, affects the electrometer, and is capable of communicating weak charges to the condenfer and Leyden phial †. In all compound circles of the firft clafs, the moft oxidable part of the metallic plates evolves the influence, appearing as pofitive electricity, whilft the leaft oxidable part feems negatively electrified. In the fecond clafs of circles it is, however, probable that this order is reverfed.

C. Galvanifm, moderately accumulated, affects the animal organs in nearly the fame manner as common electricity. When the human body is made part of the circle, a fhock is perceived at the moment of connection; and a fubfequent numbnefs and tingling fenfation denote the permanent circulation of the influence.

The frefh limbs of frogs undergo violent contractions, and foon lofe their excitability, when placed in the circuit of a powerful battery.

D. The chemical actions taking place in the compound galvanic circle, prefent curious and moft unexampled appearances; all the primary changes taking place in the different conductors being increafed, and modified to a great extent.

In all batteries of the firft order, when the connection is completed, changes take place which denote the evolution of influences capable of producing from *common* water oxygen and hydrogen, acid and alkali, in different parts of every feries ‡.

Thus, in the battery with feries of zinc plates, filver wires, and common water, oxide of zinc is formed on all the plates of zinc, whilft hydrogen is produced from the filver wires, and if the water in contact with them be tinged with red cabbage juice, it becomes green §.

And in the battery with filver, gold, and weak nitric acid, the filver is diffolved, whilft the acid becomes green, and flowly evolves gas at its points of contact with the gold.

The chemical agencies exerted in the compound batteries of the firft clafs, can be beft obferved by the fubftitution of fingle metallic wires for fome of the double plates; for, in this cafe, the changes taking place in the feries with wires,

* Profeffors Tromsdorf Böckmann, Fourcroy, and Vauquelin.

† Mr. Nicholfon and Mr. Cruikfhank.

‡ Mr. Nicholfon, Mr. Carlifle, and Mr. Cruikfhank.

§ D.

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will be exactly analogous to those produced in the series with plates; silver, and all the more oxidable metals, oxidating in water, in the usual place; and gold and platina evolving oxygen gas.

Thus, when into two small glass tubes, connected by moist animal substance, and filled with distilled water, two gold wires are introduced from a large battery, in the proper order, oxygen is produced in one quantity of water, and hydrogen in the other, nearly in the proportions in which they are required to form water by combustion *. And if the process be continued for some time, the apparatus being exposed to the atmosphere, the water in the oxygen-giving tube will become impregnated with an acid (apparently the nitrous); whilst that in the hydrogen-giving tube will be found to hold in solution an alkali, which, in certain cases, has appeared to be fixed †.

From some experiments it would appear probable that the quantities of hydrogen produced in series are small, and the quantities of alkali great, in proportion as the surfaces of contact of the least oxidable metals with the water are more extended *.

All the oxygenated solutions of bodies possessing less affinity for oxygen than nascent hydrogen, are decomposed when exposed to the action of the metal occupying the place of the least oxidable part of a series in the compound circle.

Thus, sulphur may be produced from sulphuric acid; and copper and other metals precipitated in the metallic form from their solvents ‡.

But little knowledge has yet been obtained concerning the chemical changes taking place in the second class of galvanic batteries. But from several experiments it would appear that they are materially different in the laws of their production from those taking place in the first class.

Thus, when single metallic wires with water are placed as series in powerful batteries of the second order, the influence producing oxygen seems to be transmitted by the point, in the place of that part of the plate, which was apparently incapable of undergoing oxidation; whilst the hydrogen is evolved from that point, where the oxidating part of the primary series appeared to exist §.

* Original experiment, D.

† Mr. Cruikshank, the first discoverer of the galvanic production of alkali, supposes that the hydrogen wire in common water generates ammoniac. Perhaps the presence of muscular fibre is connected with the production of fixed alkali in the experiment detailed in the text.

‡ Mr. Nicholson, Mr. Cruikshank, and Mr. Henry jun.

§ D.

§ 6.

§ 6. The agency of the galvanic influence, which occasions chemical changes, and communicates electrical charges, is probably, in some measure, distinct from that agency which produces sparks, and the combustion of bodies.

The one appears (all other circumstances being similar) to have little relation to surface in compound circles, but to be great, in some unknown proportion, as the number of series are numerous. The intensity of the other seems to be as much connected with the extension of the surfaces of the series, as with their number.

Thus, though eight series, composed of plates of zinc and copper, about ten inches square, and of cloths of the same size, moistened in diluted muriatic acid, give sparks so vivid as to burn iron wire; yet the shocks they produce are hardly sensible, and the chemical changes indistinct *. Whilst twenty-four series of similar plates and cloths, about two inches square, which occasion shocks and chemical agencies more than three times as intense, produce no light whatever.

§ 7. A measure of the intensity of the power in galvanic batteries, producing chemical changes, may be derived from the quantity of gas it is capable of evolving from water in a given time; or from the length of the fluid chain through which it can be transmitted. For the same law of diminution of activity that was applied to single circles in § 4. p. 330, may be likewise applied to compound circles.

The general relative forces of different batteries may be ascertained by connecting them in opposite orders, so as to produce a certain annihilation of power. For in all cases when the most oxidable part of one series is placed opposite to the same part of another equal series, and connected with it by means of a fluid, the galvanic agencies of both are destroyed.

IV. *General Observations.*

§ 1. From a valuable experiment lately made †, it appears, that, when common electricity is passed through water, by means of two very fine metallic points, chemical changes are effected by it, analogous to those occasioned by the transmission of the galvanic influence.

This fact, singly, presents a very strong analogy between galvanism and the common electrical influence; and, when compared with the other facts, it amounts almost to a demonstration of their identity.

On this theory, it seems most probable, that all the different

* Fourcroy, Vauquelin, and Thenard.

† By Dr. Wollaston.

galvanic phænomena owe their exiftence to electricity, excited in the different arrangements, in confequence of a change in the electrical capacity of fuch of their parts as undergo chemical action; this action being always connected with alterations in the furfaces and conducting powers of bodies.

The circulation of electricity in galvanic circles, from its different points of excitation, appears to depend, in a great meafure, upon certain fpecific attractions for it, in the different conductors, inexplicable by known laws.

§ 2. In all cafes of fimple galvanic action, negative and pofitive electricity, as refpectively exhibited by their deoxidating and oxidating influences, after being once excited, can be multiplied by a certain alternation of furface.

Thus, in the circle with zinc, iron, and water, in which, as was ftated in page 332, the zinc oxidates, and the iron undergoes no change, if a drop of water be placed on an infulated furface of the iron, it will rapidly act upon it, and produce red oxide; whereas, if it be placed upon a fimilar furface of the zinc, it will undergo very little change *.

This fact will, perhaps, in fome meafure, lead to an explanation of the caufe of the accumulation of galvanifm in compound circles. For, in thofe circles, all the feries are fo conftituted as to produce a perfect correfpondence between the electricity excited by chemical action, and that produced by compenfation.

§ 3. The general chemical agency of galvanifm is at prefent involved in obfcurity. The facts relating to the feparate production of oxygen and hydrogen, acid and alkali, in water, are totally incommenfurable with the ufually received theory of chemiftry. And, even fuppofing that the appearance of the two laft of thefe bodies is connected with the prefence of atmofpheric air, it is ftill extremely difficult to conceive, that either oxygen or hydrogen can pafs in an invifible form through fluids or organic bodies. Common phyfical facts prefent us with no analogies fufficiently diftinct to enable us to reafon on this fubject; and the elucidation of it will probably be connected with perfectly new views of corpufcular action.

§ 4. The appearance of galvanic action in living matter, particularly in the torpedinal organ, leads to curious inquiries. Chemical changes are perpetually going on in different parts of the living body, which muft be connected with alterations in their ftates of electricity; and organized beings contain all the fubftances requifite for forming fpecies of galvanic arrangements. Thefe circumftances, combined with the facts

* D.

of

of the production of muſcular contraction by common galvaniſm, and the dependance of irritability, and even life, upon the oxygenation of the blood, afford analogies which render it probable that ſome phænomena ſimilar to the galvanic phænomena may be connected with muſcular action and other proceſſes of life. Theſe analogies, however, at preſent are very indiſtinct; and they ought to be conſidered of importance only ſo far as they are likely to lead to the diſcovery of new inſtruments of experimental inveſtigation.

Concluſion.

The relations of galvaniſm to the different branches of phyſical ſcience are too numerous and too extenſive to be connected with the preceding details; and, although in their infancy, they will probably long conſtitute favourite ſubjects of inveſtigation amongſt philoſophers, becoming the ſources of uſeful diſcoveries.

The new galvanic facts have given an importance to the ſcience ſufficient to render it intereſting, and to enſure its progreſſion. The ſpirit of inquiry is awakened in the public mind, and it is difficult to imagine the exiſtence of cauſes capable of deſtroying it.

Galvaniſm may be purſued with greater facility than moſt of the other ſciences; it requires leſs time and attention; it is leſs connected with manual labour, and the moſt delicate organs are the beſt fitted for performing and obſerving its operations.

The inſtruments required for galvanic experiments are few, and but little expenſive. A battery compoſed of fifty plates, when arranged with chemical agents, forms a combination ſufficiently powerful for common experiments. With ſuch a combination, a few glaſs tubes having gold wires attached to them, and a gold leaf electrometer, may inveſtigations be purſued, and principles diſcovered, extending not alone to the laws of dead matter, but even to thoſe of animated nature.

If, to render galvaniſm a popular ſtudy, it were neceſſary to prove that it bore relations to the common wants of life, it might be ſtated, that its agencies are likely, at ſome period, to become uſeful in the arts. When our galvanic inſtruments are rendered more perfect and more powerful, we may be readily enabled, by means of them, to procure the pure metals; and to form immediately from their elements, nitrous acid and alkali. The connection of galvaniſm with philoſophical medicine is evident. The electrical influence in its common form, as excited by machines, has been employed with advantage in the cure of diſeaſes; in a new ſtate of existence it may possibly be possessed of greater, and of different powers.

But, independent of the immediate applications of this science, much is to be hoped from the elucidations which it may bestow upon the kindred sciences. And a discovery so important as to excite our astonishment, cannot fail of becoming, at some period, useful to society. All the different branches of human knowledge are intimately connected together, and theoretical improvements cannot well be made in them without being accompanied by practical advantages.

Royal Institution, Sept. 1, 1801.

LIV. *An Account of a Method of constructing Simple and Compound Galvanic Combinations, without the Use of Metallic Substances, by means of Charcoal and different Fluids. By Mr.* DAVY *.

1. IF a piece of well burned charcoal be brought in contact at one of its surfaces with a portion of water, and at another surface with a portion of nitric acid, a simple galvanic combination will be formed when the two fluids are connected together: and the powers of it are demonstrated by its agencies upon the limbs of frogs, and by its effects upon the organs of sense.

2. A compound galvanic combination or a galvanic battery may be formed from a number of series composed of the same substances: but in this case the fluid elements of each series, not being immediately in contact, must be connected with similar elements in other series in an order of regular alternation, such as water, charcoal, acid; water, charcoal, acid; and so on.

3. The best mode that has yet occurred of constructing galvanic batteries with charcoal, is by means of a number of glasses, which are made to contain, alternately, nitrous acid and water, and which are connected in pairs by means of moistened cloth. The pieces of charcoal used are made from very dense wood, such as box or lignum vitæ; and in this case, the fluids will not penetrate into them by capillary attraction much beyond the places of their primary contact. Their forms are those of arcs, so that each piece connects together two glasses; but in instances where single pieces of charcoal cannot be obtained of the proper shape, two long and thin slips may be fastened together by silk, so as to form the angle necessary to their insertion into the glasses.

* From the same.

4. Twenty

4. Twenty ſeries in a battery of this kind produce ſenſible but feeble ſhocks, and when a ſingle metallic ſeries with a gold wire and two glaſſes of water is ſubſtituted for one of the primary ſeries, hydrogen is given out by the metallic point in the glaſs of water in the place of the acid, whilſt oxygen is evolved from the point in the other glaſs.

5. In the galvanic batteries with charcoal, ſulphuric acid may be ſubſtituted for nitric acid; and ſolution of ſulphuret of potaſh for the water, without any material alteration in the nature of the agency; the ſolution of the ſulphuret indeed, ſeems, in ſome meaſure, to increaſe its intenſity, and combinations containing this ſubſtance, denſe charcoal, and concentrated nitric acid, appear to be ſuperior in activity to ſimilar combinations containing copper, and the ſame fluid elements, and to be nearly equal to thoſe compoſed of zinc, ſilver, and water.

January 9, 1802.

LV. *Obſervations on the Affinity which Earths have for each other. By* DARRACQ, *Pupil of* VAUQUELIN, *and his Aſſiſtant in the School of Mines* *.

THE analyſis of ſtones has long engaged the attention of chemiſts. The moderns, aſſiſted by more correct inſtruments, have carried this branch of chemiſtry to ſuch a degree of perfection, as ſeems to leave nothing more to be deſired; but C. Guyton, in a memoir, in which he treats with minuteneſs on the different affinities which the earths exerciſe on each other, relates ſeveral obſervations, printed in the 27th number of the *Annales de Chimie*, which excite great doubt in regard to the exactneſs of the analyſes hitherto made, and which invite all chemiſts to be on their guard, leſt they ſhould take a ſimple earth for a mixture of ſeveral. The embarraſſment which theſe affinities daily gave riſe to, determined me to examine, with great care, the different circumſtances under which they may take place; for, if it is of importance to point out theſe new combinations, it is no leſs ſo to reduce them to their juſt value.

Exp. I. C. Guyton having mixed ten centilitres of lime water with two centilitres of barytes water, there were ſoon formed white clouds, which united, and depoſited themſelves at the bottom of the veſſel containing the mixture. In con-

* From the *Annales de Chimie*, No. 118.

 ſequence

ſequence of this experiment he admitted an affinity between lime and barytes.

I repeated the ſame experiment with great care; but obtained no change in the liquors mixed, nor any ſign of precipitation, even after five months. It is to be preſumed that the precipitate formed in the experiment of C. Guyton aroſe from the impurity of the ſubſtances employed. In my opinion, it was owing to the preſence of ſome atoms of ſulphuric acid contained in the lime. It is almoſt always found in common lime.

Exp. II. By combining a ſolution of aluminated potaſh and ſiliciated potaſh, I obtained, as Guyton did, a very thick white precipitate, which ſoon after formed a gelly. This precipitate, diſperſed in water, and filtered, gave, by the teſts proper for alumine and ſilex, evident proofs of the affinity of theſe two earths diſſolved in potaſh.

Exp. III. Lime water, mixed with a ſolution of ſiliciated potaſh, gave alſo proofs of a real affinity between theſe earths by the precipitation, which was inſtantly effected. This was confirmed alſo by an analyſis of the precipitate.

Exp. IV. Strontian water with ſiliciated potaſh produced the ſame phænomenon as the preceding mixture.

Exp. V. Strontian water and lime water mixed together formed no precipitate, as was obſerved by Guyton.

Exp. VI. Strontian water and barytes water, when combined, formed no precipitate.

Exp. VII. Carbonates of lime and barytes, diſſolved in an exceſs of their acid, gave no ſign of a new combination.

Exp. VIII. This experiment, which begins the ſeries of thoſe made by Guyton on the ſolution of earths in acids, did not afford me the reſults announced by that chemiſt. Having made a mixture of equal parts of a ſolution of muriate of lime and a ſolution of muriate of alumine, he obſerved, 1ſt, That the liquor immediately became turbid, without the acid of agitation: 2d, That ſoon after it was opake, and almoſt gelatinous: 3d, and in the laſt place, that this precipitate did not diſappear by the addition of an acid.

I repeated this experiment, with all the care poſſible, to obtain the ſame phænomena. Very pure muriate of lime, mixed with muriate of alumine equally pure, gave no ſign of precipitation: the mixed liquors conſtantly remained clear.

It is probable, according to ſome eſſays which are peculiar to myſelf, that the precipitate obtained by C. Guyton was occaſioned by the ſulphuric acid contained in the muriate of alumine, and which at the time of the mixture muſt have re-acted on the lime; for when alumine has been ex-

 tracted

tracted from alum, it is difficult to free it entirely from sulphuric acid.

By extracting alumine from those natural stones which contain it, either by the nitric acid or the muriatic acid, it will be easy to discover the error into which the author was led by muriate of lime prepared with the earth of alum.

Exp. IX. By combining muriate of lime, in solution, with muriate of barytes, Guyton observed an abundant precipitate, which was not dissolved by excess of muriatic acid. The solutions of these two muriates, whether concentrated, or diluted with distilled water, gave no sign of precipitation by mixing together, nor a long time after their combination. In my opinion, the muriate of lime employed by Guyton contained some atoms of sulphuric acid, which, with the barytes, must have formed sulphate of barytes.

Exp. X. Muriate of lime mixed with that of magnesia gave, as Guyton observed, no sign of precipitation.

Exp. XI. When solutions of muriates of lime and of strontian were mixed, no change was produced. This was observed also by Guyton.

Exp. XII. Guyton announces, that the mixture of solutions of muriate of magnesia and muriate of alumine assumed, at the end of some minutes, a slight milky appearance. This experiment, being carefully repeated, exhibited nothing of the kind; my liquors retained their clearness and transparency.

Exp. XIII. The thirteenth experiment, made with a mixture of solutions of muriate of magnesia and muriate of barytes, formed no precipitate, either at the moment or long after; yet Guyton says he obtained an abundant precipitate. It is probable that sulphuric acid was still concealed in the muriate of magnesia.

Exp. XIV. This experiment, which was made with muriate of magnesia and muriate of strontian, gave no sign of any new combination. The same was remarked by Guyton.

Exp. XV. Muriate of barytes and muriate of alumine, both dissolved and even diluted in water, gave to Guyton an abundant precipitate. The same experiment, repeated with the solutions concentrated or diluted with water, did not yield me the same result: the mixture always remained clear, without forming any precipitate.

Exp. XVI. The sixteenth experiment, made by the combination of muriate of strontian with that of barytes, exhibited no change.

Exp. XVII. The seventeenth and last experiment of Guyton, on the affinity of earths in the humid way, was

 made

made by mixing a folution of muriate of ftrontian with a folution of muriate of alumine. According to his account, the liquor affumed a milky colour, and gave a precipitate which the acid did not rediffolve. This experiment, repeated, like all the reft, with a great deal of care, ftill confirmed the prefence of the fulphuric acid in the folution of muriate of ammonia he employed; for I had no precipitate when I repeated the experiment, and the liquor, for five months, loft nothing of its tranfparency.

By admitting between earthy fubftances an affinity of which nature affords a thoufand examples, I do not however think that the force which attracts them is fufficiently great to make them abandon their folvent, without evaporation, efpecially if it be acid. There is none indeed but filex diffolved in alkali that has the property of precipitating the other alkaline earths from their aqueous folutions: the other earths produce nothing of the kind; for, if alumine diffolved in an alkali be mixed with folutions of barytes, ftrontian, lime, &c. no precipitate will be formed; nothing more will take place between the ftrontian, lime, barytes, &c.

If the experiments of Guyton had really eftablifhed the facts he announces, we fhould have had no longer any certain means for analyfing earths and ftones, and every thing hitherto done on this fubject could only have been confidered as fo many incorrect refults.

I therefore think myfelf authorized to conclude, from the facts announced in this memoir, that the phænomena obferved by Guyton were occafioned in many cafes by foreign bodies contained in the matters he employed.

LVI. *Experiments concerning the Analyfis and Synthefis of Alkalies and Earths, announced by* C. Guyton *and* Desormes. *By* C. Darracq, *Pupil of and Affiftant to* C. Vauquelin *at the School of Mines* *.

The third volume of the Memoirs of the Inftitute of France contains fome experiments by C. Guyton and Desormes on the compofition and decompofition of the two fixed alkalies, and fome of the earths fuppofed to be fimple. The importance of fuch difcoveries, the period of their being made, and their utility to the arts, invite all chemifts to repeat them; and fuch were the motives by which I was guided in my refearches. To announce that my experiments were

* From the *Annales de Chimie*, No. 119.

made

made in the laboratory of the School of Mines, is to inform the public that I was affifted by the advice of C. Vauquelin.

I muft requeft thofe chemifts whofe opinions I am about to oppofe, to confider my labour as arifing only from a defire to difcover truth, and not as the refult of a vain pretenfion to alter in any manner the efteem which they deferve, and the gratitude due to them for their numerous and ufeful difcoveries. I fhall therefore content myfelf with defcribing the manner in which I repeated their experiments. I fhall fhow the refults of them, and I fhall leave to chemifts to deduce from them fuch conclufions as they may think proper.

Exp. I. The firft experiment, which confifts in the reduction of a metallic oxide by potafh, prefents nothing that can fupport the obfervation of the above authors. The confequence which they themfelves deduce from it is merely acceffory. Befides, it is known that feveral faline fubftances deftitute of potafh poffefs this property. It is known alfo, that fire alone is fufficient to reduce the oxide of lead employed in this operation.

Exp. II. Guyton and Deformes decompofed in a platina crucible fulphuret of potafh; and with the help of a ftrong heat they obtained lime.

I repeated this experiment with common fulphuret of potafh; that is to fay, the fubftance commonly fold in the fhops under the name of *liver of fulphur*; and having expofed it to a ftrong heat, I obtained a refiduum which, when treated with water, was not entirely diffolved. The infoluble part, treated by nitric acid, was attacked; the nitric folution, tried by the oxalate of ammonia, gave a great quantity of lime.

Sufpecting that the lime found by this experiment might arife from the potafh employed to make the fulphuret, I made a fecond experiment with pure fulphuret of potafh, prepared from potafh purified by alcohol and fublimated fulphur. I employed 16 grammes of alkali and 8 of fulphur. The mixture, made in a mortar, and poured by portions into a platina crucible ftrongly heated, left a matter of a reddifh brown colour, which, treated by water, was almoft entirely diffolved. The fmall refiduum, which weighed only 4 centigrammes, being carefully examined, was found to be filex coloured by charcoal; it arofe no doubt from the potafh, which, as is well known, eafily diffolves that earth.

Exp. III. To perform this third experiment, they took oxalate of potafh with fulphur. After the mixture was moiftened and pulverifed, it was treated with heat in a fand-bath, and oxalate of lime was obtained.

As the above authors do not defcribe this experiment, and give

give no proportions, I shall make known those which I employed, and the result I obtained.

To get pure oxalate of potash, I saturated potash purified by alcohol with oxalic acid. I took 18 grammes of this salt, evaporated almost to dryness, and, having mixed it with 9 grammes of sublimated sulphur, moistened the mixture with half a decilitre of water. I then placed the capsule containing it on a sand-bath, and evaporated it at a gentle heat. The residuum taken up by distilled water being filtered, there remained on the filter a blackish yellow residuum, and the liquor had assumed a slight yellow colour. The residuum was treated with nitric acid; but notwithstanding the care employed in this operation, no oxalate of lime was found. The acid liquor absolutely contained none of it. The sulphur which remained was burnt in a platina crucible, and left no residuum whatever.

Hoping that this experiment would succeed better with a stronger heat, the same quantities of oxalate of potash and sulphur were employed, and the mixture was placed in a platina crucible over a naked fire. Half an hour after, being brought to a red heat, it was taken off, and the mass obtained was treated with distilled water, which assumed a dark yellow colour, and the real smell of an hydrogenated sulphuret. The liquor was filtered, but no lime was found in the residuum. This residuum, which weighed only two centigrammes, was found to be charcoal, furnished in all probability by the potash employed, which had been purified with alcohol. I assured myself that all the oxalate of potash had been decomposed, and that there had resulted from it a sulphate of potash.

Exp. IV. They put oxalate of ammonia into very pure oxygenated muriate of potash, and caused it to evaporate at several times, taking care to maintain an excess of nitric acid to dissolve the oxalate of lime which was formed. In this manner a very large quantity was obtained.

This fourth experiment, which appears to be that which has hitherto given to the above authors the most lime, by the decomposition of the hyper-oxygenated muriate of potash, was made with great care, but always without success. As these authors do not speak of the quantities employed, I shall give an account of my experiment.

I took 16 grammes of hyper-oxygenated muriate of potash, pure and crystallized, and 8 grammes of oxalate of ammonia, also pure and crystallized, and put them into a porcelain capsule, with two decilitres and a half of distilled water. The capsule being placed on a sand-bath, the mixture

ture was evaporated to dryneſs; and towards the end of the evaporation care was taken to add a gramme of nitric acid to diſſolve the oxalate of lime which might be produced. The evaporation was repeated four times ſucceſſively, adding water at each operation, and a gramme of nitric acid. The reſiduum was then taken up by a new quantity of diſtilled water, which completely diſſolved the whole of the matter. As there were ſome atoms diſſeminated throughout the liquor, it was filtered, but it was not poſſible to appreciate the reſiduum, as the liquor was very clear. The nitric acid was ſaturated with ammonia, to precipitate the oxalate of lime; but in vain was an exceſs added, for the liquor ſtill remained clear.

The ſame experiment performed cold did not exhibit more ſucceſsful reſults, though the mixture remained fifteen days in contact, as recommended by the authors above mentioned.

Exp. V. The fifth experiment, which ſeems to have confirmed C. Guyton and Deſormes in the opinion they had formed of the converſion of potaſh into lime, is that which they performed with hyper-oxygenated muriate of potaſh and the phoſphoric acid. After treating a mixture of 20 grammes of this ſalt with 10 grammes of acid till reduced to glaſs, they diſſolved it in water; and, having ſaturated the exceſs of acid by ammonia, filtered the liquor, expoſed it to evaporation, and made it again paſs to the ſtate of glaſs. By ſix different evaporations, they obtained two or three grammes of phoſphate of lime, as they believed.

1ſt, The hyper-oxygenated muriate of potaſh, decompoſed by the phoſphoric acid, obſerving the proportions above announced, produces, by evaporation to dryneſs, a vitriform matter, a part of which remains inſoluble in the water under the form of a granulated white powder brilliant like ſalt.

2d, The liquor ſeparated from the inſoluble part being again calcined, ſtill leaves a certain quantity of inſoluble powder ſimilar to the former; a third calcination produces the ſame effect; and ſo in ſucceſſion, till the matter is almoſt entirely converted into this inſoluble powder.

3d, If the calcination is performed in a ſtrong heat, and continued a long time, the quantity of the inſoluble powder is more conſiderable, and fewer calcinations are required to convert the matter into this powder.

It is ſeen by the three preceding paragraphs, that we obtained a great deal of this ſalt; it is indeed not difficult to procure it. We ſhall now deſcribe the experiments to which we ſubjected it, and which evidently proves that this ſalt is not phoſphate of lime.

4th,

4th, This insoluble powder has no savour; it is insoluble in cold water, but it is soluble in boiling water in notable proportions.

5th, Though this matter be little soluble in water, lime-water however produces in the liquor a very sensible precipitate. Nitrate of silver furnishes one also, which is soluble in nitric acid.

6th, Ammonia occasions no change in the solution of the matter in question. As little effect is produced by the fixed alkalies.

7th, The oxalate of ammonia made no alteration in the above liquor, which it would certainly have done, had there been a suitable quantity of lime.

8th, This salt is exceedingly fusible. By the blow-pipe it fuses into a pearl, transparent while in a state of fusion, but which on cooling becomes opake.

9th, This powder is soluble in the nitric, muriatic, and even phosphoric acids. These solutions are thick, viscous, and tenacious, like clear starch. When these solutions are sufficiently diluted with water, they give no precipitates by alkalies; but, if concentrated, they give one, which redissolves in a large quantity of water.

From all these experiments we are authorized to conclude the citizens Guyton and Desormes took an alkaline phosphate for phosphate of lime. This phosphate, which is scarcely soluble, as above seen, is not a simple phosphate; for it is known that the alkaline phosphates are very soluble, especially that of potash; but it was not before known that in the experiment of which we have given an account there is formed a new phosphate, which ought to be distinguished by the name of neutral phosphate. The following experiment serves also to confirm this reasoning, and confirms in an irrevocable manner all those described.

I mixed a certain quantity of phosphate of potash with pure potash, placed the mixture in a platina crucible, and exposed it to a strong heat. I obtained for result a white substance, almost insoluble, which possessed all the properties of the salt obtained in the last experiment, and which was again ascertained to be a real phosphate of potash with an excess of base. This kind of synthesis still proves that the phosphate of lime described by the above authors is only this new salt, which, on account of its apparent properties and insolubility, has been too little examined.

Exp. VI. By treating hyper-oxygenated muriate of soda in the same manner as that of potash, these authors announce that

that they extracted from six grammes of this salt one gramme of magnesia.

To repeat this experiment, the hyper-oxygenated muriate of soda was made by saturating with oxygenated muriatic gas a solution of pure carbonate of soda. When this salt was formed, it was evaporated to dryness in a porcelain capsule; and 6 grammes of it in this state were put into a platina crucible, when fused phosphoric acid was added in small portions. The mixture immediately became yellow, and a great deal of oxygenated muriatic acid was disengaged, as was the case in the experiments made with oxygenated muriate of potash. I continued to add phosphoric acid till nothing more was disengaged; upon which, a strong heat being applied, the whole mass became liquid. After cooling there was found a vitreous matter, brittle, slightly deliquescent, and wholly soluble in water. When the liquor had become very clear, an attempt was made to precipitate from it the magnesia, by the help of potash; but having obtained not a single atom of precipitate, the experiment was repeated several times, but always without success. From what has been said, it is evident that the magnesia obtained by the above authors can not have arisen but from the soda which they employed; for it is known that soda is very often accompanied with magnesia. In this experiment, as in the fifth, the platina is strongly attacked. In two trials I had a residuum of eight centigrammes of this metal.

Exp. VII. Nitrate of soda treated by the phosphoric acid gave to the above authors magnesia. I repeated this experiment four times without obtaining the same result. I varied, but in vain, the doses of the salt and the acid, as well as the mode of operation; but by employing as much as 20 grammes at a time of nitrate of soda, and 10 of acid, I obtained only a blackish residuum weighing four centigrammes, which was found to be silex coloured by charcoal. Those who recollect the existence of this earth in the phosphoric acid discovered by C. Vauquelin, will be able to account for the silex found in this experiment.

I must inform those who may wish to repeat this experiment, that they must not employ phosphorous acid for phosphoric acid; for when the former is employed, and poured over the red and fused nitrate, a very strong deflagration is produced, the matter is thrown from the crucible, and speedily burns every thing that it meets with. This observation ought to be recollected in regard to the Experiments 5 and 6.

Exp. VIII. In this eighth experiment, when potash was heated with charcoal, and when the capsule containing the mixture

mixture was brought to a red heat, they obtained, for residuum, carbonate of potash, lime, magnesia, and even some alumine.

Without describing all the fruitless trials made to find the three earths produced in this experiment, I shall relate some of them, which prove that these pretended conversions arise only from the impurity of the charcoal employed.

Having made choice of light charcoal from white wood, it was pulverized and strongly calcined. Sixteen grammes of pure potash and eight grammes of this calcined charcoal were then exactly mixed, by moistening it with a little water; after which it was put into a platina capsule, and brought to a red heat. The residuum, after being edulcorated with water, to separate from it all the potash, was treated with nitric acid. The solution, when filtered, gave by evaporation a glutinous matter, found to be silex. The nitrates separated from this silex gave lime and magnesia.

Presuming that the charcoal might be the cause of the presence of the lime and magnesia found in this experiment, I thought it my duty to assure myself of it by analysing that employed.

I burnt 216 parts of the same charcoal to reduce it to ashes, taking care to employ a platina crucible; the ashes obtained weighed only 91 parts. This residuum was treated with the nitric acid. By proper evaporations, the presence of a great deal of carbonate and sulphate of lime was ascertained. The products obtained separately by the common processes gave silex 41, magnesia 16, lime 12, alumine 3.

For the sake of greater precision, and to be more fully convinced in regard to the existence of lime in the charcoal, as well as to prove that the lime found by C. Desormes arose only from the charcoal employed, I made the following experiment:

I took four grammes of pure charcoal obtained from the decomposition of the carbonic acid by phosphorus, and, having mixed it with 18 grammes of pure potash, put it into a platina capsule, and brought it to a red heat, having previously taken care to moisten it. The matter which thence resulted being treated with water, there remained a black residuum, which was treated with nitric acid, and the solution diluted with water was filtered. This nitric solution was mixed with oxalate of ammonia; but this salt produced no alteration in the above liquor, even when the excess of acid was saturated by ammonia; which it could not have failed to do had there been ever so little lime, or even magnesia and alumine. This would be the place for calling the reader's attention

attention to the difference of the phænomena by pure ſubſtances and thoſe which are not ſo; but I ſhall confine myſelf to mention that exhibited by the experiment in queſtion.

When the pure charcoal is treated with potaſh, as above ſaid; and when an acid is added to the ſolution ſeparated from the charcoal, in order to ſaturate the potaſh, no other phænomenon is obſerved than a diſengagement of carbonic acid gas, which the potaſh has abſorbed during the courſe of operation.

But, inſtead of pure charcoal, if that which we firſt employed be made uſe of, that is to ſay, charcoal of white wood; by ſaturating the potaſh employed, a very different reſult will be obtained. The acid, as ſoon as poured in, produces a diſengagement of carbonic acid. When a larger quantity is added, a great deal of ſulphurated hydrogen gas is produced; and by ſaturating the potaſh a large quantity of ſulphur is precipitated. This again explains the ſtate in which the lime is found, and perhaps the magneſia, in the charcoal; for there is no need to ſay that the ſulphur obtained in this operation muſt ariſe from the ſulphuric acid decompoſed. There is no doubt that the ſulphates of lime and magneſia contributed to this circumſtance, ſince the quantities of theſe earths in the charcoal were conſiderable. This ſtill ſerves to explain the obſervation made by C. Deſormes, who ſeveral times found lime, magneſia, and alumine in the ſame reſiduum. By recollecting that theſe three earths exiſt in charcoal, it will be readily ſeen whence they aroſe.

Exp. IX. By treating ſoda with charcoal in the ſame manner as the potaſh was treated, the above authors obtained magneſia, alumine, and lime. It is here ſeen that this reſult is the ſame as ought to be given by common charcoal. I ſhall therefore ſay nothing further reſpecting this experiment, than merely to announce that in treating pure ſoda with pure charcoal (that in particular ariſing from the decompoſition of the carbonic acid), none of the above earths are obtained.

Exp. X. Muriate of potaſh treated by charcoal gave to Deſormes lime. It is needleſs to obſerve, in this tenth experiment, that the lime aroſe from the charcoal; ſince, when pure matters are employed, none of that ſubſtance is found.

Exp. XI. The eleventh experiment, which, beſides lime, gave magneſia and alumine, will not give any of theſe earths when pure charcoal is employed. This experiment was made with muriate of ſoda and charcoal.

Exp. XII. The twelfth experiment, which gave exactly the ſame reſults as the eleventh, was made by treating muriate

riate of soda with sulphur. As charcoal had not been here employed, I took great care to repeat this experiment several times; but always without success. I cannot presume to say what may have been the cause of the result obtained by the above authors: it can be ascribed only to the matters they employed, since, with pure muriate of soda and sublimated sulphur, I could discover, in the two experiments I made, no traces either of lime, magnesia, or alumine.

Exp. XIII. As the thirteenth experiment is of no use in regard to the theory of Guyton and Desormes, it is needless to give any account of it.

Exp. XIV. By mixing sulphate of soda with lime, these authors thought they perceived a disengagement of ammonia: I also thought I observed the same disengagement in the vapours produced by the muriatic acid when brought into contact with the mixture. Lime alone, though extracted from marble, gave by the same acid, when moistened, white vapours. I am not convinced that this disengagement, sensible only on the approach of the muriatic acid, arose from ammonia; for, though I employed a large mass of matter, I never could distinguish that alkali by the smell. The vapours occasioned by the muriatic acid were perhaps owing only to the heat produced at the time of the mixtures, and also to the great affinity which the acid employed has for water. How, indeed, can we explain the formation of this substance by the combination of lime with pure water? or how can we conceive, if it is not formed, that it can co-exist with lime, which, as is well known, possesses the property of expelling ammonia wherever it is found?

Exp. XV. In the fifteenth experiment, these authors thought also that they remarked a disengagement of ammonia, by combining intimately together lime, sulphur, and water.

Exp. XVI. The sixteenth experiment makes mention of an analogous trial as simple as the preceding. In my opinion, the explanation of these experiments will be sufficiently developed in the eighteenth experiment, since it is that which, according to these authors, gave a great deal of ammonia.

Exp. XVII. By this experiment the above authors, instead of decomposing the lime, attempted to compose it. The processes which they employed for this purpose were as follow:

They formed nitrate of alumine, which they decomposed, at several times, by heat: by this simple means they obtained lime.

Nitrate of magnesia was employed with the same success.

After having carefully prepared the two nitrates, which they made use of for this composition, I took twenty grammes of the nitrate of alumine, and exposed it to a strong heat in a platina crucible. After three different and strong evaporations to dryness, I obtained an earth perfectly white, and insoluble in water. When treated with nitric acid, it was entirely dissolved. This nitric liquor, which was very clear, was mixed with oxalate of ammonia; but this re-agent produced no change. The addition of ammonia, so as to saturate the acid, was tried, but in vain; it was only by an excess that a precipitate was obtained, but which, being soluble in potash, was found to be alumine; consequently I did not find an atom of lime. It is true, that to prepare my nitrate of alumine I employed an earth perfectly pure, extracted from a natural stone at the time of my experiments on the affinity of earths. The acid employed was also very pure.

The nitrate of magnesia, notwithstanding all my care, did not give a more satisfactory result: the lime found by Desormes was, no doubt, accidental.

Exp. XVIII. This experiment, which calls to mind the 14th, 15th, and 16th, on account of the same phænomenon by which they are connected, is that which increased the opinion of the above authors on the recomposition of simple earths. Having instantaneously made sulphuret of potash, and added oxalic acid, they obtained, as they announce, a sensible disengagement of ammonia instead of sulphurated hydrogen, and the residuum was magnesia.

After making two trials in a porcelain mortar without discovering ammonia, but a product extremely fetid, which I suspected might conceal the alkali I sought for, to assure myself of it I made the following experiments:

1st, I mixed 30 grammes of pure potash with 15 grammes of sublimated sulphur, and the mixture exactly made was put into a tubulated retort with two decilitres of water. To the beak of the retort I adapted a bent tube, which was immersed in a solution of sulphate of iron at its maximum of oxidation. By the disposition of my experiment it is seen that I must have perceived the smallest portion of ammonia; for it is known, that if the sulphurated hydrogen does not precipitate the sulphate of iron green, an atom of this gas combined with ammonia will immediately manifest itself. My apparatus being thus arranged, I poured into the retort six grammes of pure and crystallized oxalic acid. A strong disengagement of sulphurated hydrogen was immediately produced; the flask containing the sulphate of iron was agitated a long time by this gaseous fluid; but when the operation was finished there was not a single atom of precipitate produced in the liquor. This experiment, which I think one of the most exact for ascertaining the ammonia that might have existed or been produced in this operation, evidently proves, that if the above authors obtained any, this alkali must have arisen from some other cause than that of a decomposition of the substances employed, if they were pure.

The residuum examined in three operations gave no magnesia. I must here observe that I employed sulphur, which I sublimated myself.

To remove all doubt respecting this experiment, and to be more certain of my results, I made a second, but still without success.

The apparatus being arranged as in the preceding experiment, I put in the place of the sulphate of iron muriatic acid, and began my operation. After all disengagement had ceased, I took the muriatic acid, which I evaporated at a gentle heat; but, not being able to obtain an atom of saline residuum, I poured lime into the small quantity of liquor which remained. I added more than was necessary, but notwithstanding all my precautions I had no disengagement of ammonia. It is now seen, by these two experiments, that no disengagement of ammonia is produced; for, I must repeat it, muriate of ammonia would have been formed in the last, and in the first iron would have been precipitated: nothing is disengaged but sulphurated hydrogen gas in very large quantity, and the residuum contains no magnesia.

Exp. XIX. This last experiment the above authors made by taking the charcoal of sugar, which they mixed with lime extracted from marble: this mixture being exposed to a strong heat in a platina crucible well stopped, they obtained alumine and magnesia.

As they do not give the proportions of the matters employed in this experiment, nor the quantities of the products obtained, I presume that they considered it only as a new proof in favour of the theory they have established.

I repeated this experiment, and employed charcoal of sugar and pure charcoal. With the charcoal produced from 40 grammes of crystallized sugar, I obtained a little alumine: the product collected weighed only six centigrammes; perhaps it contained magnesia. In my second operation, when I employed pure charcoal, I had not a single atom of the above-mentioned earths: it is therefore seen that the products obtained in this experiment arose from the impurity of the matters employed.

Conclusion.

Conclusion.

If the experiments I have described in the course of this memoir are exact, and if I have properly observed the phænomena they exhibited, the result will be, that the facts announced by C. Guyton and Desormes, though for the most part true, cannot lead to the conclusions which they have deduced from them, on account of the nature of the substances which they employed: we must therefore conclude, till otherwise shown, that the alkalies and earths must be retained in the class of simple or indecomposed bodies; that the experiments of these chemists do not prove that potash is formed of lime and hydrogen; that magnesia and azote produce lime; that azoted alumine constitutes magnesia; and, in the last place, that magnesia and hydrogen give birth to soda. I do not, however, pretend that these substances are simple, but only that the facts adduced as a proof are not proper to prove it.

LVII. *Extract of a Letter from* M. Von Humboldt *to* Lalande.

Caraccas, Dec. 14, 1799.

A FEW weeks after my arrival in South America, I transmitted to Delambre an extract from my astronomical observations, because I hoped that some of them might be interesting to the Board of Longitude: as I have, however, learned that the brig by which I sent my letters was wrecked near Guadaloupe during the storm which lately prevailed in these regions, I think it necessary to transmit to you a copy of them.

After a passage of about six weeks, in the frigate Pizarro, I arrived at the coast of Paria. My plan was to proceed first to the Havannah, and thence to Mexico; but I could not withstand the strong desire I had of seeing the wonders of the Oronoko, and the lofty Cordilleras, which extend from the high land of Quito to the rivers of Guarapeche and Arco. As I have now returned from a very interesting journey to the interior of Paria, through the Cordilleras of Cocolar, Tumeri, and Guiri, and to the settlement there of the Capuchins, never before visited by any naturalist, during which I carried with me, on three mules, my astronomical instruments, viz. a quadrant by Bird, and sextants, telescopes, and micrometers, by Ramsden and Troughton, you will perhaps expect that I have done a great deal for astronomy; but you know that this science is too remote from my principal objects—philosophy in general, geology, eudiometry, and the physiology of plants and animals; and under the latitude of 10° it is impoſſible to labour ſo inceſſantly as under 49°. I choſe rather to make a few obſervations with accuracy, than a great many imperfect ones; and to inſert them with great minuteneſs in my journals, that, in caſe I ſhould die in the courſe of my travels, no doubt might remain in regard to their exactneſs.

In the two papers which I tranſmitted to Delambre, you will find my obſervations made in Europe with Borda's new *inclinatorium.* I found by it, that local circumſtances have a greater influence on the inclination of the magnetic needle than on its declination; and that no connection ſeems to exiſt between the ſituation of the place of obſervation and the inclination. This I find confirmed in the new world in the interior of New Andaluſia, and therefore the obſervations made by Nouet in Egypt ſeem to be eſtabliſhed. The declination, however, is affected by local cauſes, but much leſs: at ſea they are much more regular, and the variations are much more uniform.

I have here given you my obſervations of the inclination only, which are true to 15′. During calms I obtained them at ſea with more accuracy, and the periods of the vibrations of the needle could be determined very exactly. If the ſame number is counted in the ſame time during five or ſix repeated trials, and if this is ſtill the caſe when the inſtrument is moved from its place, the reſults, in my opinion, may be conſidered as correct. Though calms are not uncommon between the tropics, during the courſe of forty days I was able to make only ten very accurate obſervations.

Places of Obſervation, 1799.	Latitude.	Longitude from the First Meridian.	Inclination. Centigrade Degrees.	Inclination. Old Degrees.	Magnetic Power. Number of Vibrations in Ten Minutes.
Paris - - - -	48° 50′ 15″	20° 0′ E	77·15°	69° 28′	245
Nîmes - - - -	43 50 12	21 59	72·65	65 23	240
Montpellier - -	43 36 29	21 32·5	73·20	65 53	245
Marſeilles - - -	42 17 49	23 3·5	72·40	63 10	240
Perpignan - - -	42 41 53	20 33·5	72·55	65 18	248
Barcelona - - -	41 23 8	19 52	71·80	64 37	245
Madrid - - -	40 25 18	13 58	75·20	67 41	240
Valencia - - -	39 28 55	17 29	70·70	63 38	235
Medina del Campo	——	——	73·50	66 9	240
Guadarama - -	——	——	73·50	66 9	240
Ferrol - - - -	43 29 0	9 24·5	76·15	68 32	237
	38 52 15	3 40	75·18	67 40	242
	37 14 10	3 30	74·90	67 30	242
	32 15 54	2 52·5	71·50	64 21	——
In the Atlantic	25 15 0	0 36 W	67	60 18	239
Ocean between	21 36 0	5 39	64·20	57 49	237
Africa and	20 8 0	8 34	63	56 42	236
America.	14 20 0	28 3	58·80	52 55	239
	12 34 6	33 14	50·15	45 8	234
	10 46 0	41 24	46·40	41 46	229
	10 59 30	44 31·5	45·50	41 57	237

In

In the year 1776, according to Cavallo, the inclination of the needle in the undermentioned latitudes and longitudes was as follows:

Latitude.	Longitude.	Inclination.
24° 24′	18° 11′	59°
10	22 52	44 12
0	37 38	30 3

Since Coulomb and Caffini no longer employ themfelves with obferving the declination, I know no place in the earth where the declination has been determined with certainty to 10″, and not ten where it has been determined to 1′. What uncertainty ftill prevails refpecting the real declination of the needle at Paris! The ten obfervations which I made at fea, will ferve hereafter to determine whether the inclination fpeedily changes. The longitude and latitude of the places of obfervation were always accurately determined at the time by means of a Ramfden's fextant, divided from 15 to 15 feconds; and a Berthoud's chronometer. You will fee by them that the inclination decreafes very rapidly from the latitude of 37°, and that it increafes lefs towards the eaft than to the weft from the latitude of 37° to 48°.

It appears to me, that in the higher chain of the calcareous mountains of this province, not far from the equator, fmall elevations above the level of the fea derange the inclination much more ftrongly than the higher mountains in the Pyrenees and Old Caftile. As a proof, I fhall here give obfervations made at four places which lie pretty nearly in an arch from north to fouth (comprehending 24′.)

Places.	Height above the Sea, Paris Feet.	Inclination. Centigrade Degrees.	Inclination. Old Degrees.	Vibrations in Ten Minutes.
Cumana	24	44·20°	39° 47′	229
Zueteppe	1111·2	43·30	38 58	229
Impoffibile	1470·	43·15	38 50	233
Cumanacoa	636	43·20	38 53	228
Cocollar	2352	42·60	38 20	229

Borda is of opinion, as appears by the memorandum drawn up for the men of fcience who accompanied La Peroufe, that the intenfity of the magnetic power is the fame all over the earth; as he afcribes the fmall variation which he found in it at Cadiz, Teneriff, and Breft, to the imperfection of the compafs. He requefted me to verify this circumftance. You here

here fee that the magnetic power is fo different, that between Paris and Cumana it decreafed from 245 to 229 vibrations in ten minutes, though it does not decreafe with the inclination. This decreafe cannot certainly be afcribed to any change in the goodnefs of the needle, or to other accidental caufes: for the fame needle made in the fame time at Paris 245 vibrations; at Girona, 232; at Barcelona, 245; at Valentia, 235; and, after a journey of feveral months, gave at the fame places exactly the fame number of vibrations as before my departure. Thefe are always the fame, in the open fields, in the houfe, or in a cavern, fo that the magnetic power in any place is always the fame, and remains a long time without change; and feems to be a general power, like that of gravity.

I had the mortification of not being able at fea to make any good obfervations of the declination. Notwithftanding all the trouble I took, I could not find an azimuth compafs on which I could depend within 40′. This is the true reafon why I have made no mention to you of the declination at fea. The point, however, where the inclination vanifhes, certainly lies further to the weft than marked in Lambert's chart, in the Berlin aftronomical almanac for 1779. A good obfervation, made in the year 1775 aboard an Englifh fhip from Liverpool, places this vanifhing point in latitude 29° north, long. 66° 40′ weft. I obferved, with great care, the declination at two places on the coaft of America with a compafs by Lenoir, in which the needle is fufpended by a thread, according to the method of Prony and Von Zach. At noon, October 1799, it was at Cumana, and twenty leagues further eaft, 4° 13′ 45″ eaft: at Caripe, the chief place of the Capuchin miffion among the Chaimas and Caribs, 3° 15′ eaft.

During the earthquake at Cumana, on the 4th of November 1799, the inclination of the magnetic needle was altered, but not the declination. The former, before the fhock, was 44°·20 of the new divifion; after the fhock it remained at 43°·35. The number of the vibrations of the dipping needle, however, was the fame as before, viz. 229 in ten minutes. This, united to other obfervations, feems in my opinion to prove, that during the earthquake this fmall part of the ball of the earth was changed, and not the needle; for, in diftricts where no figns of an earthquake ever appeared in the primitive chain of accumulated maffes of granite, the inclination was as great after as before.

On account of the intereft which you take in every thing that

that relates to navigation, the following obſervations will not be unacceptable.

I have carefully proved what Dr. Franklin and Captain Jonathan Williams aſſert in the Tranſactions of the American Society * reſpecting the uſe of the thermometer for diſcovering ſhallows at ſea; and am able to confirm, in the fulleſt manner, what they have ſaid. I was aſtoniſhed to ſee how the water became evidently colder as its depth decreaſed, and how the neighbourhood of ſhallows, and of the coaſts, could thereby be announced. The worſt ſpirit of wine thermometer, if only ſenſible, will therefore, in the hands of the moſt ignorant mariner, be a very uſeful inſtrument in the night-time, and during ſtorms, or when it is difficult or impoſſible to heave the lead. This obſervation I cannot too ſtrongly recommend to the attention of the Board of Longitude. Our whole crew were aſtoniſhed to ſee how ſpeedily the thermometer fell, when we approached the large bank which extends from Tobago to Grenada, and eaſt from Margarita. Theſe obſervations may be made with more eaſe, as the water of the ſea, in an extent of 12000 ſquare miles, has always the ſame temperature day and night; ſo that the moſt ſenſible thermometer, during from four to ſix days ſail, does not riſe or fall above 0·3°. In the neighbourhood of ſhallows it was from 2 to 3 degrees, and more, colder. This obſervation of Franklin, hitherto forgotten, may at ſome period be of great uſe to navigation; not that ſeamen ſhould throw aſide the lead entirely, and truſt to the thermometer, for this would be folly; but becauſe the obſervations may be ſo eaſily repeated, and becauſe the thermometer will announce the danger much ſooner than the lead, as the colder water above the ſhallows leſſens the temperature in the water in the neighbourhood. I can aſſert that this new mean is not more uncertain than the log, and the helps already employed in navigation. If the thermometer does not fall, navigators ought not to depend that they are entirely ſecure from ſhallows; but if it falls, they muſt be on their guard. This warning is certainly more valuable than our ſea charts, where the ſhallows are in general laid down in a very incorrect manner; and to immerſe a thermometer in a bucket filled with ſea water is certainly much eaſier. I have alſo meaſured ſeveral times the ſpecific gravity and temperature of the ſea water, at the ſurface and at certain depths, by means of Dollond's balance and thermometers, which are placed in caſes furniſhed with a valve. As my inſtruments were compared with the beſt Pariſian ones, and as I could be more

* Vol. iii. p. 32.

certain in regard to my longitudes than is usual, the small chart in which I intend to make known the result of these experiments will be interesting. In the latitude of 17° and 18° north, there is a zone in the sea between Africa and the West Indies, where, without an extraordinary current prevailing, the water is denser than under a less latitude. The following are some of my results respecting the temperature of the sea water:

North Lat.	Long. from the First Meridian.	Temperature of the Sea Water at the Surface.	Temperature of the Air.
43° 29′	9° 29′ E.	12°	18°
39 10	3 41·5	12	13
36 3	2 57	12	14
35 8	2 15	13	16.5
32 15	2 52·5	14·2	13·5
30 35	3 6	15	16
28 55	2 37·5	15	17
26 51	— 47	16	15
20 8	8 33 W.	17	16
18 53	10 5	17·4	17
18 8	13 2	17·9	19
17 26	15 26	18	16
15 22	22 49	18·5	20
14 57	24 40	19	17
13 51	30 2·5	19·8	18·9
10 46	41 24	20·7	20·3
10 28	46 31	21	17 to 27
10 29	46 35	17·8	23

This letter was written at Cumana, but for want of a proper opportunity I was obliged to bring it with me to this large capital of the province of Caraccas, which is situated in a fertile valley abounding with cacao, cotton, and coffee, 2400 feet above the level of the sea, and in a climate perfectly similar to that of Europe. The thermometer in the night-time falls to 11°, and in the day never rises above 17° or 18°.

The cloudy weather, which here daily follows the sun's passage over the meridian, makes the observation of corresponding altitudes very difficult and uncertain, as the afternoon observation is often lost. The cloudy weather after the earthquake of Cumana deprived me of an opportunity of observing the immersions of the second satellite of Jupiter on the 2d and 9th of November. I shall here give you the principal

cipal determinations of the longitude, which I made by means of Berthoud's chronometer, from the obſerved horary angles.

	Long. weſt.		
Cumana, caſtle of St. Antonio, lat. 10° 27′ 37″ and (taking the long. of Madrid at 13° 58′)	46°	31′	
Puerto Eſpana, in the iſland of Trinidad	43	49	30″
Tobago, the eaſtern extremity -	42	47	30
Macanao, the weſt part of the iſland St. Margarita	46	35	30
Punto Araya, in New Andaluſia -	46	35	30
Iſland of Coche, eaſtern extremity -	46	12	
Bocca de Drago (not certain) -	44	23	
Cabo de tres Puntas - -	44	54	30

Caraccas à la Trinité 10° 31′ 4″ latitude, very good.

I ſhall here alſo mention, that neither wind, ſtorms, nor the earthquake, have had the leaſt influence on the regular daily variation in the ſtate of the barometer; and that, according to C. Richard, this daily variation amounts at Surinam to two lines.

Theſe obſervations are the more intereſting, as all the charts of this part of the world are very incorrect; and the agreement of my longitude of Teneriff and Tobago, with the obſervations of Borda and Chabert, within from 2″ to 5″ of time, is a proof of the excellence of my chronometers.

The ſerenity of the nights between the tropics gave me an opportunity of comparing with each other the ſtrength of the light of the auſtral ſtars; which in ſome of them, ſuch as the Crane, Altar, Toucan, and the Foot of the Centaur, ſeems to have changed ſince the time of La Caille. I employed the method propoſed by Dr. Herſchel, and diaphragms of the ſame kind as thoſe uſed for the ſatellites. If the light of Sirius be ſuppoſed equal to 100 parts, and that of Procyon 88, the light of the following ſtars, according to my obſervations, are as below expreſſed.

	Parts.		Parts.
Canopus -	98	α in the Phœnix -	65
α in the Centaur -	96	α in the Peacock -	78
Acharnar - -	94	α in the Crane -	81
α in the Indian -	50	β - - -	75
β - - -	47	γ - - -	58
α in the Toucan -	70		

LVIII. *Description of a portable Barometer. By Dr. J.* Guerin, *of Avignon, Professor of Natural History in the Central School of Vaucluse, and Member of several Societies* *.

No philosophical instruments are so varied in their construction as barometers, and especially the portable ones. But if the latter leave no room for improvement in regard to exactness, they are exceedingly complex, and liable to be deranged; so that travellers are often obliged to renounce the observations they depended on making during the course of their journeys.

It is very rare that the best barometers are not either deranged or broken in the course of a long journey. Nothing however but a violent fall can break the one which I am about to describe. It may be transported on any carriage, and along the worst roads, provided care be taken to keep it inverted; that is, with the cup uppermost. Naturalists who travel stood in need of this simple instrument, the only barometer really portable being that of Deluc, the construction of which is more difficult; and which, in regard to observation, is liable to inconveniences which in mine I have endeavoured to avoid.

I shall say nothing of a great number of other barometers more or less complex, as well as defective, and in general inconvenient for transportation. They all have faults, which it would be too tedious to enumerate. It has been proposed to use for this purpose iron tubes with glass tubes cemented to them; but it has not been remarked that the mercury at length oxidates, loses its brilliancy, and adheres partially to the tube.

My barometer, the simplest possible, will be attended with this advantage, that it can be procured at a moderate price. The description of it is as follows: A, B, C, (Plate IX, fig. 2.) is a glass tube about 33 inches in length, bent from B to C in such a manner as to leave the distance of an inch between its branches.

D is a stop-cock, made of iron, ivory, box-wood, or any other substance not susceptible of being attacked by mercury, and which serves for intercepting at pleasure its passage into the cup. Box-wood, dipped in boiling wax, appears to me very proper for making a good stop-cock, as it can permit the mercury to pass between the cock and its key-stopper (if the mercury contained in the tube should dilate too much), without

* From the *Journal de Physique*, Frimaire an. 10.

suffering

ſuffering it to eſcape when it does not dilate. Ivory ſtop-cocks are attended with the ſame advantage. E is a cylindric cup, the diameter of which muſt be known; about an inch appears to me to be the moſt convenient dimenſion for a barometer of this kind.

F is a piece of ſhamoy leather with which the cup is covered. It permits the air to exerciſe an action on the mercury, and prevents it from eſcaping when the inſtrument is carried.

After having boiled the mercury in the tube, the cock and cup muſt be cemented at the interior bending. A little mercury is then to be added, that it may float above the ſtop-cock, when it is ſhut after the inſtrument has been inclined.

It may readily be conceived that the interior diameter of the long tube, and that of the cup, being known, the greateſt preciſion may be obtained when they are perfectly cylindric; for, the diameter of the two cylinders being known, the ratio of their ſolidity may be known *.

Thoſe who wiſh to avoid the trouble of this ſhort calculation may cement to the ſtop-cock a very large cup; and then the error occaſioned by the elevation of the level of the mercury will be a very ſmall fraction of a line, even if the barometer ſhould deſcend ſeveral inches.

In caſe it may not be thought proper to adapt to the ſtop-cock a large cup, which would render the barometer much heavier, nothing will be neceſſary but to meaſure the height of the mercury above the line of the level, and to ſubtract it from that of the mercury in the long tube. Let us ſuppoſe, for example, that the barometer marks 23 in. 6 lin. and that the mercury in the tube is elevated only one line; the remainder will be 23 in. 5 lin. for the height of the mercury.

By taking a view of the height of the barometer, without making the leaſt calculation, it will be eaſy to deviſe how the ſcale may be graduated ſo as to make this correction needleſs.

A nonius, to indicate the $\frac{1}{20}$th or even $\frac{1}{100}$th of a line may be adapted to this inſtrument; but the variations in the upper ſtrata of the atmoſphere are too well known to admit of our having recourſe to ſo exact a graduation in the meaſuring of heights. Ought we not to conſider as very correct a barometer that indicates $\frac{1}{10}$th of a line? The errors in the meaſure-

* Let us ſuppoſe that the diameter of the barometric tube is two lines, and that of the cup twelve, the aſcenſion of the mercury in the cup will be to the depreſſion of that in the tube as the ſquare of the ſurfaces; which in this caſe is as 4 to 144, or 1 to 36. When the barometer indicates 25 inches, we muſt then ſubtract from that height 1-36th, that is to ſay, a line, and we ſhall have the real height, 24 inches 11 lines.

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ment of great heights arise rather from the unequal densities of the strata of the atmosphere, and the attraction of the mercury in the tube, than from defects in the subdivision. A barometer will always be pretty good, if the mercury it contains is well purified and dry; and if it has been boiled in the tube; if the tube is at least $\frac{3}{4}$ths of a line in diameter; if, by a proper construction, the air is prevented from introducing itself into it, notwithstanding the agitation it is exposed to in the course of a long journey; and, in particular, if the interval between the surface of the mercury contained in the tube and that in the cup can be exactly measured.

This barometer differs very little from that of Deluc; but it is much simpler in the construction, and less expensive. The cup which I have substituted for the tube, covered with leather, serves to retain a little mercury in the reservoir, that no vacuum may remain between the stop-cock and the bending, when the barometer is inclined for the purpose of being transported. As it is not deep, and has a cylindric form, it may be easily cleaned in case the metal should become oxidated. Deluc acknowledges that his barometer is inconvenient for daily observation. The one here described has not the same fault. It is both portable and fit for the closet. I have already said that, neglecting all correction, the error resulting from the difference of the levels would amount only to $\frac{1}{32}$d—a difference which may be neglected in making meteorological observations.

One of the greatest advantages of this instrument is, that it may be rendered comparative, by choosing tubes of about two lines diameter, and cylinders of an inch. It is of no importance whether a cup-barometer maintains itself a little higher or a little lower than a syphon one, if the depression is constant, and its progress regular. I have carried both barometers on mountains more than 1800 toises in height.

I made a great many experiments on this subject on Mount Ventoux. Mine I confess always maintained itself a line lower; but was this depression of a line a fault of the cup-barometer, or did the fault consist in the greater elevation of the other? I do not know whether the solution of this problem would be favourable to Deluc's barometer, or to mine.

If the barometric observations do not require too much time and preparation; and if a simple, strong, and cheap construction supply the place of complex instruments liable to be deranged; I can assert that all these advantages are united in this barometer, by means of which correct observations may be made, which will supply our scientific institu-

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tions with portable and comparative instruments, and with which the level of the whole surface of France might be taken without any sensible error, and without having recourse to long and tedious operations. The society of Manheim sent meteorological instruments to various distant observers; but the construction of them rendered this excellent plan fruitless; as the barometers, when they arrived, were all broken or deranged.

Would it not be of advantage to science, if the French government would send to all the schools, learned societies, and other establishments, barometers, by means of which comparative meteorological tables might be constructed; by which the height of the mountains and different positions might be ascertained, and valuable observations made, which have been omitted in most topographical surveys, or have been made so badly, in consequence of the inaccuracy of the barometers, that no dependance can be placed on them?

The advancement of philosophy and natural history is the object I proposed in publishing these reflections. Comparative observations might render the study of meteorology less dry and more agreeable. The want of correct observations is the cause of our being as yet so little acquainted with the extent of the variations in the atmosphere, the point of their maximum or minimum, &c. &c. There is reason to think that such data would increase the probability of meteorological prognostication, and render more interesting a branch of natural philosophy yet little known. The corresponding heights of mountains and plains might be of great service to the geologist, and enable him to resolve the most curious problems.

P. S. I have learnt, from C. Dolomieu, that professor Pictet employed a barometer nearly similar to that here described. But as I am unacquainted with that instrument, and have used mine in journeys for four years without its being deranged, I publish this description of it with confidence, being persuaded that it may be of use to philosophers and naturalists.

LIX. *Ana-*

LIX. *Anatomical Obſervations on the Structure of the Ornithorynchus Paradoxus. By* J. F. BLUMENBACH, *Profeſſor of Medicine at Göttingen, and Member of the Medical Society of Paris* *.

THE ſpecimen of this animal in my poſſeſſion is about 19 inches in length; the head is 4 inches; the tail nearly the ſame; and the neck and trunk 10 inches †.

It is covered with two kinds of hair. The interior gray, ſhort, and very ſoft, like down; that on the outſide, mixed with others longer and ſtiffer, browniſh on the back, and yellowiſh on the belly.

The limbs are ſhort. The fore feet are about $2\frac{1}{2}$ inches in length; the hind feet are ſomewhat longer. The feet have five toes, palmated (*natatorii*). The natatory membranes of the fore feet are very broad, not inſerted between the toes, but attached below them, ſo as to be better calculated for diving.

The tail flat, and covered with ſtiff hair like briſtles.

But what appears moſt ſingular and anomalous is the conformation of the head, being furniſhed with a broad beak reſembling that of a duck. The mandibles, ſpatula-formed, are flat. The upper one, about 2 inches in length, and $1\frac{1}{3}$d in breadth.

Theſe mandibles are covered with a coriaceous membrane, which extends to the corners of their aperture.

The under mandible, which is narrower than the upper, is ſerrated (*a*, fig. 1. Plate IX.) on the edges, as is the caſe in a duck's bill. The palate is furrowed acroſs.

My ſpecimen is deſtitute of teeth, like that deſcribed by Dr. Shaw; but I have lately been informed, in a letter from Sir Joſeph Banks, that in another ſpecimen two ſmall *dentes molares* have been found on each ſide of each mandible.

The whole beak at the root is edged with an undulated membranaceous border running acroſs it (*b*).

* From *Mémoires de la Société Médicale d'Emulation, quatrième année.*

† Dr. Bobba, a very reſpectable phyſician now reſident at Göttingen, had before ſent us a very intereſting note on the *ornithorynchus paradoxus*, in which he ſays that there are three ſpecimens in Great Britain ſimilar to that in the poſſeſſion of profeſſor Blumenbach; viz. one in the Britiſh Muſæum; one in the poſſeſſion of Mr. Dobſon, an amateur of natural hiſtory at London; and a third in the univerſity of Edinburgh. But the information he ſent us having been already publiſhed in the *Bulletin des Sciences*, we did not think it neceſſary to inſert it in this work. We have confined ourſelves therefore to theſe new obſervations of profeſſor Blumenbach.—*Note of the French Editor.*

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The conformation of the cranium, taken from the occipital condyli (*cd*), and the intermaxillary bones (*ef*), has, on the firſt view, a great reſemblance to that of the duck. The bones of the ſkull are divided by no ſutures; but the interior chamber of the brain is divided by an oſſeous hook (*g*) running lengthwiſe, which is found in no other quadruped *, and ſimilar to that of the *tetrao urogallus*.

What ſeems to be highly worthy of notice, however, is the remarkable apparatus of the nerves of the ſecond branch of the fifth pair, and the diſtribution of it in three regions of the coriaceous membrane which ſurrounds the beak. The nerve (*h*) which proceeds from the lower orbital foramen goes to the tranſverſe edge: the other (*i*), which iſſues behind the intermaxillary bones, is diſtributed on the labial edges: the third (*k*), which iſſues from the ſynchondroſis, that divides the anterior hooked legs of the intermaxillary bones, proceeds to the integument of the beak.

Comparing this large apparatus of maxillary nerves, with which nature has provided this coriaceous integument, and the like ſtructure of the duck's bill, there can ſcarcely be a doubt that this highly ſenſible membrane, which ſurrounds the beak, ſerves, in a very particular manner, in the *ornithorynchus*, as in aquatic birds, for the purpoſes of feeling, by means of which theſe animals can ſearch for their food in muddy places, where neither ſight nor ſmell can be of any uſe to them in that reſpect. Hence the eyes of the *ornithorynchus* are very ſmall, but the noſtrils (*l m*) exceedingly broad.

This wonderful animal, therefore, affords an exception to the order of the internal ſenſes aſſigned to quadrupeds by Buffon †, who ſays: "In quadrupeds, ſmell is the firſt of ſenſes; taſte is the ſecond; or rather, theſe two form only one: ſight is the third; hearing the fourth; and feeling the laſt."

In the annexed figure, one of the bones of the ſkull is repreſented broken, that the interior of the chamber of the brain may be ſeen. N, denotes the orbita; O, the zygoma; P Q, the proceſſus molares of each mandible. The other parts will be underſtood from the preceding deſcription.

* I once found, but the inſtance is very rare, a ſimilar oſſeous hook in the cranium of a woman about thirty years of age.

† Hiſtoire des Oiſeaux, tom. i. p. 48.

LX. *On*

LX. *On the Analysis of Wine.* *By* C. Chaptal.

[Concluded from p. 142.]

NOW that we are acquainted with the faults in the construction of the distilling apparatus, let us try to make an application of it in order to improve the knowledge we have acquired in regard to distillation, and the art of managing the fire.

It appears to me that the whole art of distilling may be reduced to the three following principles:

1st, To heat at once, and in an uniform manner, all the points of the mass of the liquid.

2d, To remove all those obstacles which may confine the ascent of the vapours.

3d, To effect the condensation as speedily as possible.

To fulfil the first condition, it will be necessary that the mass of the liquid should have little depth; and this requires that the bottom of the boiler should present a very large surface, that the fire may be applied in a great many points.

The bottom of the still ought to be a little bent inwards. This form is attended with two advantages: the first is, that by these means the fuel is at an equal distance from every point of it, and that the heat is uniform throughout; the second is, that by this construction the bottom of the boiler has more strength, and the matters which may be deposited at the bottom of the liquor are thrown to the angles which rest on the mason work, and where, consequently, it is less dangerous. When this deposit falls upon parts immediately exposed to the direct action of the heat, it forms a crust, which prevents the liquor from being in contact with the part of the boiler covered by it, and the fire then burns the metal. This inconvenience is not to be feared, if the deposit by the curved form of the bottom is thrown into the angles, where it is withdrawn from the direct action of the heat.

The fire must be made to circulate around the boiler by means of a spiral chimney; in which case none of the heat will be lost, as the whole liquid is surrounded by it, and exposed to its action in an uniform manner.

That the column of steam which rises may experience no obstacle in its ascent, the sides of the boiler ought to rise in a perpendicular direction; and the steam must be maintained at the same degree of expansion till it has reached the refrigeratory. But the steam rising freely, and being condensed by coming into contact with the cool sides of the capital, would fall back into the boiler or body of the still, if the sides were not inclined in such a manner as to make the drops of the liquid, which attach themselves to them, trickle down into the gutter which conveys them into the worm. I have found by calculation that this inclination ought to be at least 75 degrees to the horizon. It is also necessary that the water of the refrigeratory should be frequently renewed; otherwise it will soon acquire the temperature of the steam, and be no longer able to condense it.

Though these principles in regard to distillation are incontestable, some modifications must be made in them for the purpose of facilitating the operation: by giving to the aperture of the boiler a diameter equal to that of the base, the capital becomes very wide; it is consequently necessary to give it a considerable height, that its sides may have the inclination of 75 degrees. This construction is attended with two great inconveniences: the first is, that it renders the capital heavy, clumsy, and expensive; the second, that it makes it difficult to give to the upper edges of the boiler that strength necessary to withstand the effort of the capital. These considerations induced me to make some changes in the above construction. These changes all relate to the form of the boiler: I make the sides to project a little as they rise, and then bring them nearer to each other towards the top, in such a manner that the diameter of the aperture may correspond to that of the bottom. This form removes the two effects above mentioned, and has the advantage of presenting a border at the upper part, against which the bubbles arising from too strong an ebullition may be broken, and thrown back to the centre of the boiler.

Besides this change in the form of the boiler, I thought it proper to take away the refrigeratory with which the capital was covered, and which was attended with the inconvenience of cooling the steam, and forming a cloud of vapour in the inside, which opposes its further ascension.

It may be observed, that in distilling by means of a retort in a sand-bath, it will be sufficient to apply a cool body to the retort to produce this effect: striæ are immediately seen formed on the sides, and the liquor falls to the bottom of the retort.

If I at first proposed to preserve the refrigeratory, it was because I ascribed to it a part of the effects which depended on a proper construction of the furnace. I, however, afterwards found that a still greater effect was obtained by suppressing the refrigeratory. Besides, this change was more economical, and occasioned less embarrassment.

After this I conceived that the great art of condensing the vapours was confined to enlarging the beak of the capital,

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and carefully cooling the worm. By these means the vapours escape from the still with greater facility, as they are admitted into the worm by the speedy condensation of those which preceded them.

These different improvements began to be introduced into Languedoc about fifteen years ago. The brothers Argand greatly contributed to make them be adopted. They first established stills according to these principles, and found them attended with so much saving, in regard to time and fuel, that from the results of comparative experiments it has been estimated at four-fifths.

I have myself superintended several establishments of this kind, where the same principles were adopted. In my opinion, it is hardly possible to carry improvement any further; and it is to be wished that this method of distillation should become general.

But it is not so much to the form of the apparatus as to the construction of the fire-place, and the proper management of the fire, that these extraordinary effects are to be ascribed. The posterior edge of the grate ought to correspond to the middle of the bottom of the boiler, that the flame may fall upon and heat the whole bottom in an uniform manner. The distance between the boiler and the grate ought to be from 16 to 18 inches when coals are employed; and the chimney ought to be spiral.

Besides saving in regard to time, fuel, labour, &c. this form of apparatus has an influence on the quality of the spirit. It is far sweeter than any other, and has no empyreumatic taste; which is a fault almost inseparable from the common spirits used in commerce: this quality, which renders spirits of this kind so superior to others, had almost become cause of their being rejected, because the inhabitants of the north, whose principal beverage this liquor is, found it too sweet. It was therefore necessary to mix it with *burnt* spirit, in order to give it a good name. This fiery taste may be easily communicated to it by maintaining the distillation beyond the usual time. The liquor which passes over towards the end of the operation has evidently a burnt taste.

In the arts, it is necessary to humour the taste and even the caprice of the consumer; and what among us is rejected in consequence of its bad taste, may appear exquisite and delicious to an inhabitant of the north. Burning liquors, which in the north might be accounted weak, are too powerful for the sensible nerves of the inhabitants of the south. Montesquieu has observed with great ingenuity, that to give a Muscovite sensibility you must flay him.

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By the comparative experiments I have been able to make I am convinced that a little more spirit is obtained by this method than by the old one; the cause of which is, that the spirit issues cool from the apparatus, and sustains no loss by evaporation. The distilleries therefore in which this improved apparatus is employed have no sensible smell of the spirit.

When wines are distilled, the operation is carried on till the moment when the liquor which passes is no longer inflammable.

Wines furnish more or less spirit, according to their degree of strength. Very generous wine furnishes a third of its weight. The mean term of the product of our wines in the south is a fourth of the whole: there are some which furnish a third.

Old wines give better spirit than the new; but they furnish a less quantity, especially when the decomposition of the saccharine body has been terminated before the distillation.

What remains in the boiler after the spirit is extracted is called *vinasse:* it is a confused mixture of tartar, colouring matter, dregs, &c. This residuum is thrown away as useless; yet by drying it in the air, or in a stove, very pure alkali might be extracted from it by combustion.

In some distilleries the *vinasse* is acidified, in order to be distilled, and to extract from it the little vinegar that has been formed in it.

The spirit is stronger according as it is mixed with a greater or less quantity of water; and as it is of importance in commerce to be able to ascertain easily the degrees of strength, many researches have been made to discover the means of determining it.

The distiller judges of the strength of the spirit by the number, size, and permanency of the bubbles which are formed when the liquor is shaken: for this purpose he pours it from one vessel into another, lets it fall from a certain height, or, what is more common, puts it into a long flask, which is filled two-thirds, and then shakes it violently, keeping the mouth shut with his finger. This last apparatus is called in French *la sonde.*

The proof by combustion, in whatever manner performed, is very defective. The regulation of 1729 prescribes putting gunpowder into a spoon, covering it with liquor, and setting fire to it. The spirit, if it inflame the powder, is considered to be of the first quality: when the contrary happens, it is bad. But liquor of the same quality inflames, or does not inflame, according to the proportion employed. A small quantity always inflames; a large one never, because the water which the liquor leaves is then sufficient to moisten the gunpowder, and to secure it from inflammation.

Recourse has been had to salt of tartar (carbonate of potash) for trying the strength of spirits. This alkali dissolves in water, but not in alcohol, so that the latter floats on the solution which is formed.

These methods, more or less defective, have rendered it necessary to try other means capable of determining the strength of spirit, by ascertaining the specific gravity.

A drop of oil poured upon alcohol fixes itself on the surface, or is precipitated to the bottom, according to the degree of the strength of the liquor. This method was proposed, and adopted by the Spanish government in 1770: it formed the object of a regulation; but it is liable to error, since the effect depends on the height of the fall, the gravity of the oil, the size of the drop, the temperature of the atmosphere, the dimensions of the vessels, &c.

In 1772 this important object was resumed by two able philosophers, Borie and Poujet, of Cette, who made known to the distillers of Languedoc a new areometer, which they adopted. They applied to it a thermometer, the different degrees of which indicate at every instant the corrections that ought to be made in the graduation of the areometer, in consequence of the very variable temperature of the atmosphere.

By the help of this areometer, not only may the degree of strength be ascertained, but the spirit may be carried to any point required; for this purpose different weights are provided. The heaviest is marked *Dutch proof*, the lightest *three-sevenths*: if the weight marked Dutch proof be therefore screwed to the lower extremity of the shank, and if the instrument be immersed in liquor of *three-sevenths*, it will sink a great deal too much; but it may be brought back to Dutch proof by adding four-sevenths of water.

On the other hand, if the weight *three-sevenths* be screwed to the shank, and if the areometer be then immersed in liquor Dutch proof, it will rise above the last term, and may be easily brought back to that degree by adding stronger alcohol.

When spirit is distilled for the purpose of extracting alcohol from it, a *balneum mariæ* is commonly employed: the heat is then more gentle, and more uniform, and the product of the distillation is of a better quality: this product is the common spirit of wine used in commerce.

3d, *Tartar.*

3d, *Tartar.* Tartar exists in verjuice: it is found also in must: it concurs to facilitate the formation of alcohol, as we have already observed from the experiments of Bullion. It deposits itself on the sides of the casks in consequence of rest, and forms there a crust of greater or less thickness, rough, with crystals very badly determined. Some time before the vintage, when the vessels are getting ready for receiving it, the casks are opened, and the tartar is taken out, to be employed in commerce and for various other purposes.

Tartar is not furnished by all wines in the same proportion. Red wines give more than the white. The highest coloured and thickest generally give the most.

The colour of the tartar also varies very much, and it is called *red* or *white*, according as it is produced from either of these wines.

This salt is very little soluble in cold water: it is much more so in boiling water. It scarcely dissolves in the mouth, and resists the pressure of the teeth.

It is freed from its colouring principle by a simple process, and is then known under the name of *cream of tartar.* For this purpose it is dissolved in boiling water, and, when the water is saturated, the solution is put into earthen vessels to cool: as it cools, it precipitates a stratum of crystals which are already almost free from colour. These crystals are again dissolved in boiling water: four or five per cent. of an argillaceous and sandy kind of earth, found at Murviel, near Montpellier, is diffused through the solution, and it is then evaporated to a pellicle. On cooling, it precipitates white crystals, which being exposed in the open air on cloths for some days, acquire that whiteness which belongs to cream of tartar. The mother-waters are reserved for being employed in new solutions. Such is the method practised at Montpellier and in its environs, where the principal part of the manufactories of cream of tartar are established.

The tartar is employed also as a flux: it is attended with the double advantage of furnishing the carbon necessary for the deoxygenation of the metals, and also alkali, which is one of the best fluxes known.

Tartar is purified also by calcination. Its acid is decomposed and destroyed by this process, and there remains then nothing but the alkali and the charcoal: the alkali is dissolved in water, the liquor is filtered, and, being then concentrated, that salt known in the shops under the name of *salt of tartar*, or *carbonate of potash*, is obtained.

The alkali furnished by tartar amounts only to one-fourth of its weight.

4th, *Extractive matter.* The extractive principle abounds in must. It appears to be dissolved in it by the help of the sugar; but when the fermentation changes the nature of the saccharine principle, the extractive matter sensibly decreases. A portion brought back then almost to the state of fibrine matter is precipitated: the deposit is the more sensible as the fermentation slackens, and as the alcohol is more abundant. This is what constitutes in particular the lees. These lees are always mixed with a pretty considerable quantity of tartar, which it envelops.

There always exists in wine a portion of extractive matter which is in perfect solution, and from which it may be freed by evaporation. It is more abundant in new wines than in old. The older they grow, they seem to be the more completely freed from it.

These lees, after being dried in the sun or in a stove, are well pressed, and then burnt, to extract that sort of alkali called in commerce *wood ashes.* The combustion is effected in a furnace, the sides of which are raised in proportion as it takes place: the residuum is a porous mass, of a greenish gray colour, which forms about the thirtieth part of the lees burnt.

Wines are freed from these lees by drawing them off, in order to preserve them from acid degeneration.

5th, *Aroma.* All natural wines have an odour more or less agreeable. There are some, even, which are indebted for a great part of their reputation to the agreeable odour they exhale. This is the case with Burgundy wine. This perfume is lost by too tumultuous a fermentation: it becomes stronger by age. It rarely exists in very generous wines; either because it is concealed by the strong odour of alcohol, or because the strong fermentation which has been necessary to develop the spirit has extinguished or dissipated it. This aroma does not appear susceptible of being extracted, to be communicated at pleasure to other substances. Heat even seems to destroy it; for, except the first liquid which passes over by distillation, and which retains a little of the odour peculiar to wine, the spirit which comes after has no longer the characters that essentially belong to it.

6th, *The colouring principle.* The colouring principle of wine exists in the pellicle of the grapes. When the must is fermented without the skins, the wine is white. This colouring principle does not dissolve in the vintage but when the alcohol is developed in it; and it is only then that the wine becomes coloured. The colour is stronger according as the fermentation has been more tumultuous, or as it has been left longer in the vat. Mere pressure of the grapes by treading

treading them, if done with care, may mix with must a quantity of colouring matter sufficient to make the mass assume a very intense colour. When it is required to obtain wine free from colour, the grapes are collected while the dew is upon them, and they are trod as little as possible.

The colouring principle is in part precipitated in the casks along with the tartar and the lees; and when the wine is old, it is not uncommon to see it entirely free from colour. The colour then deposits itself in pellicles on the sides of the vessels, or at the bottom: it is seen floating in the liquid like films, which render it turbid.

If bottles filled with wine be exposed to the sun, a few days will be sufficient to precipitate the colouring matter in large pellicles; and the wine loses neither its perfume nor its qualities. I have often made this experiment on the old highly coloured wines of the south.

To precipitate the principle of the colour, nothing is necessary but to pour lime water into the wine. In this case, the lime combines with the malic acid and forms a salt, which appears in the liquor under the form of light flakes. These flakes gradually deposit themselves, and carry with them all the colouring principle. The deposit is black or white, according to the colour of the wine employed for the operation. It often happens that wine is susceptible of forming a precipitate, though it has been completely freed from its colour by a first deposit; which proves that the principle of the colour has strong affinity to malate of lime. The coloured precipitate is insoluble both in cold and in warm water. This liquid even produces no change in the colour. Alcohol has scarcely any effect upon it, only it acquires from it a slight tint of brown.

The nitric acid dissolves the colouring principle of this precipitate.

When wine has been reduced to the state of extract, the alcohol poured over it becomes strongly coloured, as well as the water, though in a less degree. But, besides the colouring principle which is then dissolved, there is also a saccharine extractive principle which facilitates the solution.

The colouring principle, then, does not appear to be of the nature of resins; it presents all the characters belonging to a very numerous class of vegetable products, which approach very near to feculæ, without having all their properties. The greater part of colouring principles are of this kind: they are soluble by the assistance of extractive matter, and when freed from this medium they become fixed in a lasting manner.

 LXI. *Some*

LXI. *Some Account of* C. Fourcroy.

THE following account of this chemist is extracted from *Schmeissers Beyträge zur näheren kentnisse des gegenwärtigen Zustandes der Wissenchaften in Frankreich.*

"Fourcroy, who, as I believe, is the first public teacher of the modern chemistry in Europe, is so well known to the literary world by the service he has rendered to that science, and by the many excellent works he has written on the subject, that I can no further contribute to extending his fame than by assuring the public that this meritorious man, notwithstanding the employment he has as a member of the Council of Elders, continues to cultivate his favourite pursuit with unabated zeal.

"He indeed continues to lecture on chemistry with great zeal; and his deep knowledge of the science has been so much enriched by the rapid progress it has lately made, that the great store of facts he possesses, and which he explains to a crowded audience with great elegance and beauty of language, often obliges him to exceed the time he usually allots for that purpose. I, however, never found that he either fell short of matter, or that the patience of his hearers was exhausted, though he reads every day, and on some days three lectures, on different branches of chemistry.

"When we reflect on the various labours which his public duties require, it may readily be conceived how few unoccupied hours he has left for his daily lectures and chemical experiments. The principal places which he occupies are the following: he is a member of the Council of Elders and of the National Institute; of the Society for promoting the Arts and Sciences; of the Council of Health; professor of chemistry in the Republican Lyceum, at the National Museum of Natural History, and the General School of Arts. His indefatigable assistant Vauquelin, whose head and hands are entirely employed with experiments, supplies him with abundance of chemical discoveries, which he either confirms or has made himself.

"Sometimes I paid an early visit to Fourcroy at his own house; for this was the only time I could have an opportunity of conversing with him on chemical subjects. I always met with the politest reception, and always found him ready to enter on a discussion of topics relating to our favourite study.

"I saw C. Fourcroy's own chemical laboratory, and the hall in which he gives lectures on natural history. It is not very large, but well fitted for his purpose, and furnished with apparatus and every other thing that can serve for illustrating his

his lectures. I found him much employed at his own houſe on that part of chemiſtry which he has engaged to write for the new Encyclopédie, and which, as I ſaw by his manuſcript, is nearly finiſhed, and will no doubt meet with the approbation of the public. He informed me, that as ſoon as this work was completed he intended to begin another, viz. A Hiſtory of the Revolution effected in France in regard to Chemiſtry, and the Foundation of the Theory reſpecting the different Kinds of Gaſes. Fourcroy was one of thoſe who chiefly contributed to the eſtabliſhment of thoſe learned inſtitutions and ſchools which at preſent exiſt at Paris, and he ſtill labours for their improvement. His principal works are: An Eſſay on the Diſeaſes of Artiſts, by Ramazzini, 1777, 12mo. The Principles of Chemiſtry, 2 vols. 8vo. of which there have been ſeveral editions: the laſt conſiſts of eight volumes. The Principles of Chemiſtry, for the Uſe of the Veterinary School, 2 parts, 16mo. A Treatiſe on the acidulous Springs of Engheim. A Collection of Memoirs in Chemiſtry, 8vo. The Art of knowing and employing Medicines, 2 vols. 12mo. On the Improvement of Medicine by the Phyſical Sciences, 1792 and 1793, 4 vols. 8vo. The Philoſophy of Chemiſtry. And many ſingle papers in the Memoirs of the Academy of Sciences and the Annals of the Society of Medicine; ſome public orations on the ſciences, and a few diſſertations on ſaltpetre.

"Fourcroy is a man about 40 years of age*, of an agreeable aſpect, and great activity. At preſent he is ſo much engaged with public buſineſs, that he has little time left for domeſtic enjoyment; he has therefore ſent his wife to a country retreat, and placed his eldeſt ſon under the care of his friend Vauquelin, at whoſe houſe he enjoys the inſtruction of Stany.

"Fourcroy in the courſe of his converſation ſeemed to have a great attachment to England, on account of the great progreſs which the ſciences have made in that country, and he often expreſſed a deſire of paying a viſit to it on the reſtoration of peace."

LXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

ON the 24th of December was read a paper on friction, by Mr. Southern. He made a number of experiments on the motions of large grindſtones revolving with great rapidity, and aſcertained the power of friction on their axes from the number of revolutions which they performed

* This was written in 1797.

when

when set in motion with various velocities. He considers the results as fully confirming Mr. Vince's principles, that friction is a uniformly retarding force; although the resistance of the air and other accidental circumstances introduced great irregularities into the experiments. He found this force equal to about 1-40th of the weight: the steel spindles running on brass, with the interposition of an unctuous substance.

The Society adjourned to Thursday the 14th of January, when an algebraical paper was read, of which we can give no idea in a short notice.

A paper, by Smith Gibbs, Esq. on galvanism, has also been read before the Society; but we have occupied so large a portion of our present Number with that subject, that we cannot at present enter upon it.

ACADEMY OF THE USEFUL SCIENCES AT ERFURT.

The Academy of the Useful Sciences at Erfurt, in consequence of a prize of thirty ducats, offered by an anonymous friend of chemistry, have proposed the following question: What useful application can be made in chemistry and the arts of the temperatures below zero of Reaumur; and how far is it possible to lower the temperature by artificial means? The Society wishes that those who undertake to answer this question will repeat the experiments of Lowitz, Fourcroy, &c. and examine whether a much greater degree of cold than any yet found may be produced by changing the proportions of the mixtures, employing them in greater quantity, by new mixtures, or by placing more refrigeratory vessels within each other, and, where it can be done, by making experiments in a rarefied atmosphere: also to examine the influence of low temperatures on the different gases shut up with spirit of wine; and to observe their mixture, where possible, in a compressed state; and their influence also on liquids, on solid bodies, and on the galvanic phænomena: and, in the last place, to show what employment may be made of the low temperatures in the arts and manufactures.

The papers on this subject must be transmitted to professor Bellerman, the secretary of the academy, before the end of April 1802; three months after which the prize will be adjudged.

FRENCH NATIONAL INSTITUTE.

The National Institute of France having been formed during the war, the nomination of the foreign associates, instituted by the second article of the law of 3d *Brumaire* (Oct. 25.) year 4, was suspended. As peace has removed this suspension, in the General Sitting, which brings together, on the 5th of each month, the three classes of the Institute,

Institute, each of them presented the names of three candidates; from which the Institute, on the 5th *Nivose* (26th December) were to choose a first associate for each class.

The three candidates presented by the class of the Mathematical and Physical Sciences are, Sir Joseph Banks, Dr. Maskelyne, and Dr. Herschel.

The three candidates of the Moral and Political Sciences—Mr. Jefferson, President of the United States, Major Rennel, Count Rumford.

The three candidates named by the Class of Literature and the Fine Arts are, M. Haydn, Mr. Sheridan, and Klopstock.

Each class, in order to present its three candidates for election to the three classes united, was itself obliged to elect from a greater number of candidates presented to it by a commission composed of a member from each section. The list of the candidates proposed to each class by the commission was as follows:—

For the first class—Dr. Maskelyne, Mr. Watt, Dr. Herschel, Dr. Priestley, Mr. Cavendish, Professor Volta, Sir Joseph Banks, Professor Pallas, Mascagni, Arthur Young, Esq.

For the second class—Mr. Jefferson, Count Rumford, Norman, Kant, Herder, Sir John Dalrymple, Professor Dugald Stuart, Sir John Sinclair, Ebling, Arthur Young, Esq. Rhabec, Marini, Gaetano, Librarian of the Vatican, Champomann, Lardizobal, David Ramsay, Niebuhr, Horne Tooke, Mr. Fox.

For the third class—Arnold, Cannova the Italian sculptor, Calderari, Cesarotti, Italian poet; Haydn, celebrated musician; Heyne, Klopstock, Merian, of the Academy of Berlin; Sergelle, a Swede; Sarti, celebrated Italian musician; Mr. Sheridan, Horne Tooke.

In the General Sitting, on the 26th of December, the Institute proceeded to the election of the three associated members; when Mr. Jefferson, President of the United States of America, was chosen for the Class of Moral and Political Science. Major Rennel had 157 votes, and Count Rumford 169.

For the class of Physical and Mathematical Science, Sir Joseph Banks, President of the Royal Society of London, was nominated; and for the Class of Literature and the Fine Arts, M. Haydn.

Account of the labours of the Class of the Mathematical and Physical Sciences during the first quarter of the year 10. *Read by* C. Delambre, *secretary.*

Astronomy.—The star discovered about a year ago by Piazzi at Palermo has hitherto escaped the researches of all astronomers. Similar, in regard to the splendour of its light, to to a ſtar of the ſeventh or eighth magnitude, it exhibited none of thoſe appearances that ſerve to diſtinguiſh comets. In colour it reſembled Jupiter; and, from obſervations made in the meridian by M. Piazzi and his aſſiſtant M. Cacciatore, it would ſeem that this ſtar is a planet, the orbit of which gives reaſon to ſuppoſe that its revolution is performed in four years and a half, or five years. Towards the end of *Pluvioſe*, as this ſtar paſſed the meridian too early, it ceaſed to be viſible; and M. Piazzi, aſſiſted by Cacciatore and Cariotti, both endowed with excellent ſight, and well acquainted with the heavens, endeavoured to find it, but in vain, both with a night-glaſs and an achromatic teleſcope of a large aperture. It needs therefore excite no ſurpriſe that, nine months after, all the aſtronomers of Europe ſhould have failed in their reſearches, ſince to the difficulty of the thing itſelf there was added an uncertainty of ſome degrees reſpecting the exact place where it was neceſſary to ſearch for this ſtar, as the elements of its orbit, calculated on too ſmall an arc, cannot, after ſo long an interval, give, in a ſufficiently correct manner, the geocentric places. Beſides, the heavens have been almoſt always cloudy; and to be able to hope for ſucceſs, it would be neceſſary, conſidering the ſmall ſize of this ſtar, to make a correct enumeration of all the ſtars from the ſeventh to the ninth magnitude in the neighbourhood of which it may appear, and to repeat the examination of them every day until the ſmall planet ſhould diſcover itſelf by its motion. It will ſoon be under the ſame circumſtances in which it was at the time of its diſcovery; and if the heavens ſhould become more ſerene, we may ſtill retain ſome hope.

Winter Solſtice of the year 10. (Dec. 1801.)—The weather, ſo unfavourable to the reſearches made to obſerve Piazzi's planet, was no leſs ſo to the determination of the ſolſtitial altitude of the ſun, which was conſtantly concealed from the 19th to the 26th of December; that is to ſay, during thoſe days on which the moſt concluſive obſervations could have been made. However, by collecting thoſe made on the preceding and following days, C. Delambre was able to form nine ſeries of zenith diſtances, obſerved by Borda's circle, which gave him for mean reſult an apparent obliquity of 23° 28′ 3″. This quantity holds nearly a mean place between thoſe found ſeveral years ago by the winter ſolſtices on the one hand, and the ſummer ſolſtices on the other. On this occaſion the obſervation made at the winter ſolſtice approached very near that made at the ſummer ſolſtice. It is well known, that for a long time paſt all aſtronomers who have employed themſelves in determining the obliquity of the ecliptic have always found ſeveral ſeconds leſs in winter

than

than in ſummer. On this occaſion, however, the obſervation made at the winter ſolſtice approached very near to that made at the ſummer ſolſtice. We ſhall not attempt to explain the cauſe. We ſhall content ourſelves with mentioning the fact; but we muſt add, that, according as the thermometer by which the refraction is corrected is poſited within or without the obſervatory, or near the teleſcope, there will be a ſecond leſs or more in the above determination for which the mean of theſe thermometers was taken.

The phyſical part was read by Lacepede, ſecretary.

A commiſſion, conſiſting of Laplace, Coulomb, Hallé, Monge, &c. having been appointed to examine the phænomena of galvaniſm, and to repeat the experiments of profeſſor Volta, they communicated the reſult in a report, in which they explained the theory, and its identity with electricity. (For this report, ſee page 301.)

LXIII. *Intelligence and Miſcellaneous Articles.* *January* 1802.

MINING.

THE proſpect of peace has made conſiderable alteration in the views of the miners of Cornwall and Devon. The expected reduction in the prices of the articles of which ſuch vaſt quantities are conſumed in their works, as timber, gunpowder, iron, &c. and the probable increaſe in the value of tin at leaſt, and perhaps of copper, have given peculiar energy to the carrying on the preſent mines, and to exertions for new diſcoveries. Perhaps the ſpirit of adventure in theſe undertakings was never much higher.

Among the new diſcoveries is that of a courſe of copper ore, in the eaſtern part of Wheal Fortune mine, in the pariſh of Gwennap, Cornwall. And the ſame has been found to extend into the infant concern of Wheal Friendſhip, a mine adjoining on the ſame lode. Penandrea, under the town of Redruth, is throwing up a good deal both of tin and copper, and the extenſive mines of North Downs are ſaid to be rich in the upper levels, though the bottoms have been ſtopped now for ſome time.

A new copper mine is going on ſouth of Wheal Fortune, called Wheal Girl, and the old and great mine Wheal Buſy, which formerly has produced ſo much tin and copper in intimate mixture, it is reported will ſpeedily be put in courſe of working.

On the eaſtern borders of Cornwall, and in the part of Devon adjoining, mining has within a ſhort time been conſiderably extended. Wheal Crowndale, in the pariſh of Taviſtock,

Taviſtock, lately diſcovered, is already producing a conſiderable quantity of copper ore, as alſo are ſome other mines of not long ſtanding in the neighbourhood. A new mine is going on, on Wheal Crowndale lode, to the weſt of that concern.

Among minerals found of late, may be mentioned *fluate of lime*, cryſtallized in cubes, produced in fine pieces from Gunnis-Lake mine (copper lode running in granite) on the Cornwall banks of the Tamar, and ſome ſpecimens of ſcaly iron ore incumbent on cryſtals of quartz from Wheal Crowndale. Some quantity of the rare ſpecies of fluors cryſtallized in tetrahedral priſms has of late been collected from the waſte at the old lead and ſilver mines, not now at work, at Beerfuris in Devon.

ANTIQUES.

Mr. Holland, the architect, is in poſſeſſion of a fine collection of antique fragments purchaſed for him at Rome by Mr. Tatham previous to the revolution. It conſiſts of ſome of the beſt ſpecimens of ornamental ſculpture, and may therefore be conſidered as an acquiſition to the country, as the ſtudy of ſuch models ſerves in an eminent degree to form a correct taſte in ſtudents of architecture.

Dr. Garnett's Lectures on Zoonomia, or the laws of animal life, which commenced on Wedneſday, the 20th of January, have excited conſiderable attention, on account of the novelty of the ſubject. In the firſt lecture, Dr. Garnett gave a general deſcription of the human body, conſidered as a machine conſiſting of bones and muſcles, for the purpoſes of motion, at the inſtance of its intelligent principle. He likewiſe conſidered the various theories of ſenſation and muſcular motion, and concluded the lecture with ſome galvanic experiments, which ſeemed to ſhow that electricity has a conſiderable ſhare in producing theſe phænomena.

In the ſecond lecture, which was delivered on Saturday laſt, he proceeded to conſider the nature of reſpiration, and the cauſe of animal heat. At the requeſt of ſeveral friends in the City, he intends to deliver this courſe in Cornhill every Wedneſday evening.

A. and C. R. Aikin will begin a courſe of evening lectures on various chemical manufactures, and the outlines of general chemiſtry, on Monday the firſt of March next.

The lectures will be twenty-eight in number, and will be given every Monday and Friday, at the Alderſgate Street General Diſpenſary.

Further particulars may be learned at Mr. Aikin's, No. 4, Broad Street Buildings.

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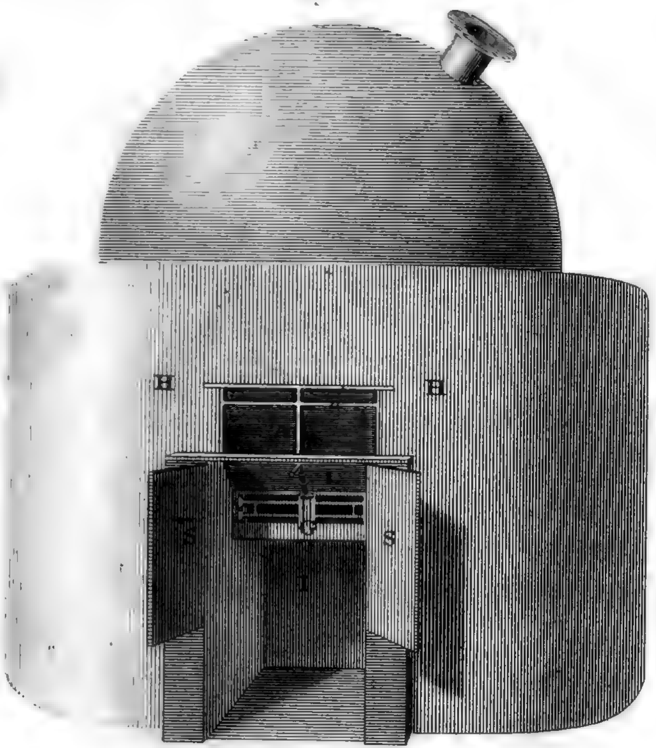

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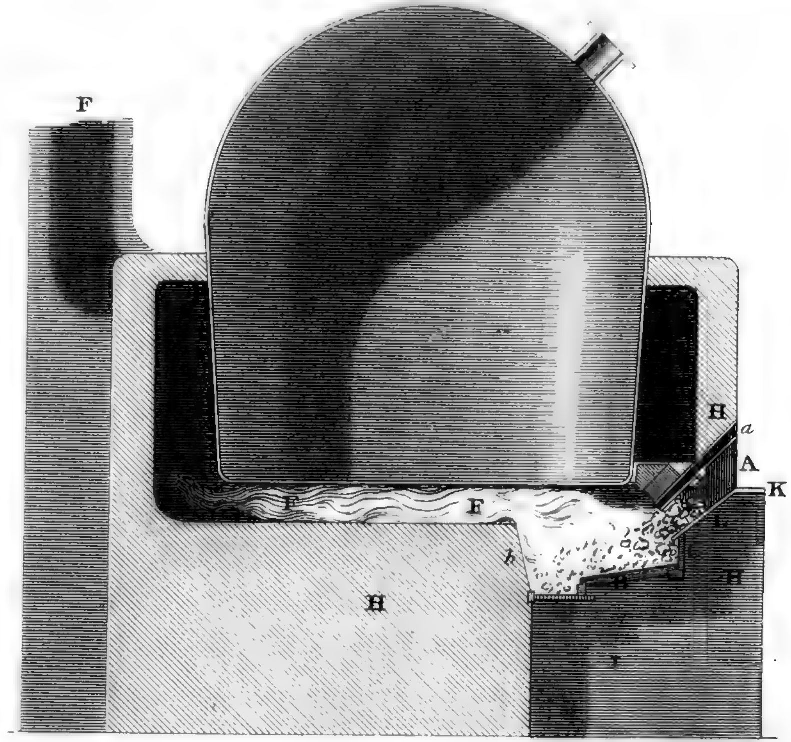

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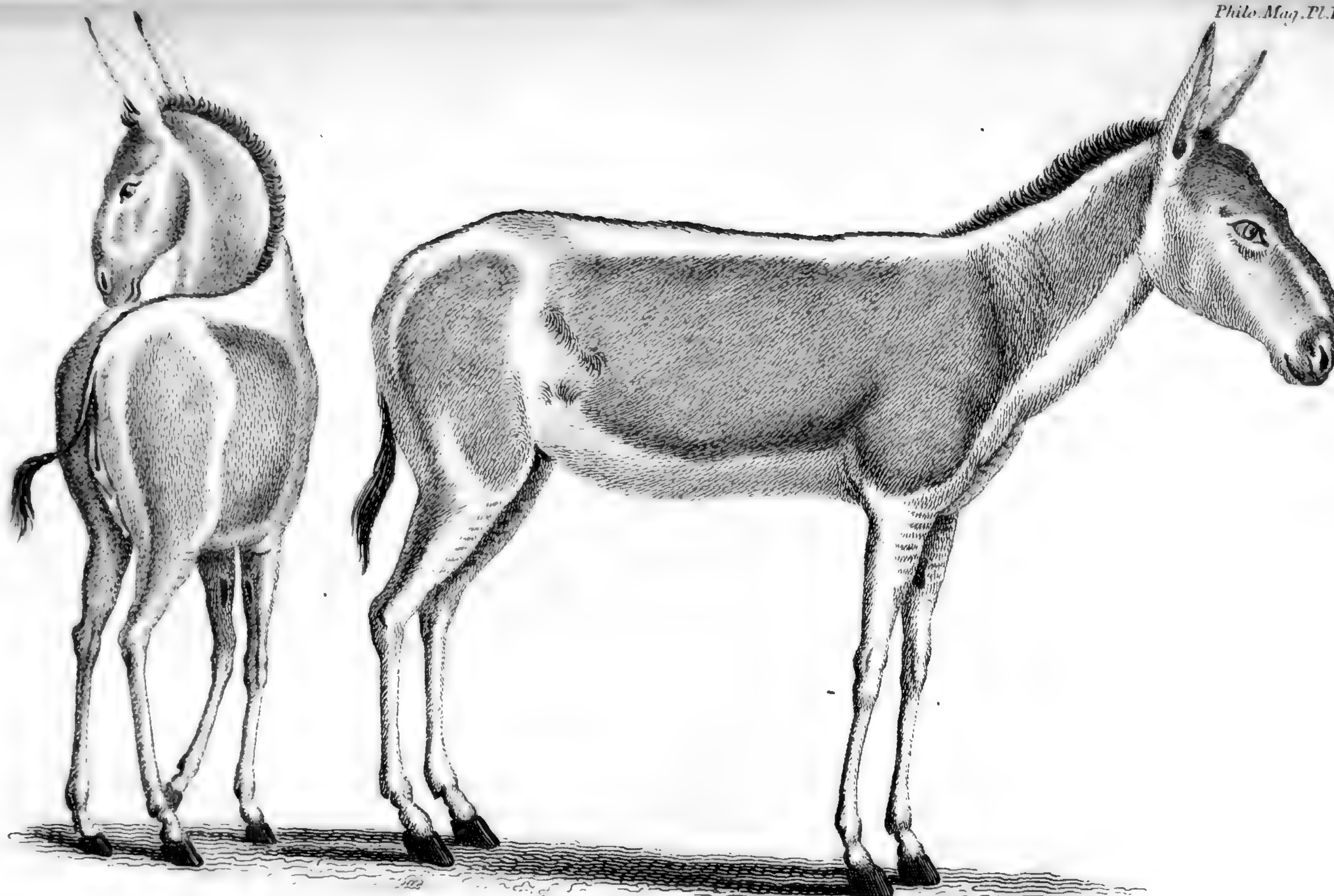

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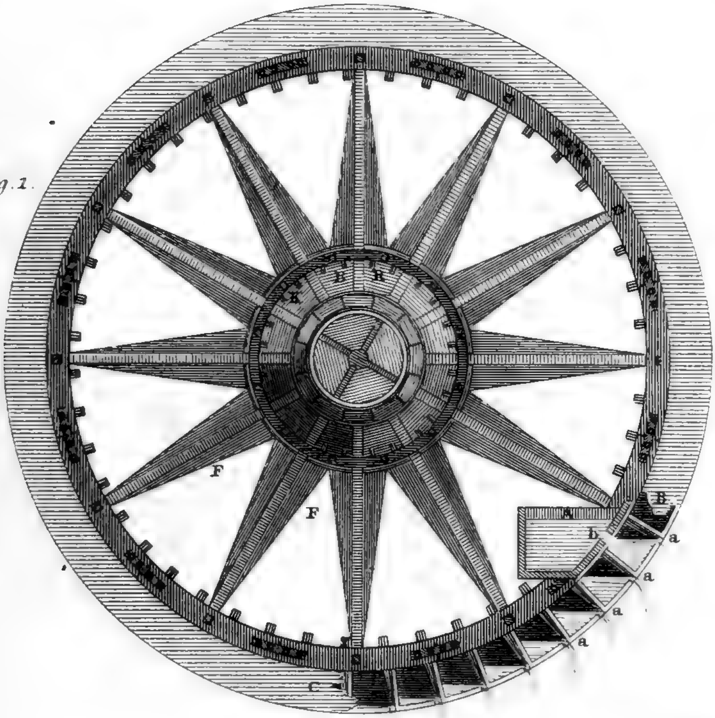

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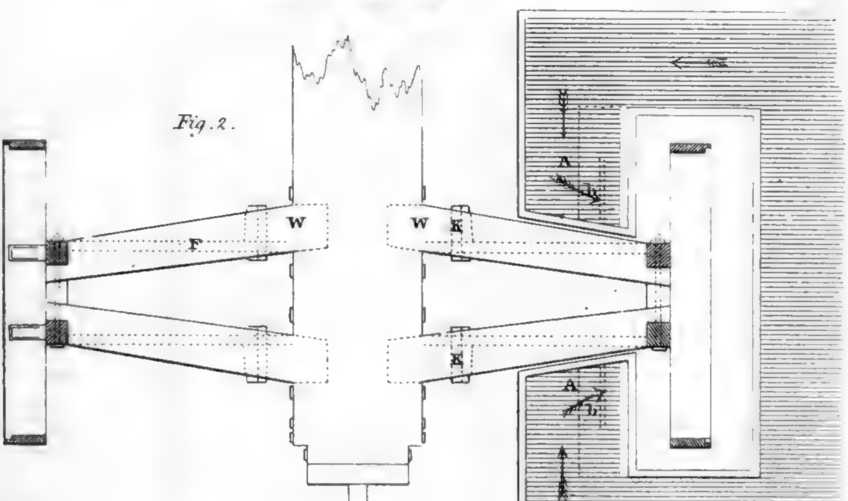

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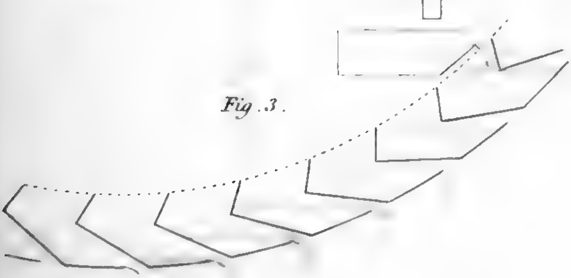

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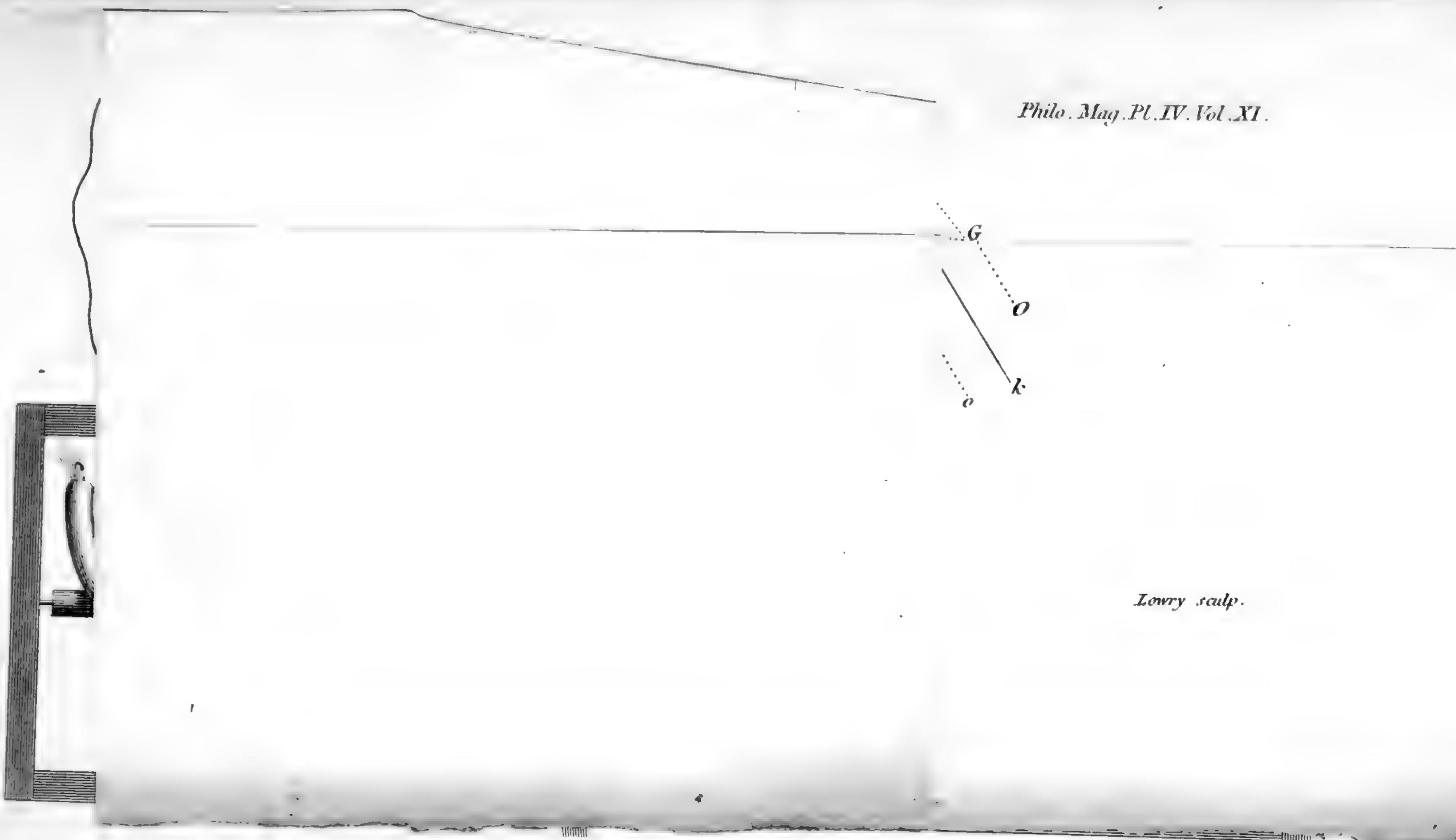
Philo. Mag. Pl. IV. Vol. XI.



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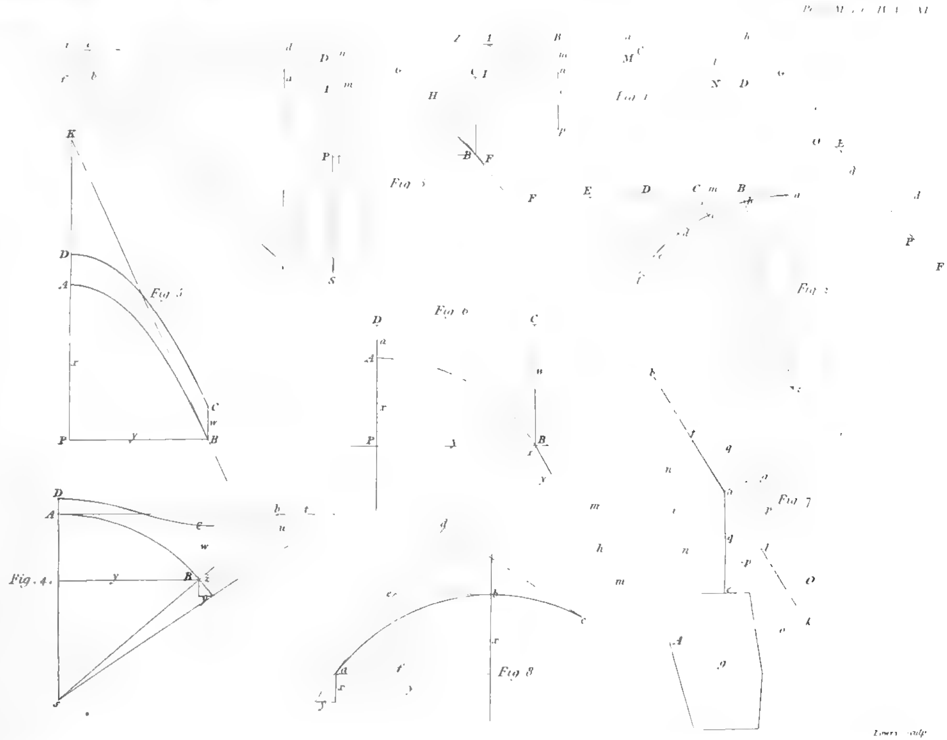
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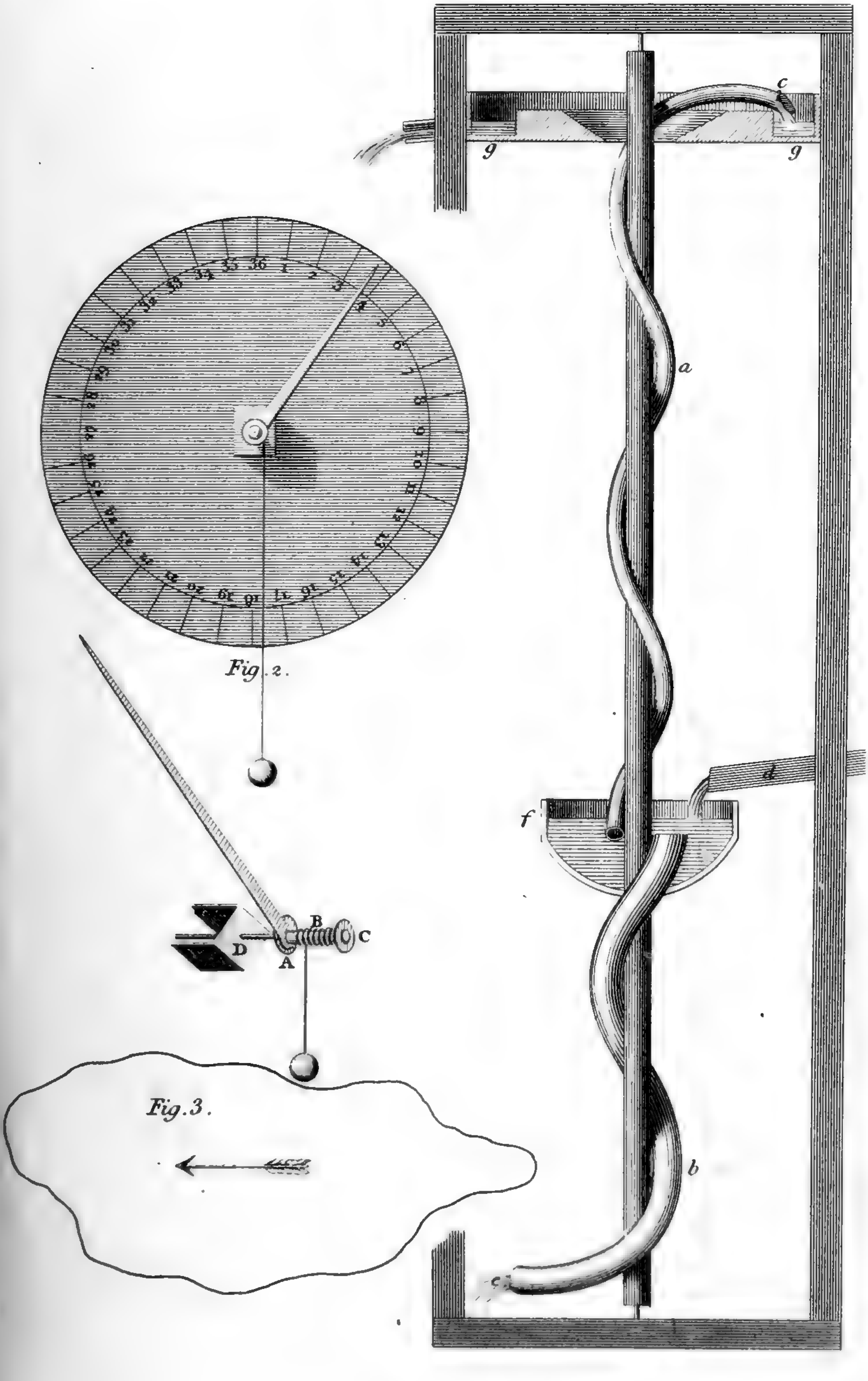

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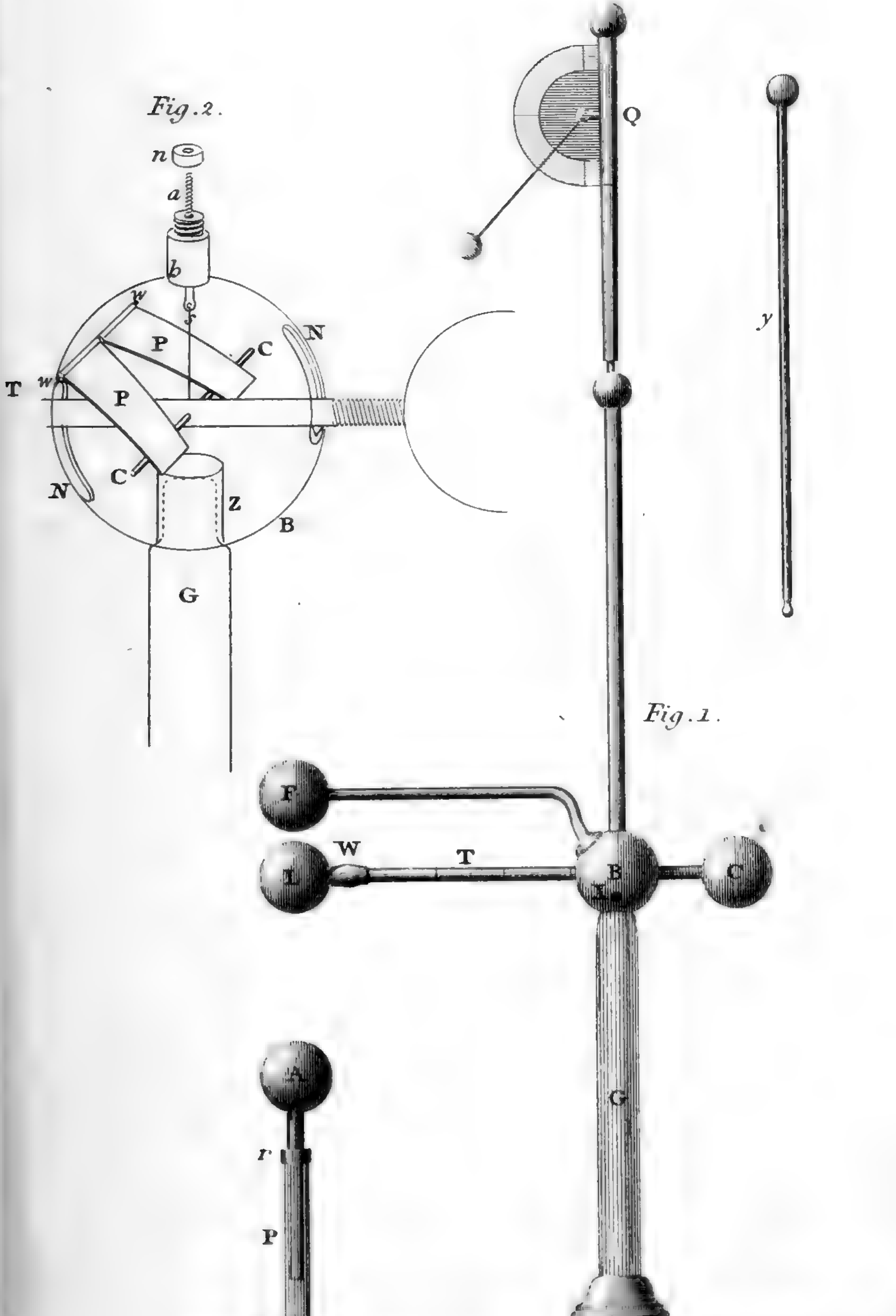

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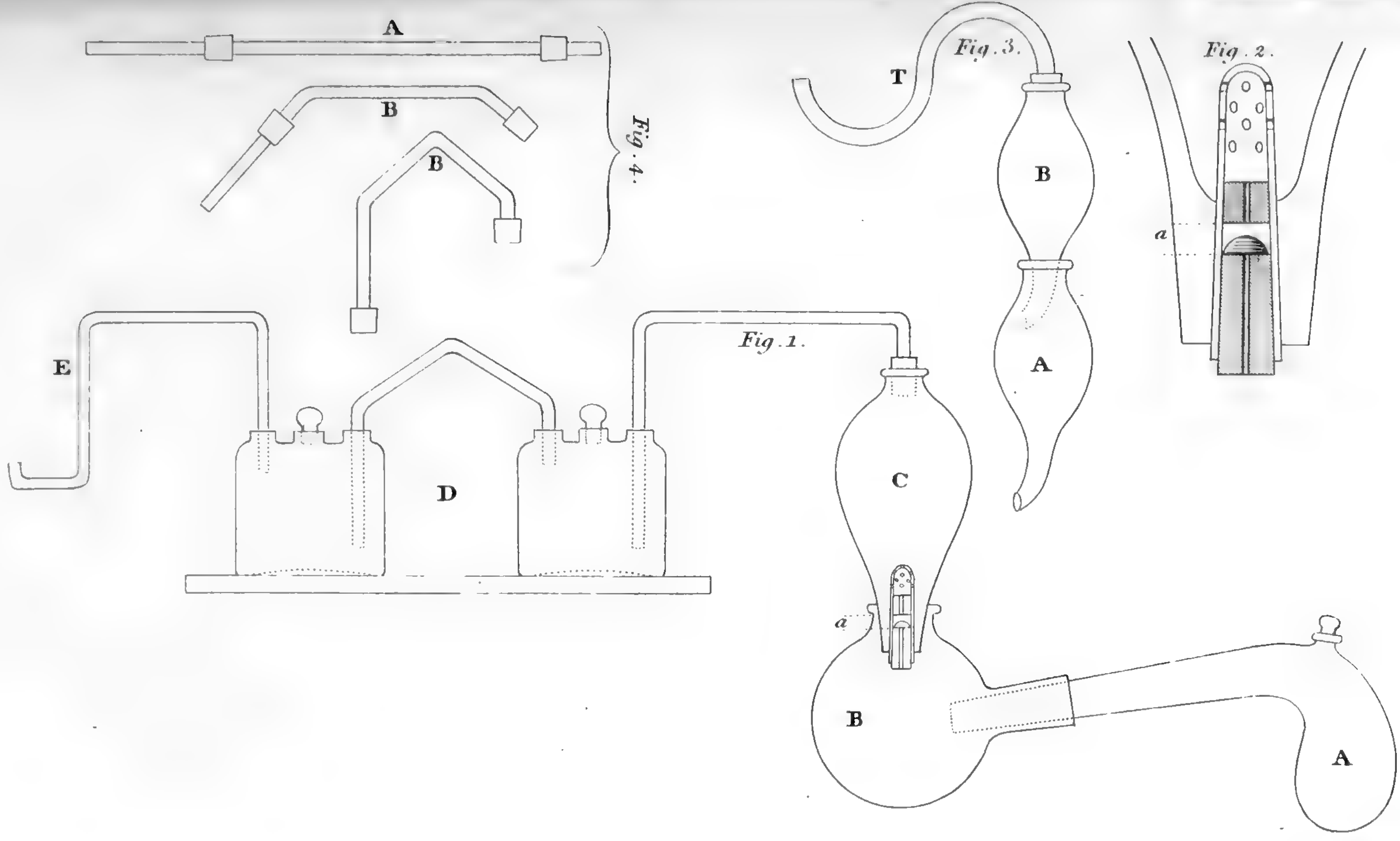

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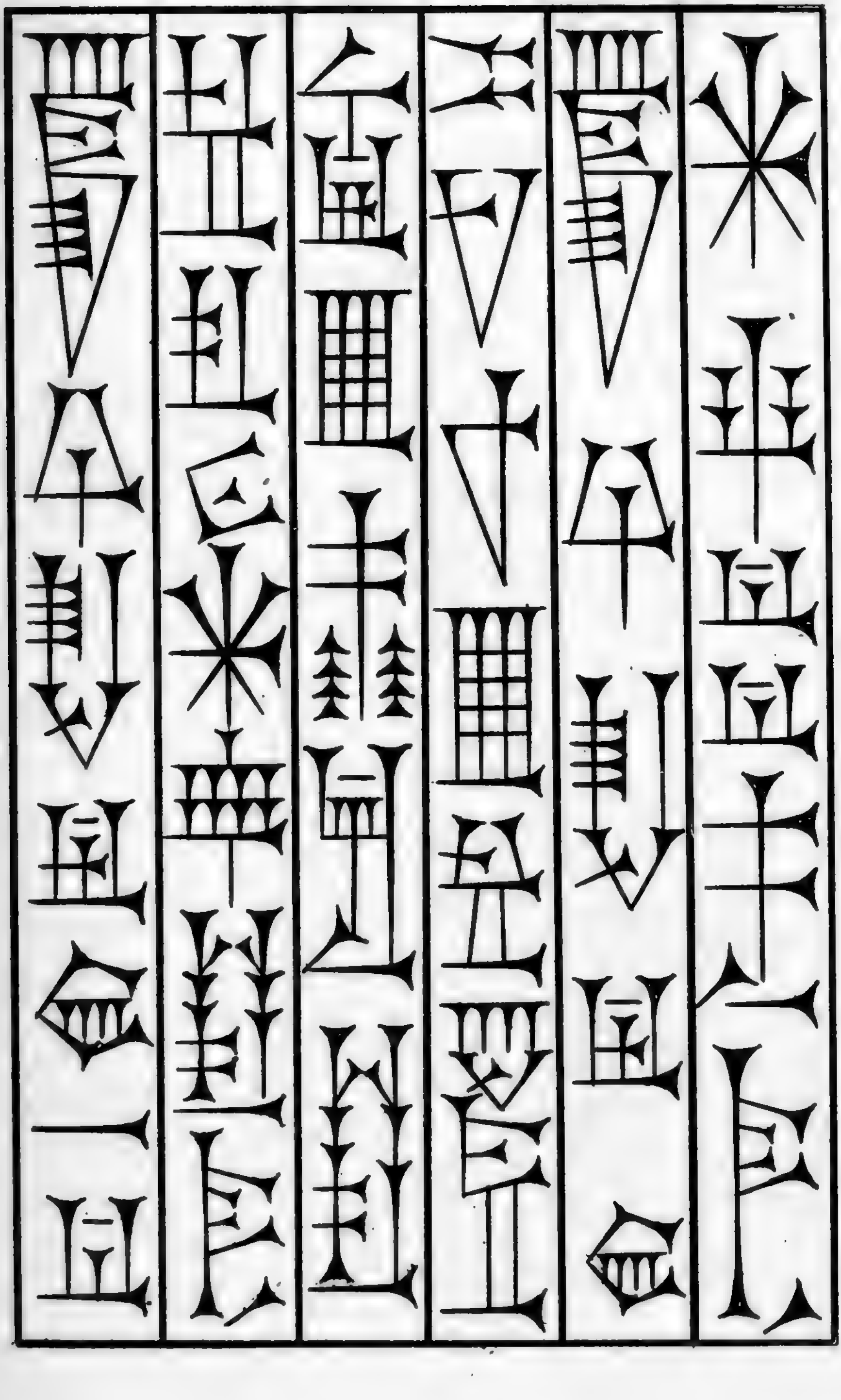

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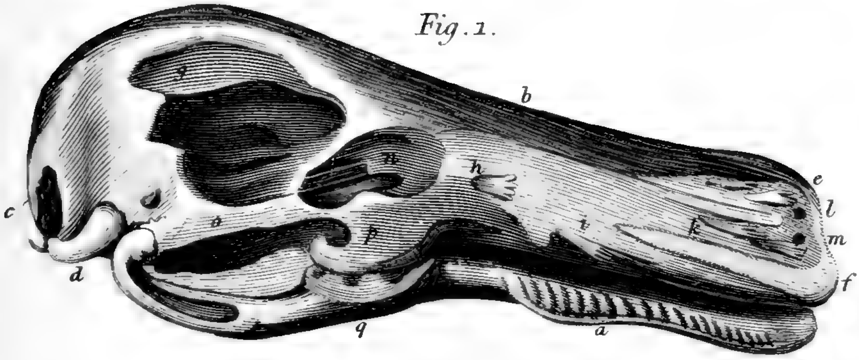

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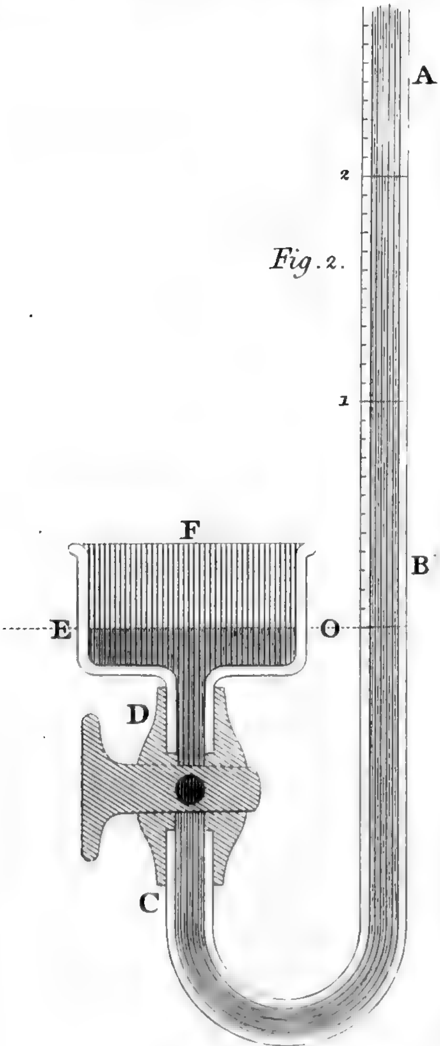

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