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BY WILLIAM NICHOLSON.

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THE Authors of Original Papers in the present Volume, are John Dalton, Esq.; Mr. J. Andrew; Sir A. N. Edelcrantz; Samuel Day, Esq.; The Right Honourable the Marquis of Exeter; W. N.; Mr. Robert Harrup; J. B.; The Rev. Wm. Gregor; Mr. Benjamin Hooke; Mr. John Gough; and Mr. Frederick Accum. Of foreign Works, M. Proust; Joses; Vauquelin; Trouset; Lagrave; Darcet; Van-Marum; Vassali Eandi; Carbonell; Parmentier; Cadett; Boullay; Ritter and Curaudau. And of English Memoirs abridged or extracted, B. H. Latrobe, F. A. P. S.; Mr. Robert Dossie; Thomas Eccleston, Esq.; Mr. John Webb; Mr. Richard Knight; The Right. Hon. Sir J. Banks, Bart. P. R. S.; William Herschell, L. L. D. F. R. S.; Richard Kirwan, L. L. D. F. R. S.; Richard Chenevix, Esq. F. R. S.; John Templeton, A. L. S.; Benjamin Smith Barton, M. D.; Philip Crampton, M. D.; H. Davy, Esq.; Mr. William Henry; M. Komarewski; and Charles Hatchett, Esq.

Of the Engravings the Subjects are, 1. An improved Gun-Lock, by Mr. John Webb. 2. An Apparatus for splitting Logs of Wood, by Mr. R. Knight. 3. Mr. Eccleston's Peat-borer. 4. The Orniscus Prægustator, as it resides in the Mouth of the Bay Alewife. 5. Outline Drawing of the Clupea Tyrannus or Bay Alewife. 6. Figure to illustrate a new Method of computing the Longitude, by Mr. Andrew. 7. Two Plates of Figures illustrative of Dr. Herschell's Memoir on the Structure of the Heavens. 8. Sir. A. N. Edelcrantz's new Statical Lamp. 9. An Ancient Magazine Gun for quick firing. 10. An Ancient Lock of thirteen hundred Combinations. 11. An Apparatus for Distillation, by Sir
A. N.

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Soho Square, August, 1803.

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A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

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THE ARTS.

MAY, 1803.

ARTICLE I.

A Drawing and Description of the Clupea Tyrannus and Otiscus Pragustator. By BENJAMIN HENRY LATROBE, F.A.P.S.*

To Thomas P. Smith, one of the Secretaries of the American Philosophical Society.

SIR,

I BEG leave, through your means, to communicate to the American Philosophical Society, an account of an insect, whose mode of habitation, at least during some part of his life, has appeared to me one of the most singular, not to say whimsical, that can be conceived. Introductory remarks.

In the month of March 1797, illness confined me for several days, at the house of a friend on York river in Virginia, during his absence. My inability to move further than to the shore of the river, gave me leisure to examine carefully, and in more than an hundred instances, the fact I am going to mention.

Among the fish that at this early season of the year resort to the waters of York river, the alewife or oldwife, called the Description of the fish called the bay alewife.

* From the Transactions of the American Phil. Society, V. 77.

DESCRIPTION OF THE CLUPEA TYRANNUS.

bay-alewife, (*clupea nondescripta*) arrives in very considerable shoals, and in some seasons their number is almost incredible. They are fully of the size of a large herring, and are principally distinguished from the herring, by a *bay* or red spot above the gill-fin. (*see the drawing*) They are, when caught from March to May, full-roed and fat, and are at least as good a fish for the table as the herring.

Large insect resident in the mouth of the fish,

In this season, each of these alewives carries in her mouth an insect, about two inches long, hanging with its back downwards, and firmly holding itself by its fourteen legs to the palate. The fishermen call this insect *the louse*. It is with difficulty that it can be separated, and perhaps never without in-

jury to the jaws of the fish. The fishermen therefore consider the insect as essential to the life of the fish; for when it is taken out, and the fish is thrown again into the water, he is incapable of swimming, and soon dies. I endeavoured in numerous instances to preserve both the insect and the fish from injury, but was always obliged either to destroy the one, or to injure the other. I have sometimes succeeded in taking out the insect in a brisk and lively state. As soon as he was set free from my grasp, he immediately scrambled nimbly back into the mouth of the fish, and resumed his position. In every instance he was disgustingly corpulent, and unpleasent to handle; and it seemed, that whether he have obtained his post by force, or by favor, whether he be a mere traveller, or a constant resident, or what else may be his business where he is found; he certainly has a *fat* place of it, and fares sumptuously every day.

The drawings annexed to this account were made from the live insect, and from the fish out of whose mouth he was taken. I had no books to refer to, then; but examining the *Systema Naturæ* of Linnæus, I was surprized to find so exact a description of the insect as follows (*see Salvii editio, Holmiæ 1763. p. 1060. also Trattner's Vienna edition, same page.*)

Defination.

" *Insect. apt.* ONISCUS, PEDES XIV.
Antennæ setaceæ
Corpus ovale.

O. Phyllodes, abdomine subtus nudo, cauda ovata.

Description of Linnæus under the name of *Oniscus Phyllodes.*

Habitat in pelago; corpus præter caput, et caudam ultimam, et septem segmentis trunci, et quinque caudæ. Antennæ utrinque duo, breves. Caudæ folium terminale omnino ovatum; ad latera utrinque

atrinque subtus auctum duobus petiolis diphyllis, foliolis lanceolatis, obtusis, cauda brevioribus. Caudæ articuli subtus obtekti numeroſis veſiculis longitudine caudæ."

From the particularity with which the oniscus phyſodes is deſcribed by Linnæus, it is evident that he had the inſect before him, or a deſcription by an attentive obſerver. It appears alſo from the "*Habitat in pelago*," that the O. Phyſodes, if this be the inſect, is found detached from his conductor. But differing in a few points and ſaid to live in the ocean. There are a few points in which the O. Phyſodes differs from my inſect. I did not obſerve the antennæ, perhaps for want of ſufficient attention, or of a miſcope. The petioli of the tail were not, to appearance, *two-leaved*, and I am certain that the ſegments of the tail, and the tail itſelf, were without the *veſiculi longitudine caudæ*.

There are many circumſtances, to aſcertain which is eſſential to the natural hiſtory of this inſect. The fiſh whoſe mouth he inhabits comes, about the ſame time with the chad, into the rivers of Virginia from the ocean, and continues to travel upwards from the beginning of March, to the middle of May; as long as they are caught upon their paſſage up the river, they are found fat and full of roe. Every fiſh which I ſaw had the oniscus in his mouth; and I was aſſured, not only by the more ignorant fiſhermen, but by a very intelligent man who came down now and then to divert himſelf with fiſhing, that, in 40 years obſervation, he had never ſeen a bay alewife without the louſe. The chad begin to return from the freſh water lean and *ſhotten*, about the end of May and beginning of June, and continue deſcending during the remaining ſummer months. No one attempts then to catch them, for they are unfit for the table. Whether the bay alewife returns with the chad, I could not learn, but it is certain that after June it is not thought worth the trouble to catch them. No one could tell me *poſitively* whether the oniscus ſtill continues with them, but it was the opinion of my informant, that, like every other paraſite, he deſerts his protector in his reduced ſtate, for he could not *recollect* that he had ever ſeen him in the mouth of thoſe accidentally caught in the ſeine in July or Auguſt. Whether the inſect be conſtantly in the mouth of the fiſh, &c.

I conſider, therefore, the natural hiſtory of the oniscus, which I now communicate, as very imperfect; and it were to be wiſhed that ſome lover of natural ſcience would follow up the enquiry, by endeavouring to aſcertain whether he con-

or be the ſame as the oniscus of Linné.

tinue with, or quit the fish before his return to the ocean, and also whether he be the oniscus physodes of Linnæus, *quæ habitat in pelago.*

Proposed name. Should he be an insect hitherto undescribed, I think he might be very aptly named *oniscus prægustator.*

The bay alewife not accurately described. The bay alewife is not accurately described in any ichthyological work which I have seen; nor can I from my drawings, which were made with a very weak hand, venture a description. From his having a regular prægustator, I would suggest that he ought to be named *clupea tyrannus.*

Leaches on the oniscus. The oniscus resembles the minion of a tyrant in other respects, for he is not without those who *suck* him. Many of those which I caught had two or three leaches on their bodies, adhering so closely, that their removal cost them their heads. Most of the marine onisci appear to be troublesome to some one or other fish. The oniscus ceti is well known as the plague of whales, and many of the rest are mentioned in Linnæus and Gmelin, as *pestes piscium.*

BENJ. HENRY LATROBE, F. A. P. S.

Qu. If mentioned in Block's History. P. S. A gentleman well skilled in entomology informs me that he believes, that in Block's History of Fishes, a work not to be had in Philadelphia, this oniscus is mentioned. But, from a late examination of Gmelin and Fabricius, I am convinced that the oniscus prægustator is a species not hitherto accurately described—Gmelin had probably seen the Linnæan insect, having changed the *antennæ utrinque duo*, to *antennis quaternis*, and left out most of the long description given by Linnæus. Neither he, Linnæus, nor Fabricius, mention the circumstance of habitation in the mouth of the fish, and the industrious and copious Fabricius, who having changed the names of the genera, calls him *cymothoa physodes*, copies the description of Gmelin, excepting the mention of the four antennæ, which in his arrangement form a character of the genus.

II.

*The Principles on which the Purification of Fish-oil may be performed, and of the Uses to which it is applicable. By ROBERT DOSSIE *.*

THAT the fetid smell of fish-oil is chiefly owing to putrefaction, it is unnecessary to show; but though this be the principal cause, there is another likewise, which is, Factor of fish-oil is from putrefaction and empyreuma. either or burning the oil, occasioned by the strong heat employed for the extracting it from the blubber of the larger fish, and which produces a strong empyreumatic scent that is not always to be equally removed by the same means as the putrid smell, but remains sometimes very prevalent after that is taken away.

In order to the perfect edulcoration of oils, there are frequently two kinds of factor or stink to be removed, viz. The same remedies do not remove both. the putrid, and the empyreumatic; and the same means do not always equally avail against both.

The putrid smell of fish-oil is of two kinds; the rancid, The oil itself may become rancid; or it may putrefy as to its mixture of other matters. which is peculiar to oils; and the common putrid smell, which is the general effect of the putrefaction of animal fluids, or of the vascular solids, when commixed with aqueous fluids.

Fish-oil has not only rancidity, or the first kind of putrid smells peculiar to oils, but also the second or general kinds; Fish-oil has both these putrid smells. It contains gelatine and oil. as the oil, for the most part, is commixed with the gelatinous humour common to all animals, and some kinds with a proportion of the bile likewise; and those humours putrefying combine their putrid scent with the rancidity of the oil, and, in cases where great heat has been used, with that and the empyreuma also.

The reason of the presence of the gelatinous fluid in fish-oil is this: that the blubber, which consists partly of adipose vesicles, and partly of the membrana cellulosa, which contains the gelatinous fluid, is, for the most part, kept a considerable time before the oil is separated from it, either from the want of convenient opportunities to extract the oil, or in order to the obtaining a larger proportion; as the putrid The gelatine is introduced from ruptured vessels &c.

* Communicated to the Society for the encouragement of Arts long ago, but now published by them in the original form.

effervescence

effervescence which then comes on, rupturing the vesicles, makes the blubber yield a greater quantity of oil than could be extracted before such change was produced; and the vesicles of the *tela cellulosa*, containing the gelatinous matter, being also burst from the same cause, such matter being then rendered saponaceous by the putrefaction, a part of it mixes intimately with the oil, and constitutes it a compound of the proper oleaginous parts and this heterogeneous fluid.

How the bile comes to be admixed.

The presence of the bile in fish-oil is occasioned by its being, in many cases, extracted from the liver of the fish; which is not to be so profitably done by other means as by putrefaction; and the bile being consequently discharged, together with the oil from the vessels of the liver containing them, combines with it, both from the original saponaceous property of bile, and from that which it acquires by putrefaction.

Train oil is particularly impure.

This holds good particularly of the cod-oil, or common train, brought from Newfoundland; which, from its high yellow colour, viscid consistence, and repugnance to burning well in lamps, manifests sensibly the presence of bile and the gelatinous fluid; which latter, by the saponaceous power of the bile, is commixed in a greater proportion in this than in any other kind of fish-oil.

Perfect oils are little disposed to putrefy

A tendency to putrefy, or at most but in an extremely slow manner, is not an absolute property of perfect oils in a simple or pure state; but it is a relative property dependant upon their accidental contact or commixture with the aqueous fluid. This is evident from the case of oils concreted into a sebaceous form; which being perfectly oleaginous and uncombined with any water, except such as enters into their

unless water be present,

component parts will not putrefy unless water, or something containing it, is brought in contact with them. But the fluid animal, and most vegetable oils, being compounded of perfect oils with other mixed substances, either sub-oleaginous or gelatinous, have always a putrescence *per se*, or tendency to putrefy, without further admixture of aqueous moisture. This commixture of heterogeneous matter in fish-oil, particularly of the gelatinous fluid and bile, gives rise to

But most oils will putrefy alone;

even after edulcoration.

a further principle of purification than *simple edulcoration*, or the removing the fætor; for the presence of such humours in the oil renders it subject to a second putrescence *per se*, supposing the

the first corrected; makes it unfit for the purpose of the woollen manufacture, as the heat through which this is in some cases employed, causes this matter to contract a most disagreeable empyreuma. It also prevents its burning in lamps, as well from its viscosity as from the repugnance which the presence of water gives to all oleaginous matter. It is therefore necessary to free the oil from this heterogeneous matter; after which it can be subject only to the rancid putrescence, or that which is proper to oils as such.

The substances which have been or may be applied to the removing or preventing the effects of putrescence, are, acids, alkalis, metallic calces, neutral salts, ethereal and essential oils, vinous spirits, water, and air. With respect to acids, though they may be applied with effect to the removal of prevention of putrefaction in mixed animal and vegetable substances, yet they have not the same efficacy when employed in the case of oils; for in a small proportion, without the subsequent aid of alkalis, they rather increase than diminish the fætor, and in a large proportion they coagulate the oils, and change their other properties as well as their consistence. Though they might therefore be employed with the assistance of alkalis, yet requiring a more expensive and complex process, and not being moreover necessary, as the same end may be obtained by the use of alkalis only, they may be deemed improper for the purification of animal oils for commercial purposes. Alkaline substances, both salts and earths, are the most powerful instruments in the edulcoration of oils; but as their action on putrid oils, and the method of applying them to this end, are not the same in both, it is proper to consider them distinctly.

Of alkaline salts it is the fixed kind only which are proper to be used for the edulcoration of oils. Fixed alkaline salts, in a dissolved state, being commixed with putrefying animal substances, appear to combine with putrid matter, and mixing with some of the principles, form instantly volatile alkaline salts. On the less putrid they seem to act, after their combination, by an acceleration of the putrescent action, till they attain the degree which produces volatile salts. This is evident by the sensible putrid ferment and smell which appear after their commixture; but which gradually abating, the oil is rendered sweeter, much lighter-coloured and thinner.

Their

Enumeration of bodies used for purifying oils: Acids, alkalis, oxides, neutral salts, vol. oil, alcohol, water, air. Acids not eligible,

but alkalis are best.

Fixed alkaline carbonates increase the putrid process;

and are very
useful in purify-
ing the oil;

but they do not
remove empy-
reuma.

The alkaline
carbonates
render part of
the oil solid;

but this is cor-
rected by com-
mon salt.

Alkaline earths.

Lime is good but
it coagulates.

as do also soap
lecs.

Their great use in the edulcoration of fish-oil arises, there-
fore, from their converting such parts of the gelatinous fluid
and bile as are highly putrefied, instantly into volatile salts,
and causing a rapid putrefaction of the other parts; by which
means the oil is freed from them by their dissipation. They
do not, however, equally act on the parts of the oil on which
the empyreumatic scent depends, unless by the assistance of
heat; for when they are commixed with the oils without heat,
in proportion as the putrid smell diminishes, that becomes
more sensibly prevalent. The ultimate action of lixiviate
salts on animal oils, except with respect to the empyreuma,
seems to be the same either with or without the medium of
heat; for the same urinous and putrid smell, gradual diminu-
tion of the colour, and fetid scent, happens in one case as in
the other, except with regard to the acceleration of the
changes; and such salts, where the purification is required
to be made in a great degree, are a necessary means, as they
are more effectual than any other substance that can be em-
ployed.

The use of lixiviate salts alone is not, however, the most
expedient method that can be pursued for the edulcoration
of oils, for several reasons. If they be used alone, cold, in
the requisite proportions, they coagulate a considerable part
of the oil, which will not again separate from them under a
very great length of time; and when they have destroyed
the putrid scent, a strong bitter empyreumatic smell remains.
The same inconvenience, with relation to the coagulation of
part of the oil, results when they are used alone with heat.
The super-addition of common salt, (which resolves the
coagulum and counteracts the saponaceous power of the
lixiviate salt, by which the oil and water are made to combine)
is therefore necessary; and the expence arising from the larger
proportion of lixiviate salt, requires it to be employed if no
other alkali be taken in aid, and renders the junction of alkaline
earths with it extremely proper in the edulcoration of oils
for commercial uses. Lime has also an edulcorative power
on animal oils; but it has also so strong a coagulative action,
that the addition of a large proportion of alkaline salts be-
comes, when it is used, necessary to reduce the concreted
oil to a fluid state; and therefore this substance alone is not
proper for that purpose. The combination of lixiviate salt
with

with lime, or the solution commonly called soap-lye, has an effectual edulcorative action on fetid oils; but it makes a troublesome coagulation of part of the oil, if no common salt be employed, and must be used in such large proportion, if no alkaline earth be added, as renders the method too expensive.

Lime has a power of combining with and absorbing the putrid parts of the gelatinous fluid and bile, when commixed with oil; and effects, either with or without heat, a considerable edulcoration of fetid oils; but it combines so strongly with them, either cold or hot, that the separation is difficult to be effected, even with the addition of brine; and the oil, when a large proportion of it is used, can scarcely be at all brought from its concretion to a fluid state, but by an equivalent large proportion of lixiviate salt: the use of lime therefore, alone, is improper, or even in a great proportion with other ingredients. But when only a lesser degree of edulcoration is required, a moderate quantity, conjoined with an equal or greater weight of chalk, which assists its separation from the oil, may, on account of its great cheapness, be employed very advantageously: it will in this case admit of precipitation from the oil by the addition of brine. It may be also expediently used; when lixiviate salt is employed with heat for the most perfect purification of oils; for it will in that case give room for the diminishing of the quantity of lixiviate salt, though the proportion be nevertheless so restrained as not to exceed what the proportion of lixiviate salt (just requisite for the edulcoration) can separate from the oil.

Chalk has an absorbing power similar to lime, but in a less degree on the putrid substance of oil; it does not, however, combine so strongly with the oil as to resist separation in the same manner, and is therefore very proper to be conjoined either with lixiviate salts or lime, as it renders a less quantity of either sufficient, and indeed contributes to the separation of the oil from them.

Magnesia alba or the alkaline earth which is the basis of the sal catharticus, and the singular earth which is the basis of alum, both have an edulcorating power on fetid oils; but, like lime, have too strong an attraction with them to be separated so as to admit of the reduction of the oil from the concretion to which they reduce it; and therefore, as they are not superior in efficacy to lime and chalk, but much dearer

or more difficult to be obtained, they may be rejected from the number of ingredients that are proper for the purifying of oils, with a view to commercial advantages,

Sea-salt useful
in mixtures but
not alone.

Sea-salt has an antiseptic power on the mixed solid parts of animals; but used alone, or dissolved in water, it does not appear to lessen the putrid fœtor of oils, but on the contrary rather increases it. If, after their commixture with it, they are subjected to heat, it rather depraves than improves the oils; but though by its own immediate action on them it conduces so little to the edulcoration of oils, yet it is a medium for the separation of water and the alkaline substances requisite to be employed to that end. It is of great utility in the edulcorative processes: for when alkaline salts or earths combine with the water necessary to their action on the oils, or themselves form coagulums or corrections with it, a solution of salt will loosen the bond and dissolve the close union; so that the oil being separated will float on the aqueous fluid, while the earth, if any be in the mixture, will be precipitated and sink close together to the bottom of the containing vessel.

Muriate of
potash, sulphate
of soda or of
potash, &c. of
little value.

Sal catharticus, glauber salt, nitrum vitriolatum, tartar, and other neutral salts, though they counteract putrefaction in the mixed or solid parts of animals, seem to have little effect on oils with respect to their edulcoration, and cannot therefore be ranked amongst the substances proper to be used for that purpose.

Oxide of lead is
very powerful,

Lead reduced to the state of a calx, either in the form of minium or litharge, has a strong edulcorative power on fetid oils, and is indeed applied to that end, with respect to one kind of vegetable oil, for a very bad purpose, considering its malignant qualities on the human body.

In the case of train-oil, which will scarcely ever be considered among the esculent kinds in this country, the same objection against its use would not lie; and employed either with or without heat, it is a powerful absorbent both of the putrid and empyreumatic parts that occasion the fœtor.

but was rejected
from prejudice.

As however there may be some prejudice against its use, even in any way, and as it is not absolutely necessary, I have not given it a place among the ingredients of the processes I recommend.

The

The ochrous earth of iron, commonly called red ochre, has an absorbing power on the putrid parts of oil, but combines so strongly, that the separation is tedious even with the addition of brine; if, nevertheless, it is added when chalk and lime have been some time commixed with the oil, as in process the first, it will promote the edulcorative intention, and will subside along with them; and, as it has some advantage without increasing the expence, unless in the most inconsiderable degree, its use may be expediently admitted in that process.

Red ochre may be used in mixture.

Essential and ethereal oils are applicable to the prevention of putrefaction in the mixed and solid parts of vegetables, but are not so to the edulcoration of fetid oils; and if they had the desired effect, they would not, on account of their price, answer the commercial end, unless the due effect was produced by adding them to the oils in a very small quantity.

Volatile oils inefficient and costly.

The same holds good of spirits of wine as of essential and ethereal oils, both with respect to their efficacy and the expence.

So likewise are alcohol and ether.

Water has an edulcorative action on fetid oils, by carrying off the most putrid parts of the gelatinous fluid or bile, in which, as was above explained, the principal factor resides, if the quantity added be large, and an intimate commixture be made of them by stirring them together for a considerable time: this only partially removing those heterogeneous putrescent substances, the remaining part soon acquires the same state and the oil again grows fetid, though not to the same degree as before.

Water by agitation improves oils, but they soon putrefy again.

Water is, however, a necessary medium for the action of salts and the separation of alkaline earths and calces of metals, when they are employed for the edulcoration of oils, as will appear from a consideration of my processes.

It is necessary in the compound processes.

Air edulcorates oil by carrying off the most putrid parts, which are necessarily extremely volatile. It may be made to act on them either by simple exposure of them to it with a large extent of surface, or by forcing it through them by means of ventilators as has been practised by some dealers; but is now, I believe, neglected on account of their finding the improvement of oils by it not adequate to the trouble, as the gelatinous matter and bile, not reduced to a certain degree of putrefaction, being left behind, putrefy again to nearly the same degree as before.

Air is still less effectual.

Resumption.
Lime, chalk,
sea-salt, and
alkali, are the
materials for
purifying oils.

It appears from these several observations, that the cheapest ingredients which can be used for theedulcoration of train-oils, are lime and chalk, which may, with the addition of a proper quantity of solution of sea-salt or brine, be made to procure a separation of them from the oils, according to process the first, so as to answer for some purposes; that the lixivate salt is the most powerful purifier of oils, and, with the assistance of chalk and brine, will, without heat, according to process the second, effect a very considerable degree ofedulcoration; and that lixivate salt used with heat, with the addition of lime and chalk, to save a part of the quantity which would otherwise be necessary, and of brine to procure a quick separation, will perform anedulcoration sufficient for all commercial purposes, according to process the third; but that calcined lead and the ochrous earth of iron may, perhaps, be applied in some cases with advantage, where the oil is not designed for esculent use.

PROCESS THE FIRST.

For purifying Fish-oil in a moderate Degree, and at a very little Expence.

Process 1.
Moderate puri-
fication.
Chalk and
slacked lime
agitated with
oil; salt water
added; repose;
decauntation.

TAKE an ounce of chalk in powder, and half an ounce of lime slacked by exposure to the air; put them into a gallon of stinking oil, and, having mixed them well together by stirring, add half a pint of water, and mix that also with them by the same means. When they have stood an hour or two, repeat the stirring, and continue the same treatment at convenient intervals for two or three days; after which, superadd a pint and a half of water, in which an ounce of salt is dissolved, and mix them as the other ingredients, repeating the stirring, as before, for a day or two. Let the whole then stand at rest, and the water will sink below the oil, and the chalk subside in it to the bottom of the vessel. The oil will become clear, be of a lighter colour, and have considerably less smell; but will not be purified in a manner equal to what is effected by the other processes below given; though, as this is done with the expence of only one ounce of salt, it may be practised advantageously for many purposes, especially as a preparation for the next method, the operation of which will be thereby facilitated.

PROCESS

PROCESS THE SECOND.

To purify, to a great Degree, Fish-oil without Heat.

TAKE a gallon of crude stinking oil, or rather such as has been prepared as above mentioned, and add to it an ounce of powdered chalk; stir them well together several times, as in the preceding process; and after they have been mixed some hours or a whole day, add an ounce of pearl-ashes, dissolved in four ounces of water, and repeat the stirring as before. After they have been so treated for some hours, put in a pint of water, in which two ounces of salt are dissolved, and proceed as before: the oil and brine will separate on standing some days, and the oil will be greatly improved both in smell and colour. Where a greater purity is required, the quantity of pearl-ashes must be increased, and the time before the addition of the salt and water prolonged.

If the same operation is repeated several times, diminishing each time the quantity of ingredients one half, the oil may be brought to a very light colour, and rendered equally sweet in smell with the common spermaceti oil.

By this process the cod-oil may be made to burn; and, when it is so putrid as not to be fit for any use, either alone or mixed, it may be so corrected by the first part of the process, as to be equal to that commonly sold: but where this process is practised in the case of such putrid oil, use half an ounce of chalk and half an ounce of lime.

PROCESS THE THIRD.

To purify Fish-oil with the Assistance of Heat, where the greatest Purity is required, and particularly for the Woollen Manufacture.

TAKE a gallon of crude stinking oil, and mix it with a quarter of an ounce of powdered chalk, a quarter of an ounce of lime, slacked in the air, and half a pint of water; stir them together; and when they have stood some hours, add a pint of water and two ounces of pearl-ashes, and place them over a fire that will just keep them simmering, till the oil appears of a light amber colour, and has lost all smell except a hot, greasy, soap-like scent. Then superadd half a pint of water

in

in which an ounce of salt has been dissolved; and having boiled them half an hour, pour them into a proper vessel, and let them stand till the separation of the oil, water, and lime be made, as in the preceding process. Where this operation is performed to prepare oil for the woollen manufacture, the salt may be omitted; but the separation of the lime from the oil will be slower, and a longer boiling will be necessary.

If the oil be required yet more pure, treat it, after it is separated from the water, &c. according to the second process, with an ounce of chalk, a quarter of an ounce of pearl-ashes, and half an ounce of salt.

Observations on Process the First.

Observations.

This process may be performed on any kind of fish or sea-oil that is putrid and stinking, and will improve it in smell, and generally render the colour lighter, if previously dark and brown: it will also conduce to render these oils fitter for burning, which are, in their crude state, faulty in that point; but it will not meliorate them to the full degree they admit of even without heat, and should therefore be practised when only a moderate improvement is required.

The dregs may be saved.

Secondly, When the oil is taken off from the dregs and brine, the dregs which swim on the brine should be taken off it also, and put into another vessel of a deep form; and on standing, particularly if fresh water be added and stirred with them, nearly the whole remaining part of the oil will separate from the foulness; or, to save this trouble, the dregs, when taken off, may be put to any future quantity of oil that is to be edulcorated by this method, which will answer the same purpose.

Observations on Process the Third.

Process 3 is best for train-oil.

First.—This is most advantageously performed on train-oil, called vicious whale-oil; and the more putrid and foul it may be, the greater will be the proportional improvement, especially if there be no mixture of the other kinds of fish-oils, particularly the seal, which do not admit of being edulcorated perfectly by means of heat, but require other methods: but when the vicious oil is pure from admixture of others, however stinking it may be, the bad smell will be removed by this process duly executed, and the brown colour changed to a very

very light amber; and these qualities will be much more permanent in this than in any crude oil, as it will not, from the degree of purity to which it is brought, be subject to putrefy again under a great length of time, whether it be kept open or in close vessels.

The oil in this state will burn away without leaving the least remains of foulness in the lamp; and, being rendered more fluid than before, will go further, when used in the woollen manufacture, than any other kind, and will be much more easily scoured from the wool.

If nevertheless there be any branches of the woollen manufacture which require the use of a more thick and unctuous oil, this may be rendered so by the addition of a proper quantity of tallow or fat, of which a certain proportion will perfectly incorporate with the oil, the fluidity and transparency being still preserved, as well as all the other qualities that render it suitable to the intended purpose. This may be most beneficially done by adding a proper quantity of the refuse grease of families, commonly called *kitchen-stuff*, which being put to the oil, when moderately heated, will immediately dissolve in it, and let fall also its impurities or foulness to the bottom of the vessel, and render the purified admixture a considerable saving to the manufacturers.

Secondly. The different qualities and dispositions of different parcels of vicious oil with respect to edulcoration, render various proportions necessary of the ingredients to be used. The quantities stated in the above process are the least which will effect the end in general, and frequently greater will be required; but this may always be first tried; and if it be found, after six or eight hours simmering of the mixture, that no gradual improvement is making in the smell and colour, but that the oil continues the same in those particulars, and remains also mixed with the chalk and lime, and in a thick turbid state, a fourth or third part of the first quantity of pearl-ashes should be added, and the simmering continued till the oil be perfect. As the quantity of the water is lessened by the evaporation, it is necessary to make fresh additions from time to time, that there may be always nearly the original proportion.

Thirdly. If it be inconvenient to give the whole time of boiling at once, the fire may be suffered to go out and be re-kindled at any distance of time; and if, in such case, a small proportion

It will then burn clear, &c.

Kitchen-stuff may be added when a thick oil is wanted.

The quantities of ingredients may be varied,

and the boiling performed at different times.

proportion of pearl-ashes dissolved in water be added, and the mixture several times stirred betwixt the times of boiling, it will facilitate the operation. The time of boiling may be also much shortened, if the chalk, lime, and pearl-ashes, be added for some days before, and the mixture frequently stirred.

PROCESS THE FOURTH.

Which may be practised alone, instead of Process the First, as it will edulcorate and purify Fish-oil to a considerable Degree, so as to answer most Purposes, and for Process the Third, when the whole is performed.

Process 4.
Purification by
lime water.

TAKE a gallon of crude stinking oil, and put to it a pint of water poured off from two ounces of lime slacked in the air; let them stand together, and stir them up several times for the first twenty-four hours; then let them stand a day, and the lime-water will sink below the oil, which must be carefully separated from them. Take this oil, if not sufficiently purified for your purpose, and treat it as directed in Process the Third, diminishing the quantity of pearl-ashes to one ounce, and omitting the lime and chalk.

ROBERT DOSSIE.*

III

An Essay on the Fecula of Green Plants. By Professor PROUST.

(Concluded from Page 278. Vol. IV.)

Fecula not
precipitable
by water.

I BELIEVE Parmentier was the first who doubted, and it was not unreasonable in his time, whether the tincture of fecula in alcohol was resinous, because it was not precipitated by water. But when we consider that water can never detach it from the gluten, that alcohol, oils and grease have exclusively that property, and lastly, that this substance, when separated from the alcohol and left to itself, is a fat body, te-

* The dregs, remaining after the sundry processes above mentioned, will form an excellent manure; as has been since noticed in Dr. Hunter's Georgical Essays.

nacious

nacious and insoluble in water; it must be acknowledged that there is no other product in vegetables to which it approaches so nearly. But we shall find, that, in order to give it this character more perfectly, it is only necessary to add to it a little oxygen.

The oxygenated muriatic acid, in a few days, bleaches and hardens the green resin; it then becomes ropy, like boiled turpentine and its colour is very diffusible in water; but if the green part of the fecula belonged to the coloured juices, contained in dying ingredients, oxygen would not convert it into a resin. At present, when experience has taught us not to fix such strict limits to vegetable products as heretofore, because we so often find them united by intermediate qualities, we shall not be surpris'd to see that a resin, at its maximum of divisibility, can mix with water. Do we not find that camphor, essential oils, both animal and vegetable, sarcocolle, &c. are completely dissolved by water? Yet we do not from that circumstance exclude such products from the classification which analysis assigns to them.

But rendered more completely resinous by oxygenation.

The green fecula acquire, by the action of oxygenated muriatic acid, that tawny brown colour which indicates the decay of the leaves in winter, and their aqueous diffusion becomes abundantly turbid. On the whole, then, we may infer, that though the colouring matter of the fecula cannot resist the action of water, when extracted by alcohol, it is nevertheless, in all its other qualities, a substance truly resinous; and though this product, which is one of the most curious of the vegetable kingdom, because it adorns it by its various combinations, has not been admitted in the *system of chemical knowledge*, yet the chemists who have been the most occupied with it, such as Rouelle, Danel, Sage, Parmentier, &c. have not thought it less worthy of their investigations.

When it turns brown.

This resin, when dissolved in potash, quits it to unite with silk, which it tinges of a clear green, but too fugitive to become useful; notwithstanding which it resists the action of verjuice; but its attraction for gluten in preference to the vegetable fibre, is conformable to established principles; for, in general, colouring bodies attach themselves to animalised substances, rather than to the fibres of flax, hemp, or cotton. There is then something in the fecula analogous to wool, to silk, &c. It is the gluten.

This resin quits potash, to unite in preference with animal matter like a dye.

V. Let us now examine the fecula by tests more adapted to disclose the new characters of animalisation.

The fecula putrefies like an animal body.

If, in summer, the fecula, either boiled or raw, be kept under water, in less than twenty-four hours it yields a disagreeable smell, and soon exhales an excrementous fetor which continually increases, and, to which it might be dangerous to be long exposed. The infectious miasma arising from it, instantly discolours metallic writings; and its liquor, which may be compared to a cadaverous sanies, also rapidly blackens plates of silver.

Steeping of flax. It is, without doubt, from the corruption of this principle, rather than from any other cause, that the pernicious exhalations from hemp and flax, while steeping, arise. Running waters, which are equally proper to separate the flax as standing ones, quickly carry off their extractive juices; there can only remain the fecula intangled in the green fibres which is susceptible of being destroyed by steeping.

The liquor, which at the end of a year, remains above the rotten fecula, contains sulphurated hydrogen, carbonate of ammonia, and gluten dissolved by the intervention of the latter principle.

The liquid very obstinately stercoral.

This liquor has also this peculiarity, that it preserves its stercoral smell after long boiling. The product of its distillation contains carbonate of ammonia, combined with a principle of infection, which does not blacken metallic solutions, and with the nature of which I am not at all acquainted.—Acids, by precipitating the fecula and saturating the ammonia, do not weaken it; this has, for a long time, induced me to think, that though the effluvia from a mass of animal putrefaction may serve as a vehicle to the phosphorus and sulphur contained in it, yet these combustibles are not alone, the cause of the infection. For there is a great difference, for example, between the smell of rotten fish or flesh, of corrupted fecula, of stinking cheese, and those of phosphorated and sulphurated hydrogen.

The infection peculiar in putrefying bodies.

On Putrefaction.

What, then, is putrefaction? A change of which we have very few clear ideas.

Putrefaction is not destruction,

When fecula, curd, flesh, or any organic matters in general, have passed through a certain period of this change, which we are

are accustomed to call putrefaction, all at once they stop in a state of permanence, in which, unknown combinations seem to fix, and as it were to salt them, to embalm them, so as to insure their duration in this new state and to secure them from all subsequent destruction.

When, for example, the curd, the fecula, the gluten, or the flesh, after having passed through all the changes of an infection, which is often deadly, and those derangements of colour and of form which disfigure them, have at last arrived, the one to a cheesy state; the vegetables, the dung-hills to mould, turf, or poudrette;—flesh itself has not been annihilated after an ichorous stagnation of fifteen years: all stop at this point, without being able to pass the limit, without ever attaining, at least under our eyes, that final dissolution which must terminate their existence, or reduce them to a mere earthy matter, inert *non ab similem cineribus*, to use the expression of Stahl, in short, to a state in which no trace of the radical of their organization can be discovered.

Putrefaction of this kind in strictness takes place nowhere. No sooner do we perceive the derangement of the organization of an animal or vegetable matter, lividity, infection, but we conclude that this process has commenced; and without consideration, we confound these appearances, which belong to a species of fermentation, little known, with the effects of that which alone is the true putrefaction, if its end agrees with the notions we form of it; if it be truly an operation determined by nature to analyse and resolve into their last principles, those substances which are submitted to it.

Hence we may conclude, that absolute putrefaction is a thing we are totally unacquainted with. But let us return to the fecula; it is time to attend to that which is in such a state of division as to pass the filtre.

VII. We will take as an example, the filtered juice of cabbage, one of those plants which yields it most abundantly. And, at the same time, the better to demonstrate the difference between this second fecula and albumen, we will submit the latter to the same proofs. The white of an egg beat up with a pound of water, and filtered, will furnish the test liquor we want.

1st. Into water heated to 50 degrees, immerse two matrasses, one with the filtered juice, the other with albumen. In an instant

but a permanent state.

Complete destructive putrefaction is nowhere seen.

Fecula from cabbage juice; compared with albumen.

It separates from water by less heat.

instant the juice will be clouded with cheese-like flocks which fall to the bottom; but at this temperature, the albumen does not experience the slightest change.

Fecula, though greatly diluted falls by heat.

2d. Place on a furnace two matraffes, the one with juice mixed with twenty parts of water, and the other with albumen. The fecula, however diluted it may be, will nevertheless be entirely separated by the heat; thus clearly proving its insolubility. With respect to the water of the albumen, as it heats, it opalises, without losing its transparency; it boils, and becomes contracted, but does not deposit flocks, or any thing resembling fecula. And, if at last the evaporation be completed in an open vessel, it ends by leaving only a coat of white egg. Darcet has already shewed that albumen dispersed in a great quantity of water, is no longer separable by heat. Albumen is a soluble mucilage, fecula is not; and the temperature which coagulates the latter produces no change on the state of the first.

Fecula continually separates.

3d. The water of albumen remains several days unaltered; the juice of plants, on the contrary, is in a continual state of change which never ceases to disturb its transparency. Filter the juice, it becomes turbid; repeat the filtering, it again becomes clouded; in short it continually deposits white fecula.

Albumen acts on tests like an alkali: fecula not.

4th. Albumen changes the juice of violets green, and restores reddened turnsole to blue.

The white fecula, washed, does not produce either of these changes. And how should it? The juices of cabbage, hemlock and many others redden turnsole. Besides the property of changing it to green is not possessed by the albumen itself; it is known to be owing to a mixture of alkali.

They differ in habitude with alcohol;

5th. Alcohol separates light, transparent, glary flocks from the water of albumen, which retain, on the filtre, the appearance of boiled white of egg. The juice of plants only yields to alcohol a whitish opaque powder, which quickly falls to the bottom of the vessel.

with acids;

6th. All the acids, hidro-sulphurated water, and ammonia precipitate the fecula dissolved in the juices; but these re-agents operate no change on the water of albumen.

with ox. mur. acid;

The oxygenated muriatic acid precipitates and oxidates the white fecula; the same acid first oxidates and then precipitates the albumen.

7th. Crystallized

7th. Crystallized carbonate of potash, magnesia, sea-salt, ^{and with com-} muriate of potash, sal-ammonia, saltpetre, &c. thrown into ^{pound salts.} a filtered juice, cause the fecula, which by its nature is but little soluble, to precipitate in proportion as they dissolve. The water of the albumen is not disturbed by any of these salts.

Consequences.

The white fecula deposited spontaneously, or by alcohol, acids, salts, &c. is insoluble in water. The albumen is the reverse: the acids which precipitate the fecula, do not alter the solution of albumen. Fecula insoluble in water; albumen soluble.

No salt is capable of depriving the albumen of water; but with the fecula it is different; its affinity for water is so weak that there is none which does not destroy it; and, consequently precipitate it.

The white of egg dried and afterwards softened is restored, in bulk, in opacity, in whiteness to boiled albumen. Not so the white fecula; it becomes of a deep brown. Nay the greatest number are entirely blackened in drying, such as those of cabbage, cresses, *solanum lico persicum*, &c. and if they are softened in water, they will never assume the appearances which are known in white of egg. In short, this fecula is nothing but a part of the gluten which forms the basis of the green fecula. If, for example, the fecula of white cabbage, separated by the filtre, be compared with that which is obtained from its juice by heat, and both be deprived of their colouring parts, the smallest difference will not be discovered. But, above all, the white fecula is the most easily dissolved, because it is not, like that which is green, in a state of combination which opposes it. All plants contain a portion of gluten, which, not having been vivified by the light, remains without colour. Other striking differences.

The cabbage, the endive, the (escarolle) and the plants blanched by the art of the gardener, also yield white fecula, but in much less quantity than when they are permitted to remain green. The stalk of the cabbage and that of hemlock afford pale fecula in comparison with that obtained from their leaves. But in general it is not necessary that vegetables should show much colour outwardly to indicate their possessing much gluten. Green and white fecula.

Separation by heat is not an exclusive character of albumen.

gluten. The small houfeleek, as we shall see presently, yields an abundant fecula of a deep colour, and particularly rich in wax.

VIII. But it will be said, that the albumen being the only product in which the property of coagulating by heat has been noticed, it would appear natural to conclude that, &c. But the milk of almonds also separates by heat, alcohol, acids, &c. This is a fact which has been always known in pharmacy, and nevertheless a conclusion has never been formed from this slender appearance that emulsions contained white of egg, because even if the characters of animalisation had been perceived in the principles of the emulsions, they ought likewise to have exhibited other striking marks of resemblance before they could have been considered as albuminous*.

The characters of fecula and gluten are not the same throughout, &c.

It is also in this point of view that the gluten of fecula must be regarded, because it is neither tenacious, nor elastic, nor fermentable, like that of cheese †. In announcing it to chemists as a product analogous to that from wheat, Rouelle only brought forward part of the characters which constitute the agreement, those alone which belong to the nature of the component parts, because the external marks of resemblance do not exist; for the same reason, my object, in defending the labours of this great master, is much more to retain in the catalogue of his discoveries that of an animalised matter found more immediately in the leaves, than of gluten properly so called, because this in fact, is the discovery which the author of the "System" has rendered doubtful in his work. Break

* Rouelle had a much better foundation than he was aware of, for comparing the green juice of plants to an emulsion. The cheese separated from the milk of almonds, by some one of these means, being washed and dried, yields an oil by expression, and afterwards all the products of caseum, by distillation. This, without doubt, is the reason why almonds and all sorts of nuts, afford so great a quantity of nitrogen, with nitric acid.

The thin milk of almonds contains gum, a little extractive matter, and sugar, which is either that of the cane, or that which I discovered in the grape, and which I shall describe when treating of fermentation.

† The gluten of wheat is susceptible of a fermentation which is peculiar to it. The gases disengaged are carbonic acid and tolerably pure hydrogen in abundance. I shall hereafter resume this subject.

the

the aggregation of animalised matters, take away the forms of silk, horn, wool, feather, &c. and it is clear that then being considered only as to their constituent parts, they would be albumen, gluten, fibre, and all that can be stated, because if the constituent parts are, in all respects, the same, (which even now has not hitherto been examined, and which nevertheless is the only proper means of distinguishing them from each other,) nothing would remain but to ascertain the proportions in which nature has combined them, in order to give them their respective properties or being.

But, it may be added, that if the albumen be not separated from the juices with characters as distinct as may be wished, this must be ascribed to the extract, the salts, and the acids which are constantly united with it, and which cannot but disguise it a little; it is particularly in the washings of flour that it must be sought, to be obtained in such a state of purity as to put its nature past all doubt. Let us see then what the washings of flour will afford.

IX. The water of flour like a recently filtered juice, is in a state of change continually increasing, and which does not cease until the acid arising from the fermentation of the saccharine principle has completed the precipitation of the gluten.

All the acids, all the salts which have been applied to the juices operate in a similar manner upon the washings of flour; alcohol does the same, but not vinegar, because it dissolves the gluten. In a word, it is not by coagulating the gluten that acids separate it from juices and the water of meal, because ammonia and salts do the same, but rather by attracting the solvent of a substance which appears to borrow its solubility from a pure and simple division, but not from an affinity similar to that which unites gums, sugar, or albumen to water.

The water from flour exposed to a heat of fifty degrees, parts with its gluten like the juice of plants. Neither can solubility be imparted to the gluten by diluting it with a large quantity of water. It is precipitated by the slightest impression of heat.

I have collected to the quantity of an ounce of gluten, separated by heat from such washings, to be kept in its own moisture; it fermented and produced vinegar and ammonia. At present, after two years, it is a dark mass, cellular, odorous and savoury, like the cheese from gluten.

To

Albumen has not been discovered in vegetables.

To conclude; albumen has not yet been discovered in vegetables. Though it is not to be thence inferred, that it is impossible it should be formed there as well as in animals. The time in which we live, more fertile than ever in observations, convinces us daily that there are few products, either of one kingdom or the other, which can be truly considered as exclusive. Nevertheless it must be admitted that in endeavouring to establish the existence of albumen to the exclusion of the gluten of green plants, the learned author of the "System" has depended too much on the feeble support of a simple appearance. It appears to me that, before stating the existence of the albumen, he ought to have strengthened his first opinion with facts more conclusive than the single one of concretescibility. Let us not however overlook that, in so vast an enterprise as his, it is very difficult for an author to bring all his materials together with equal precision.

Remarks on other objects.

It will be in the same disposition that I shall extend these conclusions to the other products, placed by Fourcroy, without sufficient examination, among the glutinous substances of vegetables.

There exists, says he, an observation more exact and more positive than that of Rouelle upon the presence of this glutinous matter in the vegetable texture which forms linen, paper, &c. p. 296. vol. vii. It is sufficient to recall this passage to the recollection of its author. More details on my part, would have too much the appearance of censure. I think Fourcroy will suppress it in a new edition as well as that relative to the paste of mallows. If this paste be entitled to be placed in the class of the animalised products of vegetables, almond paste, and those of frangepane, eggs, marmalade, &c. are equally deserving to be so arranged.

Bird-lime is a kind of turpentine.

With respect to the bird-lime mentioned in the same chapter, it is universally known to be only a kind of turpentine, an inflammable aromatic resin, soluble in alcohol, formed by vegetation in the fibrous texture of the holly-oak, in the fruit of the elder, perhaps in its bark, and in other plants, but in no respect a glutinous substance.

Green fecula soluble in potash and leaves woody matter.

X. Potash easily dissolves green fecula, and divides it into two parts; one unites itself to the solvent and the other is separated in the form of a green powder which is not affected by a new quantity of potash. This powder, being washed and dried,

dried, yields by distillation the products of white wood, of linen, that is to say, nothing ammoniacal. It is the woody part which usually accompanies the fecula in consequence of the trituration.

This solution has all the characters of an animal solution; it exhales ammonia, an odour which is that of a woad vat; it blackens the silver evaporating basin, and emits, by the action of acids, an effluvia which darkens the writing of white metals.

The solution has animal characters.

But in this case, as in the soap of wool, great part of the fecula experiences a loss which deranges the nature and proportion of its radicals. Acids only separate a small quantity of the fecula. The remainder acquires an extractive character which disposes it to unite with water. Neither alcohol nor acids can separate this new extract from salts. It is of a fawn colour, and muriate of tin precipitates from it a dusky lake.

But the fecula cannot be recovered by acids.

With respect to the other, when collected and washed on the filtre, it is worthy of notice that it has not lost the property of hardening by the heat of boiling water.

Properties of the separated matter.

Alcohol acquires a deeper green from the precipitated fecula than from that which is fresh. This arises from the resin, which not being destructible like the gluten, is united in greater quantity to that which escapes the destruction. As to its other properties, this fecula, by distillation, yields ammoniacal products.

XI. An acid of 18 or 20 degrees disengages azote abundantly from green fecula; a stronger acid dissolves them with facility, and separates a little powder which is the ligneous fragments of the plant.

Acids separate azote from fecula, &c.

However sparingly the nitric acid is used, it is but seldom that crystallised oxalic acid can be obtained. It is resolved into carbonic acid and water.

The solutions of fecula always contain the yellow bitter principle of Welter, sulphuric acid, benzoic acid, oxalate of lime and fat. If the solution of a fecula containing iron, such as the solanum lycopersicum, be precipitated with acetite of lead, a powder composed of oxalate and phosphate of lead, and oxide of iron is obtained. On heating it with the blow-pipe it burns, even the lead is dissipated and nothing remains but a globule of phosphate of iron.

When

When a vegetable product contains nitrogen, sulphur, phosphorus, benzoic acid, fat, yellow bitter, iron in abundance, no doubt can remain of its belonging to the class of animalised substances.

Of Wax.

Wax is a vegetable matter.

XII. Wax is the work of vegetation, and not of the bees. It is, I think, in deriving nourishment from the gluten which accompanies it in the dust of the stamina that they make the separation. This dust yields ammonia in abundance, which has led me to believe that it contains gluten; and now that I have discovered wax in certain fecula, I am of opinion, that if this dust was treated with nitric acid, wax would be obtained.

It may be had from fecula.

The fecula of the small houseleek yielded a quantity that surprised me. This wax is white, dry, brittle and without smell; it cannot be confounded with the sebaceous products obtained from other fecula, such as those of hemlock and folanum. Messrs. Fernandez and Chabaneau examined it to satisfy themselves; they chewed it, and were convinced that this product was a perfect wax.

It covers plants;

The fecula of green cabbage also yielded it, but in much less quantity. The wax appears to me to be the varnish spread over the plants by vegetation, doubtless to preserve them from the effects of wet, which might injure them. It is this varnish which divides the rain and the dew into the silvery pearls upon the leaves of cabbages, poppies and so many other plants which afford this agreeable spectacle in our gardens. It is also the wax which the curious gardener who gathers a plum, a fig, or a grape is careful not to rub off with his fingers.

At Paris, an orange on removing the paper in which it has been wrapped from its leaving Portugal, will be found covered with a farinaceous coating, which may be taken off with the blade of a knife, and then brought to a candle and melted to ascertain its nature.

The fecula of opium also contains a fat which from its firm consistence, nearly resembles wax, and which has been known to many opiologists.

and silk.

Finally, raw silk is also covered with a coating of wax which is carried off, with the colour, by alcohol, and is separated from that fluid by cooling.

Of some other fecula less known.

When five or six pounds of saffron are operated upon at the same time in order to obtain the volatile oil and extract, a fine powder is observable in the decoction, which renders it cloudy, precipitates, and may be separated by straining. This powder washed assumes in drying the contraction and horn-like appearance of the green fecula in summer; it putrefies rapidly and becomes food for worms if care be not taken. In the fire this fecula yields all the products of gluten. With alcalis and lemon-juice, it tinges silk of very brilliant yellow.

Fecula from Saffron.

Borage.

A plant may contain gluten in two states: the one, in the fecula; the other, dissolved in its juice by means of potash. Such is the juice of borage; clarified, it is thick and of a light blue colour; a few drops of acid separate from it a cheesy curd which may be collected on the filtre, and is nothing but gluten.

from borage;

Elder. Dwarf Elder.

The berries, in the midst of an highly coloured, very gummy, and slight by saccharine juice, contain a fecula as green as that of spinach when it has been well purified from its red colour. Alcohol extracts from it a green tincture; the residue is a gluten not at all different from that of fecula.

from elder and dwarf elder.

On crushing these berries their bird-lime sticks to the fingers; it is of the same consistence as that from the holly-oak: submitted to fermentation these juices yield a very small quantity of spirit of a disagreeable smell. It is succeeded by an astonishing quantity of very good distilled vinegar.

Buckthorn.

Its juice, which contains a very nauseous bitter extract with gum and a little sugar, is thickened with a greenish mud, which is separated from it by heat and fermentation. This pulp, well washed, is of a clear green; it is gluten mixed with a small quantity of fibre. It affords carbonate of ammonia, &c.

from Buckthorn:

The Rose.

Its petals yield by trituration a fine lightly coloured fecula, which affords the same products as gluten.

Grapes.

Grapes.

from grapes.

Fecula is found abundantly in them, it is that which makes the ley of wines; but to speak of this product would be to anticipate what I have to say on fermentation. Lastly, gluten is found in quinces, apples and, without doubt, in other fruits; it is found in the acorn, the horse-chestnut, the Spanish chestnut, rice, barley, rye, pease, and beans of all sorts. I shall return to this subject in treating of the difference between wheat which has germinated and that which has not undergone this operation.

IV.

Description of a Borer for draining Boggy Land.

By THOMAS ECCLESTON, Esq. *

Chief difficulty
of draining bogs.
The newly cut
drain rises up.

THE greatest obstacle to the effectual draining of many boggy lands, consists in the earth in the bottoms of the ditches or drains when newly cut, and more especially if made to any considerable depth, rising from the pressure of the waters contained in the bog, by which the new-cut drains and ditches are frequently so nearly filled up, as to impede the flowing of the water they were intended to carry off, and thereby rendering the work comparatively ineffectual.

Remedy by
boring.

There are different layers, or strata, in moss or peat lands, which will not allow the water easily to filtrate through them, yet are of so soft and spongy a nature as to rise from the pressure of the water contained in the bog.

It becomes necessary to give a free vent to the above confined water, effectually to drain such lands. This has been most successfully done by the Augre, a model of which I have herewith presented to the society.

Method insuffi-
cient.

A common augre, or even a pole, will force a passage, and give vent to the water for a short time; but owing to the peat being only pressed sideways, and not cut out, the parts soon join again, and the passage of the water of course becomes completely obstructed; but by means of this augre, a cylindrical column of peat of six inches diameter will be clearly cut out and taken thence, and a free passage maintained for a very considerable space of time.

* Soc. of Arts. 1801.

The first experiment made, produced a clear augre-hole of the above dimenſions, four yards in depth, in one hour; and the water, which had been pent up in the bog, roſe above the level of the bottom of the ditch, from four to fix inches; and the bottom of the ditch, which was previously very ſoft, and had begun to ſwell and riſe, in a few days became more firm and ſolid, and this in ſo great a degree, that when cleared it remained without ſwelling or riſing in the leaſt. It will conſiderably reduce the expence of draining ſuch lands, by rendering them ſo firm as to cauſe the firſt end-drains to ſtand.

but the large
augre cuts a
durable hole.

The moſt proper depth to bore, depends on the ſituation. Where the moſs lands lie low, and are in danger of being flooded, no greater depth than what is abſolutely neceſſary for draining the ſurface ſhould be bored, as, by deep boring, the land may be ſunk ſo low as to be liable to inundation.

A. Plate II. Fig. 2. The cutter of the borer, which penetrates the peat. B. The body of the borer, ſix inches in diameter. C. The aperture through which the peat introduced by boring is drawn out. D. A portion of the iron bar of the borer, to the upper part of which a croſs handle is to be fixed.

Description.

V.

Description of an Improvement in the Gun-Lock by which the casual Diſcharge of the Piece is prevented. By Mr. JOHN WEBB.*

I HAVE taken the liberty of ſending to the ſociety an invention of mine, to prevent the accidents which frequently attend the uſe of fire-arms, and which may be applied to the gun-locks now in common uſe. It is contrived on ſuch a principle, that when it is on full cock, and the trigger pulled in the common manner, it returns to the half cock only, unleſs, at the ſame time that the trigger is pulled, the preſſure of the thumb is applied on a ſpring placed upon the butt or ſtock of the gun; in which caſe it gives fire in the uſual

The inventors
letter.

* Transactions of the Society of Arts, 1802. To the Secretary; and the Society gave the inventor a bounty of twenty guineas.

manner. The intent of this invention is to guard against the casualties which arise when fire-arms are left loaded, or the misfortunes which frequently happen from twigs of trees or bushes catching the trigger when sportsmen are passing through hedges.

I hope it will meet the approbation and encouragement of the Society, and am,

S I R,

Your humble Servant,

JOHN WEBB.

Description of Mr. JOHN WEBB'S Gun-Lock, Plate I. Fig. 1, 2, 3, 4, 5. The letters of the several Figures correspond together in the general Description.

Description of
the lock.

A is the cock—B, the hammer—C, the main spring—D, the tumbler—E, the large sear—F, the small sear—G the sear spring—H, the shank or arm of the large sear—I, the shank or arm of the small sear—K, the thumb-piece—L, the trigger—M, the lever of the thumb-piece—N, the spring which holds the thumb-piece up, when not pressed upon by the thumb.

Fig. 1, is an interior view of the lock at full cock.

Fig. 2.—The same lock at half cock.

Fig. 3.—The lock when down.

Fig. 4.—The lock fixed in the gun-stock, in order to show the thumb-piece K and the trigger L, with their mode of action. When the gun is held cocked in the usual manner, ready to fire, and the trigger L is pulled by the finger, the thumb, being pressed at the same time on the piece K, raises, by means of the lever M, moveable on a pin in its centre, the shank I of the small sear, and admits the cock to give fire as in the common way; whereas, if only the trigger L is pulled, the lock stops at the half cock I; further motion being prevented by a notch in the small sear. A spring, N, screwed to the stock, returns the thumb-piece to its place, when the thumb is taken off.

Fig. 5 shows, on a larger scale, the construction of the tumbler, large and small sears, the sear-spring, and the manner in which they rise out of the bents of the tumbler.

VI. *Description*

VI.

Description and Account of a simple Apparatus for breaking-up Logs of Wood by the Explosion of Gunpowder. By Mr. RICHARD KNIGHT.*

S I R,

I HAVE frequently observed the great difficulty, labour, and loss of time experienced in breaking-up logs of wood, particularly for the purpose of fuel; such as the stumps and roots of large trees, which remain after the felling of timber, many of which, especially such as consist of the harder and more knotty kind, as oaks, elms, yews, &c, are frequently left to rot in the ground, in order to avoid the necessary expence of breaking them to pieces in the common way, which is generally effected by the axe, and driving a succession of iron wedges with a sledge hammer; a laborious and tedious process. Sometimes gunpowder is used, by setting a blast in a similar way to that in mines or stone-quarries. This method, though less laborious than the former, is tedious, is attended with several difficulties, and requires considerable experience and dexterity, or the plug will be more frequently blown out than the block rent by the explosion. With a view, therefore, to obviate these difficulties, I have constructed an instrument, a sketch and description of which I now inclose for your approbation. The simplicity of its construction and application is such as almost to preclude an idea of its originality; but as it has hitherto appeared entirely new to all my acquaintance, and as I do not know that any thing of the kind has ever before been presented to the public, I am induced to think it may not be unacceptable; and should it appear to you an object worthy the attention of the Society of Arts, I shall be happy in making it public through a channel so highly respectable; and will, immediately on being favoured with your opinion, transmit to the society a complete instrument with the necessary appendages, and a more minute description of its mode of application.

Remarks on the object of Mr. Knight's apparatus.

I am, SIR, Your obedient humble Servant,

RICHARD KNIGHT.

Foster-lane, March 16, 1802.

* Transactions of the Society of Arts, 1802. The Inventor received the Silver Medal.

*The following Articles were sent to the Society. See Plate II.
Fig. 1 and 2.*

A, the rending or blowing screw, with a wire B, for the purpose of occasionally clearing the touch-hole, previous to the introduction of the quick-match.

C, an auger proper to bore holes, to receive the charge of the screw.

D, a gouge, to make an entrance for the auger.

E, a lever, to wind the screw into the wood, with a leather thong F attached to it, in order to fasten it occasionally to the screw, to prevent its being lost, in case it should be thrown out when the block is burst open; a circumstance which does not often occur: for in all my experiments, when the wood has been tolerably found, I have always found the screw left fixed in one side of the divided mass.

A roll of twine is to be steeped in a solution of nitre, for the purpose of a quick match, or train, to discharge the powder, by thrusting a piece thereof down the touch-hole, after taking out the wire B.

Account of the
instrument
actually used.

The first that was made was for J. Lloyd, Esq. of St. Asaph, the late member for Flint, who, having a great quantity of timber on his estate, considers it a considerable acquisition; and at Overton-Hall, last summer, spoke so favourably of it, in my presence, to Sir Joseph Banks, that he immediately sent for his smith, and requested I would give him the necessary instructions for making one; but as I left that part of Derbyshire soon after, I had not an opportunity of seeing it finished. Since my return home, I have had several made, similar to that which I now present to the Society, which are better finished, and have sharper threads than smiths in general have an opportunity of giving them.

Letter from Mr. Lloyd on the same subject.

DEAR SIR,

Account of the
use and applica-
tion of the blast-
ing screw.

AFTER you left us last autumn, at Sir Joseph Banks's, his smith, who is a remarkably good workman, bestowed much needless time and trouble in making a blasting-screw; for he finished it in the highest style of polish, and, I think, made the thread of the worm too fine, or at least finer than was needful. However, it answered most completely, and very much to Sir Joseph's satisfaction, who lamented he had not seen such a contrivance

contrivance many years ago, when a relation of his used to amuse himself with splitting the roots of trees, &c. in the common way. I have used the *blasting-screw*, for so I shall call it, all the last and preceding winter, with the greatest success, and have gained many loads of fuel, which otherwise would have been suffered to rot, as the expence and labour in clearing the roots in the ordinary way renders the fuel so procured too expensive; and since I have had the screw, I have observed some hundreds of roots in a rotting state in other places, from the want of knowing that there was such a contrivance as the screw. I think you would serve the public in no small degree, by devising some method of making its use known to the world.

When I was at Overton, some pieces of very tough, knotty, close-grained oak were picked from the timber-heap, for the use of the Gregory lead-mine, by Sir Joseph Banks's direction, and the screw severed some pieces four or five feet in length, and nine or ten in diameter, throwing them some feet asunder, to the surprize of the miners, who were assembled on the mine-bank. Sir Joseph took the screw with him to Revelby-Abbey, in Lincolnshire, where, I understand, he had some large roots, that had lain by many years as useless; and I dare say he will give you a good account, and bear testimony to the utility of the invention. We have used it without a single accident; but my neighbour, Lord Kirkwall, having procured one to be made by that which I had from you, one of his servants, in his Lordship's absence, I presume, put too much powder into the hole, and the screw was blown as high as a one-pair of-stairs window, and passed through it into an apartment where a person then happened to be, but without any farther mischief than the loss of a pane of glass. Any one who uses the instrument will soon learn what depth of screw will be sufficient to split any root in proportion to its strength, taking care that the screw has sufficient hold to resist the force of the gunpowder, before the root is cleft. I think much powder may be saved by using a cotton match, impregnated by a solution of saltpetre, or any of the combustible matters generally made use of in fire-works; and by the use of the cotton the hole through the screw may be lessened, which will add to the action of the confined powder, though a straw filled with powder, in the manner in which the miners use it, answers very well. Should any one be timid in using the screw, a chain or rope may easily be attached to the screw, and that fixed to any log, or fastened to a stake driven

into the ground. If wood is rotten, the screw cannot act. I assure you, that when I go abroad, I constantly see great quantities of roots in a rotten state, about almost every farm-house, which would not be the case if the utility of the instrument were made public. I am,

Your much-obliged friend, and humble servant,
Wyefair, March 26, 1802. J. LLOYD.

VII.

Letter from Mr. DALTON, containing Observations concerning the Determination of the Zero of Heat, the thermometrical Gradation, and the Law by which dense or non-elastic Fluids expand by Heat.

Manchester, April 20th, 1803.

To Mr. NICHOLSON.

SIR,

Remarks on a letter in our last number, concerning the real zero of heat.

IN your last Number, page 221, a correspondent has adverted to an hypothesis which I suggested in my account of experiments on the expansion of elastic fluids. It was, that “the repulsive force of each particle [of gas] is exactly proportional to the whole quantity of heat combined with it,” when subject to a given pressure. Having previously found that 1000 parts of any elastic fluid, at temperature 55° , were expanded to 1325 at 212° , it followed from the hypothesis, that the quantity of heat in air, of 55° , is to the quantity in air of 212° , as $\sqrt[3]{1000} : \sqrt[3]{1325}$, or as 10 : 10.9834 : from which the real zero is deduced at 1573° below the freezing point of water. I had moreover noted, that for $78\frac{1}{2}^{\circ}$ (as the abovementioned writer has justly corrected it) the expansion of air was 167 parts, and for the other $78\frac{1}{2}^{\circ}$, only 158.—From these data, calculating, as he says, *upon my hypothesis*, he has deduced two other points for the real zero; namely, 1714° , and 1486° : these inconsistent results, he thinks, tend to discredit, either the hypothesis, or the accuracy of the experiments. Now he must give me leave to observe, that the two last deductions are *not* derived from my hypothesis, but from another totally inconsistent with it; namely that the mercurial thermometer is an accurate measure of heat. For, upon the hypothesis that the real temperature is as the cube root

root of the aërial space, it will be found, that when the mercurial thermometer stands at $133\frac{1}{2}^{\circ}$, a proper equidifferential thermometer would stand at 139° 3. Hence no inconsistent results can be derived from the above data.

Before the merits of the hypothesis in question can be decided, we must have an instrument to measure equal increments of heat. The mercurial thermometer equally divided is not that measure. De Luc has shewn that the true mean heat betwixt freezing and boiling, is several degrees below 122° per scale. Even Dr. Crawford, who has manifested more desire than any other person to find the mercurial thermometer an accurate measure of heat, has concluded from a variety of experiments, all of which prove that mercury expands more in the higher than in the lower part of the scale, that the thermometer is *nearly* accurate only. Now it has been shewn, that elastic fluids observe a general law of expansion, whether by heat or diminished pressure. Why should not liquids do the same?—I expect to be able in a short time to demonstrate that water and mercury agree in one and the same law or rate of expansion from the two points at which they freeze and boil respectively: and yet these two liquids are apparently the most discordant in this respect. It would certainly be a most extraordinary anomaly, and inconsistent with the simplicity of the other laws of nature, if mercury alone should be excepted from a common law by which all other liquids expand, not uniformly, but increasingly, with equal increments of heat. The truth is, I believe, that the same quantity of heat which raises the mercurial thermometer 2° at the freezing point (of water), will raise it about 3° at the boiling point. A mercurial thermometer graduated according to the hypothesis I have suggested, that the increments of heat are as the increments of the cube roots of the manometrical spaces, (in which case it no where differs more than 7° from the common equal graduation for 180) does, I apprehend, in all cases, indicate the true mean temperature, when hot and cold waters are mixed together; at least, I have not been able as yet to find any deviation.

With respect to the discordance, exhibited by Seguin, in the results of experiments to determine the real zero, it may arise from an error in the theory, or from errors in the experiments.

The mercurial thermometer is not a correct measure of heat.

Expectation that all liquids will be found to observe the same law of expansion.

Seguin's determination of zero.

riments. I am inclined to ascribe it to the latter. The simplicity of the doctrine of heat, maintained by Drs. Irvine and Crawford, is to me so strikingly apparent, when contrasted with the now prevailing, but, I think, unfounded distinctions of *combined* and *uncombined*, or free caloric, that I should be very unwilling to discard it; but upon clear proof of its being incompatible with the phenomena.

I am your obliged friend,

J. DALTON.

VIII.

Memoir containing the Physical and Chemical Examination of the Teeth. By CIT. JOSSES, of Rennes *.

The bones long ago analysed;

AMONG those animal matters which have been analysed with the most attention, the bones may be selected. For a long time, they were believed to be formed of earthy substances, the particles of which were connected by a peculiar *gluten*: at present it is ascertained, that they are a true saline concretion, known by the name of phosphate of lime, mixed with a certain quantity of gelatine. The experiments made to prove this have been so often repeated, particularly in the extraction of phosphorus, and are so well known, that they cannot now be called in question.

but not the enamel of the teeth.

But though there be no longer any doubt as to the osseous substance, the same cannot be said of the covering which cloaths the surface of the exposed part of the teeth. This covering, constantly confounded in the general class of bones, seems never to have undergone a thorough examination, alone, by an accurate decomposition. All the authors who have treated of this subject, appearing to have done it too inattentively, have committed errors, which it is essential to correct. Though it has been supposed, that this covering was formed of materials analogous to the osseous part, to which it adheres, it is nevertheless true, that it differs from it essentially, in its physical and chemical qualities, and that it possesses peculiar properties, as may be easily shewn.

This last is the author's object.

The intention of this work is, therefore, to ascertain the composition of this covering, to shew its physical and che-

* Annales de Chemie, xliiii. 2.

mical characters, and to point out the error of considering it as a common osseous body, having no other difference but the density arising from the compactness of its constituent parts.

I shall first concisely point out the physical characters of the enamel of the teeth, and then proceed to the chemical investigation. This analysis must more fully elucidate the physiological inferences I shall then offer, to prove that nature has assigned particular functions to this substance, which differs absolutely from the bones.

Physical characters of the enamel of the teeth.

When examined on the surface of the teeth, the enamel is white, smooth, polished, transparent, very brittle, and of extreme hardness: its fracture shews a very distinct regular chrySTALLIZATION, formed of an assemblage of small brilliant crystals, very compact, and inclining to the shape of needles. On all the surfaces which it covers, as well as in the interior of certain teeth where it is found, it is disposed in radii, a little oblique and horizontal, almost perpendicular to the body of the bone, forming at the point of contact with it two angles, the upper one re-entering and acute, the other inferior, re-entering and obtuse. This substance is found in this manner, in all animated beings which have teeth.

It appears chrySTALLIZED in its fracture.

From this short sketch of the physical characters of the teeth, it will be seen, that it is impossible to avoid concluding, that the enamel is very different from the osseous body to which it adheres; for which reason I have often proposed to myself, to make a scrupulous examination of it. A favourable circumstance offered itself, and furnished me with an opportunity of making the experiments I had long intended: an opportunity which I should not easily have found any where but at Paris. Before recounting the means I have employed, I ought to say that these experiments have been performed in the laboratory of the medical school.

Its characters differ from those of the bony teeth.

The analogy of the work I wished to undertake, with the interesting researches on animal substances, which continually occupy the professors of chemistry of the medical school: the flattering approbation and eagerness expressed by professor *Deyeux*, in assisting me with his advice; the zeal manifested by the pupils of this laboratory, to whom the care of performing the operations is confided: in short, a multitude of facilities left me no doubt that my hopes would be crowned with success.

One of the first things to be attended to, was to procure the enamel pure: this operation was not without difficulty as will be easily conceived, from the close adherence which this substance preserves with the osseous body, from the time the germ of the tooth is developed and has passed through the regular periods of the formation of the enamel, and of ossification. Nevertheless, a sufficient quantity may be disengaged from the bony part, by the assistance of chemical agents, which, without producing any effect on the animal, act very readily on the bone; such are the means which will be pointed out, and which I have employed to perform the experiments I am about to describe.

Having procured some human teeth, as well as those of several animals, I exposed them to the action of various chemical agents.

The enamel afforded a very slight indication of gelatine.

I then filed off the enamelled surface of some teeth, without reaching the osseous part, I caused the filings resulting from this operation to be boiled in water, and I submitted the liquor to the action of tannin. I obtained so slight a precipitate, that this operation scarcely showed the presence of gelatine. On the contrary, the osseous body, in the same proportions, and by the same process, yielded an abundant precipitate.

Papin's digester softens the bone, but not the enamel of teeth.

I exposed whole teeth in water, to a degree of heat superior to that of ebullition, by means of Papin's digester; the enamel preserved its hardness and its figure, but the bony part was softened and became friable.

The enamel does not burn like bone.

In a naked fire, the enamel does not burn like a bone; the smell arising from the igneous decomposition of an animal substance, is scarcely perceptible; it is not converted into charcoal, but is slightly browned and decrepitates.

Distillation extricates scarcely any thing from it.

When carefully separated from the body of the bone, and distilled in a retort, this substance shews only a faint trace of animal product; it does not afford, like the bone, a great quantity of phlegm, of oil, of carbonate of ammoniac, or of carbonate hydrogen gas; and in all its physical and chemical characters, it presents differences which keep it distinct from the osseous body.

It is soluble in acids.

However unsusceptible of change the enamel may appear to be, it is nevertheless soluble in all the acids, but with remarkable differences, which depend on their radicals, their

concentration,

concentration, their combined or insensible caloric, and on that caloric which being only interposed between their constituent particles, is the cause of their various temperature. Hence arise varieties in the affinities, which change the attractions, and produce different variations in the results, of which I shall have occasion to speak.

Having plunged human teeth and those of several animals into nitric acid, I observed that it acted rapidly on the enamel, and slowly on the bone. The first was soon entirely dissolved, and I could only perceive the osseous part, which in its turn, but in a much longer time, disappeared in an excess of acid. When the solution was completed, I tasted it; it appeared to me still strongly acid, and I found that its taste differed from that of nitric acid. To judge of the nature of this liquor, I filtered it, and submitted it to some experiments, which showed that it contained phosphoric acid, disengaged, without doubt, by the nitric acid.

Nitric acid acts rapidly on the enamel, and slowly on the bone.

The nitric solution affords phosphoric acid.

Teeth digested with muriatic acid, gave similar results to the former.

Muriatic acid,

But the same effects were not produced when I employed sulphuric acid: whether concentrated or diluted with water, hot or cold, the effervescence with the enamel, which I noticed in the other acids, did not take place with this. It appeared at first to refuse to dissolve this substance, but I soon saw it act on the bony part. In a little time I observed it precipitate a whitish salt, which I knew to be a true sulphate of lime. This solution also preserved its acid taste, of which I could not deprive it, notwithstanding the pains I took by adding fresh teeth. This acidity was owing to the phosphoric acid which had been disengaged.

Sulphuric acid acts less on the enamel, and more on the bone.

The experiments which I have just described with the three acids, did not satisfy me; they had been performed on teeth furnished with their enamel. The results were therefore confused, and it was necessary to examine them in a direct manner. I resolved, therefore, to operate upon the enamel perfectly freed from the osseous part: I endeavoured to separate it with a file, but this method appeared too laborious and too tedious to furnish me with a sufficient quantity. I had recourse to the sulphuric acid: the preference I gave to it, was pointed out by the last experiment I have noticed above, which had left me considerable portions of the enamel undissolved,

The enamel separated by sulphuric acid.

undissolved, and perfectly deprived of the osseous substance. This method succeeded beyond my hopes, by weakening the acid a little, which I made warm to hasten the effect. Thus I soon procured detached enamel in sufficient quantity, amply to supply the experiments I proposed to make.

Its appearance.

The enamel, separated in the manner I have described, was washed in several waters before it was used, by which means it was freed from the sulphate of lime, attached to its surface; it was then dried. Examined then with a magnifying glass, it showed transparent parts, the fracture of which, when broken, presented very well-defined needle-shaped crystals. This enamel was very hard, and perfectly resembled the fragments which I had broken in splinters, from the upper part of a tooth, by striking it with a hammer. It only differed from it in having a whiter appearance superficially, owing to the lime having been made more evident. Some pieces of this enamel were laid on burning coals; they decrepitated, and their particles were thrown to a great distance.

Ignition left a white powder;

Other fragments, put into a red-hot crucible, having produced the same effect, I pulverized a certain quantity, and heated it for some minutes, in another crucible. This powder first lost its very white colour, and became a little grey; but by continuing the fire, it regained its whiteness: it then crumbled easily between the fingers; its taste was alkaline; when mixed with water, it was partly dissolved, and the solution resembled lime-water.

which was lime.

Another part of this powder triturated with muriate of ammonia, soon produced the decomposition of this salt, and discovered the ammonia; and lastly, the calcined enamel was equally soluble in nitric, muriatic, and sulphuric acids, without any apparent difference. The solutions, examined by different re-agents, corresponded with solutions of lime. Though it was evident, from these experiments, that lime was the basis of the enamel, yet it remained to be discovered what it was united to, before calcination.

Distillation of this enamel drove off no fluid.

To obtain proofs on this head, I weighed four grammes of enamel, prepared, as I have described, with sulphuric acid; and, after having washed them well with a brush, to remove the small quantity of sulphate of lime, which was found during the separation of the osseous part, and precipitated on its surface, I pulverized them, and proceeded to distillation in a coated

coated glass retort. The fire was urged so as to bring the retort to a red-heat, and after more than an hour, I remarked in the neck of it, a white sublimation, very light. This was the only product I obtained, and notwithstanding my care in keeping up the fire, no fluid was condensed.

When I judged that the operation was at an end, the apparatus was disengaged, and I hastened to examine the sublimate formed in the neck of the retort. The quantity was so small, that I had much trouble in collecting it; it was dissolved by nitric acid. I then had reason to believe that it was lime, volatilized by the action of the fire; nevertheless, as I had been struck with a slight ammoniacal odour, I introduced into the neck of the retort, a match moistened with muriatic acid, and I observed that some white vapours were quickly formed.

A small portion of white sublimate, containing some ammonia.

The residue of the distillation was a whitish powder, whose taste was slightly caustic: it was dissolved by water, and more readily by acids, and changed tincture of violets to green. Its solution, treated with different re-agents, yielded a true lime.

Residue of lime.

On reflecting upon the products obtained during the operation, I have given an account of, I thought I observed a kind of analogy with those afforded by oxalate of lime. To satisfy myself how far this opinion might be well founded, I determined to proceed in the analysis of the enamel, according to the method pointed out by *Fourcroy* and *Vauquelin*, in the interesting work published by them on urinary calculi, in which they shew that the species of concretion called mural stone (*pierre murale*), is entirely formed of oxalate of lime and an animal substance.

Whether the enamel be oxalate of lime.

I therefore pulverized some enamel, and boiled a certain quantity in liquid carbonate of potash. The liquor was scarcely boiling, when I perceived a slightly penetrating odour, nearly resembling that of ammonia. I then introduced into the neck of the matras, a paper match, dipped in concentrated nitric acid; immediately an abundant vapour was formed, such as is always seen when ammoniacal gas is brought into contact with that disengaged from nitric acid.

Enamel dissolved in potash.

Ammoniacal vapour.

When the ammoniacal odour had ceased, I withdrew the matras, and placed it on a sand-bath. After twenty-four hours digestion, I found at the bottom of the matras, a white precipitate, above which floated a limpid liquor; it was decanted

canted and filtered, and then poured upon a new quantity of powdered enamel, and I proceeded as before: at the end of a second digestion of twenty-four hours, its taste did not appear to be any longer alkaline.

Examination of the alkaline solution, in order to detect oxalic acid, without success.

Following the directions of the same authors, I examined the carbonate of potash in which I had digested the enamel, with acetite of lead and barytes; and obtained a very abundant white precipitate. These results, similar to those obtained by the above two chemists, from mural or oxalite calculi, or calculi of oxalate, gave reason to suppose that the oxalic acid being combined with the lime in the enamel of the teeth, formed there, also, an oxalate of lime. To ascertain this, I tried to decompose the two precipitates I have mentioned, but all my endeavours on this point were without success*.

Enamel separated by the digester.

Fearing that the sulphuric acid, which I had used in my preparatory processes, had altered some of the component parts of the enamel of the teeth, I procured some by another method, and recommenced my operations. Papin's digester justified my hopes; it afforded me an easy method of obtaining an abundant quantity, completely disengaged from the osseous part, and which had not experienced any change from an acid. The enamel obtained by this new method, and submitted to the same experiments, no longer yielded similar results.

* If my suspicions had been realised, it will be perceived, that the oxalate of lime being thus discovered completely formed by nature in living animals, and necessary in the organisation of the teeth, the oxalic acid would become a striking part of the beautiful theory of the two learned men whom I have cited. This would have afforded a natural explanation of the calculous oxalates in the urinary concretions, as well as of the oxalic acid sometimes observed in humours, though their phenomena are always thought to be unusual, and to arise from morbid affections.

As the formation of the enamel takes place in the first years of life, and necessarily ceases after the last dentition, it may easily be conceived, that the reflux of the oxalic acid, or the liquid oxalate lime, by way of urine, which has so many points of resemblance to osseous substances, must have given rise to the concretion of the mural calculi, which are more common in youth than in age, as *Fourcroy* has well observed in his works. But after much labour, I found myself compelled to abandon my first expectations, and to endeavour, by new investigations, to discover the truth.

This

This substance distilled in a retort, did not now emit any ammoniacal odour, nor did it produce any fluid. The residue of the distillation was of a grey colour, which became white by calcination, and by the intense heat of the fire it acquired the hardness of porcelain; it no longer had the alkaline taste, it no longer decomposed muriate of ammonia; it no longer changed syrup of violets green, water no longer acted on it; it shew'd, the enamel not distilled and submitted to these latter experiments, afforded the same results as the residue of the distillation: I again placed some in digestion with diluted sulphuric acid, for the purpose of treating it like the ossaceous substances from which phosphoric acid is to be separated, and I obtained similar results; that is to say, acid phosphate of lime in solution in the liquor, with sulphate of lime, and fragments of the enamel not decomposed. These are the salts whose presence has been ascertained from examinations by the usual methods.

The residue of this when distilled was hard like porcelain and resembled the original enamel, &c.

It seemed to be phosphat of lime.

From the great difference of these results obtained in both methods of treating the enamel of the teeth, it will be seen that at first I was led into an error by the effect of the sulphuric acid, which, in the preliminary preparation left considerable portions of enamel, whose physical characters caused me to believe them unchanged.

The enamel separated by sulph. acid is not pure.

I have thought it useful for the interest of science, to make known these surprising differences, they may be advantageously applied in the medical art, and assist in explaining the phenomena, as I had occasion to point out elsewhere.

IX.

A New Method of Clearing observed Lunar Distances of the Effects of Parallax and Refraction, for the Purpose of determining the Longitude at Sea or Land. By Mr. J. ANDREW. Communicated by the Author.

LET A, B, C, D, Pl. 3. Fig. 3. be four given points on any sphere, and so constituted that they shall all lie on the same imaginary plane; and let the sphere be supposed to be cut so by this plane, that the lines AB, BC, CD, DA, and BD, joining the given points, may be all of them right lines, or chords of great circles which may be supposed to have passed through

Principle of the method demonstrated.

through those points before the cutting of the sphere. Farther, let the chord AB be parallel to the chord DC , and since the four points A, B, C, D , lie in the section of a sphere, that is, in the circumference of the same circle, the chord AD will be equal to the chord BC , and the diagonal BD to the diagonal AC . For the same reason (because $ABCD$ is a quadrilateral figure inscribed in a circle) the rectangle under the diagonals BD and AC is equal to the sum of the rectangles under the opposite sides AD, BC , and AB, DC ; or, because BD and AC are equal, as also AD and BC , the square of BD is equal to the square of BC , together with the rectangle under AB and DC .

Now let the point B represent the moon's apparent place, and D the apparent place of the sun or a star, and let the chords AB and DC be parallel to the horizon; then is BD the chord of the arc of the apparent distance, and AD or BC are the chords of the arcs of azimuth circles passing through the sun or a star and the moon, intercepted between the parallels of apparent altitude; or in other words, AD or BC is the chord of the difference of altitude. The length of the chord AB is to that of DC in the ratio of the cosine of the altitude of the object at B to the cosine of the altitude of the object at D , and the rectangle under the chords AB and DC will be to the rectangle under any two assumed chords parallel to AB and DC , and terminated by the same azimuth circles which pass through the points B and D , as the rectangle under the cosines of the altitudes of the points B and D is to the rectangle under the cosines of the altitudes of the two assumed chords. And since BD and BC are had by observation, the rectangle under AB and DC is also known, being equal to $BD^2 - BC^2$, or to $\frac{BD + BC}{2} \times \frac{BD - BC}{2}$.

Make b the moon's true place, situated somewhere in the same azimuth circle that passed through B , and d the sun or star's true place, situated in the same azimuth circle that passed through D ; and analagous to $ABCD$ draw the quadrilateral Figure $abcd$, which will have all the properties of the former, and ad or bc will be the chord of the arc of the difference of the true altitudes, and bd the chord of the arc of the true distance. And it has been already shewn, that the rectangle under the cosines of the apparent altitudes is to the rectangle under the cosines of the true altitudes, as $AB \times DC$ is to

$ab \times dc$, wherefore the rectangle under ab and dc being found and added to the square of bc or ad , which is supposed to be known, the sum will be equal to the square of bd , the chord of the true distance, which may consequently be determined.

Theorems for clearing the distance of parallax and refraction.

If the half chords, or sines of half the arcs be used instead of the chords of the whole arcs, the true distance will, from the foregoing principles, be obtained by the following theorems.

1. $\text{Sine}^2 \left(\frac{\text{app. dist. } \textcircled{P} \ \& \ \textcircled{Q} \ \text{or} \ *}{2} \right) - \text{fine}^2 \left(\frac{\text{diff. app. alt. } \textcircled{P} \ \& \ \textcircled{Q} \ \text{or} \ *}{2} \right) = Q$
2. $\frac{\text{Cos. } \textcircled{P}'\text{s true alt.}}{\text{Cos. } \textcircled{Q}'\text{s app. alt.}} \times \frac{\text{Cos. } \textcircled{Q} \ \text{or} \ *'\text{s true alt.}}{\text{Cos. } \textcircled{Q} \ \text{or} \ *'\text{s app. alt.}} \times Q = q$
3. $\text{Sine}^2 \left(\frac{\text{diff. true alt. } \textcircled{P} \ \& \ \textcircled{Q} \ \text{or} \ *}{2} \right) + q = \text{fine}^2 \left(\frac{\text{true dist. } \textcircled{P} \ \& \ \textcircled{Q} \ \text{or} \ *}{2} \right)$

If a table of chords, or of natural sines of the 2^a power, were constructed, the square numbers corresponding to certain given arches, and conversely, the arches corresponding to certain given square numbers, to be employed in the solution of the above theorems, might be had without farther trouble, by inspection. A second table might also be constructed so as to fit the 2^d theorem, and simplify the calculation for finding q , but this happens to have been already done, as Table IX, with the short, additional, corrective Tables X. and XI. in the 3^d edition of the requisite tables fully answer this purpose, by readily furnishing a logarithm, which added to that of Q gives the logarithm of q . The nature of these tables is explained near the end of that work. Tables necessary.

The method now proposed of solving the problem of the longitude, so far as a solution depends on finding the true distance between the moon and the sun, or any given fixed star, has several advantages, it is conceived, over the common methods, as the answer will in every possible case, be determinate; the diverse affections of the sides of the spherical triangles commonly used never occasioning in this way any embarrassment to the operator, and as there never happens any change of signs from $+$ to $-$, or from $-$ to $+$, in any part of the calculation, it is plain the operation must always be clear of ambiguity. It will also be found on trial, that the nature of the numbers employed in this mode of calculation tends very much to preclude small errors, or to render their effects insensible. A table of logarithms of natural numbers, a table of the squares of chords, and Table IX. with its corrections

Advantages of the method.

from

from the requisite tables, are all the tables necessary (after having found the moon and the sun or stars true altitude) according to the new method, for finding the true distance required. And as they require to be opened only about seven times, the whole operation might be easily performed in about five minutes; and if carefully done, the answer will not vary one second of a degree from the truth.

EXAMPLES.

Examples.

1. Given the apparent altitude of the moon's center, $35^{\circ} 49' 57''$, and the true altitude $36^{\circ} 33' 9''$; the apparent altitude of the sun's center $28^{\circ} 49' 54''$, and true altitude $28^{\circ} 48' 19''$, and the apparent distance of their centers $95^{\circ} 31' 2''$ req^d the true distance.

App. Alt.	Log. Cofines,	True Alt.	Log. Cofines
\odot $35^{\circ} 49' 57''$	— 9.9088773	\odot $36^{\circ} 33' 9''$	— 9.9048840
\odot $28^{\circ} 49' 54''$	— 9.9425241	\odot $28^{\circ} 48' 19''$	— 9.9426341
2) $7^{\circ} 0' 3''$	19.8514014	2) $7^{\circ} 44' 50''$	+ 19.8475181
$3^{\circ} 30' 1\frac{1}{2}''$	Nat. sine ² 0037278	$3^{\circ} 52' 25''$	— 19.8514014
$\frac{1}{2}$ App. dist. $47^{\circ} 45' 31''$	Nat. sine ² 5480725		— 0.0038833
	Difference 5443447	—————	Log. + 9.7358740
			= 9.7319907
$\frac{1}{2}$ Diff. true alt. $3^{\circ} 52' 25''$	N. S. ² 0045638		
$\frac{1}{2}$ True dist. $47^{\circ} 31' 40''$	N. S. ² 5440628		
	2		
	$95^{\circ} 3' 20''$		= True distance.

2d. Example from the Requisite Tables, page 38, 3d Edition, *ad fin.*

App. alt. * $11^{\circ} 17'$	True alt. * $11^{\circ} 12' 21''$
— — \odot $9^{\circ} 38'$ Ho. Par. $54' 42''$	— — \odot $10^{\circ} 26' 28''$
2) $1^{\circ} 39'$	2) $0^{\circ} 45' 53''$
$0^{\circ} 49' 30''$	$0^{\circ} 22' 56\frac{1}{2}''$
$\frac{1}{2}$ App. dist. $21^{\circ} 47' 51''$	N. S. 0142530
	N. S. 3713273
Log. 9.5861148	= 3855803 = S. 0000445 = 5.6486956
Log. 9.5527587	= 3570743 = D. 1373746
Tab. IX. Log. 9.9990330	1374191 = 19.1380470
19.1379065	= 1373746
	$21^{\circ} 45' 31\frac{1}{8}''$
	2
	$43^{\circ} 31' 2\frac{1}{2}''$ = True distance

3. Example

3. Example from the Requisite Tables, page 41 & 43, 3d Edition, *ad fin.*

<p>App. alt. * $\overset{\circ}{24} 48$ $\ominus \frac{12}{2} 30$ Ho. Par. $\overset{\circ}{56} 15$ — — $\ominus \frac{13}{2} 20 42$ $\frac{6}{2} 9$ Nat. fine 1071318</p>	<p>True alt. * $\overset{\circ}{24} 45 57$ $\ominus \frac{13}{2} 20 42$ $\frac{5}{2} 42 37\frac{1}{2}$ L. S. 8.9978259</p>
<p>$\frac{1}{4}$ App. diff. $25 44 17\frac{1}{2}$ — 4342618 Log. 9.7335131 = 5413936 = S. 0099004 = 7.9956518 Log. 9.5147204 = 3271300 = D. 1765564</p>	<p>$\frac{2}{2}$ 1864568 = 19.2705783 Sine $25 34 56 = 9.635289\frac{1}{2}$ $\frac{2}{2}$</p>
<p>Tab. IX. Log. $\frac{9.9986500}{19.2468835} = 1765564$</p>	<p>True distance = $\frac{51 9 52}{2}$</p>

4. July 6th, 1790, at 9^h 13' 15" A. M. apparent time, the apparent altitudes and distance of the sun and moon's centers were observed as follows: Moon's alt. 25° 29' 11", Sun's alt. 15° 54' 5" distance 66° 19' 32", Moon's Ho. Par. 54' 35" req^d the true distance.

<p>App. alt. $\ominus \overset{\circ}{25} 29 11$ $\ominus \frac{15}{2} 54 5$</p>	<p>True alt. $\ominus \overset{\circ}{26} 16 28$ $\ominus \frac{15}{2} 50 55$</p>
<p>Difference 9 35 6 Tab. II. 0069802</p>	<p>10 25 33 Tab. II. 0082550</p>
<p>App. diff. 66 19 32 Tab. II. 2992304</p>	<p>2903825</p>
<p>Log. 9.4657548 = 2922502</p>	<p>T. diff. $\frac{66 15 4}{2}$ Tab. II. 2986375</p>
<p>Tab. IX. Log. $\frac{9.9972157}{9.4629705} = 2903825$</p>	

Note.—In the three first examples the computation is carried on by the help of the tables that are in common use, but in this last example, by referring to the proposed table of the squares of chords, under the title of Table II. some idea may be had of the labour required in the more improved method of computation. There is nothing farther to add than that, if after the many modern improvements in the way of clearing the observed lunar distances of the effects of parallax and refraction, the present mode should be thought of utility, and worthy of adoption, the necessary tables, or at least that part which is new, will be published in a separate octavo volume.

X.

*Analysis of the Propolis or Mastic of Bees.** By Cit.

VAUQUELIN.

Propolis or bee mastic.

THE propolis is well known to be, the first matter collected by the bees of a swarm, newly placed in a hive. This substance is resinous, ductile, odorant and of a reddish brown colour, more or less deep.

The propolis adheres so strongly to the legs and feet of the bee, which has collected it, that it is not able to free itself from it. Other working bees, to whom this bee seems to present its legs, carry off, with their jaws, this tenacious matter, and apply it round the inside of the hive, particularly over all the projections: hence this name of propolis, which signifies in the Greek *before the city*.

Its general properties and application.

This propolis is at first soft and very extensible; but it hardens, and at last becomes very solid. It is not yet known from what part of plants it is extracted: it is believed that it comes from that species of gum-resin which coats and defends most of the buds of trees and shrubs from wet. By continuing this work within the hive, all the foreign bodies, which are introduced into the common habitation and are too heavy to be removed to the outside, are covered over with this matter, and thus put out of the boundary.

The following are the observations made by Cit. Vauquelin on this subject, and which he has inserted in a report made to the Society of Agriculture, in concert with Cit. Lasteurie.

In the mass it is blackish; but it is semitransparent when in thin plates. The heat of the fingers is sufficient to soften it and give it all the ductility of wax: but it is more rosy and tenacious. Like wax it may be kneaded between the teeth, without any perceptible taste. Its odour is aromatic, resembling that of melilot, of balsam of Peru, or of the Banana poplar.

Analysis:
Solution in alcohol:
filtration.

One hundred grammes of this substance were digested, for twenty-four hours, in very pure alcohol. The liquor acquired a deep red tint: and was thus filtered. Fresh spirit of wine was put to the residue, and left to digest again in the cold during twenty-four hours. As it had gained but little colour,

* Abridged in the Bulletin de la Société Philomathique, from the Memoirs of the Society of Agriculture of the Department of the Seine.

a third quantity of alcohol was added to the marc, then boiled for a few minutes: and filtered quite hot, Six portions of alcohol were successively added to the marc and boiled: finally to deprive it of the fat matters it retained from fragments of bees, as well as some vegetable substances and grains of sand, boiling sulphuric ether was poured on it, and the mass was pressed through a fine strainer. The residue dried, weighed 14 grammes.

To obtain the substance which had been dissolved by the alcohol, the whole quantity that had been employed in the different washings was collected, and passed through a fine strainer, which stopped all the matter precipitated by the cold. This residue was wrapped in unsized paper and pressed; when dried and melted, it weighed 14 grammes.

Precipitate by cooling.

All the alcohol was then distilled and reduced to three-fourths of its quantity. The liquor which came over had an aromatic odour; but it did not render water turbid, nor was it acid. What remained in the retort was of a deeper colour. Its precipitate by water was ropy like the resins which are obtained by the same means. By diluting this liquor with water and boiling it, a resiniform mass was obtained on cooling, of a red brown colour, semi-transparent and very brittle, which weighed 57 grammes. The water in which this matter had been dissolved, contained an acid.

Distillation gave aromatic spirit. Three fourths which remained in the retort gave a resinous precipitate by water. The water was acid.

This resinous mass, or pure propolis melts readily on the fire: it yields by distillation a volatile oil, white and of a very agreeable smell. The fixed part then acquires a deeper colour and becomes harder: it is soluble in fixed and volatile oils. It is a true resin, very similar to balsam of Peru, of which it contains the acid.

Properties of the resin.

The 14 grammes of precipitate produced by cooling, were true wax, possessing all its properties. It remains to be known whether this wax is actually mixed with the propolis by the bees; or whether, in collecting the latter with too little care the wax is not united to the resin.

The first precipitate was wax.

	grammes.	
Pure wax - - - -	14	Component parts.
Pure resin of propolis - -	57	
Residuum of extraneous bodies	14	
Loss; acid; aroma, - - -	15	
	100	

XI.

*Memoir on the cutaneous aeriform Transpiration. Read at the public Sitting of the Society of Health, at Grenoble, the 4th Frimaire, in the year 11. By CIT. TROUSSET, M. D. etc.**

Introductory observations.

PHYSICIANS have in all ages endeavoured to ascertain the influence of the air on the human body; but how it is to be conceived that the ancients, who were not even acquainted with the gravity of that fluid, should be able to determine its action? For the same reason, if we except Hippocrates, who in his work says plainly that the air is digested in the lungs, like the food in the stomach, his cotemporaries and their successors have only left behind them, on this subject, incoherent fancies, often ridiculous, and always wrong; the offspring of imaginations undirected by any certain experience.

If it were my intention to consider the influence of the air, in all its relations, I should first attend to respiration, a function of such importance, that without it life could not be sustained, while alone it would require some time for its discussion.

But modern chemists, after having made an accurate analysis of atmospheric air, have formed a theory of respiration, so ingenious, so complete, and founded on such exact experiments, that every effort which has hitherto been made to overturn it has only served to render it more solid.

I could, then, only repeat here what has been already said by Lavoisier, Seguin, Crawford, Fourcroy, Chaptal, &c. for this subject has been so well elucidated by the labours of these celebrated chemists, that it may be said to be exhausted; and their doctrine, on this topic, has been so disseminated, that, at present, it is known and adopted by all who employ themselves in scientific pursuits; I shall, therefore, dispense with a repetition that would be fastidious and useless.

Whether gas be emitted from the skin.

But though the operations of the lungs are very accurately known, the functions of the skin have not been so distinctly elucidated. Independent of the cutaneous transpiration so carefully and diligently observed by Sanctorius and many others, we may enquire whether one or several aeriform fluids escape

* Annales de Chimie, XLV. 73.

by the skin? And if so, what may be their nature? These are the questions I propose to examine in the present memoir.

The ancients did not even suspect the cutaneous transpiration of air, and make no mention of it in any of their works.

The count de Milly first announced, in 1777,* the discovery of an elastic fluid which passes off by the skin. He asserts that, being in a warm bath, half a pint might have been collected in the space of three hours; and from his analysis, which was both inaccurate and very incomplete, he concluded that it was fixed air (carbonic acid gas.)

C. de Milly first observed it.

M. Ingenhouz, some time afterwards noticed this transpiration of air by the skin, but he believed it to be phlogisticated air. (nitrogen gas.)

D. Ingenhouz.

M. M. Priestley and Fontana repeated the experiments of the two preceding philosophers, but could not perceive any aeriform emanation from the skin.

Priestley, Fontana.

M. Jurine, surgeon at Geneva, intending to be a competitor for the prize proposed by the Royal Society of Medicine, repeated the experiments of M. M. Milly and Ingenhouz, not only on himself but on several individuals of all ages, using different kinds of water and varying the temperature; he informs us that he never discovered any emanation of air. Presuming that the water, by its pressure might have obstructed the passage of the air; or that it contracted the exhaling air-vessels of the skin, he continued his inquiries, by varying the processes before used by M. M. Priestley and Fontana, and he thought he had proved by experiments, the inaccuracy of which may be easily shewn, that a small quantity of fixed air (carbonic acid gas) is constantly emitted by the skin.

Jurine.

Cit. Fourcroy expresses himself thus: "it is not true, as some moderns have asserted, that elastic fluids and particularly carbonic acid gas are transpired by the skin" *.

Fourcroy.

Such, a very few years ago, was the state of the question which now engages us. Incorrect experiments, the contradictory results of which were disputed either wholly or in part,

* Memoirs of the Royal Academy of Sciences at Berlin, for 1777, p. 32.

† See History and Memoirs of the Society of Medicine, Vol. X. p. 54, and fol.

* Fourcroy, System of Chemical Knowledge, Vol. IX. p. 203:

left philosophers in uncertainty, and appeared to require new observations, in order that the opinions of chemists might be finally settled on this subject.

Observation of
gas emitted
from the skin in
the bath.

I had often meditated on this subject with an intention of engaging in it, when in the spring of the year eight, while attending one of my patients in the bath, I perceived that he was entirely covered with bubbles of air; the hairs on his body were surrounded with bubbles decreasing from the base to the summit, so that many of them presented the appearance of pyramids more or less elevated. I displaced all these bubbles, and in half an hour afterwards there appeared an equal quantity.

It was nitrogen.

On witnessing this phenomenon, I engaged my patient to continue the bath, and collected successively several jars of this gas, which I examined with much care and at different times. I was satisfied that it was perfectly pure nitrogen gas, without any mixture of carbonic acid.

I wished then to know whether this phenomenon was general, or depended on the pathologic state of the subject. Experiments made on myself and on several other individuals did not offer any thing similar.

I communicated my experiments and their results to Cit. Fourcroy, in the beginning of the year nine, who encouraged me to repeat them. Stimulated by the approbation of this celebrated professor, I undertook the task. It was not enough for me, in fact, to be certain of the accuracy of my experiments, it was also necessary to convince others.

Detail of experi-
ments on this
gas.

In the spring of the year nine, having collected a certain quantity of this gas with great care, I filled a small glass jar 10 lines in diameter, and eight inches high, with it; a taper was instantly extinguished in it eleven times successively.

I endeavoured to decant a similar measure of this gas, but could not succeed, which I could easily have done had it been carbonic acid.

It was passed through lime-water without rendering it turbid or diminishing in volume.

It underwent no change with ammoniacal gas.

It did not affect blue vegetable colours.

Phosphorus which had remained immersed in it for more than a month, was scarcely diminished in bulk.

I judged

I judged these experiments to be more than sufficient to show that it was nitrogen gas.

I again transmitted the result of these new experiments to Cit. Fourcroy, in *Fructidor* of the year nine; probably his occupations have prevented him from sending me any reply.

Having since that period reflected much on the importance of this discovery, I have continued to attend to it, but my notions are entirely changed with regard to the consequences to be drawn from it.

Formerly in the letters written to Professor Fourcroy, and especially in the latter, I considered as a particular fact, dependant on the pathologic state; at present I am compelled to consider it as a general phenomenon belonging to the whole human species.

The emission of gas from the skin considered as a general phenomenon.

1st. Because it is probable that the gas so abundantly yielded by the skin of M. le Comte de Milly, was nitrogen gas, as may be seen by an attentive perusal of his memoir.

2d. M. Ingenhouz, convinced by his own experiments, that an aeriform fluid is emitted by the skin, believed it to be phlogificated gas (nitrogen gas.)

3d. My experiments confirm the suspicion of M. Ingenhouz.

4th. Some time ago I met with another person who transpired abundantly in the bath; the bubbles with which he is constantly covered are not dissolved in the water, it is probably nitrogen gas, I acknowledge, however, that I have never made any exact experiment on it.

5th. The experiments made by M. M. Priestley, Fontana and Jurine, which consisted in placing open vessels under their arm-pits, do not prove any thing in opposition to what I have before advanced, because it is evident that these vessels being full of atmospheric air, it could not be displaced by nitrogen gas, whose specific gravity is less; while this effect would have taken place with carbonic acid gas, if, as M. Jurine infers from his experiments, that this last fluid is constantly disengaged by the skin.

Those made by M. Jurine, by placing his arm in a glass cylinder, are not more conclusive, because they were performed with a view to prove that it was carbonic acid gas which escaped, and the proper means were not taken to point out the presence and the quantity of nitrogen gas, whose disengagement he did not even suspect.

But

The water supposed to obstruct air in most persons.

But why was not this phenomenon, which I believe to be general, observed by M. M. Priestley, Fontana, Jurine, &c. and why is it not equally perceptible in all individuals placed in the same circumstances? It is probable, as I have said in the beginning of this memoir, after M. Jurine, that the water acting by its weight upon the exhaling air-vessels of the skin differently upon different subjects, may obstruct the passage of all gaseous bodies in the greatest number of individuals.

I shall here conclude this memoir, which I might have extended much farther, had I been inclined to discuss all the questions to which the phenomenon naturally gives birth; but having had no other design but to call the attention of natural philosophers to the subject, and satisfied with having began the research, I leave the completion to more skilful chemists.

SCIENTIFIC NEWS.

Decree of the French Government relative to the new Organization of the National Institute.

New organization of the French institute.

THE government of the republic, upon the report of the minister of the interior, having advised with the counsel of state, decree as follows:

I. The national institute at present divided into three classes, shall from henceforth be divided into four; that is to say,

First Class. Class of physical and mathematical sciences.

Second Class. Class of French language and literature.

Third Class. Class of history and foreign literature.

Fourth Class. Class of the fine arts.

The actual members and foreign associates of the institute, shall be divided into these four classes.

A committee of five members of the institute, named by the First Consul, shall adjust this division, which shall be submitted to the approbation of government.

II. The first class shall be formed of the ten sections which at present compose the first class of the institute, with a new section of geography and navigation, and eight foreign associates.

The sections shall be composed and called as follows :

Mathematical Sciences -	{	Geometry - - - -	6 members.	New organiza- tion of the French institute;
		Mechanics - - - -	6 ditto.	
		Astronomy - - - -	6 ditto.	
		Geography and Navigation -	3 ditto.	
		General Physics - - - -	6 ditto.	
Physical Sci- ences - -	{	Chemistry - - - -	6 members.	
		Mineralogy - - - -	6 ditto.	
		Botany - - - -	6 ditto.	
		Rural Economy and Veterinary Art	6 ditto.	
		Anatomy and Zoology - - - -	6 ditto.	
		Medicine and Surgery - - - -	6 ditto.	

The first class shall appoint, with the approbation of the First Consul, two perpetual secretaries, one for the mathematical sciences, the other for the physical sciences. The perpetual secretaries shall be members of the class, but shall not form a part of any section.

The first class may elect six of its members from the other classes of the institute.

It may name an hundred correspondents selected from the learned men of France and foreign countries.

III. The second class shall be composed of forty members.

It is particularly charged with the composition of the dictionary of the French language. It shall examine with respect to language, the important works of literature, history and the sciences. The collection of its critical observations shall be published at least, four times in a year.

It shall name from its own body, and with the approbation of the First Consul, a perpetual secretary, who shall continue to be of the number of the forty members which compose it.

It may elect twelve of its members from the other classes of the institute.

IV. The third class shall be composed of forty members and eight foreign associates.

The objects of its researches and labours shall be learned languages; antiquities and monuments; history and all the moral and political sciences connected with history. It shall particularly apply itself to the enriching of French literature with the works of Greek, Latin and Oriental authors, which have not yet been translated.

It shall employ itself in the continuation of diplomatic collections.

New organiza-
tion of the
French institute.

It shall name from its own body, under the approbation of the First Consul, a perpetual secretary, who shall be of the number of the forty members which compose the class.

It may elect nine of its members from the other classes of the institute.

It may name sixty correspondents, natives or foreigners.

V. The fourth class shall be composed of twenty-eight members and eight foreign associates.

They shall be divided into sections, described and formed as follows :

Painting	-	-	-	-	-	10 members.
Sculpture	-	-	-	-	-	6 ditto.
Architecture	-	-	-	-	-	6 ditto.
Engraving	-	-	-	-	-	3 ditto.
Music (composition)	-	-	-	-	-	3 ditto.

It shall appoint, with the approbation of the First Consul, a perpetual secretary, who shall be a member of the class, but shall not be a part of a section.

It may elect six of its members from the other classes of the institute.

It may name thirty-six correspondents, national or foreign.

VI. The foreign associated members shall have a deliberative voice only on subjects of science, literature and the arts ; they shall not form part of any section, nor interfere in any usage.

VII. The present actual national associates of the institute shall form part of the one hundred and ninety-six correspondents attached to the classes of the sciences, *belles lettres*, and fine arts.

Correspondents may not assume the title of members of the institute.

They shall lose that of correspondent when they shall be domiciliated at Paris.

VIII. Nominations to vacant places shall be made by each class in which the vacancy happens, the persons elected shall be confirmed by the First Consul.

IX. The members of the four classes shall enjoy a reciprocal right to assist at the particular sittings of each class, and may deliver lectures when they are requested.

They shall re-unite four times in a year into one body, to communicate their proceedings.

They

They shall elect in common the librarian and under librarian of the institute, as well as all those agents who belong to the institute in common. New organization of the French institute.

Each class shall present for the approbation of the government, the particular statutes and regulations of its internal police.

X. Each class shall hold one public sitting every year, at which the other three shall assist.

XI. The institute shall receive annually from the public treasury 1,500 fr. for each of its non-associated members, 6,000 fr. for each of its perpetual secretaries; and for its expences, a sum which shall be fixed every year, upon the demand of the institute, and comprised in the estimates of the minister of the interior.

XII. There shall be an administrative committee for the institute, composed of five members, two from the first class, and one from each of the others, named by their respective classes.

This committee shall regulate in the general sittings prescribed by Art. IX. all that relates to the administration, to the general expences of the institute, and to the division of its funds between the four classes.

Each class shall afterwards regulate the application of the funds assigned to it for its expences, as well as all that concerns the printing and publishing its memoirs.

XIII. Every year the classes shall distribute prizes, the number and value of which shall be regulated as follows:

The first class, a prize of 3,000 francs.

The second and third class, each a prize of 1,500 francs.

The fourth class, grand prizes of painting, sculpture, architecture and musical composition. Those who shall have gained one of the grand prizes, shall be sent to Rome, and maintained at the expence of government.

The above decree was followed by a second, appointing the members of the different classes, and regulating the days of their meeting.

The sittings of the first class are to be held every Monday; those of the second class every Wednesday; those of the third every Friday; and those of the fourth every Saturday.

The sittings are to be held in the same place, and to last from three o'clock till five.

New organiza-
tion of the
French institute.

The first sitting of the first class, in Vendémiaire; that of the second, in Nivose; that of the third, in Germinal; and that of the fourth, in Messidor; are to be public.

In conformity to these decrees, the national institute has been re-organised.

The first class, that of physical and mathematical sciences, held its first sitting on Monday the 11th Pluviose.

C. Chaptal was chosen president; and C. Cuvier, perpetual secretary of the section of physical sciences; and C. Delambre, for that of mathematical sciences.

The committee to propose regulations are CC. Laplace, Monge, and Fourcroy.

The sitting of the second class, that of French language and literature, was held on Wednesday the 13th.

President: C. Lucien Bonaparte. Perpetual secretary: C. Suard.

Committee, &c. CC. Morallet, Bigot-Preameneu and Andrieux.

The third class, that of history and foreign literature, on Friday the 15th.

President: C. Le Brun. Perpetual secretary: C. Dacier.

Committee, &c. CC. Pastoret, Dutheil, and Sylvestre de Sacy.

The fourth class, that of the fine arts, on Saturday the 16th.

President: C. Vincent. Perpetual secretary: C. Labretton.

Committee, &c. CC. Vincent, Danon, Dufourny, Gretry and Moitte.

Galvanic Information.

Galvanic re-
searches.

THE *Moniteur* of the 2d Floreal (April 22,) says "the galvanic society continues its labours with zeal and with the most marked success."

"Cit. Gautherot has performed a succession of experiments tending to prove that the developement of electricity is proportionate to the surfaces.

"New discoveries have been published by Professor Aldini relative to the existence of a galvanic atmosphere, and to the contractibility of the heart.

"Cit.

" Cit. Nauches, the president, with his co-labourers BORNET and Pajò-Laforêt, have succeeded, by the assistance of two *homogeneous* metallic conductors, in subtracting the electric fluid of the brain, and spinal marrow of an ox, immediately after its death, and conveying it to the thighs of a frog, where it produced muscular contractions. This operation was equally successful upon the palpitating muscles, and could not be prolonged beyond a quarter of an hour after death. The senator Lamartilliere has given an explanation of the disengagement of mucosity by one of the poles of the pile, and has made it appear that depends on a chemical decomposition.

Cit. Paroisse took notice that, of the muscles, the diaphragm preserved its galvanic excitability the longest.

Cit. Izaer gave an account of the construction of a pile, discovered by Cit. Alizeau, in which the roundels, impregnated with a saline solution, are replaced by moistened salt, and which preserves its power for a month, without being cleaned.

The committee of medical application, consisting of M. M. Guillotin, Dudaumon, Petit-Radel, &c. have made a number of experiments on *asphyxia* produced by strangulation.

" The application of galvanism to the cure of diseases which had been suspended on account of the unfavourable season, is to be resumed in a ward which has been appropriated, by the minister of the interior, to that purpose, in the hospital of the *Ecole de Medicine*, and in a laboratory belonging to the society."

Prize Question.

The Society of Arts and Sciences at Utrecht, has proposed the following Prize Question for the Year 1803.

Prize question.
Electricity.

" What is the true nature of the electric matter? Is it a compound? What are the chemical changes it undergoes, when united with other bodies, and which it produces in those bodies?"

The prize, which is to be adjudged on the first of October, 1803, consists of 30 ducats; and the memoirs are to be addressed to Dr. Luckman, at Utrecht.

Hermaphrodite

Hermaphrodite Fish.

Hermaphrodite fish.

In Vol. XXI. of the "New Transactions of the Royal Academy of Sciences at Stockholm," is an account of an eel (*gadus lota*) in which eggs and soft roe were found at the same time. It is by professor J. G. Pipping.

Experiment calculated to prove that the Laws of Galvanism appear to differ from those of Electricity. By LAGRAVE, Member of the Galvanic Society*.

Supposed difference between the electric and galvanic fluids.

The experiments which I submit to the public have appeared to me to differ from the laws of electricity, in this respect, that the galvanic fluid seeks the most intimate affinities, and that electricity appears to endeavour to escape at the first touch.

My Experiments were as follows :

Experiments with a long conductor.

I raised a pile of seventy pair of plates, of copper and zinc, perfectly free from oxidation; I then applied, to the zinc end, a conductor six ells long, which I passed once round my arm, then twice round my body, above my cloaths, from whence I conducted it round my thigh and my leg, also once round each, and secured its lower extremity to my foot, in a bucket of warm water: I wetted my right hand, (the great conductor was round my arm, thigh and leg, on the left side) and having brought my fore-finger to the copper end, I experienced the most lively shock in this hand and arm, but felt nothing from the great conductor of the zinc end. Surprized not having had the slightest sensation from this pole, I repeated the experiment with the same effect; and at first believed that, in this case, the galvanic fluid was governed by the laws of electricity; that is to say, that it was disposed to pass off by the shortest way; but I quickly discovered another cause, when, on taking a copper wire, two feet long, in my right hand, and placing it in contact with the copper pole, I felt the shock in the foot immersed in the bucket of water. I was equally astonished at receiving the shock in my foot, notwith-

Galvanic shock perceived in the moistened hand;

in the foot, when the circuit was made with a second dry conductor,

* From the Journal de Physique, Ventose, An. XI.

withstanding

notwithstanding the wire was so repeatedly wound round my arm, my body, my thigh, and my leg, without yielding, in this long course, the slightest effect: I repeated this experiment with the same success; and soon discovered that the shock I received in my foot, to the exclusion of the other points of contact, was only occasioned by the affinity of the water for the galvanic fluid, and because it softened the epidermis: but that which convinced me of it, was, that on putting drops of water alternately, on the turns of the wire, and then bringing that connected with the copper pole in contact with it, I instantly felt the shock at the places where the drops of water were put, and experienced no sensation in my foot. I repeated this experiment, putting a drop of water upon the several turns of the wire, and connecting the copper pole with it: similar effects were produced without my perceiving the slightest sensation lower down; although I had neglected to wipe off the moisture occasioned by the different drops of water. I observe, in this operation, the rule which governs electricity, that is to say, that it is disposed to give the spark by the shortest way; but, on the other hand, I also find, that the galvanic fluid is inclined to seek an affinity which does not seem to agree with the electric fluid; because, we obtain a spark from the first contact, but here, the galvanic fluid passes through a considerable space of the most intimate points of contact, without losing any of its power, and produces no sensation until it has found an affinity favourable to its disengagement. I felt also the most distinct effects, on taking a wire of great length in one hand, with which I touched the positive pole, as in the former experiment I made a long circuit with it entirely round my room, and having brought back the end to my left hand, I put the right in contact, and felt a similar shock in my right hand, but if I took a wire of two feet in that hand, without being careful to wet it as well as the other, I felt nothing. The fluid passed through the long circuit of the wire to reach that point which offered the greatest affinity; thus I placed a conductor at either pole, and uniformly felt the shock from that pole which I touched with my naked and moistened hand.

and in various parts, in contact with the great conductor, by placing drops of water on it.

Conclusions.

Such were my observations on this series of experiments.

It will be the greatest satisfaction to me, if they should give rise to such clear opinions as may tend to the good of humanity and to the progress of the arts and sciences.

Galvanic Experiments, tending to ascertain the Existence of Two Fluids in the Animal Economy, the one positive and the other negative, which, by their Union, appear to produce the Agency of Vitality, by LA GRAVE, Member of the Galvanic Society.*

Galvanic pile formed of muscle and brain with cloth interposed.

BEING desirous of satisfying myself concerning the phenomena which are exhibited in the experiments made on frogs, by bringing part of each of their thighs in contact upon the cervical nerves, and persuaded that this fact could only proceed from the concurrence of two fluids, the one positive and the other negative, as we find it in the pile of Volta, I felt inclined to form a pile, composed of the nervous and muscular parts alternately. With this view, I procured a body, and prepared and uncovered, as much as possible, a certain number of the pectoral and intercostal muscles, which I cut into the form of discs; I then took the brain, not being able in this experiment to make use of the nervous cords, and cut, as thin as I could, the same number of discs as I had prepared from the muscular parts. Of these I raised a pile to the number of fifteen or sixteen couple, placing between the pairs, pieces of cloth moistened in salted water. It was in vain that I endeavoured to raise my pile with the pieces of cloth; the soft parts of the brain and their flexibility prevented it. I attempted it several times, because the experiment appeared to me to be curious, but I could not succeed. Being still desirous to overcome these difficulties, it occurred to me that pieces of leather or of hat might fulfil my wishes, as they possess a proper stiffness to support the soft part of the brain. In fact, I tried both, but those of hat soon obtained the preference over the pieces of leather, because they have the quality of being porous, of retaining the moisture better, and of possessing a degree of elasticity, which the leather has not. I then made some fresh preparations of muscle and brain, and raised a pile, making use of discs of hat for separating them; but after having raised my pile to twenty couple, I experienced new difficulties. The cerebral parts were crushed and began to escape from the weight of the pile. I need not observe that I took care to examine whether this small number of pairs was sufficient to produce the taste. I obtained still effect from the

Hat substituted for leth.

* From the Journal de Physique, Ventose, An. XI.

twentieth couple: it was necessary therefore to determine to abandon the experiments, or to devise some other mode. For this purpose, I thought of making a layer of small string the length of the pile, tied to the glafs of the insulater of the pile of Volta to sustain my rounds and my pairs of the fleshy and cerebral parts; this idea succeeded. I continued then to raise my pile upon these new supports: I was astonished and much more impatient to perceive nothing at the thirtieth couple. Obstinate bent on succeeding or being convinced that my opinion was erroneous I persisted and continued to heighten my pile; I did not perceive the least effect until I had added the fortieth pair. Encouraged by this success, I redoubled my attention and was thoroughly convinced of the existence of the taste at the fiftieth couple. Nevertheless I continued increasing the pile to the sixtieth, and the evidence of its effects was most decisive.

Other contrivances.

At the fiftieth pair the taste was perceived and at the sixtieth decisive.

Such were my disagreeable but curious experiments.

ACCOUNT OF NEW BOOKS.

Transactions of the American Philosophical Society held at Philadelphia for promoting Useful Knowledge. Vol. V. 1802, p. p. 328. Quarto.

American philosophical transactions.

THE contents of this volume are I. Experiments on the mission of acids, and other liquids, in the form of vapour, over several substances in a hot earthen tube—II. Experiments relating to the change of place in different kinds of air through several interposing substances—III. Experiments relating to the absorption of air by water—IV. Miscellaneous experiments relating to the doctrine of phlogiston—V. Experiments on the production of air by the freezing of water—VI. Experiments on air exposed to heat in metallic tubes—VII. Some account of the poisonous and injurious honey of North America—VIII. On the ephoron leukon, usually called the white fly of Passaick river—IX. Remarks on certain articles found in an Indian tumulus at Cincinnati, and now deposited in the museum of the American philosophical society—X. A drawing and description of the *clupea tyrannus* and *oniscus prægustator* (in the *Present Number*)—XI. A description

description of a newly invented globe time-piece—XII. A description of the pendant planetarium—XIII. On the use of the thermometer in navigation—XIV. Sur les vegetaux, les polypes et les insectes—XV. Memoir on the analysis of black vomit—XVI. Observations on the soda, magnesia and lime, contained in the water of the ocean; shewing that they operate advantageously there by neutralizing acids, and among others the septic acid, and that sea-water may be rendered fit for washing cloaths without the aid of soap—XVII. Description of a stopper for the openings by which the sewers of cities receive the water of their drains—XVIII. A memoir on animal cotton, or the insect fly-carrier (*see Phil. Journal for April*)—XIX. Note concerning a vegetable found underground—XX. Astronomical and thermometrical observations, made at the confluence of the Mississippi, and Ohio rivers—XXI. Astronomical and thermometrical observations made on the boundary between the United States and his Catholic Majesty—XXII. Observations on the figure of the earth—XXIII. Description of some improvements in the common fire-place, accompanied with models offered to the consideration of the American philosophical society.—Appendix, I. An account of a method of preventing the premature decay of peach trees. II. Description of a method of cultivating peach-trees, with a view to prevent their premature decay; confirmed by the experience of forty-five years, in Delaware state and the western parts of Pennsylvania.

Parkinson's
chemistry.

The Chemical Pocket Book; or Memoranda Chemica; arranged in a Compendium of Chemistry, By JAMES PARKINSON, HOLBORN. The third Edition, with appropriate Tables and Accounts of the latest Discoveries. 1803 p. 272. 12mo.

THE character of this work is already established. In the present edition the newest discoveries are inserted. The author has adopted the nomenclature of Chenevix.

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

JUNE, 1803.

ARTICLE I.

A Report of the State of His Majesty's Flock of Fine-wooled Spanish Sheep during the Years 1800 and 1801; with some Account of the Progress that has been made towards the Introduction of that valuable Breed into those Parts of the United Kingdom where fine Clothing Wools are grown with Advantage. By the Right Hon. Sir JOSEPH BANKS, Bart. P. R. S. &c. &c..*

ON the 9th of June, 1800, when his majesty's Spanish flock was shorn, it consisted of 100 ewes and wethers, which produced as follows: State of his majesty's Spanish flock of sheep, June 9, 1800.

Wool washed on the sheeps' back	-	-	398 lb.
Lofs in scowering	-	-	104
Amount of scowered wool	-	-	294

Which produced, when sorted, } 65l. 11s.

prime	234 lb.	at 5s. per lb.	}	65l. 11s.
choice	34,	at 3s.		
fribbs	26,	at 1s. 6d.		

* From a printed copy with which I was favoured by Sir Joseph. In our Journal, Vol. IV. p. 289 (Oct. 1800) the reader will find a copy of a project for extending the breed of fine-wooled Spanish sheep, now in the possession of his majesty, into all parts of Great Britain where the growth of fine clothing wool is found to be profitable, which was also drawn up and circulated by the same gentleman. The present report may be considered as a sequel to the interesting history, and proposal contained in the former paper. W. N.

Sheep disposed of to extend the breed, in 1800.

Eight rams and nine ewes were this year disposed of, which were all that could be spared from the flock. Two of the rams went into Dorsetshire, where the breed is much approved by some skilful judges of sheep, and seems likely to produce considerable advantage by crossing with the common sheep of the country.

Mr. Bridge's account of the improved value of sheep and their wool, by crossing with the Spanish.

Mr. Bridge, of Winford Eagle, communicated this year the result of an experiment he had made on three kinds of sheep, viz. Dorset, half Spanish and half Dorset, and half Spanish and half Mendip.

He kept these sheep from the year 1798, when they were lambed, till February 1800, when they were butchered as fat sheep; and having valued them in June 1798, he found the carcasses of each sort, with two years wool which had been shorn from them, to yield at that time the following increase in value:

Real Dorset	-	-	-	-	-	4l. 5s. 6d.
Half Spanish half Dorset	-	-	-	-	-	4 3 8
Half Spanish half Mendip	-	-	-	-	-	3 19 2

In these experiments Mr. Bridge's woolstapler values the Dorset wool at 1s. 2½d. a pound, and the half Spanish wool at 1s. 4d½. only; but as the Spanish crosses in both cases increased the quantity of wool, and as half Spanish wool has never, when its value was properly known, been sold for less than 1s. 9d. and generally more than 2s. a pound, there can be no doubt that the improvement in value, arising from the crosses, is in both cases considerable.

Mr. Ridgway's statement of similar results.

Mr. J. Ridgway, of Upperton, in the parish of Yazor in Herefordshire, communicated an experiment, in which two sheep, the one a Ryeland, and the other half Spanish and half Ryeland, of equal weights, were fed by him together; the half Spanish sheep produced in a year 2 lb. 12 ozs. more wool and 5 lb. more mutton than the Ryelander. This gentleman, whom his majesty graciously permitted to have rams from the Spanish flock some years ago, has also shewn by his accounts that the wool of his flock of about 16 score sheep, has been so much increased both in quantity and in value by the Spanish crosses, as to have produced nearly twice as much money for each clip after the Spanish blood was established in it, as it usually did before.

In June 1801, the Spanish flock consisted of 108 ewes and wethers, His majesty's Spanish flock, June 1801.

Which produced in wool, washed on the sheeps' back 397lb.

Loss in scowering - - - - - 112

Amount of scowered wool - - - - - 285

Which produced, when sorted, prime, 237lb. at 5s. 6d. per lb. }
 choice, 31, at 3s. 6d. } 72l. 1s. 9d.
 fribbs, 17, at 1s. 9d. }

The wool of the rams and fattening wethers which had been kept separate, was prepared for sale at the same time, and produced in

Wool washed on the sheeps' back - - - 220lb.

Loss in scowering - - - - - 82

Amount of scowered wool - - - - - 138

Which produced, when sorted, prime, 96lb. at 5s. per lb. }
 choice, 30, at 3s. 6d. } 30l. 6s.
 fribbs, 12, at 1s. 9d. }

This year, eight rams and twenty-two ewes were sold. If the foot rot had not unfortunately damaged the rams very materially, more of them would have been disposed of. It is, however, observable, that although the rams that are kept at Windsor, in rich land are occasionally attacked by this harassing disease, the ewes and wethers that feed on the dry and hilly pastures of Oatlands have never been subject to lameness of any kind. If Sheep disposed of to extend the breed.

Eleven wethers that had been sent to the marshes, in order to try the effect of rich pasture in fattening sheep of this breed, were slaughtered this year by Mr. King of Newgate Market, previous to the Smithfield meeting, which usually takes place the week before Christmas. Two of the carcases were given to persons who had been useful in ascertaining the value of the Spanish breed; the remaining nine were sold to Mr. Giblet, butcher, in Bond Street, whose judgment in selecting, and liberality in purchasing the best carcases, is well known, both to those of whom he buys, and to those who buy of him. The sale bill is as follows: Account of the fattening and sale of eleven wethers.

				£.	s.	d.
1 sheep, 6 stone 6lb.	at 6s. per stone	-	-	2	0	6
1 ditto, 7 0	6s. - - -	-	-	2	2	0
1 ditto, 6 1	6s. - - -	-	-	1	16	9
1 ditto, 7 2	6s. - - -	-	-	2	3	6
1 ditto, 5 6	6s. - - -	-	-	1	14	6
				9	17	3

	£.	s.	d.
Brought over,	9	17	3
1 sheep, 5 stone 2 lb. at 6s. per stone	-	1	11 6
1 ditto, 5 7 6s. - - -	-	1	15 3
1 ditto, 5 4 6s. - - -	-	1	13 0
1 ditto, 6 2 6s. - - -	-	1	17 6
11 heads and plucks, at 1s. - - -	0	11	0
10 stone 4 lb. fat, at 3s. 10d. - - -	2	3	0
	£.	19	8 6

Respecting the goodness of the mutton, enquiry must be made of Mr. Giblet, at whose shop the carcases were shewn for several days, and of his customers who purchased the joints. Experience has, however, demonstrated already, both at Windsor and at Weybridge, that Spanish mutton is of the best quality for a gentleman's table.

Sale of their pelt wool. The pelt wool of these 11 sheep was taken off, in order that its value might be ascertained.

It weighed in the yoke	-	-	-	36 lb.
Lofs in scowering	-	-	-	8
Amount of scowered wool	-	-	-	28

It was sold as skin wool for 4s. 6d. a pound, and of course produced 5l. 19s. or 10s. a sheep, all expences deducted. The amount of this profit was quite unexpected, and holds forth a source of advantage in this breed, that has not probably hitherto been calculated upon.

The commendable exertions of Dr. Parry of Bath, in advancing his majesty's patriotic views as to this object. Of all who have laboured to render his majesty's patriotic views in importing Spanish sheep permanently useful to his subjects, Dr. Parry of Bath deserves the highest commendation. Amidst the labours of a profession always toilsome when successful, and particularly so at Bath, where persons, whose diseases cannot be ascertained by the faculty elsewhere, continually resort, the doctor found leisure to employ himself in the improvement of the British fleece, by crossing various breeds with Spanish rams presented by his majesty to the Marquis of Bath, and to the Bath Agricultural Society.

The prizes the doctor has continually obtained from the judicious and respectable body from whom he borrowed rams, for cloths made of his own wool, in the midst of a manufacturing country and amongst abundance of able competitors, prove to a demonstration, that he has brought the fleeces of the mixed breed very nearly to the value of the original Spanish;

nish;

nish; nor is this to be wondered at, when we recollect that the effect of a mixture of breeds operates in the following proportions.

The first cross of a new breed gives to the lamb half of the ram's blood, or 50 per cent.		Rate of amelioration of wool by the Spanish cross.
The second gives - - - - -	75 ditto.	
The third - - - - -	87½ ditto.	
The fourth - - - - -	93¾ ditto.	

At which period it is said, that if the ewes have been judiciously selected, the difference of wool between the original stock and the mixed breed is scarcely to be discerned by the most able practitioners.

More need not be said of the doctor's merit. His book, which every man who wishes to improve wool ought to read, will give a more just idea of the acuteness of his discrimination, the diligence with which he pursued his purpose, and the success that finally attended his judicious management, than can be stated in the brief form of a report like this.

Much, however, as Dr. Parry deserves the gratitude of all who honour the fleece, Lord Somerville's merit stands at least as eminently conspicuous. Emulating the example of his sovereign, his lordship, whose just discrimination of the value of different breeds of stock is admitted by the most experienced agriculturists, made a voyage to Portugal, for the sole purpose of selecting by his own judgment, from the best flocks in Spain, such sheep as joined in the greatest degree the merit of a good carcase, to the superiority in wool which the Merino flocks are allowed to possess.

Lord Somerville's active and judicious labours in establishing a Spanish flock.

His lordship succeeded, and brought home, more than two years ago, a flock of the first quality, which will probably repay with advantage the costs of the undertaking, as some of his lordship's rams are said to have been already sold for 100 guineas each.

As ten crops of wool have now been shorn from his majesty's Spanish flock, and not a single sheep from Spain has been introduced into it during the whole of the ten years that have produced them, and as the tenth crop afforded nearly five-sixths of prime wool and only one-fourteenth of fribbs, it is to be hoped that the deep-rooted prejudice which has for ages deceived the people of England into an opinion that Spanish wool degenerates in this climate, will now be finally lodged in that

His majesty's flock has proved by ten years experience, that Spanish wool does not degenerate in this climate, and that the mutton is excellent.

catalogue

catalogue of vulgar errors, which the increase of human knowledge daily enlarges. It is to be hoped also, that a bold assertion hazarded here, that the mutton of Spanish fine-wooled sheep is coarse, tough, and little better than carrion, will be contradicted by the evidence of Mr. Giblet and his customers, to the satisfaction of those who have unwarily given credit to it.

His majesty having been pleased to permit the sale of such sheep as can be spared from the Spanish flock to be continued, the rams will be delivered at Windsor, and the ewes at Oatlands, in the latter end of August. As, however, it has been suggested to his majesty that the carcases of the sheep are evidently improved, and that the wool has rather gained than lost in value, six guineas will be in future the price of a ram, and two that of an ewe. And as his majesty has been graciously pleased to continue to entrust the management of the flock to Sir Joseph Banks, all letters on the subject of it, addressed to him in Soho Square, will be answered, and the utmost endeavours used to consult the convenience of those who wish to become purchasers.

July, 1802.

JOS. BANKS.

II.

Description of a New Process of Refining. By CIT. DARCEY,
Nephew*.

Causes of perfection in the arts.

THOUGH the practice of an art is alone sufficient to bring it to perfection, it must also be admitted, that the greater number of uses to which its products are applied, as well as the greater number of manufactories wherein its processes are carried on, are the principal causes of its arriving quickly at that desirable point; in fact, if the manufactured articles be of general utility, if the processes be generally followed, a competition is produced, and every manufacturer endeavours to be victorious in the contest in which he is engaged: it is then that individual interest simplifies the operations of art and brings them to perfection, it succeeds in affording at a lower price products of a similar kind, or in many cases of a sort preferable

* From the Journal de Physique, Vendemaire, An. XI.

to those obtained by the original processes, and it is thus that we daily see a multitude of the arts arrive at a great degree of perfection. I might produce as examples, the art of the pin-makers, that of the needle-maker, &c. &c. but I mean at present to confine myself to a new process for refining *, the details of which will sufficiently illustrate what I advance.

Instances.

It is known in commerce, that articles of gold and silver, after long use upon a change of fashion, or from other circumstances, are melted together without attending to any rule or proportion but that which arises from the variable course of trade.

Standard of melted plate uncertain.

The ingots obtained in these operations must, therefore, at first, contain different proportions of gold and silver, but they must at last be united with those metals, which in conformity to the law will require to be added, in order to their employment in the arts of the jeweller and the silversmith.

To bring these metallic compounds into circulation again, that is to say, to qualify them to be reconverted into works of gold or silver, they must undergo an operation which reduces them to the standard prescribed by law, there are two methods of accomplishing this end.

Method of bringing metals to a legal standard.

The first and most natural is that which points out the addition of the quantity of gold, silver, or copper, necessary to restore the fixed proportions of the alloy.

In the second all the metals are separated from each other, and after reducing them to the state of pure or simple bodies, the alloy is formed according to the legal proportions, but the first of these processes constitutes part of the second, inasmuch as it employs the same materials as are purified by means of this last, which is used to separate an alloy of gold, silver, &c.

It is to this operation that the name of refining is given.

All the operations of refining are founded upon the peculiar properties of those bodies on which the refiner is to work. The chief process is that which bears the name of *parting*; its basis is the insolubility of gold in the nitric acid; this acid, by dissolving the silver and copper, leaves the gold, which was alloyed with these metals, at the bottom of the vessel in which the solution is carried on.

Refining properly so called. Parting.

* I here speak of refining as an art, without reference to its connection with the financial operations of the state.

Gold not completely purified by the usual processes of parting.

This is seen by a single experiment; but practice soon shows that for this solution to take place, it must be made upon an alloy of one part gold with four parts silver, and that it will be more perfect the greater quantity of concentrated acid is employed, or the higher the degree of heat which is applied to the vessels. These observations improve the art, but do not bring it to that point which will enable us to obtain the gold in its greatest purity, for this metal very rarely comes from the crucible more pure than 998 or 999 in a thousand.

Antient processes.

In this state it was that I found the art at my entrance into the national refinery, where being placed as it were in competition with the refiners of commerce, I sought the means of improving the processes already known, and I now offer to the arts, that which has constantly succeeded with me during two years operations carried on in the laboratory of Cit. Dizé.

I shall begin with describing the old processes of refining, and afterwards explain the method which I have substituted in their stead.

Quartation.

The ingots received in commerce by the refiners are more or less rich, and more or less mixed with the fine metals, but their purity is generally between 850 and 950 thousandth parts. They mix these ingots and unite them by fusion, so that the alloy intended to be formed shall contain four parts of silver to one of gold; they pour these alloys out in grains, and add to every five kilogrammes 500 grammes, to the amount of 650 grammes of nitrate of potash of the second boiling*.

Fusion with nitre.

Granulation.

The mixture is then melted in crucibles, where it is suffered to cool in a mass, and these masses are again fused and granulated, to multiply the surfaces of the alloy to be submitted to the action of the nitric acid which is employed in the parting.

Parting by nitric acid.

To perform this operation, the refiner distributes the grains in pots of stone-ware, and pours two parts of nitric acid of 30 degrees over one part of the alloy. He places these pots in a sand-bath, in order to assist the action of nitric acid upon the silver, by means of the heat. When the solution is ef-

* The intention of this operation is to oxidate the copper and the other metals which are mixed with the gold and silver; and in fact, by this fusion, which is called (*pouffée*) the purity of the mass is brought to about .978: hence the quantity of acid necessary for its solution is much diminished.

fectcd, he decants the nitrate of silver, and washes the gold until the water from the washings will no longer decompose muriate of soda.

The gold is not yet freed from all alloy, he therefore adds to it some nitric acid of the same strength, and brings the mixture to ebullition, he then decants the nitrate of silver, washes the gold, and adding to it nitric acid of 40°, he replaces the vessel on the sand-bath. When it has been sufficiently boiled, he separates the gold from the acid, and washes it with great care, dries it by a gentle heat, melts it and forms it into ingots, the purity of which, as I have said above, does not generally exceed .998, and is even often below .995, which is the limit fixed by law. In the latter case, the refiner is obliged to proceed to a new quartation and a new parting; his expences are then doubled and may be even trebled, if in the second operation, he does not surmount the inconveniencies which caused the first to fail.

This is the department of refining which I have brought nearest to perfection, as will be seen in the description of my process. Improved process.

I mix, as in the old operation, the ingots of commerce, in such a manner, that when they are melted together, the gold in the alloy shall bear proportion of one to four parts of silver. Quartation and fusion with only one hundredth part of nitre.

I melt the ingots thus prepared, and when the matter is very liquid, I project thereon about 200 grammes of nitrate of potash for every 20 kilogrammes of the alloy. This small quantity of saltpetre is sufficient, as I have remarked, to oxidate the tin, which is always combined in greater or less quantity with the ingots of commerce. It is essential to separate this tin, because in the operation of parting it becomes oxidated and mixed with the gold, and at the time of that metal being melted, it may perhaps be partly reduced and render it harsh and of less purity.

When the whole is in perfect fusion, I immediately granulate and distribute it, as in the former method, into pots. Granulation.

There I add to it the same quantity of nitrid acid of the same strength, following exactly the same manipulations as those I have described above: in one word I observe the aptient process to the moment, when the gold, after having been well washed, is put into the crucible: I collect the gold in this state Parting as before.

into as small a vessel as possible, and add to it as much sulphuric The parted gold is boiled with sulphuric acid;
ric

ric of 66° as will cover its surface; I increase the temperature of this acid by placing my vessel on a sand-bath, I raise it to ebullition, and keep it in that state for about an hour; I then *and washed.* suffer the whole to cool *, I decant the acid and wash the gold until the water from the washings will not yield any more precipitate with muriatic acid or alkalies. *It is then pure.* Nothing then remains but to dry this gold, which is in powder, and to reduce it into ingots, which will constantly be of the purity of 1.000, or 24 carats.

These are all the details necessary to direct the performance of the operation which I propose: the following are the advantages which I believe is actually possesses.

*Advantages of
the new process.*

I make no *pouffé*, consequently I save both charcoal and crucibles; I also am enabled to perform the operation without pounding the metal so often, and have fewer washings and meltings: but the greatest advantage of this improvement is the considerable diminution of the fire left in the scorixæ; for in the former method the great quantity of nitrate of potash that was added, promoted the oxidation †, or the extreme division of a considerable part of the silver, which then remained in the scorixæ with the oxide of copper, the potash, &c. and this waste amounted, in each operation, to 2400 grammes on silver of the purity of .350 to .400.; and this silver remained, without yielding any profit, frequently for several years; that is to say, until the time when the refiner had a sufficient quantity of washings to make it worth refining.

I have already asserted that I constantly obtain the gold in its greatest purity, or at 1.000. which is known to be a great

* I must also observe that it is essential to suffer the sulphuric acid boiled with the gold, to be completely cold before it is decanted, without this precaution, as the stone-ware vessels used in France are not capable of withstanding the great degree of heat acquired by the acid in boiling, there is much hazard of being wounded by the fracture of the vessels, and the drops of acid which are scattered in all directions.

For greater safety, the bottom of the vessel may be covered with sulphuric acid of 66 degrees, and at the temperature of the atmosphere.

† I dare not yet affirm that the silver is in the state of the scorixæ of the *pouffée*; I wait the result of several experiments which I have undertaken on this subject in order to fix my opinion.

advantage

advantage in commerce, but it is of still more benefit to the refiner, by his operation being certain; and he no longer runs any risque, if the calculations of his alloy have been accurately made, of being ruined by unsuccessful operations, and by the necessity of repeating two or three times a process of which nothing can insure the success.

III.

Remarks on the Construction of the Heavens. By WILLIAM HERSCHFL, LL. D. F. R. S. From the Philos. Transactions, 1802. p. 477.*

IT has hitherto been the chief employment of the physical astronomer, to search for new celestial objects, whatsoever might be their nature or condition; but our stock of materials is now so increased, that we should begin to arrange them more scientifically. The classification adopted in my catalogues, is little more than an arrangement of the objects for the convenience of the observer, and may be compared to the disposition of the books in a library, where the different sizes of the volumes is often more considered than their contents. But here, in dividing the different parts of which the sidereal heavens are composed into proper classes, I shall have to examine the nature of the various celestial objects that have been hitherto examined, in order to arrange them in a manner most conformable to their construction. This will bring on some extensive considerations, which would be too long for the compass of a single paper; I shall therefore now only give an enumeration of the species that offer themselves already to our view, and leave a particular examination of the separate divisions, for some early future occasions.

Celestial objects have not hitherto been classed according to their respective nature.

Introductory observations.

In proceeding from the most simple to the more complex arrangements, several methods, taken from the known laws of gravitation, will be suggested, by which the various systems under consideration may be maintained; but here also we shall confine ourselves to a general review of the subject, as obser-

* To this Paper is annexed "a catalogue of 500 new nebulæ, nebulous stars, planetary nebulæ, and clusters of stars," for which the reader is referred to the Transactions.

vation

vation must furnish us first with the necessary data, to establish the application of any one of these methods on a proper foundation.

ENUMERATION OF THE PARTS THAT ENTER INTO THE
CONSTRUCTION OF THE HEAVENS.

I. *Of insulated Stars.*

1. Insulated stars;

of which the sun is one.

They are too far afunder to be sensibly affected by gravitation.

Illustration: Sirius and the sun would require 33 millions of years to fall together;

even if there were no contrary attractions.

In beginning our proposed enumeration, it might be expected that the solar system would stand foremost in the list; whereas, by treating of insulated stars, we seem, as it were, to overlook one of the great component parts of the universe. It will, however, soon appear that this very system, magnificent as it is, can only rank as a single individual belonging to the species which we are going to consider.

By calling a star insulated, I do not mean to denote its being totally unconnected with all other stars or systems; for no one, by the laws of gravitation, can be intirely free from the influence of other celestial bodies. But, when stars are situated at such immense distances from each other as our sun, Arcturus, Capella, Lyra, Sirius, Canopus, Markab, Bellatrix, Menkar, Shedir, Algorah, Propus, and numberless others probably are, we may then look upon them as sufficiently out of the reach of mutual attractions, to deserve the name of insulated stars.

In order not to take this assertion for granted, without some examination, let us admit, as is highly probable, that the whole orbit of the earth's annual motion does not subtend more than an angle of one second of a degree, when seen from Sirius. In consequence of this, it appears by computation, that our sun and Sirius, if we suppose their masses to be equal, would not fall together in less than 33 millions of years, even though they were not impeded by many contrary attractions of other neighbouring insulated stars; and that, consequently, with the assistance of the opposite energies exerted by such surrounding stars, these two bodies may remain for millions of ages, in a state almost equal to undisturbed rest. A star thus situated may certainly deserve to be called insulated, since it does not immediately enter into connection with any neighbouring star; and it is therefore highly probable, that our sun is one of a great number that are in similar circumstances. To this may be added, that the stars we consider as insulated are also surrounded by a magnificent collection of innumerable stars, called

the

the milky-way, which must occasion a very powerful balance of opposite attractions, to hold the intermediate stars in a state of rest. For, though our sun, and all the stars we see, may truly be said to be in the plane of the milky-way, yet I am now convinced, by a long inspection and continued examination of it, that the milky-way itself consists of stars very differently scattered from those which are immediately about us. But of this, more will be said on another occasion.

From the detached situation of insulated stars, it appears that Insulated stars may be the centres of planetary systems. they are capable of being the centres of extensive planetary systems. Of this we have a convincing proof in our sun, which, according to our classification, is one of these stars. Now, as we enjoy the advantage of being able to view the solar system in all its parts, by means of our telescopes, and are therefore sufficiently acquainted with it, there will be no occasion to enter into a detail of its construction.

The question will now arise, whether every insulated star be Probably they all are such : a sun like ours, attended with planets, satellites, and numerous comets? And here, as nothing appears against the supposition, we may from analogy admit the probability of it. But, were we to extend this argument to other sidereal constructions, or, still farther, to every star of the heavens, as has been done frequently, I should not only hesitate, but even think that, from what will be said of stars which enter into complicated sidereal systems, the contrary is far more likely to be the case; and but the complicated stars are probably not. that, probably, we can only look for solar systems among insulated stars.

II. Of Binary sidereal Systems, or double Stars.

The next part in the construction of the heavens, that offers 2. Double stars, itself to our consideration, is the union of two stars, that are formed together into one system, by the laws of attraction.

If a certain star should be situated at any, perhaps immense, distance behind another, and but very little deviating from the line in which we see the first, we should then have the appearance of a double star. But these stars, being totally unconnected, would not form a binary system. If, on the contrary, two stars should really be situated very near each other, and at the same time so far insulated as not to be materially affected or stars revolving round a common center. by the attractions of neighbouring stars, they will then compose a separate system, and remain united by the bond of their own mutual

mutual gravitation towards each other. This should be called a real double star; and any two stars that are thus mutually connected, form the binary sidereal system which we are now to consider.

It is easy to prove, from the doctrine of gravitation, that two stars may be so connected together as to perform circles, or similar ellipses, round their common centre of gravity. In this case, they will always move in directions opposite and parallel to each other; and their system, if not destroyed by some foreign cause, will remain permanent.

Four cases of orbits.

Figure 1 (Plate V.) represents two equal stars a and b , moving in one common circular orbit round the centre o , but in the opposite directions of at and bt . In Fig. 2. we have a similar connection of the two stars ab ; but, as they are of different magnitudes, or contain unequal quantities of matter, they will move in circular orbits of different dimensions round their common center of gravity o . Fig. 3. represents equal, and Fig. 4. unequal stars, moving in similar elliptical orbits round a common centre; and, in all these cases, the directions of the tangents tt , in the places ab , where the stars are, will be opposite and parallel, as will be more fully explained hereafter.

In these cases the center of gravity is considerably remote from the larger star.

These four orbits, simple as they are, open an extensive field for reflection, and, I may add, for calculation. They shew, even before we come to more complicated combinations, where the same will be confirmed, that there is an essential difference between the construction of solar and sidereal systems. In each solar system, we have a very ponderous attractive centre, by which all the planets, satellites, and comets are governed, and kept in their orbits. Sidereal systems take a greater scope: the stars of which they are composed move round an empty centre, to which they are nevertheless as firmly bound as the planets to their massy one. It is however not necessary here to enlarge on distinctions which will hereafter be strongly supported by facts, when clusters of stars come to be considered. I shall only add, that in the subordinate bodies of the solar system itself, we have already instances, in miniature, as it may be called, of the principle whereby the laws of attraction are applicable to the solution of the most complicated phenomena of the heavens, by means of revolutions round empty centres. For, although both the earth and its moon are retained in their

orbits by the sun, yet their mutual subordinate system is such, that they perform secondary monthly revolutions round a centre without a body placed in it. The same indeed, though under very narrow limits, may be said of the sun and each planet itself.

That no insulated stars, of nearly an equal size and distance, can appear double to us, may be proved thus. Let Arcturus and Lyra be the stars: these, by the rule of insulation, which we must now suppose can only take place when their distance from each other is not less than that of Sirius from us, if very accurately placed, would be seen under an angle of 60 degrees from each other, They really are at about 59°. Now, in order to make these stars appear to us near enough to come under the denomination of a double star of the first class, we should remove the earth from them at least 41253 times farther than Sirius is from us. But the space-penetrating power of a 7-foot reflector, by which my observations on double stars have been made, cannot intitle us to see stars at such an immense distance; for, even the 40-foot telescope, as has been shewn *, can only reach stars of the 1342d magnitude. It follows, therefore, that these stars could not remain visible in a 7-foot reflector, if they were so far removed as to make their angular distance less than about 24¼ minutes; nor could even the 40-foot telescope, under the same circumstances of removal, shew them, unless they were to be seen at least 2½ minutes asunder. Moreover, this calculation is made on a supposition that the stars of which a double star is composed, might be as small as any that can possibly be perceived; but if, on the contrary, they should still appear of a considerable size, it will then be so much the more evident that such stars cannot have any great real distance, and that, consequently, insulated stars cannot appear double, if they are situated at equal distances from us. If, however, their arrangement should be such as has been mentioned before, then, one of them being far behind the other, an apparent double star may certainly be produced; but here the appearance of proximity would be deceptive; and the object so circumstanced could not be classed in the list of binary systems. However, as we must grant, that in particular situations stars apparently double may be composed of such as are

If insulated stars be assumed to be as far asunder as Arcturus and Lyra, they cannot by their remoteness from the earth appear double;

because they would become invisible before they were remote enough.

It is highly improbable that the double stars should consist of stars considerably asunder.

* See Phil. Transf. for 1800, Part I. page 83.

insulated,

insulated, it cannot be improper to consult calculation, in order to see whether it be likely that the 700 double stars I have given in two catalogues, as well as many more I have since collected, should be of that kind. Such an inquiry, though not very material to our present purpose, will hereafter be of use to us, when we come to consider more complicated systems. For, if it can be shown that the odds are very much against the casual production of double stars, the same argument will be still more forcible, when applied to treble, quadruple, or multiple compositions.

Computation founded on the improbability that the remotest star should be as much larger than the mean size as is requisite ;

Let us take α Aquarii, for an instance of computation. This star is admitted, by Flamsteed, De la Caille, Bradley, and Mayer, to be of the 4th magnitude. The two stars that compose it being equal in brightness, each of them may be supposed to shine with half the light of the whole lustre. This, according to our way of reckoning magnitudes *, would make them $4m \times \sqrt{2} = 5\frac{2}{3}m$; that is, of between the 6th and 5th magnitude each. Now, the light we receive from a star being as the square of its diameter directly, and as the square of its distance inversely, if one of the stars of α Aquarii be farther off than the stars of between the 6th and 5th magnitude are from us, it must be so much larger in diameter, in order to give us an equal quantity of light. Let it be at the distance of the stars of the 7th magnitude; then its diameter will be to the diameter of the star which is nearest to us as 7 to $5\frac{2}{3}$, and its bulk as 1,885 to 1; which is almost double that of the nearest star. Then, putting the number of stars we call of between the 6th and 5th magnitude at 450, we shall have 686 of the 7th magnitude to combine with them, so that they may make up a double star of the first class, that is to say, that the two stars may not be more than 5" asunder. The surface of the globe contains 34036131547 circular spaces, each of 5" in diameter; so that each of the 686 stars will have 49615357 of these circles in which it might be placed; but, of all that number, a single one would only be the proper situation in which it could make up a double star with one of the 450 given stars. But these odds, which are above $75\frac{1}{2}$ millions to one against the composition of α Aquarii, are extremely increased by our

and the much greater improbability that the position of the two stars should agree with the phenomena.

The odds are many millions to one.

* The expressions 2m, 3m, 4m, &c. stand for stars at the distance of 2, 3, 4, &c. times that of Sirius, supposed unity.

foregoing

foregoing calculation of the required size of the star, which must contain nearly double the mass allotted to other stars of the 7th magnitude; of which, therefore, none but this one can be proper for making up the required double star. If the stars of the 8th and 9th magnitudes, of which there will be 896 and 1134, should be taken in, by way of increasing the chance in favour of the supposed composition of our double star; the advantage intended to be obtained by the addition of numbers, will be completely counteracted by the requisite uncommon bulk of the star which is to serve the purpose; for, one of the 8th magnitude, ought to be more than $2\frac{1}{4}$ times bigger than the rest: and, if the composition were made by a star of the 9th magnitude, no less than four times the bulk of the other star which is to enter the composition of the double star would answer the purpose of its required brightness. Hence therefore it is evident, that casual situations will not account for the multiplied phenomena of double stars, and that consequently their existence must be owing to the influence of some general law of nature; now, as the mutual gravitation of bodies towards each other is quite sufficient to account for the union of two stars, we are authorised to ascribe such combinations to that principle.

It will not be necessary to insist any further on arguments drawn from calculation, as I shall soon communicate a series of observations made on double stars; whereby it will be seen, that *many of them have actually changed their situation with regard to each other, in a progressive course, denoting a periodical revolution round each other; and that the motion of some of them is direct, while that of others is retrograde.* Should these observations be found sufficiently conclusive, we may already have their periodical times near enough to calculate, within a certain degree of approximation, the parallax and mutual distance of the stars which compose these systems, by measuring their orbits, which subtend a visible angle.

Before we leave the subject of binary systems, I should remark, that it evidently appears, that our sun does not enter into a combination with any other star, so as to form one of these systems with it. This could not take place without our immediately perceiving it; and, though we may have good reason to believe that our system is not perfectly at rest, yet the causes of its proper motion are more probably to be ascribed to

Actual observation indicates a revolution of double stars round their center.

The sun is not thus combined; though our system is not absolutely at rest.

some perturbations arising from the proper motion of neighbouring stars or systems, than to be placed to the account of a periodical revolution round some imaginary distant centre.

III. *Of more complicated sidereal Systems, or treble, quadruple, quintuple, and multiple Stars.*

3. Complicated sidereal systems: or treble, quadruple, &c. stars.

Those who have admitted our arguments for the existence of real double stars, will easily advance a step farther, and allow that three stars may be connected in one mutual system of reciprocal attraction. And, as we have from theory pointed out, in figures 1, 2, 3, and 4, how two stars may be maintained in a binary system, we shall here shew that three stars may likewise be preserved in a permanent connection, by revolving in proper orbits about a common centre of motion.

Observations and inferences respecting the nature of the possible revolutions governed by an attractive force, directed to a center.

In all cases where stars are supposed to move round an empty centre, in equal periodical times, it may be proved that an imaginary attractive force may be supposed to be lodged in that centre, which increases in a direct ratio of the distances. For since, in different circles, by the law of centripetal forces, the squares of the periodical times are as the radii divided by the central attractive forces, it follows, that when these periodical times are equal, the forces will be as the radii. Hence we conclude, that in any system of bodies, where the attractive forces of all the rest upon any one of them, when reduced to a direction as coming from the empty centre, can be shewn to be in a direct ratio of the distance of that body from the centre, the system may revolve together without perturbation, and remain permanently connected without a central body.

Hence may be proved, as has been mentioned before, that two stars will move round a hypothetical centre of attraction. For let it be supposed that the empty centre o , in Fig. 1 and 3, is possessed of an attractive force, increasing in the direct ratio of the distances $oa : ob$. Then, since here ao and bo are equal, the hypothetical attractions will be equal, and the bodies will revolve in equal times. That this agrees with the general law of attraction, is proved thus. The real attraction of b upon a is $\frac{b}{ab^2}$; and that of a upon b is $\frac{a}{ab^2}$; and, since

$b = a$, it will be $\frac{b}{ab^2} : \frac{a}{ab^2} :: ao : bo$; which was required.

In Figures 2 and 4, when the stars a and b are unequal, and their distances from o also unequal, let $oa = n$, and $ob = m$; and let the mass of matter in $a = m$, and in $b = n$. Then the attraction of b on $a = \frac{b}{ab^2}$, will be to the attraction of a on $b = \frac{a}{ab^2}$, as $n : m$; which is again directly as $ao : bo$.

Observations and inferences respecting the nature of the possible revolutions governed by an attractive force, directed to a center.

I proceed now to explain a combination of three bodies, moving round a centre of hypothetical attraction. Fig. 5 contains a single orbit, wherein three equal bodies abc , placed at equal distances, may revolve permanently. For, the real attraction of b on a will be expressed by $\frac{a}{ab^2}$; but this, reduced

Figure of orbits, &c.

to the direction ao , will be only $\frac{b \cdot by}{ab^3}$; for, the attraction in the direction ba is to that in the direction by , parallel to ao , as $\frac{b}{ab^2}$ to $\frac{b \cdot by}{ab^3}$. The attraction also of c on a is equal to that of b on a ; therefore the whole attraction on a , in a direction towards o , will be expressed by $\frac{2b \cdot by}{ab^3}$. In the same manner

we prove, that the attraction of a and c on b , in the direction bo , is $\frac{2a \cdot by}{ab^3}$; and that of a and b on c , in the direction co , is $\frac{2c \cdot by}{ab^3}$. Hence, ab and c being equal, the attractions in

the directions ao bo and co will also be equal; and, consequently, in the direct ratio of these distances. Or rather, the hypothetical attractions being equal, it proves that, in order to revolve permanently, ab and c must be equal to each other.

Instead of moving in one circular orbit, the three stars may revolve in three equal ellipses, round their common centre of gravity, as in Fig. 6. And here we should remark, that this centre of gravity will be situated in the common focus o , of the three ellipses; and that the absolute attraction towards that focus, will vary in the inverse ratio of the squares of the distances of any one of the stars from that centre, while the relative attractions remain in the direct ratio of their several distances from the same centre. This will be more fully explained, when we come to consider the motion of four stars.

A straight-lined orbit or oscillatory motion.

A very singular straight-lined orbit, if so it may be called, may also exist in the following manner. If a and b , Fig. 7, are two large equal stars, which are connected together by their mutual gravitation towards each other, and have such projectile motions as would cause them to move in a circular orbit about their common center of gravity, then may a third small star c , situated in a line drawn through o , and at rectangles to the plane described by the stars ab , fall freely from rest, with a gradually acquired motion to o ; then, passing through the plane of the orbit of the two stars, it will proceed, but with a gradually retarded motion, to a second point of rest d ; and, in this manner, the star c may continue to oscillate between c and d , in a straight line, passing from c , through the centre o , to d , and back again to c .

In order to see the possibility and permanency of this connection the better, let o be the centre of gravity of the three bodies, when the oscillating body is at c ; then, supposing the bodies a and b to be at that moment in the plane pl , and admitting m to represent a body equal in mass to the two bodies a and b , o will be the common centre of gravity of m and c . Then, by the force of attraction, the body c and the fictitious body m will meet in o ; that is to say, the plane pl , of the bodies a and b , will now be at $p'l'$. The fictitious body m may then be conceived to move on till it comes to n , while the body c goes to d ; or, which is the same, the plane of the bodies a and b will now be in the position $p''l''$, as much beyond the centre of gravity o , as it was on the opposite side m . By this time, both the fictitious body m , now at n , and the real body c , now at d , have lost their motion in opposite directions, and begin to approach to their common centre of gravity o , in which they will meet a second time. It is evident that the orbit of the two large stars will suffer considerable perturbations, not only in its plane, but also in its curvature, which will not remain strictly circular; the construction of the system, however, is such as to contain a sufficient compensation for every disturbing force, and will consequently be in its nature permanent.

In order to add an oscillating star, it is not necessary that the two large stars should be so situated as to move in a circular orbit, without the oscillating star. In Fig. 8, the stars a and b may have such projectile forces given them as would cause them to describe equal ellipses, of any degree of excentricity.

If

If now the small star c be added, the perturbations will undoubtedly affect not only the plane of the orbits of the stars, but also their figures, which will become irregular moveable ovals. The extent also of the oscillations of the star c will be affected; and will sometimes exceed the limits cd , and sometimes fall short of them. All these varieties may easily be deduced from what has been already said, when Fig. 7 was considered. It is however very evident, that this system also must be permanent; since not only the centre of gravity o will always be at rest, but ao , whatever may be the perturbations arising from the situation of c , will still remain equal to bo .

Observations and inferences respecting the nature of the possible revolutions governed by an attractive force, directed to a center.

It should be remarked, that the vibratory motion of the star c will differ much from a cometary orbit, even though the latter should be compressed into an evanescent ellipsis. For, while the former extends itself over the diameter of a globe in which it may be supposed to be inscribed, the hypothetical attractive force being supposed to be placed in its centre, the cometary orbit will only describe a radius of the same globe, on account of its requiring a solid attractive centre.

After what has been said, it will hardly be necessary to add, that with the assistance of any proper one of the combinations pointed out in the four last figures, the appearance of every treble star may be completely explained; especially when the different inclinations of the orbits of the stars, to the line of sight, are taken into consideration.

If we admit of treble stars, we can have no reason to oppose more complicated connections; and, in order to form an idea how the laws of gravitation may easily support such systems, I have joined some additional delineations. A very short explanation of them will be sufficient.

Fig. 9 (Plate VI.) represents four stars, abc and d , arranged in a line; a being equal to b , and c equal to d . Then, if $ao = bo$, and $co = do$, the centre of gravity will be in o ; and, with a proper adjustment of projectile forces, the four stars will revolve in two circular orbits round their common centre. By calculating in the manner already pointed out, it will be found, that when, for instance, $ao = 1$, $co = 3$, and $c = d = 1$, then the mass of matter in $a = b$, will be required to be equal to 1,3492.

It is not necessary that the projectile force of the four stars should be such as will occasion them to revolve in circles. The system

Observations and inferences respecting the nature of the possible revolutions governed by an attractive force, directed to a center.

system will be equally permanent when they describe similar ellipses about the common center of gravity, which will also be the common focus of the four ellipses. In Fig. 10. the stars $a b c d$, revolving in ellipses that are similar, will always describe, at the same time, equal angles in each ellipse about the centre of hypothetical attraction; and, when they are removed from $a b c d$ to $a' b' c' d'$, they will still be situated in a straight line, and at the same proportionate distances from each other as before. By this it appears, as we have already observed, that the absolute hypothetical force in the situation $a' b' c' d'$, compared to what it was when the stars were at $a b c d$, is inversely as the squares of the distances; but that its comparative exertion on the stars, in their present situation, is still in a direct ratio of their distances from the centre o , just as it was when they were at $a b c d$; or, to express the same perhaps more clearly, the force exerted on a' , is to that which was exerted

on a as $\frac{1}{\overline{a'o}^2} : \frac{1}{\overline{ao}^2}$. But the force exerted on a is to

that exerted on c , in our present instance, as $ao = 1$ to $co = 3$; and still remains in the same ratio when the stars are at a' and c' ; for the exertion will here be likewise as $a'o = 1$ to $c'o = 3$.

Fig. 11 represents four stars in one circular orbit; and its calculation is so simple, that, after what has been said of Fig. 5, I need only remark that the stars may be of any size, provided their masses of matter are equal to each other.

It is also evident, that the projectile motion of four equal stars is not confined to that particular adjustment which will make them revolve in a circle. It will be sufficient, in order to produce a permanent system, if the stars $a b c d$, in Fig. 12, are impressed with such projectile forces as will make them describe equal ellipses round the common centre o . And, as the same method of calculation which has been explained with Figs. 6 and 10 may here be used, it will not be necessary to enter into particulars.

Fig. 13 represents four stars, placed so that, with properly adjusted projectile forces, they may revolve in equal times, and in two different circles, round their common centre of gravity o . If $ao = bo = 4$, $co = do = 5$, and $c = d = 1$, then will the mass of matter in $a = b$, required for the purpose, be

1,5136. This arrangement, remarkable as it may appear, cannot be made in all situations; for instance, if the distance $ao = bo$ were assumed equal to 1, that of $co = do$ being 2, it would be impossible to find such quantities of matter in a and b as would unite the four stars into one system.

Observations and inferences respecting the nature of the possible revolutions governed by an attractive force, directed to a center.

As we have shewn how the arrangement in Fig. 10. may be derived from that of Fig. 9, so it will equally appear, that four stars may revolve in different but similar ellipses round their common centre, as in Fig. 14. For here the four stars, when placed at $abcd$, are exactly in the situation represented in Fig. 13; but, on account of different projectile forces, they revolve, not as before in concentric circles, but in similar elliptical orbits.

Fig. 15 represents three stars, abc , in the situation of Fig. 5, to which a small oscillating star, d , is added. The addition of such a star to Fig. 1, has been sufficiently explained in Fig. 7; and, what has been remarked there, may easily be applied to our present figure. As the fictitious body m , in Fig. 7, was made to represent the stars a and b , it will now stand for the three stars abc . If we suppose these stars to be of an equal magnitude in both figures, the centre of gravity o , of the three stars, will not be so far from m and n as in Fig. 7; and the perturbations will be proportionally lessened.

Fig. 16 gives the situation of three stars, abc , moving in equal elliptical orbits about their common focus o , while the star d performs oscillations between d and e . What has been said in explaining Fig. 8, will be sufficient to shew, that the present arrangement is equally to be admitted among the constructions of sidereal systems that may be permanent.

We have before remarked, that any appearance of treble stars might be explained, by admitting the combinations pointed out in Figs. 5, 6, 7, and 8; and it must be equally obvious, that quadruple systems, under what shape soever they may show themselves, whether in straight lines, squares, trapezia, or any other seemingly the most irregular configurations, will readily find a solution from one or other of the arrangements of the eight last figures.

More numerous combinations of stars may still take place, by admitting simple and regular perturbations; for then all sorts of erratic orbits of multiple flexures may have a permanent existence. But, as it would lead me too far, to apply calculation to them, I forbear entering upon the subject at present.

Before

The numerous combinations actually seen in the heavens prove that the preceding disquisitions is not mere surmise.

Before I proceed, it will be proper to remark, that it may possibly occur to many, who are not much acquainted with the arrangement of the numberless stars of the heavens, that what has been said may all be mere usefess surmise; and that, possibly, there may not be the least occasion for any such speculations upon the subject. To this, however, it may be answered, that such combinations as I have mentioned, are not the inventions of fancy: they have an actual existence; and, were it necessary, I could point them out by thousands. There is not a single night when, in passing over the zones of the heavens by sweeping, I do not meet with numerous collections of double, treble, quadruple, quintuple, and multiple stars, apparently insulated from other groups, and probably joined in some small sidereal system of their own. I do not imagine that I have pointed out the actual manner in which they are held together; but it will always be a desirable step towards information, if the possibility of such unions, in many different ways, can be laid before us; and, very probably, those who have more leisure to consider the different combinations of central forces, than a practical astronomer can have, may easily enlarge on what has been laid down in the foregoing paragraphs.

IV. *Of clustering Stars, and the Milky-way.*

4. Clustering stars and the milky way.

From quadruple, quintuple, and multiple stars, we are naturally led to a consideration of the vast collections of small stars that are profusely scattered over the milky-way. On a very slight examination, it will appear that this immense starry aggregation is by no means uniform. The stars of which it is composed are very unequally scattered, and show evident marks of clustering together into many separate allotments. By referring to some one of these clustering collections in the heavens, what will be said of them will be much better understood, than if we were to treat of them merely in a general way. Let us take the space between β and γ Cygni for an example, in which the stars are clustering with a kind of division between them, so that we may suppose them to be clustering towards two different regions. By a computation, founded on observations which ascertain the number of stars in different fields of view, it appears that our space between β and γ , taking an average breadth of about five degrees of it, contains more than 331 thousand stars; and, admitting them to be clustering

two different ways, we have 165 thousand for each clustering collection. Now, as a more particular account of the milky-way will be the subject of a separate paper, I shall only observe, that the above-mentioned milky appearances deserve the name of clustering collections, as they are certainly brighter about the middle, and fainter near their undefined borders. For, in my sweeps of the heavens, it has been fully ascertained, that the brightness of the milky-way arises only from stars; and that their compression increases in proportion to the brightness of the milky-way.

We may indeed partly ascribe the increase, both of brightness and of apparent compression, to a greater depth of the space which contains these stars; but this will equally tend to shew their clustering condition: for, since the increase of brightness is gradual, the space containing the clustering stars must tend to a spherical form, if the gradual increase of brightness is to be explained by the situation of the stars.

V. *Of Groups of Stars.*

From clustering stars there is but a short transition to groups ^{5. Groups of stars.} of stars; they are, however, sufficiently distinct to deserve a separate notice. A group is a collection of closely, and almost equally compressed stars, of any figure or outline; it contains no particular condensation that might point out the seat of an hypothetical central force; and is sufficiently separated from neighbouring stars to shew that it makes a peculiar system of its own. It must be remembered, that its being a separate system does not exclude it from the action or influence of other systems. We are to understand this with the same reserve that has been pointed out, when we explained what we called insulated stars.

The construction of groups of stars is perhaps, of all the objects in the heavens, the most difficult to explain; much less can we now enter into a detail of the numerous observations I have already made upon this subject. I therefore proceed in my enumeration.

VI. *Of Clusters of Stars.*

These are certainly the most magnificent objects that can ^{6. Clusters of stars.} be seen in the heavens. They are totally different from mere groups of stars, in their beautiful and artificial arrangement: their

their form is generally round ; and the compression of the stars shews a gradual, and pretty sudden accumulation towards the centre, where, aided by the depth of the cluster, which we can have no doubt is of a globular form, the condensation is such, that the stars are sufficiently compressed to produce a mottled lustre, nearly amounting to the semblance of a nucleus. A centre of attraction is so strongly indicated, by all the circumstances of the appearance of the cluster, that we cannot doubt a single moment of its existence, either in a state of real solidity, or in that of an empty centre, possessed of an hypothetical force, arising from the joint exertion of the numerous stars that enter into the composition of the cluster.

The number of observations I have to give relating to this article, in which my telescopes, especially those of high space-penetrating power, have been of the greatest service, of course can find no room in this enumeration.

VII. *Of Nebulæ.*

7. *Nebulæ.*

These curious objects, which, on account of their great distance, can only be seen by instruments of great space-penetrating power, are perhaps all to be resolved into the three last mentioned species. Clustering collections of stars, for instance, may easily be supposed sufficiently removed to present us with the appearance of a nebula of any shape, which, like the real object of which it is the miniature, will seem to be gradually brighter in the middle. Groups of stars also may, by distance, assume the semblance of nebulous patches; and real clusters of stars, for the same reason, when their composition is beyond the reach of our most powerful instruments to resolve them, will appear like round nebulæ that are gradually much brighter in the middle. On this occasion I must remark, that with instruments of high space-penetrating powers, such as my 40-foot telescope, nebulæ are the objects that may be perceived at the greatest distance. Clustering collections of stars, much less than those we have mentioned before, may easily contain 50,000 of them; and, as that number has been chosen for an instance of calculating the distance at which one of the most remote objects might be still visible*, I shall take notice of an evident consequence attending the result of the compu-

* See Phil. Transf. for 1800, page 83. N. B. In the same page, line 22, for 5000 read 50,000.

tation; which is, that a telescope with a power of penetrating into space, like my 40-foot one, has also, as it may be called, a power of penetrating into time past. To explain this, we must consider that, from the known velocity of light, it may be proved, that when we look at Sirius, the rays which enter the eye cannot have been less than six years and $4\frac{1}{2}$ months coming from that star to the observer. Hence it follows, that when we see an object of the calculated distance at which one of these very remote nebulae may still be perceived, the rays of light which convey its image to the eye, must have been more than nineteen hundred and ten thousand, that is, almost two millions of years on their way; and that, consequently, so many years ago, this object must already have had an existence in the sidereal heavens, in order to send out those rays by which we now perceive it.

Immense magnitude of the heavens, or space containing it.

Light employing six years in its passage from Sirius; and near two millions of years in arriving from remote nebulae, those objects must have existed long ago.

VIII. *Of Stars with Burs, or Stellar Nebulae.*

Situated as we are, at an immense distance from the remote parts of the heavens, it is not in the power of telescopes to resolve many phenomena we can but just perceive, which, could we have a nearer view of them, might probably shew themselves as objects that have long been known to us. A stellar nebula, perhaps, may be a real cluster of stars, the whole light of which is gathered so nearly into one point, as to leave but just enough of the light of the cluster visible to produce the appearance of burs. This, however, admits of a doubt.

3. Stellar nebulae.

IX. *Of milky Nebulosity.*

The phenomenon of milky nebulosity is certainly of a most interesting nature: it is probably of two different kinds; one of them being deceptive, namely, such as arises from widely extended regions of closely connected clustering stars, contiguous to each other, like the collections that construct our milky-way. The other, on the contrary, being real, and possibly at no very great distance from us. The changes I have observed in the great milky nebulosity of Orion, 23 years ago, and which have also been noticed by other astronomers, cannot permit us to look upon this phenomenon as arising from immensely distant regions of fixed stars. Even Huygens, the discoverer of it, was already of opinion that, in viewing it, we

9. Milky nebulosity.

saw,

saw, as it were, through an opening into a region of light*. Much more would he be convinced now, when changes in its shape and lustre have been seen, that its light is not, like that of the milky-way, composed of stars. To attempt even a guess at what this light may be, would be presumptuous. If it should be surmised, for instance, that this nebulosity is of the nature of the zodiacal light, we should then be obliged to admit the existence of an effect without its cause. An idea of its phosphorical condition, is not more philosophical, unless we could shew from what source of phosphorical matter, such immeasurable tracts of luminous phenomena could draw their existence, and permanency; for, though minute changes have been observed, yet a general resemblance, allowing for the difference of telescopes, is still to be perceived in the great nebulosity of Orion, even since the time of its first discovery.

X. Of Nebulous Stars.

10. Nebulous stars.

The nature of these remarkable objects is enveloped in much obscurity. It will probably require ages of observations, before we can be enabled to form a proper estimate of their condition. That stars should have visible atmospheres, of such an extent as those of which I have given the situation in this and my former catalogues, is truly surprising, unless we attribute to such atmospheres, the quality of self-luminous milky nebulosity. We can have no reason to doubt of the starry nature of the central point; for, in no respect whatever does its appearance differ from that of a star of an equal magnitude; but, when the great distance of such stars is taken into consideration, the real extent of the surrounding nebulosity is truly wonderful. A very curious one of this kind will be found in the 4th class, No. 69.

XI. Planetary Nebulæ.

12. Planetary nebulæ.

This seems to be a species of bodies that demands a particular attention. To investigate the planetary nature of these nebulæ, is not an easy undertaking. If we admit them to contain a great mass of matter, such as that of which our sun is composed, and that they are, like the sun, surrounded by dense luminous clouds, it appears evidently that the intrinsic brightness of these clouds must be far inferior to those of the sun. A

* See *Systema Saturnium*, page 8 and 9.

part of the sun's disk, equal to a circle of 15" in diameter, would far exceed the greatest lustre of the full moon; whereas, the light of a planetary nebula, of an equal size, is hardly equal to that of a star of the 8th or 9th magnitude. If, on the other hand, we should suppose them to be groups, or clusters of stars, at a distance sufficiently great to reduce them to so small an apparent diameter, we shall be at a loss to account for their uniform light, if clusters; or for their circular forms, if mere groups of stars.

Perhaps they may be rather allied to nebulous stars. For, should the planetary nebulae with lucid centres, of which the next article will give an account, be an intermediate step between planetary nebulae and nebulous stars, the appearances of these different species, when all the individuals of them are fully examined, might throw a considerable light upon the subject.

XII. *Of planetary Nebulae with Centres.*

In my second catalogue of nebula, a single instance of a planetary nebula with a bright central point was mentioned; and, in No. 73 of the 4th class, is another of very nearly the same diameter, which has also a lucid, though not quite so regular a centre. From several particularities observed in their construction, it would seem as if they were related to nebulous stars. If we might suppose that a gradual condensation of the nebulosity about a nebulous star could take place, this would be one of them, in a very advanced state of compression. A further discussion of this point, however, must be reserved to a future opportunity.

12. Planetary nebulae with centres.

IV.

Description of a New Statical Lamp, which of itself raises and keeps the Oil at a constant Height. By M. D'EDELCRANTZ, of Stockholm, Member of several Academies and Literary Societies, Knight of the Order of the Polar Star, &c.*

SINCE the first invention of the lamp with a double current of air by M. Argand, many attempts have been made to find the means of placing the reservoir of oil below the socket of

Advantages of placing the reservoir of a lamp below the flame.

* Communicated by the Author.

the lamp, and to raise it, in order not only to prevent the disagreeable shadow of lamps on the ordinary construction, which always produces darkness in some part of the apartment, but also to alter the construction, and afford the manufacturing artist an opportunity of giving it those elegant forms of which the instrument is capable.

A new lamp
different from
those of Kier
and of Carcel.

I shall not speak of * Keir's hydrostatic lamp, nor of that with wheel-work by † Carcel, as they are both well known, but confine myself to the description of this new lamp, and which I have considered with the same design as these philosophers.

Description with
reference to the
drawing.

The body of the lamp, Fig. 1, consists of three vessels, *aahh*, *dehh*, and *bbfg*. These may be either cylindrical or rectangular, and made of sheet iron, they differ little in height, and about a line in their diameters, the vessels *aahh*, and *dehh*, are joined at their lower edges *hh*;—*dehh* is closed at *de*; and *aahh* is terminated at *aa*, by a small gallery or balustrade by way of ornament. The third vessel *bbfg*, which passes easily between the other two, is also closed at *fg*, by a plate which projects about three lines; from the center of this there rises a tube *kkll*, to the extremity *ll* of which is screwed the socket of a common Argand's lamp. In the center of this is fixed another tube *pq*, made of tin, in which the iron wire *mn* is placed, and is besides fixed to the plate *de*, at right angles to its surface, the end of the wire is furnished with a nut and screw *oo*. This wire screws to direct the vessel *bbfg* in its motion between the two others, and the position of the nut is intended to determine the limits of that motion.

Description of a
new statical
Lamp.

To use this lamp, let us suppose the nut *oo* to be so fixed, that when the vessel *bbfg* is at its greatest height, there shall only remain a space of 14 or 15 lines between the two other vessels, that is to say, a length equal to *db*. Now pour mercury into the space between the two vessels to within the distance of a few lines at *de*, as to *rr*; the edge of the middle vessel will then be immersed in mercury, and all communication between the inside of this vessel and the outer air will be cut off. And if after having unscrewed the socket of the lamp, oil be poured into the instrument by the aperture *ll*, it will

* Philof. Journal, Quarto, III. 467.

† Philof. Journal, New Series, II. 108.

occupy the space between the two ends fg and de , and the oil by pressing on the surface of the mercury at rr , will raise it a little on the outside from its height and specific gravity. Description of a new statical lamp.

The specific gravity of oil to that of mercury is about as one to 16, a column of oil therefore of 16 inches would not raise the mercury above an inch*.

If, after having filled the reservoir with oil, and screwed on the socket, the Plate fg be so adjusted with weights, that it shall become equal in weight to a column of oil whose base is fg , and height ks ; it is evident that the oil in the reservoir will under this pressure rise to ss , and will constantly remain at that height while any remains, and while the weight of fg continues unaltered; notwithstanding the fall of fg , which when the oil is consumed, will be depressed to de , and rest on the Plate de . utt , is a small tin tube, intended to receive the oil which may be spilled; it answers the purpose of the small round glass usually hung beneath other lamps.

The height to which the oil may be raised in a lamp of this kind is arbitrary, since to increase the height of the socket it is only necessary to increase the weight in proportion, always providing a space for the rise of the mercury, equal to a 16th part of the rise of the oil. This rise will be constant, the weight being always the same, as it does not in any respect depend on the quantity of oil in the reservoir.

It is necessary, however, to observe respecting the invariability of the counterpoising weight, that it will not be geometrically accurate. We know by the laws of hydrostatics, that a solid body loses by immersion in any fluid as much of its weight as is equal to the weight of the fluid displaced by it; now as the oil is consumed, the middle vessel sinks deeper into the mercury, and consequently loses of its weight, it will therefore after some time be evidently incapable of supporting the column of oil at the same height as before. But if this middle vessel be made of very thin sheet iron and has not much range, this diminution will be inconsiderable, and may in practice be entirely neglected. The lamp in the figure being adjusted to 10 lines, will contain oil sufficient for eight hours, at the end of this time, the surface of the oil in the socket will

* *i. e.* Half an inch rise without and half an inch depression within. N.

Description of a
new statical
lamp.

have fallen but two or three lines, whilst the flame will not be materially injured by a fall of four or five. By admitting a larger interval between *de* and *fg*, the lamp will hold more oil and burn longer. To raise the oil to its proper height on these occasions, it will be necessary to add more weights; for example, one ounce for each line of the height required.

The method of filling and using the lamp will naturally be understood from the foregoing description. I shall only add, that the quantity of mercury required will be about a pound, and that when once put into the lamp it need not be removed from thence, unless it should be thought proper to clean the inside of the lamp at intervals. In this case the oil and mercury may be poured together into a glass or earthen vessel, and the mercury afterwards separated from the oil by a paper strainer. The instrument may be cleaned with warm water and a feather, it may then be filled again, and when the proper weights are placed, the rise of the oil must be produced by a gentle shaking of the lamp, any unusual pressure for this purpose must be avoided. The oil will not rise to its proper and constant height till a few moments after being lighted. The length of the lower current of air requires high chimneys to produce a clear light. These must be taken off when the lamp is extinguished. To remove it with safety, the column or upper part of it must be taken off, and the instrument be carried carefully by its socket.

It will easily appear, that various ornamental and elegant forms may be given to this lamp, the determination of which will remain with the manufacturing artist. Our present design represents a truncated column, on a square base, ornamented with four balls, which also serve for the counterpoising weights.

I have thought proper to call this instrument a statical lamp, as depending on the equilibrium of three different bodies, two of them fluid, and one solid.

V.

On Oily Hydrogen. By Professor PROUST.*

THE perusal of the memoir of the Dutch chemists having suggested to me the idea of making two experiments on the gas obtained by the distillation of olive-oil, I apprehend, doctor, that you will see the result with pleasure. With respect to the inferences which I have ventured to draw from them, if they be not true, they will nevertheless contribute to extend the scale of this new order of facts, and increase the means of studying them with better effect.

Gas by distillation of olive oil.

I call this gas oily, because it seems to me that its great weight; its white, sooty, heavy flame; its strong smell; and above all, its property of becoming lighter by being passed several times over alcohol, point out a simple solution of the oily vapour in carbonated hydrogen.

Called oily gas.

The purest oil must yield a considerable quantity of carbonic acid with the oily gas. Berthollet was perfectly right in saying that a certain dose of oxygen would be found in the oils: besides, the absorption which they continually make authorises it. It may perhaps be attempted to connect this acid with the principles of that mucous body which Scheele separated from them by oxide of lead; but if on the one hand, the nature of the ingredients in this operation be well considered, and on the other, that of the sugar of oils, I believe it will not be unreasonable to suppose that the sugar was entirely formed during their coction with the oxide.

Pure oils yield carbonic acid with the oily gas: they contain oxygen.

Sugar of oils.

It is free from every metallic matter, since it does not render hydro-sulphurated water turbid; but it is particularly distinguishable from all our vegetable sugars, because it is not susceptible of any fermentation, corruption or mouldiness. I have kept three or four ounces of it full ten years, which is of the consistence of a tolerably clear syrup, without its having experienced the slightest alteration in the greatest heats. In a word, I am very much disposed to believe that this species of sugar is rather a new product than a substance separated from the oils.

not the same as the vegetable sugars.

Its unchangeable nature.

* From the Journal de Physique, Germinal, An. XI.

Distilled oil again discovers characters to which little attention has been given. It is changed into a volatile odorous or essential oil, doubtless by dissolving a certain quantity of carbonated hydrogen. The following are some of its properties :

Properties of
distilled oil.

When heated with water in a retort, part of it comes over in the distillation. It has a very powerful but disagreeable smell, with a degree of lightness which places it in the rank of volatile oils, because its weight to that of olive-oil is not more than as 91 to 100. It swims upon alcohol, which dissolves it with ease. Lastly, it enters into ebullition as readily as spirit of turpentine ; and if a lighted taper be brought to the mouth of the matrass, the vapour of it burns like that of volatile oil.

Sebate of potash
remains after the
distillation of
soap.

I have not particularly examined the acid liquor which accompanies the distilled oil : I shall only insert a fact relative to its history. If soap be distilled to the destruction of its oil, the saline residue which I should have expected to be the carbonate, is found to be the sebate of potash. It is crystallizable, and the sulphuric acid instantly disengages that penetrating vapour called sebatic acid. But to return to the oily hydrogen, which holds in solution the facitious essential oil which I have mentioned at the beginning. When the heat suddenly strikes that portion of the oil which is found there, as if in a red hot tube, it passes from the vaporous state to that of carbonated hydrogen, in the same way as happens to ether, alcohol, radical vinegar, essence of turpentine, &c. when they pass through a red-hot tube.

I shall conclude this by an experiment which Rouelle made in his lectures, and which I also show in mine ; because in addition to the agreeable spectacle, the explanation of its causes is perfectly conformable to the theory of inflammations.

Beautiful ap-
pearance from
the sudden com-
bustion of oil ;

Pour half a spoonful of olive-oil into a small melting crucible obscurely red, or at that degree of heat which shall have been discovered by some previous experiments. A thick cloud of white smoke will immediately rise which takes fire at its summit, that is to say, four or five feet distant from the crucible. If on the contrary, the combustion of the cloud arises from the crucible, it is occasioned by the heat being too great. In that case we must wait a few minutes, and the phenomenon will appear in all its magnificence. A piece of wax will give the same effects, but it seems to me to require more previous preparations than the oil.

and wax.

On the Dioxide of Iron.

The simplest truths of chemistry are not always those which are the most easily shown in public teaching. For example, nothing can be more evident to the most numerous auditory than that, in the tube experiment, the iron carries with it an overcharge of oxygen, because every one of the spectators can satisfy himself by his eyes, and acquire an absolute conviction of that which is intended to be shown; but when amidst the connection of ideas and proofs, we come to those which relate to the dioxide of this metal, we must quit facts and replace them by suppositions, because it is not possible to execute them with facility in public. In fact, it is impossible either to apply a degree of heat in a few instants to the oxide of iron, sufficient to deoxidate it, nor can it even be easily reduced by melting. But by the following process this difficulty is partly overcome.

The simplest truths of chemistry not always easy to be manifested in a lecture.

Instance. Dioxide of iron

Heat for an hour a mixture of one ounce of iron ore of Elba, and two gros of charcoal, in a small coated glass retort, at the bottom of which the clay has been left of the thickness of two inches. Place it immediately on the grate of the furnace, and its neck will then project from the furnace by the door of the fire-place. Close it with fragments of bricks and earth, which will keep the retort in its situation. If the neck be not long enough to reach into the pneumatic-trough, which in this case ought to be a salad-dish or a soup-plate covered with a plate of lead perforated, add to it another which must be luted with paper and starch.

Easy process for this deoxidation by Elba.

Iron ore heated in a glass retort with charcoal.

Having done this, cover with the usual precautions, the grate and the retort with charcoal. It receives from this disposition a much more intense heat than if it had been placed on the bars. The carbonic acid and carbonated oxide are then obtained with the greatest facility.

When the retort is cold, its belly will be found to be nearly filled with pure iron of a spongy appearance, which acquires the metallic brilliancy by friction with any polished substance. This mass is broken with difficulty, and notwithstanding the separation of its parts, they will be all found to be soldered together. On heating it dexterously by the blow-pipe, it burns with sparkling. The magnet attracts the whole of it. Part of it may be used to give hydrogen with sulphuric acid, in

State of the reduced iron.

short every one is convinced on touching it, that it is iron reduced to its primitive state, and that if it were formed into a mass, it might constitute bar-iron.

As carburet is formed in this operation, the dissolution of the iron yields the same bituminous smell as that which is observed in crude iron.

If instead of wood-charcoal, that of blood well washed be mixed with the oxide, ammoniacal carbonate will be deposited in the neck of the retort. I have explained the cause of this product in my second memoir on Prussian blue. The solution of iron then affords phosphate with the potash at the first moment of its precipitation. The bituminous odour of the solution of cast iron as well as the gas, are also obtained from recently made charcoal of blood, by the application of sulphuric acid, it takes fire at the mouth of the matras. The reason of this is so evident, that it would be superfluous to dwell on it.

Four inches of this gas, and one of oxygen, cannot be inflamed in a tube with the charge of a pocket electrophorus; neither can it with two of oxygen, nor even with three. My object in these experiments was to discover if charcoal or oil would be precipitated.

Lastly, with four inches of oxygen or equal parts an inflammation takes place; but it causes an agreeable surprize to observe that, on opening the stop-cock, the gaseous residue instead of being diminished, is on the contrary, increased to thirteen or fourteen inches. This sudden expansion is accompanied by another fact equally curious, the explanation of which is obvious, on recollecting the observation of Monge, on an atmosphere more or less compressed. This is a cloud or thick mist which fills the tube as the residue escapes from its former compression. Let us now examine our eight inches of mixture which has increased in bulk to thirteen or fourteen by the inflammation.

On transferring it through lime-water, it will be found that it scarcely troubles it, and even at sometimes not perceptibly at all. It also, within a few lines, preserves its dimensions of thirteen or fourteen inches.

If a light be presented to it, it will be seen that it is not oily hydrogen. Its blue flame descends slowly, and it is consumed without the least detonation. Lastly, it has but a weak odour very different from that of oily hydrogen.

This

This gas burnt with oxygen is wholly converted into carbonic acid, and is no longer dilatable however small the quantity of oxygen mixed with it may be.

The expanded gas produces carbonic acid when burned with oxygen.

The following are the conjectures which I form at present from these results.

I do not think that the residue retains the least trace of oxygen after the inflammation, otherwise it would burn with rapidity, which is far from being the case.

Four inches of oxygen generally consume seven and a half of pure hydrogen in my eudiometer, and as our residuary gas contains only a very small quantity of carbonic acid, it follows, that these four inches have consumed a quantity of hydrogen which would have been at least equal to seven inches, had it been in the usual state of dilatation at which the pressure of the atmosphere keeps it.

Conjectures or inferences.

Four inches of oily hydrogen therefore contain, according to this, seven inches of pure hydrogen condensed into the bulk of four.

Again our residue after the combustion of four inches of oily gas, is all at once changed into thirteen or fourteen inches of carbonated hydrogen. We must therefore conclude, that the seven inches of hydrogen compressed into the bulk of four, held in solution a quantity of oil capable of being converted, by a high temperature, into thirteen or fourteen inches of carbonated hydrogen.

I say a high temperature, because in fact, that alone is capable of changing oil into carbonated hydrogen.

Whence I conclude, that in the inflammation of four parts of oily gas, by an equal quantity of oxygen, nothing is really burnt but the hydrogen.

VI.

Letter from FORTIS to J. C. DELAMETHERIE, on a Shower of Mud which fell at Udina.*

I HAVE just received at the same time, the number of the Journal de Physique, in which you notice the different opinions respecting stones fallen from above, of which almost every

* In the Journal de Physique, Germinal, An. XI.

Muddy shower
which extended
over several
leagues.

country has a tale or history to relate, and the details by my friend Cernazaï, a good mineralogist of Udina, of a slimy rain which actually fell there in the evening of the 6th of this month, the wind having blown with violence from the east for several days. The surface of the land which has been entirely watered with this strange rain, appears to be from ten to twelve leagues in diameter from the sea-shore to the foot of the Alps of Carnia. Here, my dear friend are the means of establishing a brick furnace in the higher regions of the atmosphere, by those who are already convinced that materials are suspended there, susceptible of a fusion similar to that of the interior of a volcano. I know not if the partisans of that opinion which brings down the lavas from the moon, will be able to derive any support from the mud which has just coated the fields of Friuli; but for my own part, I was at first of opinion, that the wind having been charged, in Sicily or near Naples, with volcanic dust thrown up by a whirlwind, had deposited it at the foot of the mountains of Carnia, which had obstructed the further progress of the clouds. But having since examined a specimen of the sediment in question, sent to me by my friend at Udina, and which I forward to you with this letter*, by a very powerful magnifier, I am convinced that there is not the least resemblance between this argillaceous earth and the *detritus* thrown by volcanoes into the upper regions of the atmosphere. It appears to me much more probable that a hurricane, or perhaps a water-spout had pumped up the slimy water left by the large rivers when they overflow our richest plains, and raised it into these regions in which the wind had acted upon it. It is in consequence of such very natural and common events, that worms, tadpoles, and small fishes are frequently seen to fall from on high, without any one dreaming of their proceeding from an aerial race or from another globe.

Its probable
nature.

You, my dear and learned friend, may make what use you think proper of this simple unskilful explanation.

* This earth is light, of the colour of brick-dust, and seems to be argillaceous. *Note of J. C. Delametherie.*

VII.

Letter from M. VAN MARUM to M. BERTHOLLET. Containing an Account of some Experiments, shewing the Method of extinguishing violent Fires with very small Quantities of Water, by Means of Portable Pumps.

SIR,

WHEN I had the pleasure to see you, during my stay in Paris, in September last, you informed me that my experiments, made on a large scale some years ago, to shew the practicability of extinguishing very violent fires with an inconsiderable quantity of water, were entirely unknown in France. You at the same time requested me to send you an account of the experiments to be read at the national institute, and inserted in the *Annales de Chemie*. My compliance with your request has been prevented till now by the want of time. The circumstances which gave rise to the experiments were the following:

Nine years ago, Van Aken, a Swede, published at Stockholm, Copenhagen, and Berlin, that he could very quickly extinguish large artificial fires with an inconsiderable quantity of a liquid which he called anti-incendiary, the composition of which he had for some time kept secret. Having observed in the journals, that M. Van Aken had repeated his experiments with much success in Berlin, before some members of the academy of sciences, I wrote to the celebrated M. Klaproth, requesting, if he knew the composition of the anti-incendiary liquid, he would communicate it to me; intending to shew, at this place, the merit of the invention, by an experiment on a large scale. With this intention, as soon as I had received M. Klaproth's communication, I caused Van Aken's liquid to be prepared under my immediate inspection. The composition was a solution of 40lb. of sulphate of iron, and 30lb. of sulphate of alumine, mixed with 20lb. of the red oxide of iron, and 200lb. of clay. I then commenced a series of comparative experiments, by forming two combustible masses, alike in all circumstances; setting them on fire, and extinguishing one of them with the liquid of Van Aken, and the other with common water, I was much surprised to find, in several trials, that,

History of an experiment for extinguishing fire.

Anti-incendiary water of Van Aken.

Its composition.

by

On trial it proved less effectual than common water.

Experiment to shew that a very small quantity of water is needful in extinguishing fire.

by using the two liquids in the same manner, the fire was always extinguished more quickly by the common water than by the anti-incendiary liquor; but I observed, at the same time, that a very inconsiderable quantity of water, if judiciously directed, would extinguish a very violent fire. The result of my first experiments in this respect, led me to make others on a larger scale, and which I shall describe alone: I took two casks, which had been full of pitch, and of which the inside was yet well covered with that inflammable substance; the heads of these were then taken out, and in order to assist the operation of combustion, I gave them a conical shape, placing the larger aperture of 20 inches diameter uppermost, the other of 16 inches diameter below, mounted on a three-legged stand, a few inches from the ground, so that the fire might be kept as brisk as possible by the free current of air passing through the cask. I covered the inside of each cask with a fresh coat of pitch, and having placed shavings of wood in each cask, I lighted them one after the other. I began to extinguish this fire when it was at its height. For this purpose I made use of an iron ladle capable of holding two ounces of water, and provided with a very long handle, being kept at the distance of four or five feet from the apparatus by the violence of the fire. I carefully poured the water from this ladle in very small streams over the inside of the cask, placing the ladle on the edge of the cask, and moving it gradually along the edge as the flames ceased. In this way the first ladle full of water extinguished nearly one half of the fire, and what remained was effectually put out by a second ladle full, used in the same manner. The striking success of this experiment, induced me to repeat it in the presence of many persons, and with care and attention in the application and management of the water, I have more than once succeeded in extinguishing an highly ignited cask with a single ladlefull of two ounces of water.

Theory. The steam keeps off the access of air.

It will appear surprising at first that so violent a fire could be extinguished with so small a quantity of water; the reason will, however, be easily understood when we reflect that, according to well-known facts and principles, the flame of a burning body will cease whenever its contact with the atmospheric air is prevented: now, when a small quantity of water is thrown upon an highly-ignited body, part of that water is instantly

instantly reduced to steam, which, rising from the surface of the burning body, prevents the contact of the atmospheric air, and by that means puts out the flame, which cannot appear again while the production of the steam is continued.

According to these experiments, it appears that the art of extinguishing a violent fire with a small quantity of water, consists in this: that the water be thrown on that part of the fire which is the most violent; so that the quantity of steam produced, which suppresses the flame, may be the greatest possible: that water be continued to be thrown on the neighbouring inflamed parts, as soon as the fire has ceased in that on which the operation was began, and that all the burning parts be visited in this way as quickly as possible. By thus following the flames regularly with streams of water, they may be every where suppressed before the part on which the operation was began, shall have entirely lost by evaporation the water with which it was moistened: this is often necessary to prevent the parts from breaking out afresh; for, on the principle above-mentioned, a burning body, of which the flames are suppressed, cannot be again in flames until the water thrown on it be totally evaporated.

Being convinced by these experiments, that a small quantity of water is sufficient to extinguish ordinary fires, especially in an early stage. I have endeavoured to produce the same conviction among my fellow-citizens, by repeating the experiments I have described, and have advised them to provide themselves with small portable pumps for their use in case of necessity. Many immediately followed my advice, and their good effect being proved, the number has gradually increased in several towns in Holland, especially after an experiment which I made in this place in May, 1797, to shew, on a larger scale, the advantages to be derived from a well-regulated direction of streams of water, in the extinguishing even the most violent fires, and with very small quantities of water, where portable pumps are used.

The following is the experiment:

I prepared a shell of dry wood, forming a room of 24 feet long, 23 feet wide, and fourteen feet high, having two doors on one side, and two windows on the other; this box, provided with the wood-work of a roof, was uncovered above, and its partition raised six inches from the ground; so that, having a current

Instructions grounded on the facts and theory.

Experiment on a larger scale.

A wooden edifice set on fire and extinguished.

rent of air from the bottom upwards, the combustion might be rendered as brisk as possible when the building should be set on fire. The inside of the partitions were strongly pitched, and were beside covered with twisted straw, which was also pitched. To the inner part of this straw covering, I fastened wood shavings, and besides this a coarse cotton cloth, soaked in oil of turpentine, in order that the whole inside of the building might be quickly on fire. Very soon after lighting it, the flames being rendered more brisk by the wind, were every where so violent, that it was considered by my assistants as impossible to extinguish them: I succeeded, however, after the method above directed, in little more than four minutes, and with five buckets of water, a part of which was wasted by the negligence of my assistants, as was proved by the following experiment:

Repetition:

Having invited only a few persons to witness this, my first experiment, on the 8th May, I repaired my apparatus, and repeated it on the 11th, before a great number of spectators: the flames were no less violent than in the preceding experiment. I now undertook to direct the jets of water in person, and succeeded in extinguishing the fire in three minutes, and employed only three buckets of water, each containing about eighteen pints.

Experiment at
Gotha.

Being at Gotha in July 1801, the duke and duchess of Gotha pressed me much for a repetition of my experiment at their own expence, the details of which they had seen in the German journals; in order to make the matter more fully understood in that part of Germany, where, as in every other place, fires often make the most dreadful ravages, in consequence of the ignorance of the method of properly using the small quantity of water which they may have at hand. The obliging manner in which their highnesses made their request, and my own wish to render it more generally useful, induced me to repeat it. The celebrated astronomer, Von Zach, assisted me in this experiment, and had the goodness to insert an account of it in a German periodical paper, intitled *Reichs Anrieger*, 6th August, 1798, No. 119.

M. Lalande arriving at Gotha on the 30th July, four days after this experiment, was well acquainted with its results, and, as he has since told me, spoke of it at the National Institute, soon after his return to Paris; but informed me, at the same time, that

that the truth of his account was doubted. In order to clear away all doubt on that head, I subjoin the following liberal translation of the verbal process of the experiment which Von Zach had drawn up, and inserted in the above-mentioned periodical paper :

Doctor Van Marum * being at Gotha, in the course of a literary journey, which he was making in Germany in 1798, the duke of Gotha, well known as an amateur of the physical and mathematical sciences, signified his desire to see, on a large scale, that experiment of extinguishing fires, the effect of which M. Van Marum had shewn him, in extinguishing a pitched cask, set on fire, with a small ladle of water. He caused a building to be constructed under M. Van Marum's direction, in the duches's garden, of equal dimensions in all respects with that used for the same experiment at Harlaem, which was of 24 feet long, 20 feet wide, and 14 feet high. It had two doors on the north-east side, and two window-like openings on the north-west side ; the top was left entirely open to give the flames a free vent ; the inside of this receptacle was plastered with pitch, and afterwards covered with straw mats, on which melted pitch had been poured. To the bottom of these mats, cotton wicks, soaked in spirits of turpentine, were suspended, that so the building might be every where on fire at the same time. In this state the fire, excited by the wind, was soon so violent, that the flames, with thick clouds of smoke, were carried several feet above the opening of the roof, and so fiercely, that the spectators assembled about the building quickly drew back ; many were of opinion that it would not be possible to extinguish it, but that the building must be reduced to ashes. When the straw mats were entirely consumed, the interior wood work of the building was soon on fire in every part. The most unfavourable circumstances attended this experiment ; for the wind drove the flames directly through the two doors on the north-east side, by which it had been intended to introduce the streams of water to extinguish them : but notwithstanding this, M. Van Marum placed a small portable pump (or engine) before the door in that part of the north-east side of the building nearest to the south-east-side ;

Verbal process of
Von Zach on the
method of ex-
tinguishing fire.

* The remainder of this memoir being the words of the report, it has not been marked as a quotation.---N.

Verbal process of
Von Zach, on
the method of
extinguishing
fire.

without regarding the apprehensions of his assistants, he set it in action, and placing himself before this door, as near as the intense heat of the fire would admit, he directed the streams of water first towards the south-east side, and as near the door as was possible, and continued until the flames were extinguished on the side, and sprinkled with water in the same direction; after which the water was directed along the south-east side, and afterwards the north-east, so that in a few minutes the flames were got under, and the burning partitions were extinguished. After this the pump was placed before one of the openings in the north-west side. He also very soon extinguished the south-east side; and lastly, coming to the centre of the building, where the fire appeared here and there in the chinks of the boards and the holes left by the nails, breaking out at intervals in small flashes, he entirely extinguished them, and thus perfectly subdued this violent fire. It was estimated by several of the spectators, that the fire was extinguished at most in three minutes from the commencement of the action of the pumps to the time when the wood just remained burning, and broke out afresh in some places; these renewals were however so inconsiderable, that the burning parts were quenched by means of some wet rags fastened to a stick. Before setting the pump to work, its reservoir was filled with four buckets of water; but in carrying it to the first window of the building, and from thence to the middle of it, near a bucket full was spilt; so that it may be positively affirmed, that this violent fire was extinguished with three buckets of water, excluding that which was afterwards used to quench these parts of the building which remained red. It was easily observed, when the fire was out, that not only the straw mats were burned, but the entire wood work of the building had been on fire, in so much that the smallest part of wood could not be found in the inside of the building which had not been more or less severely burnt. The north-east side in particular, against which the wind drove the flames with the most violence, was entirely charred.

The experiment made at Gotha differed materially from that at Harlaem, as the flames and thick smoke, which came out at the doors on the latter experiment, rendered access to the building with the pump very difficult; so that it was only by means of persuasion, and the courageous example which

M. Van

M. Van Marum himself set in leading his assistants and directing the operations of the pump, that he could prevail on them to face the danger, which they considered as very dreadful.

Verbal process of Von Zach, on the method of extinguishing fire.

The result of the foregoing facts is, that in applying the method of extinguishing fire, the circumstances to be observed are these, that, to extinguish the most violent fire, it is only necessary to wet the surface of the burning matter in the part in which the flames are seen, and that for this purpose only a small quantity of water is needful, if the parts be wetted in the proper manner. In operations of this kind, therefore, particular attention must be paid to throwing the water in such a way, that the entire surface of the burning part shall be wetted and extinguished, and that in such a way that an extinguished part shall never be left between two others which are in flames; for if attention be not paid to this, the heat of the flames burning here and there will quickly change the water with which the part has been wetted into steam, and the whole will again take fire. In order, then, to extinguish a fire in all cases, no more water must be thrown on the burning part than is needful to wet its surface; and this I conceive to be all that is requisite to extinguish a fire whatever may be the circumstances of its origin.

VIII.

*Report, presented to the Class of Accurate Knowledge of the Academy of Turin, in the sitting of the 2d Nicose, (Dec. 23, 1802) in the Year 11, on the Action of Galvanism, and on the Application of this Fluid, and that of Electricity, in the Healing Art, by ANTOINE-MARIE VASSALI EANDI.**

THE galvanic experiments, made the 22d and 26th of last Thermidor, before a considerable number of spectators, by your associates Giulio, Rossi, and myself, on the heads and trunks of three persons who had been decapitated, the report of which you have caused to be printed, excited many questions respecting this agent, and, by analogy, on electricity. These two fluids, and their uses, have furnished the subject of

Historical introduction.

* From the Journal de Physique, Germinal, An. XI.

the ordinary conversations of well-informed people; and, as always happens, when a discovery is the topic, some have exaggerated its advantages, and others have despised it. This diversity of opinion led our associate, citizen Charles Bossi, (qu. Roffi) to propose two judicious questions to me, which I have endeavoured to resolve in the following letter, written to him in the beginning of Vendemiaire. I could easily have swelled it to greater extent by additional reasons and numerous instances of cures obtained by means of galvanism and electricity, as well as with others of misfortunes, occasioned by these fluids, and with the constant correspondence which I have observed between the moral powers, that is to say, the strength of mind and courage of the victims of justice, and the effects of the galvanic fluid upon their bodies; but these observations belong to the general report of the experiments we have made since the 26th Thermidor; I shall, therefore, confine myself to presenting you the letter which I wrote at the above-mentioned time.

Questions for
discussion.

“ You asked me, in one of our late academic sittings, whence it arose that, after the number of experiments made by the most able philosophers of the past age, on the electric fluid, the discovery of which is one of its most brilliant conquests, so little is determined relative to the medical action of this fluid on the human body; and whether galvanism appears really already to promise results of more utility in the art of healing? I shall inform you what my opinion is, or rather, I shall submit to you the inferences, which the different experiments made by me alone, or in which I have assisted and witnessed, have enabled me to draw with more certainty than I ever dared to hope when I began seriously to engage in this pursuit.

Galvanism con-
sidered as a mo-
dification of elec-
tricity.

I consider galvanism as a modification of electricity, which renders that fluid more active; as the small flame, separated by the blow-pipe, is infinitely more powerful than the large flame from which it proceeds. I have read several experiments to the class, which support this parallel between electricity and galvanism. Animals which had been only stunned by the most powerful explosions of the magic table, were killed in less than three minutes by galvanism, which was far from being strong.

Proofs of its ef-
fects on animals,

The

The fluid of a pile of 25 pair of plates of silver and zinc, on metals and of the size of a crown piece, separated by discs of pasteboard ^{water,} moistened in water, saturated with muriate of ammonia, oxidates the metals by decomposing the water, while it is scarcely perceptible to the fingers, and shews but very small sparks. Brilliant electric sparks, which are sharply felt by our bodies, do not oxidate the metals, or decompose the water, if they are not so strong as to explode. On passing the galvanic stream through the body of a frog, it has happened that his humours ^{on frogs,} have been decomposed, and I have seen him swell so prodigiously, that he was unable to sink himself below the water, though perfectly lively; this I have never observed in frogs tormented by exploding sparks. All these facts, to which I could add many others, prove the superior activity of galvanism, when compared with electricity. Hence it results, that the fluid from ^{Utility of Volta's} the electro-motor of Volta may be highly useful in cases where ^{electro-motor.} the ordinary electricity is not sufficiently active. You are acquainted with some of the experiments which I have made with my colleagues, Julio and Roffi; we have others still more interesting, which have determined physiological facts that have always been doubtful from a want of the means of verifying them. Lastly, we have tried the application of it in several disorders with the greatest success. Here are three instances:

A lady, about 30 years of age, after a continuation of violent pain in the head, lost the sight of the right eye. Citizen Roffi was consulted on the complaint, and after a careful examination of this eye, which to appearance was as healthy and clear as the left, he concluded that it could be only a paralysis of the optic nerve, or what is called a *gutta serena*, which, allowing the patient to see only a thick mist, increased her misfortune, while it also contributed to derange the sight of the other eye, so that she was in continual fear of falling, and was not even able to discern with the right eye those objects which were within her reach. Roffi, being indisposed, sent to me to apply galvanism. I formed a pile of thirty pair of such plates as are mentioned above, and making use of gold cords for conductors, I caused the galvanic stream to enter close to the external angle of the eye, and to pass out sometimes through the eye-brow; sometimes exactly at the ophthalmic vein, which goes through the hole of the orbit; and sometimes close to the Cure of a gutta serena by galvanism. internal

internal angle of the same eye. The operation was attended with some pain; it caused tears to flow abundantly; but after having received successive galvanic shocks for half an hour, the sight of the eye began to be restored. Being unwilling to fatigue the lady, and to allow time for nature to act, the operation was suspended until the evening, when it was repeated for thirty minutes also. The following day the eye began to perceive the outlines of bodies. Having repeated the operation for three successive days, she not only was capable of seeing objects and the physiognomy of persons, but even the balls of their eyes. Before this operation, in consequence of a consultation with doctor John Baptiste Anfori, first physician to the Hospital de la Charité, I had galvanised a girl of 27 years of age, of a melancholy habit, who, after some slight attacks, had a hemiparalysis of the right side, which particularly affected the arm, the cheek, and the eye. The other symptoms had yielded to bleeding, and the use of such other remedies as had been indicated, but the eye remained constantly fixed, with pains in its muscles. Ten minutes of galvanisation were sufficient to excite an abundant flood of tears, and a discharge of water through the nostril on that side which had been always closed after the above attack; the pains in the muscles were also much alleviated; she could even turn the eye to either side, but had great difficulty to raise or depress it, accompanied with a sensation of weight round the eye. The operation was repeated after a day's repose, which restored the complete freedom of motion to the eye, and freed it from every unpleasant sensation.

Cure of a paralytic affection of the eye by galvanism.

These two operations were performed in the presence of several persons, and executed almost wholly by citizen Heacinte Carena, teacher of physic in the national college of Turin.

Cure of the hydrophobia by galvanism.

The advantages of galvanism will appear still more decisively to you by the cure of the hydrophobia, lately accomplished by citizen Roffi, who will give a detailed and exact account of it in an interesting memoir, on which he is employed.

A man who had been bitten on the middle finger by a mad dog came to consult him for the pains he felt in his arm and back, and particularly in the finger, which had been bitten a month before. The actual cautery, applied to the finger, removed the pains, but in a few days they returned, accompanied by

‡

symptoms

symptoms of hydrophobia. He could no longer bear the sight of water without shuddering; an inflammation of the throat obstructed his swallowing even chewed bread, and a violent desire to bite appeared every instant.

In this state he was brought to citizen Roffi, who perceiving that he could not endure the sight of water, or even of any shining body, prepared, in another room, a pile of 50 pairs of zinc and silver plates, separated by 50 discs of pasteboard, moistened in a solution of muriate of ammonia; he then made use of small strips of moistened white brown paper for a conductor, on which he caused the naked feet of the patient to be placed, and at the moment he opened his mouth to bite, he thrust in the end of the conducting-rod, which communicated with the other extremity of the pile. The patient suffered much during this operation, which, after several shocks, rendered him so weak, that he was unable to stand: being then extended on the floor, he was galvanised with facility. The operation produced a copious perspiration. After some time, Roffi caused the man to be carried home, with directions that he should be brought again the following day, that the operation might be repeated. It was at two o'clock in the afternoon that the patient was galvanised, at six o'clock on the following morning he came alone to citizen Roffi, to tell him he was perfectly cured, for he felt no pain, nor difficulty in swallowing, and had entirely lost all aversion to water and other liquids: but no arguments could induce him to submit to another operation.

In a few days after, some slight pains having caused him to dread a new attack of the hydrophobia, he returned to Roffi, who, by galvanism, again removed all the symptoms. This cure was likewise performed in presence of several people. The patient is possessed of such sensibility, that more than a month afterwards he felt the sensation of galvanic shocks as far as his shoulders, which I could scarcely perceive beyond the third joint of the finger, and I am not among the least susceptible. From these essays you will perceive what benefits may be hoped from galvanism. I do not doubt that an agent

Benefits to be expected from galvanism.

This will become more obvious from the explanation of the medical action of electricity on the human body.

Difference of opinion concerning electricity, arising from its misapplication.

Several celebrated writers have placed electricity among the most certain and active remedies; others have demonstrated the inutility and even danger of this fluid as a remedy; each of them have supported their opinion by well-ascertained facts.

Electricity may do harm when injudiciously applied.

Nothing is more easy to be accounted for when we reflect on the very great number of those, who, in the application of electricity as a medicine, have only acted empirically without having consulted the nature of the disorder, or of the agent by which they intended to cure it. It is for this reason that, in the memoir which will appear in the volume of the academy, I have recommended the greatest caution in the use of galvanism, which, like electricity, may do a great deal of harm; and that I have asserted that this remedy, though very excellent in itself, has done more harm than good in consequence of improper application. I shall not speak of the chimerical properties which, in the enthusiasm of novelty, have been attributed to electricity, such as that of conveying to the human body, by friction with them, the action of remedies sealed up in glass tubes.

New remedies injudiciously over-rated, &c.

It is notorious that it is the fate of every new discovery to be so over-rated, that its opponents are at no loss for reasons to object to it, but after some time things are brought to the proper level, established by the more intimate knowledge of the agent. Thus those who are sufficiently acquainted with the properties of electricity, know how to distinguish the cases in which it can be employed to advantage from those in which it would be injurious. Of ten patients affected with the same complaint, who undergo a similar course of electricity, five may be entirely cured, and the other five shall be very ill.

Those who are cured extol electricity as the best of remedies; those who suffer say that it only increased their complaint; both speak from accurate experiments; both are right and wrong at the same time from making the application too general, that is to say, because they do not distinguish the cause of the disorder which requires to be opposed by the use of electricity. I will explain myself: The same complaint,

for instance *sciatica* *, may be occasioned either by obstructed humours, by their too great quantity, or by a defect in the reaction of the solids; it may also arise from organic defects, from a change in the humours, from a venomous, or as it is called an *acid* principle, or from a peculiar virus which is found in the humours.

Those five who are affected from an obstruction in the humours receive the greatest benefit from electricity, which sets them in motion; the other five, who are tormented by a *sciatica* proceeding from vitiated humours will grow worse under the electric treatment, which increases the acidity of the humours, carrying off part of the water which kept the poison diluted. This theory of the effects of electricity on the human body is founded on the nature of this fluid, and on the properties it manifests in numerous experiments.

The electric fluid tends constantly to an equilibrium, and this tendency is so strong, that it is seen to penetrate through the air to a certain distance, and to spread itself on idio-electric bodies. It is this tendency which causes electrified water to run in a continued stream through capillary syphons, whence it passes slowly in drops, if the electrization is stopped. It is by the same tendency that electricity accelerates the circulation of the blood in animals †, and of the humours in vegetables. It is, lastly, by the same tendency that when the electric spark passes from one conductor into another by connecting wires, if the cohesion of the body be not sufficiently great to prevent it, it takes with it, in its passage, some of the conducting particles, which serve it as a vehicle. This property, which is manifested by the ordinary effects of thunder, and of several experiments, shews the cause of the increased evaporation of electrified liquids, and the greater transpiration of animals and vegetables under similar circumstances. It is clear, therefore, that in every case of obstructed humours, if other symptoms do not forbid, electricity will be a very good remedy; on the

Constant tendency of the electric fluid to an equilibrium, urged as the mode of its action.

* Doctor Balbis observed to me, that all the species of *sciatica* could be very well accounted for without having recourse to the hypothesis of corrupted humours; I replied to my learned brother, that it was my desire to compare my theory with the chief theories of sciatic affections without attending to their probability.

† Van Marum seems to have ascertained that it does not produce this effect. See his account of the Haarlem machine.—N.

contrary, if the complaint proceeds from vitiated humours, or from a diluted virus being contained in them, then electricity, either by evaporating the diluting humour, or by causing a greater change, will be hurtful. Hence it is evident, that electricity and galvanism ought to be used with the greatest circumspection, and that the nature of the disorder, as well as of these fluids, must be considered, before it can be decided, whether the application is proper or not. It must also be observed that this remedy, on account of its activity, like all other remedies, however good they may be, becomes dangerous when abused.

An instance of injury from an imprudent application of galvanism.

I might bring many instances of injury occasioned by the misapplication of electricity, even in cases where a short time before it had been indicated. But I shall only produce one case relative to galvanism. A young female had been cured of pains in the muscles of her face by galvanism; but after the cure, in consequence of continuing the application she did herself harm, which increased by the galvanisation, and did not cease until she left herself intirely to the powers of nature, assisted by nourishing food. The patient, who is not capable of forming an opinion on the state of his health, must therefore consult an able physician, one who does not despise nature and new discoveries. By this caution he will never be hurt by the application of electricity or galvanism, which as Boerhaave says (*Elem. Chemiæ, pars. 3. processus* 198.) of another very active remedy: *mirè præstat in multis incurabilibus: at prudenter à prudenti medico, abstinere si methodum nescis.*

IX.

Account of two Musquets of peculiar Construction for the Purpose of quick firing.—W. N.

Two musquets belonging to the earl of Warwick.

THE two pieces are the property of the Rt. Hon. the Earl of Warwick, who has permitted me to examine and describe them: both have the name *Wm. Martindale, Londini*, in gold upon the barrel, and the fashion of mounting them is little different from that at present used. This name might perhaps fix the time of their fabrication nearly, if the registers of the Armourer's Company, or other similar books were to be consulted.

fulted. I should suppose them to have been made about a century ago. Neither of them is in good condition.

One of these pieces is constructed precisely on the principle, and with the same parts, as the pistol belonging to Lord Camelford, which was described in our last Volume, page 250, but the workmanship is not so good. The chamber piece is of brass, and the barrel is $30\frac{1}{2}$ inches long. I think the magazine would hold nine or ten balls.

One resembles Lord Camelford's pistol.

The second piece is on a different construction. It seems to have been invented before the other, probably by the same person. The barrel, which is 39 inches long, has no breech pin, but is perforated clearly through. The usual charge of powder and ball is lodged in a separate iron cylinder, (Fig. 3, Plate VIII.) two inches and three quarters long, and of the same bore as that of the piece itself, namely a little more than half an inch, or 0.54 inch. The metal of this cylinder is about one twelfth of an inch thick, and it is lodged for actual service in the posterior part of the bore of the gun, which is enlarged for the purpose of receiving it (see Fig. 2, letter G.) The gun is provided with five of these cylinders, four of which are lodged in cells in the stock ready for use, while the fifth is supposed to be placed in the gun itself. There is a touch hole in the cylinder which answers to another in the barrel that opens into the pan; and this last is a cylinder fixed to the barrel itself, having an excavation on its upper surface for lodging the priming (B, Fig. 1 and 2.) Immediately behind the breech end of the barrel there is a receptacle in the stock, or rather in the barrel itself (E), for lodging the charged cylinder previous to sliding it into the chamber of the barrel. It is to be observed, that the metal of the barrel is continued back four inches farther than the termination of the bore, and that it is in this strong metallic part that the receptacle E is made. The lock itself is attached to the piece by having the part immediately beneath the shutting face of the hammer, fitted upon the fixed cylindrical pan B, and secured by an end screw so as to move round upon that cylinder. So that it is to be understood, that the lock plate can either remain in the usual position, or may be brought up to a position in which its length shall be rather more than at right angles with the length of the barrel, as in the line H. In this last position the priming is given, and the piece cocked exactly by the same means as in

Description of the other. It cocks and primes at once.

Description of a musquet for quick firing.

Lord

Description of a
musquet for
quick firing.

Lord Camelford's pistol, excepting that here the lock itself performs the motion, instead of the cylinder that carries the pan, which continues unmoved. In Fig. 1. the stud D proceeding from the barrel reacts upon the hammer, and shuts it at the same time that the flat bar proceeding from the cock comes to rest on a notch in the barrel, and causes it to acquire the position of full-cock. Fig. 2. is a section of the barrel seen endwise, and of the lock raised up. When the lock, and the piece to which it is fixed are returned to the usual situation, the receptacle E is filled by a solid piece of iron, which forms a portion of A C, and effectually resists the recoil which the piece C would otherwise undergo. At A is a catch that serves to secure the lock and apparatus in the last-mentioned position; but when the gun has been discharged, and the subsequent operations are to be resumed, that catch is drawn back, and the spring C throws the apparatus of the lock a little way up, in order that it may be then raised, and the empty charge-piece taken out and another put in, &c.

It may perhaps be considered as loose conjecture to suppose these guns were originally invented by the Marquis of Worcester, whose "Century of Inventions" was published about 1655, and hath since been reprinted by various publishers, and in periodical works. Out of this work I copy the following, which will at least amuse the reader.

Accounts of in-
ventions of
quick firing by
the Marquis of
Worcester.

" 58. How to make a pistol discharge a dozen times with once loading, and without so much as once new priming requisite, or to change it out of one hand into the other, and stop one's horse."

This seems to have been the pistol of Lord Camelford before described in our Journal, and the first of the guns here mentioned.

" 69. Another way as fast and effectual, but more proper for carabines.

" 70. A way with a flask appropriated unto it, which will furnish either pistol or carabine with a dozen charges in three minutes time, to do the execution of a dozen shots as soon as one pleaseth proportionably.

" 71. A third way, and particular for musquets, without taking them from their rests to charge or prime, to a like execution, and as fast as the flask, the musquet containing but one charge at a time."

I suppose this last to have been the contrivance, of which the sketch is given in Plate 8.

“ 72. A way for a harquebuzs, a crock, or ship musquet, fix upon a carriage, shooting with such expedition, as without danger one may charge level, and discharge them sixty times in a minute of an hour two or three together.

Accounts of inventions of quick firing by the Marquis of Worcester.

“ 73. A sixth way, most excellent for fakers, differing from the other, yet as swift.

“ 74. A seventh way, tried and approved before the late king (of ever blessed memory) and an hundred lords and commons, in a cannon of eight inches half quarter, to shoot bullets of 64 pounds weight, and 24 pounds of powder twenty times in six minutes; so clear from danger, that after all were discharged, a pound of butter did not melt being laid upon the cannon-breech, nor the green oil discoloured that was first anointed and used between the barrel thereof and the engine, having never in it, nor within six feet, but one charge at a time.”

The diameter of this piece is fully sufficient to carry an iron ball of 64lb. I suppose this contrivance to have been the same as one which was used in China much later, viz. in the year 1725, in the third * year of the emperor *Yong-tcheng*, and made from a model presented by the chief of one of the provinces. Its bore was about one inch, and consequently it threw a ball of less than four ounces of iron, or six of lead. A receptacle was made in the hinder part of the barrel, similar to the musquet just described; but instead of the charge being lodged in the kind of iron cartridge, Fig. 3, Plate VIII. there was an actual breech piece for containing the charge, which piece was lodged in the receptacle when in service, and could be placed or replaced by an handle like that of any other vessel. The gun was provided with four of them. If the Marquis of Worcester's great gun was of this description, the heat would be chiefly produced in the shifting piece, and consequently the gun would, as he says, continue cool. This shifting piece must have been very weighty, even without its charge, and little durable in its fitting.

A Chinese cannon with a number of separate chambers.

“ 75. A way that one man in the cabin may govern the whole side of ship musquets to the number, if need require, of 2 or 3000 shots.

Inventions of the Marquis of Worcester for quick firing.

* *Memoirs concernant les Chinois*, vol. 7. Paris, 1782.

“ 76. A way that against several advances to a fort or castle one man may charge fifty cannons, and stopping when he pleaseth, though out of sight of the cannon.

“ 77. A rare way likewise for musquets, fastened to the pummel of the saddle, so that a common trooper cannot miss to charge them with twenty or thirty bullets at a time, even in full career.

“ When I first gave my thoughts to make guns shoot often, I thought there had been but one only exquisite way inventable, yet by several trials and much charge I have perfectly tried all these.”

X.

Of Rain. By RICHARD KIRWAN, LL. D. F. R. S. President of the Royal Irish Academy, &c. *

Rain is not caused by refrigeration;

DIMINUTION of the temperature of air, replete with moisture, below the degree at which its saturation takes place, whether this refrigeration were caused by rarefaction, or by the intermixture with colder air, has been generally supposed the cause of rain; but this hypothesis in both its branches has been satisfactorily refuted: the first by Saussure † and the second by De Luc ‡. And in fact refrigeration will indeed separate moisture from air cooled below the temperature necessary to hold it in solution. But this separation will terminate, if gradual, in the production of *dew*, as already seen, or, if sudden, in the production of a *cloud*, as in the experiment of Tornea mentioned by Maupertuis, but cannot nor has in any instance produced *rain*.

for this produces dew or clouds.

Rain is caused by the privation of electricity.

Rain is the immediate result of the union of the particles which form clouds; and this union is the consequence of the subtraction of the electric atmospheres which keep them at a distance from each other; and this subtraction is itself the consequence either of the superior attraction of better conductors or of the attraction or repulsion of other clouds through the causes mentioned in the first section of this chapter. The

* Irish Academy, 1802, p. 487.

† Hygrom. § 224.

‡ De Luc *Idées de Meteorologie*, p. 43, &c.

consequence of the *attraction* of clouds is their *incorporation*, and the result of their incorporation is the increased volume of their constituent particles, an increase proportioned to the attraction that produced it; the increased volumes, thus produced, form those drops whose collection we call *rain*. The weight of these being superior to the resistance of air, they necessarily descend, and the cause of their different size is thus clearly discerned.

The repulsion of clouds similarly electrified, and not greatly differing in magnitude, terminates in a bare increase of distance; but, if their magnitudes be much disproportioned, it may terminate in *attraction*, or at least in forcing the constituent particles into closer contact, and thus by increasing their magnitude effect the same result. Development of the effects.

When the attraction takes place between clouds differently and highly electrified, and within what electricians call the *striking distance*, the electric fluid is set free, the coalescence of the nubilous particles is more rapid and complete, and hence the large drops that follow flashes of lightning, or even floods, where the quantities both of vapour and electron are considerable, as between the tropics.

Upon these principles most of the phenomena relative to rain appear to me easily explicable; of these the most remarkable are:

1. That rains are more copious but less frequent in the southern parts of our hemisphere not much elevated over the sea, than in the more northern latitudes. They are more copious when their productive causes occur, evidently because the quantity of suspended vapour is much greater in the hotter than in the colder regions; but they are less frequent, because the variations of wind in different directions which introduce and intermix clouds indifferently electrified are less frequent; this might be proved by instancing the rainy seasons between the tropics, were it not that this illustration would extend this paper to too great a length. Even in moderately elevated situations between the tropics, if insulated and of small extent as the island of St. Helena, it seldom rains. Phenomena of rain.
More copious but less frequent in low latitudes.

2. That, in the temperate latitudes, rains are also more copious, though commonly less frequent, in *summer* than in *winter*, for the reasons already assigned. *Dry* summers are then the consequence of uniform winds, from whatever quarter Most copious though least frequent in summer.

ter they may blow, as *wet* summers are of their variation, particularly if in opposite directions, and if they reach heights sufficient to intermix the clouds that subsisted during the reign of their antagonists.

Southerly winds
bring rain in
Europe.

3. Southerly winds are most frequently accompanied with rain, in most parts of Europe at least, and probably in most parts of our hemisphere; but *northerly* and *easterly*, with clear, dry, and serene weather. Because southerly winds are not only warmer, proceeding from warmer climates, but also more highly electrified than the soil of the colder countries into which they flow. Hence the copious vapours they contain are quickly deprived of part of their electron, and thus converted into clouds; but the superior strata of the atmosphere under which the southern air is introduced, not being supported by air as dense as that which subsisted under them before their introduction, necessarily descend and mix with the inferior southern air; by this intermixture they are warmed, and deprive the clouds already formed and in its vicinity of part of their electron, or perhaps in conformity to the eleventh principle, they are themselves deprived of part of their electron by those clouds, and the vapours they contain are thus converted into clouds; in either way clouds differently electrified must be formed. Hence proceeds their gradual attraction to each other which terminates in those gentle showers that usually accompany this wind. Northerly and easterly winds on the contrary, proceeding from colder countries are less highly electrified than the soil of the countries they invade; and hence from the opposite reasons to those just mentioned they introduce serene weather and a disposition adverse to nubification.

The reasons hitherto adduced to explain the different effects of these different winds, evidently arose from an ignorance of the origin and progress of these winds. It was imagined that southerly winds, flowing into colder countries were suddenly cooled by an intermixture with the colder air of those countries, and that thus their vapours were condensed into rain, yet, even so, this intermixture could only produce clouds and not rain, but in fact this intermixture cannot take place, except with the superior and unmoved strata of the atmosphere, and these alone could not produce numerous clouds, much less copious rains; for the air of the countries into which these
southerly

southerly winds flow, must itself have flown northwards, before the more southern air could enter upon them, as shewn p. 129.

Moreover, southerly winds retain much warmth, and northerly winds are so much colder in the countries into which they are introduced, that their temperature cannot be supposed sufficiently altered to deposit much vapour in the one case, or dissolve much of that already condensed in the other; on the contrary, the warm southerly wind should dissolve the clouds already formed, and the northerly, by their increased cold, should produce many more.

Hence electrical agency must of necessity be recurred to, though I do not doubt but it may be more correctly applied by persons better versed in electrical knowledge than I can pretend to be. Currents of air flowing in different directions at different heights in the atmosphere, must undoubtedly be intimately connected with these effects, but with these we are at present too little acquainted.

4. That a disposition to rain is generally connected with a diminution of the weight of the atmosphere, as is a disposition to serenity with the increase of its weight. Because under the diminished weight of the atmosphere, the eruption of vapours both from land and water is much more copious, a disposition highly favourable to nubification, and the clouds already formed descend lower, are more concentrated, and hence more disposed to react upon and attract each other and thus produce rain. The increased weight of the atmosphere must produce opposite effects and induce a disposition adverse to the production of rain.

The diminished weight of the atmosphere increases evaporation and causes rain.

5. That more rain falls on the surface of the earth than on small elevations above it, as from 30 to some 100 feet: see Phil. Transf. 1769, p. 361; and of 1771, 297; and of 1777, p. 256. This effect seems to me to proceed from the greater stillness and tranquillity of the air near the surface of the earth than at greater elevations. To prove this, it is only necessary to collect the rain that falls in moderate weather on both situations, with that which falls on both, in more stormy weather. If this explanation be just the difference between the quantities collected in both situations will be found greater in the latter than in the former case. This experiment I shall make, and communicate the result to the academy.

More rain falls on the ground than at small elevations.

6. That

Rain on mountains, and plains near them.

6. That the quantities of rain collected at the top of high mountains, and on plains about half a mile distant from those mountains are nearly equal, but in summer there falls somewhat more on the plains, and in winter somewhat less. Phil. Transf. 1771, p. 295. The greater quantity of rain collected in summer on plains appears to me to proceed from the cause just mentioned, the less disturbed state of the atmosphere; but the quantity of rain gained through the influence of this cause is often, in great measure, compensated by that arising from the condensation of fogs formed on the summits of mountains, particularly at night, when neither fog nor rain exist on the plains. But in winter, these mists being much more frequent and denser on the summits of mountains, the quantity of moisture which they deposit is far more considerable.

More rain on the westerly coasts of Europe.

7. That it rains much more on the western coasts of most parts of Europe, particularly if mountainous, than in the interior parts of those countries, or on the eastern coasts of the Britannic islands.

The cause of these phenomena is very obvious. Westerly winds are by far the most frequent in most parts of Europe; these flow from the Atlantic which bounds it, and generally convey marine clouds electrified differently from the soil or land over which they flow, as also from that of the higher clouds under which they reign. Hence proceeds their mutual attraction, and thence rain. This effect must take place principally on the western coasts; when they proceed further, this different electrical state must either cease or be diminished. When the coasts are mountainous, these mountains quickly absorb the electric matter contained in the western blasts, and by collision, condense the vapours they contain, first into clouds, and finally into rain. Hence it often happens that westerly winds, particularly in summer, produce no rain, either because they introduce no clouds, or meet with none differently electrified.

Countries where it never rains.

Peru.

8. That in some countries it scarce ever rains.—This arises from local circumstances, as is apparent in the following instances: 1. It never rains on the plains of Peru from the gulf of Guayaquil, nearly under the equator, up to latitude 23° south, nor is thunder ever heard there, though these plains border on the Pacific Ocean, but they receive a slight dew every night. Bouguer, fig. de la Terre XXIII. 2 Ulloa's

Mem.

Mem. p. 157. 2 Phil. Transf. Abr. 132. Plainly from the following reasons: These plains are entirely sandy, and, consequently, emit very little vapour, being soon parched by the heat that there prevails; consequently the intermixture of marine vapours can produce no effect. Again, the clouds in these tracts are elevated to a great height, and are attracted by the electrical agency of the Cordellieres that border on these plains, to their lofty summits, and there produce copious rains; hence also the sandy and extensive deserts of Arabia ^{Arabia.} and Africa are seldom refreshed by rain. 2. It scarce ever ^{Africa.} rains in Egypt, particularly in Upper Egypt. Now it is to ^{Egypt.} be observed, that Egypt is so situated betwixt lofty mountains that no wind can enter it without passing over them, but the northerly winds, which issue from the Mediterranean; for a southerly wind must pass over the mountains of Abyssinia; an easterly, over those that intercede between the Red Sea and the Nile, and proceeding from the Deserts of Arabia, can convey little or no vapour; and westerly winds must pass over the Deserts of Africa and Mount Atlas. Now the northerly wind does not begin to blow until the month of June, when Egypt is so scorched as to emit scarce any vapour, and the few clouds it may convey are attracted by the mountains of Abyssinia. Towards the middle of June the inundation of the Nile, it is true, commences, and then, as the northerly wind still continues, perhaps rain might be expected, but little attention being paid to it then, we are not informed whether any falls or not, perhaps the clouds then also pass to the mountains of Abyssinia, whither this wind conveys them, and which they deluge with rain; all other winds deposit their moisture on the reverse of the mountains they pass over.

Prognostics of Rain.

When the barometer falls, and the hygrometer rises, *rain* is ^{Prognostics of} announced. _{rain.}

When the barometer rises, and the hygrometer falls, we may expect fair weather, if farther changes do not appear in these instruments, as sometimes there suddenly do.

If the barometer falls and the hygrometer also, *windy weather* will probably follow—particularly if the barometer falls much below its natural height, which in Dublin is from 29,9 to 29,98.

Again,

Again, in the morning the hygrometer is generally higher than at noon, by reason of the difference of temperature; but if it stands *lower at noon* in a greater proportion than the difference of temperature demands, it prognosticates *fair weather*.—On the contrary, if at noon it be higher than it stood in the morning, *rain* may be expected. Sauff. Hygr. p. 356.

To foresee the Rise or Fall of the Barometer in Day-time.

Prognostics of change in the barometer.

Observe it at seven o'clock in the morning, and afterwards at nine and at ten. If it remains steady, its next motion will probably be *downwards*. So also if it falls within that interval of time, the probability is, that it will *sink* still lower. But if it rises within that interval, the chances of a greater rise or of a greater fall are equal.

Again, observe the barometer at one in the afternoon, and again at three; if it remains unmoved, it is probable that it will *rise*, but if it has fallen, the chances of a farther rise or fall are equal.

XI.

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, F. R. S. and M. R. I. A. (Irish Acad. 1802.)

Uncertainty in the analysis of pyrites from the combustion of the sulphur.

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.

The burned sulphur, or sulph. acid, precip. by barytes.

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

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 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 chymists, 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.

Deduction of the sulphur from the acid, by the received proportions did not agree with the experiments.

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 chymists, 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 analysis 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

The real quantity of acid formed by burning its radical, determinable by combining it in some known salt, or else by obtaining the acid dry. The first is uncertain from complexity; the latter in most cases impracticable.

them

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.

Exp. 100 sulphur was acidified by nitric acid;

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

and the acid thrown down with nitrate of barytes.

Results, that 100 parts sulphate of barytes contain 14.5 sulphur. and also the oxygen.

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 have 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 chymical 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 purposes of scientific chymistry.

I. Investigation.
Pure lime was had by solution of marble in muriatic acid, then precip. by carb. of potash, and washed and calcined.

One hundred parts of this lime were dissolved in dilute muriatic acid, in the same platina-crucible, previously weighed; and then sulphuric acid was added in sufficient quantity. Sulphate of lime was precipitated; and the vessel was exposed to a heat, at first gentle, to evaporate the liquor; and then, by degrees, raised to a temperature, which could expel every thing but the combined sulphuric acid, and leave the sulphate of lime completely calcined. The crucible with the salt was then weighed and the augmentation was 76.—It appears to me, that, if we admit (and I see no reason that we should not admit it) that calcined lime and calcined sulphate of lime are wholly exempt from water, it must be clear, that the 76 additional weight were sulphuric acid; and, that the sulphuric acid must in this state, more than in any other, approach nearer to what may be termed, absolutely real acid. One hundred parts of calcined sulphate of lime contain therefore

II. 100 p. lime diss. in mur. acid, were precip. by sulph. acid, and the mur. acid and water driven off by ignition.

The dry sulphate was 76 heavier, which were dry acid.

Lime	-	-	-	-	-	-	57
Sulphuric acid	-	-	-	-	-	-	43

Component parts of sulphate of lime.

100

By the former experiments (those made upon sulphur converted into acid, and then united to barytes) we had the quantity of sulphur, contained in sulphate of barytes. By the latter

III. 100 parts sulph. of lime were dissolved in water

by the addition of oxalic acid and a little muriatic acid.

muriate of barytes being added, threw down the acid in sulph. of barytes weighing 183. This acid was known to be 43 parts.

The proportion in 100 parts barytes is therefore 23.5 acid, and the sulphur in this, by the first exp. was 14.5. Real sulph. acid contains 14.5 sulph. and, the rest = 90 oxygen : or $61\frac{1}{2}$ to $38\frac{1}{2}$ = 100.

latter (those made by directly combining lime with sulphuric acid) we had the proportion of real acid, contained in calcined sulphate of lime. Consequently, by knowing the ratio, that sulphate of barytes bears to sulphate of lime, with regard to the acid in each, we shall arrive at the knowledge of the quantity of sulphur, contained in real sulphuric acid. For this purpose, I attempted to dissolve, in water, 100 parts of sulphate of lime. But finding, in this method of proceeding, a considerable inconvenience arising from the great quantity of liquor, necessary to effect the solution of that salt, I had recourse to the following expedient. Upon 100 grains of calcined sulphate of lime, I poured some oxalic acid, which attracts the basis with an affinity superior to that exercised by sulphuric acid. Oxalate of lime was here formed; but oxalate of lime is soluble in a very small excess of any acid. A little muriatic acid operated a complete solution; and thus a great quantity of sulphate of lime required but little water to dissolve it. Into this liquor, muriate of barytes was poured, and suffered to remain some time, gently heated. By these means, any oxalate of barytes, that might have been formed, was retained in solution, by the original excess of acid; and the entire quantity of sulphate of barytes was deposited. Of the exactness of all these methods, which I used, as the instruments by which I ascertained these results, I convinced myself by various preliminary experiments. After the usual filtration, washing and drying at the gentle heat of a sand-bath, I obtained in one experiment 185, in another 183, and, lastly, in another 180. This difference does not exceed the limits of what all persons, conversant in analytic chymistry, will allow to experiments of this nature. We may therefore take 183 as the mean proportion; consequently, we shall say, that 183 of sulphate of Barytes contain the same quantity of sulphuric acid, as 100 of sulphate of lime; and $183 : 43 :: 100 : 23.5$. Therefore 23.5 is the proportion of acid in 100 of sulphate of barytes. But we have before seen, that 14.5 of sulphur, acidified by nitric acid, form that portion of sulphuric acid, contained in 100 of sulphate of barytes: viz. 23.5. We must now say, that $23.5 : 14.5 :: 100 : 61.5$, and the fourth term will be the proportion of sulphur = 61.5, which combined with 38.5 of oxygen will form 100 of real sulphuric acid.

In

In neither of the proportions, whether it be of the acid contained in the salt, or of the combustible basis contained in the acid, do I agree with the two chymists whom I have quoted. This justly excited some doubts in my mind, and led me to repeat my experiments. Nor should I yet be thoroughly satisfied, If I could not, upon other grounds, than by supposing inaccuracy in them, account for the apparent differences. We must ever expect to see the errors of our predecessors corrected by men, much inferior in abilities; but who, by possessing more certain means, supply the want of genius and invention. At the time in which the experiments were made, that determined the proportion of 33 per cent. of sulphuric acid in sulphate of barytes, it was not known that we had never obtained any barytes pure; and that a considerable portion of carbonic acid resisted the action of every degree of heat that had been applied to carbonate of barytes. The fact was, I believe, first observed by Pelletier; but the method of avoiding the inconvenience was pointed out by Vauquelin. He decomposes nitrate of barytes by lime, and a moderate degree of heat is sufficient to expel all the acid and the water. The chymists, I have mentioned, performed synthetic experiments, by combining, directly or indirectly, sulphuric acid, and such barytes as they imagined to be pure. The constant similarity of their results is sufficient to prove the accuracy of their operations; but working upon an impure substance, they must have been contented with a similarity of error.

Former experiments of others were inaccurate, because barytes had not then been obtained pure,

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.

and because Lavoisier, in burning sulphur in oxygen,

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

might have volatilized some,

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

and imperfectly acidified some,

3d. In rectifying, some acid may come over along with the water; or, *vice versa*, 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.

and been embarrassed by water in rectifying.

Expts. of Tassaert, Thenard, and Guyton.

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 No. 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.

From the determination of the acid in an insoluble sulphate, it becomes easy to extend it to others.

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 domestic art or analytic chymistry 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, they will serve as proofs of their mutual validity; and perhaps demonstrate, that sulphuric acid, as well as water, and some other bodies, may of itself enjoy liquidity, at the temperature and pressure which act upon our globe.

SCIENTIFIC NEWS.

Improvement on Lamps.

A VERY great improvement in the construction of lamps and reflectors, has been lately made by Mr. Nicholas Paul of Geneva, who in conjunction with Mr. Smethurst, an eminent lamp contractor has lately made a public experiment, by illuminating the upper part of New Bond Street.

New lamp of
Mr. N. Paul.

Fifteen only of the new lamps with reflectors, were substituted in place of more than double that number of common ones; the effect of which was, that the street was enlightened with at least twice the usual quantity of light.

This effect is produced, not by the combustion of an extra quantity of oil, but by the scientific construction of the apparatus; the lamp being formed upon the principles of the best air furnace; so that the whole of the combustible material employed, is converted into light and heat, without smoke; and that portion of light which would have ascended and been lost, is distributed to those situations where it is required, by means of reflectors so formed as to distribute the same uniformly over the illuminated space, and to obliterate the shadow that would be formed by the body of the lamp.

These lamps and reflectors are not merely adapted to lighting the public streets, but by various modifications, are accommodated to every circumstance where lamps or candles can be made use of*.

Improvement on the Mode of Watching Cities.

Samuel Day, Esq. of Charter-house, Hinton, in Somersetshire, in the commission of the peace for the county, has directed his attention to the application of a mechanical check upon the diligence and precision of watchmen, labourers, and all other classes of men, whose duty requires that they should attend at certain places at appointed times. His own account of this interesting arrangement is as follows †.

Mr. Day's
watchman's re-
gulator.

* I have received the above from Mr. Paul, but expect to give a fuller account from my own examination, in our next Number, W. N.

† From a printed paper with which he has favoured me. W. N.
From

Mr. Day's
watchman's re-
gulator.

From the concurrent testimony of many Individuals, the present system of watching cities is languid and inadequate: no house is secure, when depredation is determined on, or if there be any security, it is more from the means taken within, than from the watching without. Magistrates have seen and pointed out the defects in the system, and have at times applied what means they could to counteract the evils arising from those defects, but their means have been ineffectual: and householders have been obliged to submit to their risques with no other consolation but that of thinking, that though the plan of watching was bad, it was better than none. In attentively considering the plan, the defects seem to reduce themselves to the following heads; 1st, The too long intervals which watchmen take, between their going their rounds—by which it appears that considering any individual house, that house has not the benefit of actual watching more than ten minutes through the whole night.

2^{ndly}. The Watchman's call of the hours,—from which no service arises to any, but to the depredators of the night, as is obvious to any one who reflects, that of the many hundred housebreakings and street-robberies committed in London in the year, how few of the depredators are detected or taken by the watchmen themselves, from no fault perhaps of these last, but because the thieves have taken advantage of the watchman's repose in his box, and what is more, of the notice which he gives, by vociferation, of his distance or approach, by which they hasten, or delay their attack, or carry off their plunder accordingly.

3^{dly}. The uncertainty of the watchman's doing his duty—who either from intoxication, drowsiness, or indolence, or induced by the badness of the night, may miss his rounds, without detection.

4th. The use of the lanthorn, which answers no purpose but that of adding to the signal of the watchman's approach. And lastly the use of the watch-box, which answers no end but that of promoting drowsiness, and perhaps disease, from the chills which are increased by inaction in a cold damp house.

For these defects it will be asked what effectual remedy can be proposed? To double the number of Watchmen would be the most obvious, but is out of the question when we consider
the

the enormous expence which would attend it, and is unnecessary when we reflect that the usual number of men employed will be sufficient, if any mode can be adopted of fixing and ascertaining their vigilance. The instrument called the *watchman's noctuary*, or *labourer's regulator*, offers a mode for this purpose. By one of such being placed at each end of a watchman's round, it will be ascertained how the man continued his movements through the night, to a nicety of ten minutes at any period of the watch—and the slightest irregularity or omission will be visible the next morning to the inspector or constable, whose office it shall be to open the machine. The test of regular and well-sustained vigilance is given by the Watchman's dropping a token as he passes, every half hour, quarter, or half quarter, into a receiver or cell; each half hour or quarter presenting it's own cell to receive the same, and each cell, like time itself, irrecoverable when passed. No trick or fraud on the watchman's part can counteract the movement of the horizontal wheel formed of these cells, and completing a revolution once in twelve hours. He has no command over it, and each cell (as it moves under the receiver) will be a kind of speaking witness of his diligence and fidelity in going his rounds, answering the next morning to the exact periods he either was or ought to have been there.

Mr. Day's
watchman's re-
gularor.

By this means the calls of the Watchmen, which were only instituted for the purpose of his giving notice of being on his duty, will be superseded; and a considerable expence of animal exertion will be saved to the individual, which might better be converted into that of going his rounds twice, where he now only goes once. Warnings to the nightly thief of timely attack or retreat will likewise be taken away, and if instead of an open the watchman was to carry a dark lanthorn, the robber would have no security whatever in calculating the moment of his depredation, and might be detected in the very outset of his attack, as the slightest sound would alarm the watchman walking in silence, and not drowning distant noise by that of his own voice.

Of the objections to this new mode of ameliorating the watching of cities, the only one seems to the expence of the time pieces,—and considering the number which the larger parishes will have occasion for, this expence will be important;

Mr. Day's
watchman's re-
gulator.

portant *; but let it be considered that it will not amount to more than three pence in the pound of a rate on houses, and that the first will be the sole expence—probably to be saved by diminishing the number of patrols to one half (or less) of what they now are. But trifling indeed will be the expence when compared with the losses sustained by the public in depredations, which, according to a late work on the police of the metropolis, amounts to two millions and upwards.

The best situation for these machines will be at each end of a watchman's round, perhaps certain rounds will require three. They ought to stand in a convenient recess in the street, secured by rivetings of iron, or let into a wall, or placed on a strong bracket within the iron railing of an area; and, if the dial plates were suffered to appear, would be useful in the day as well as in the night: as an eight-day clock it would require no attention to its movement but once a week, and the morning inspector might attend to the slight duty of winding it up.

The annual expence of keeping it in repair is too trifling to be taken notice of.

I have to remark that the same machine will answer in customhouses, warehouses, banking-houses, docks, and every place where vigilance, to be useful, must be exact.

Some Account of a pretended new Metal offered for Sale, and examined by RICHARD CHENEVIX, Esq.

Interesting paper
on the so called
palladium.

AN extremely curious and interesting paper by R. Chenevix, Esq, was read on Thursday the 12th, and the following week, at the Royal Society. It purported to be an inquiry into the properties of a supposed new metal called *palladium*, or *new silver*.

The manner in
which it was
offered to the
public.

This substance announced to the public in a small printed paper, partially circulated, was sold at an extravagantly high price. The notice is as follows; it is given *verbatim*, as the language in which it is written is not unworthy of attention †.

* It is presumed that each time piece will amount to not less than twelve guineas, and each round will require two. D—. I apprehend they may be made for much less than half the money. W. N.

† I received a small piece by the post. The piece of the specimen was at the rate of about one shilling per grain.

PALLADIUM, OR NEW SILVER,

Has these Properties among others, that shew it to be a new noble Metal.

1. It dissolves in pure spirit of nitre, and makes a dark red solution.

2. Green vitriol throws it down in the state of a regulus from this solution, as it always does gold from *aqua regia*.

3. If you evaporate the solution you get a red calx that dissolves in spirit of salt or other acids.

4. It is thrown down by quick-silver and by all the metals but gold, platina, and silver.

5. Its specific gravity by hammering was only 11.3, but by flattening as much as 11.8.

6. In a common fire it tarnishes a little and turns blue, but comes bright again, like other noble metals, when stronger heated.

7. The greatest heat of a blacksmith's fire will hardly melt it.

8. But if you touch it while hot with a small bit of Sulphur it runs as easily as Zinc.

It is sold by Mrs. Foster, at No. 26, Gerrard Street, Soho, London. In samples of five shillings, half a guinea, and one guinea each.

Immediately on the receipt of this paper, Mr. Chenevix went to the place where it was advertised to be sold, and bought a piece which he submitted to the trials proposed by the author of the imposition. Mrs. Foster, it appears, is only the vender, and totally unacquainted with the person who brought the metallic substance and the printed paper to her house.

In every respect but one, the assertions in the paper Mr. Chenevix found to correspond with his own experiments. The specific gravity he found to vary in different pieces from one to two per. cent.

The resemblance which, in many of its properties, this singular substance bore to Platina, induced a suspicion in the mind of Mr. Chenevix that the last mentioned metal formed a part of the compound, if indeed it was a compound body; but the essential characters of difference were so striking and

Examined by
Mr. Chenevix;

Suspicion that it
might contain
platina.

numerous as almost entirely to remove this doubt, or to convince him that palladium was really a simple substance; as its concealed author pretended, and not an imposition upon the public, as Mr. Chenevix has now proved it to be.

It is platina and mercury.

It appears then, from Mr. Chenevix's experiments, that this pretended simple metal is a combination of platina and mercury; and it must excite not only astonishment, but humility in the cultivation of natural knowledge, to find that a combination of two metals, each upon so high a specific gravity as those which are combined in palladium, should produce a compound, the specific gravity of which is less than that of the least weight of the component substances; and, moreover, that mercury whose affinity for caloric had hitherto been found to baffle all attempts to destroy its volatility; could, when combined with platina, become as fixed as any metallic substance, with which we are acquainted. But nature laughs at our theories, and forces us from time to time, by extraordinary discoveries of this sort, to acknowledge that we are her vassals, and that we strive in vain to bring her within our laws.

Process for composing it.

Without entering into any detail of the contents of this paper, which is interesting in many other respects, as well as in the discoveries which it announces, we shall shortly state one of the means, several being given, by which Mr. Chenevix accomplishes the production of palladium; or, at least, of a substance having every chemical or physical property of that supposed metal. He dissolves platina, procured from the ammoniacal salt, in nitro muriatic acid. To this he adds twice its weight, at least, of red oxide of mercury, but if the acid be not saturated he adds more till the whole liquor is perfectly neutralized. This combination of muriate of platina and mercury, is then to be poured into a solution of green sulphate of iron, and the black metallic precipitate which is thereby formed is, when collected, and the excess of mercury driven off, at a red heat, fused in a Black's furnace. It melts into a metallic button at a white heat, and becomes palladium; a compound as it seems, of two parts of platina to one of mercury.

Variations in the compound.

Mr. Chenevix finds it difficult always to combine with platina the same quantity of mercury; and the specific gravity of his palladium varies like that which is sold by Mrs. Foster;

but

but he has found, when the experiment completely succeeds, that palladium, containing one third of mercury, is of the specific gravity of 11.5,—when it contains one fourth of that metal, the specific gravity is one part higher; and the higher the specific gravity, the greater is the proportion of platina, and the less the ductility of the compound. Above 12.5, it so far ceases to have the properties of palladium, that it is no longer soluble in nitric acid.

The remaining particulars of this highly interesting paper we must defer till after its publication by the Royal Society.



A new Method of Painting, proposed by M. CARBONELL, a Spanish Physician, capable of being advantageously substituted for Painting in Distemper.*

THE disagreeable smell perceived on entering rooms newly painted in distemper is universally known. It is only after having left these rooms exposed to the action of the air, that they are habitable. A process which remedies these two inconveniences, deserves therefore to be adopted, and it is with that view that we insert that lately published by *M. Carbonell*, a Spanish physician.

The mode of operation, described by the author, is very simple, it consists in substituting the serum of the blood of oxen, instead of the solution of glue commonly used to mix up the colouring matter.

To succeed, the following methods must be used:

1st. The butcher must be directed to receive the blood of one or more oxen in proper vessels. When the blood is quite cold, that is to say three or four hours after it is drawn, the vessels must be gently inclined, by which means, a clear, amber-coloured liquor will be decanted. This must be passed through a strainer to separate the fragments of the clot which have broken off and been mixed with it.

2d. Reduce to powder quick-lime, which has been sprinkled with a little water to diminish the adhesion of its integrant parts. Pass this powder through a sieve, and deposit it immediately in boxes or bottles well closed.

* Bibliothèque de Sonini, No. V.

3d. When

Manner of using
it, &c.

3d. When these two materials are to be used, the serum is first poured into a wooden or earthen vessel, and mixed with a sufficient quantity of the lime, pulverized as directed above, taking care to preserve the mixture of a proper fluidity to be easily spread with the brush over the surfaces to be covered.

4th. Too great a quantity of this paint must not be prepared at once, because it thickens very fast, and when it has acquired too much consistence it cannot be used. This inconvenience may, however, be remedied by keeping the fluidity at the same point, by the addition of a sufficient quantity of serum, which should always be kept in a vessel near that containing the paint, that it may be used when necessary.

Addition of co-
lour.

5th. The colour so prepared, should be used as quickly as possible.

General re-
marks.

6th. As the colour resulting from the application of this preparation is always white, and as there are circumstances in which a different colour is wished for, this may be obtained, by adding a solar earth of a red, black, green, or yellow species to the serum, at the same time as the lime is mixed. Even a beautiful blue may be obtained by using the blue glass prepared from oxide of cobalt, provided it be reduced to an impalpable powder.

7th. The strength of the composition being necessarily lessened by the addition of the solar colouring matters, the same degree of solidity may be preserved by adding to the serum used for diluting this composition, a few whites of eggs; but particular care must be taken not to put too many, otherwise the paint will be subject to scale off.

8th. This paint can only be applied on wood-work or coatings of plaster, which have not been previously covered with oil-paint.

9th. As one layer will not be sufficient, two or three may be laid on those surfaces which are required to be well painted; but before laying on a second coat, that which has been already applied must be perfectly dry.

10th. A beautiful polish may be given to this paint by friction, in the same manner as to other sorts of paint; it need only be observed, that it is better to grease the cloths used for rubbing it, with clear whale oil, rather than with any other kind of oil.

11th. To

11th. To temper this paint whether white or coloured, it is necessary that the serum should be fresh and not have undergone any change, otherwise the paint will be of a bad quality and not durable.

The preservation of the serum, particularly in the summer, requires much caution, because this fluid has a great tendency to putrefy. It is therefore essential to keep it in a cool place, and to examine before using it, whether it has begun to emit a bad smell; for, in that case, care must be taken not to use it.

For the same reason the vessels in which the serum is kept must be carefully cleaned, and washed frequently with hot water, to remove the spoilt portions of this fluid, with which the pores of the vessels might be impregnated.

M. Carbonell asserts that this paint is so durable, that when prepared with good materials, it may be used to paint the walls of damp houses, without fear of its coming off; an advantage certainly not possessed by distemper painting.

The same author also asserts that he has made many experiments with this same paint, and has obtained satisfactory results; and in all cases so constant, that he doubts not, when it shall be known, that it will be generally adopted. He instances, among other examples, the use which is made of it at Barcelona, as well in the exterior of buildings as within-side, and he has always observed, not only that the sun, the air, moisture or drought, produce no change on it, but also that it is free from bad smell; so much so, that places where it has been used may be inhabited without danger the very day of its application.

One might be disposed, at the first glance, to believe that the new paint proposed by M. Carbonell, is nearly the same with that of milk, described some years ago by Cit. Cadet Devaux. This latter paint may also have succeeded; but on reflecting upon the essential difference which exists between the serum of the blood and that of milk, it will soon be perceived that if the milk paint is good, that of M. Carbonell must be better.

But experience must decide in this respect, and it is to be presumed that a short time will make known which of the two methods deserves the preference.

The work in which M. Carbonell has given the details relative to the preparation and use of his paint with serum, is dedicated

dedicated to the queen of Spain. This work has been printed in the Spanish language: it would be very worthy of being translated into French, which would be the means of giving it greater publicity.

ACCOUNT OF NEW BOOKS.

North London
canal.

Report of the intended North London Canal Navigation; with general Estimate, &c. &c. By R. DODD, Engineer. 1802. p. 23 Quarto.

THE line of this intended canal is to communicate with the Thames near Bell-Wharf, and, passing through Ratcliffe and Whitechapel, is to join a basin near Hackney turnpike, from which the main line is to extend to the river Lea near Waltham Abbey. Two collateral cuts are also to be continued from this basin for the convenience of the eastern and northern suburbs of the city. From Waltham Abbey the river Lea is to be navigated as far as Bishop's Stortford, at which place the intended line will again commence and be continued until it forms a junction with the Cam below Cambridge. From thence that river is navigable to the wash or Lynn deeps.

The advantages to be derived from the proposed measure are numerous, whether considered as facilitating the conveyance of provisions to the capital; as furnishing a safer and cheaper communication from the wharfs and warehouses at the eastern extremity of the town, to the northern and western parts of it; as lessening very much the distance and danger of water carriage between the metropolis and the whole of the east coast; or, as affording the government a ready means of transporting troops and military stores in the event of an invasion of that part of the Island.

The general advantages of Canals as well as those peculiar to the present statute are stated with considerable perspicuity by Mr. Dodd in the present volume.

Description of an improved Armillary Sphere exhibiting at one View the true Solar, or Newtonian System, agreeably to that Order, Harmony, Beauty, and Variety observable in the Heavens; whereby the Science of Astronomy will be familiarly explained. By T. Patrick, Optician, and Manufacturer of Adam's and Senex's Globes, No. 29, King Street, Covent-Garden, 1802. pp. 24. 12mo. Patrick on the armillary sphere,

THE patent nautical angle, whereby a ship's departure, on the nautical meridional difference of latitude, &c. are obtained from inspection, with the greatest accuracy, in plain sailing, oblique, current and mercator, with a sliding scale of meridional parts, graduated to four miles. By the same author, 1803, pp. 19. 12mo. angle.

Theorie des Vents et des Ondes: A Theory of the Winds and Waves, by M. de la Coudray. 1 Vol. in 8vo. Price 3 Francs. Bernard, Paris, 1802.

Exposé des Temperatures: An Account of Temperatures, in which the various state of the atmosphere, and the influence of the air on the human frame, animals and plants, are treated on in the way of aphorism. Price sewed, 5 Francs. Barrois, Paris, 1802.

De L'Electricité Medicale: A Treatise on Medical Electricity, by Cit. Sigaud Lafond. Professor of Natural Philosophy and Chymistry, in the Central School of Chers. Associate of the National Institute, &c. 1 Vol. 8vo. 618 pages, with plates, 6 Francs sewed. Delaplace, Paris, 1802.

Nouveaux Traité sur la Construction et Invention des nouveaux Barometres, Thermometres, Hygrometres, Aereometres et autres Decouvertes de Physique Experimentales: A Treatise on the Construction and Invention of Barometers, Thermometers, Hygrometers, Areometers, and other Discoveries in Experimental Philosophy, by Affier Perricat the Elder, Engineer, &c.: with Meteorological Observations made on the Mountains by many learned Men, and by the Author himself, with comparative Tables. 1 Vol. 8vo. Price 2 Francs, 10 Cents. Paris, 1802.

- New Books:** *Traité d'Optique*: A Treatise on Optics, a new Edition revised and corrected, with Additions, particularly on the Production of the Images in Optical Instruments, on the Achromatic Telescope, and on the Iris, by many Pupils of the Polytechnic School, with all the Plates of the old Edition, and the Addition of many new Ones, by Lacaille. 1 Vol. 8vo. Price 3 Francs.
- Nouvelles Experiences Galvaniques*: New Galvanic Experiments by Nyfsten, Physician, 8vo. Price sewed, 2 Francs. 50 Cents. Levraults, Paris.

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

JULY, 1803.

ARTICLE I.

On the Naturalization of Plants. By JOHN TEMPLETON,
A L. S. From the *Irish Transactions*, Vol. VIII.

THE naturalization of plants is an object of such importance, and a subject that is at present so little understood, that any attempt to extend our knowledge of it, however trifling, may still tend towards improvement, and perhaps serve as a foundation, on which at some future period a more perfect structure may be erected. Many experiments are yet wanting, much remains to be yet done, and, like other branches of knowledge, it will require the united efforts of numbers to bring it to perfection.

The same Almighty hand that formed the earth, has scattered in far distant regions vegetables which the necessity or luxury of man excites him to endeavour to accumulate about his home. And if we at the present time survey the different nations of the earth, we will find that most of them have received great and important benefits by the introduction of foreign plants; and that there is no country, however numerous its collection of plants, but may yet receive considerable advantages by the naturalization of others.

Botany, a science which every one engaged in the study of will readily acknowledge to afford one of the purest of human pleasures, particularly to the botanist.

pleasures, from the introduction of exotics derives its principal support; and certainly whatever tends to facilitate this amiable study is truly deserving of the attention of every philanthropic mind. As all botanists cannot have an opportunity of examining plants in their native soils at proper seasons, it is therefore only by transplanting and cultivating, they can become acquainted with the productions of distant countries; and to cultivate them with success we must derive our information principally from the plants themselves: each has certainly a peculiar character, which were we truly acquainted with, those tedious experiments with each newly acquired species, which now nearly exhaust the patience of all lovers of plants, would be no longer necessary; gardening might then boast of being established on scientific principles, and would then never adopt rules contrary to what nature dictates.

Nor will she, scorning truth and taste, devote
To strange and alien soils, her seedling stems;
Fix the dark fallow on the mountain's brow,
Or to the moss-grown margin of the lake
Bid the dry pine descend. From nature's laws
She draws her own: nature and she are one.

MASON'S ENGLISH GARDEN, B. 111, line 226.

By our present imperfect knowledge of the physiology of plants, we are necessitated to accept of every assistance within our reach: and plants being so immediately connected with every modification of the atmosphere, meteorology, which has hitherto been considered as an object of curiosity, is a source from which we may derive much useful information.

The mean heats
of different cli-
mates consider-
ably govern their
vegetation,

Heat being found to increase or decrease nearly in a regular progression, according to the degrees of latitude, if the latitude of the place where a plant is found be known, by consulting Mr. Kirwan's table of the mean annual temperature of different latitudes*, we may find whether the temperature nearly corresponds with our own. Or, supposing the mean annual temperature of Dublin, lat. 52°, equal 50 of temperature, by adding one for every degree of latitude southward, and subtracting one for every degree of latitude northward, we have the temperature correctly enough for our purpose. For these

* See an estimate of the temperature of different latitudes by R. Kirwan, Esq; page 17.

calculations need not be carried to the greatest degree of correctness, as we know that, if we except a few, plants have a considerable range of latitude; those which cannot bear frost, but with considerable latitude, being found to extend from the northern to the southern verge of the torrid zone, and many of those which grow on the southern limits of the temperate, to approach the borders of the frozen zone. Thus of the Lapland plants near three hundred are found in the environs of Paris *, many of them much farther south, and some, as the water lillies (*nymphaea*), sundew (*drosera*), arrow-head (*sagittaria*), &c. even natives of India †.

In the latitude 44° on the European, and 34° on the American continent ‡, it is not unusual for water to be frozen in January; and as some seasons are much more severe than others, plants growing considerably farther to the southward would be liable to suffer by cold in such seasons, if nature had not provided a remedy by their manner of growing, which enables them to resist the cold of such rigorous seasons; and on this account many of them will be found to thrive, when transplanted nine or ten degrees further north than their native stations. Plants do greatly accommodate themselves to the climate.

From ancient authors it appears that Italy formerly experienced the same degree of cold as the American continent under the same parallel of latitude does at present. Therefore it is highly probable, that Italian plants not introduced into Italy since that time, might in a series of years be changed from their now tender to their once hardy state. Instances in the ancient and present climate of Italy.

But in conducting such an experiment as the naturalization of plants from a southern to a northern climate, so many minute circumstances require our attention, that few people have either time or patience to reach the wished for goal.

But a careful attention to the characters which the plants themselves present, will enable us to proceed with more certainty, and hopes of having our endeavours crowned with success.

By the appearance of the roots and leaves we may nearly determine in what kind of soil the plant is most likely to thrive. In naturalizing plants particular attention must be paid to the soil. Robust roots and fleshy or rigid leaves require a dry soil, ac-

* See Flora Lapponica & Thuillier's Flore des Environs de Paris.

† See Hunters Evlyns Sylva, p. 552.

‡ See Kirwan on the Temperature of different Latitudes, p. 50.

Instructions.

cording to their thickness; stiff clay or sandy loam, as beans, peach, and apple trees; robust spongy roots which have a tendency to mat near the surface with thin leaves, as the alder (*betula alnus*), willows (*salix*), require a somewhat stiff soil with moisture; many of the *salix* genus will not grow with their accustomed vigour, in a light turfy or peat mold soil, for want of the necessary resistance to the roots, although suitable in respect to moisture. Slender, hard and wiry roots, as those of the pine, cistus, &c. require dry, sandy, or gravelly soils. And extremely fine and hairlike roots, as those of erica, hal-mia, rhododendron, &c. must have a soil whose particles will not impede the shooting of their tender fibres, and with a small but regular degree of moisture, that the roots, which by their form cannot resist the slightest drought, may not be destroyed. Plants in a warm climate perspire more than in a cold one; so in a warm they require much, and in a cold one little moisture. Therefore, when transplanted from a warm to a cold climate, they should have a dryer soil, and from a colder to a warmer, a moister one, than their native station.

In transplanta-
tion to a cold
climate the soil
must be dryer;
and the contrary.

In the first case, not being able to perspire the superabun-dant moisture, they will be rotted; and in the last, not having moisture sufficient to supply the loss by perspiration, the growth will be slow, disease and death will follow, unless they receive a timely supply of moisture: by the red or yellow colour of the leaves we may discern the approach of the first evil, and by the stunted growth, and small curled leaves, that of the last. A large quantity of pure circulating fluid seldom injures plants, but stagnant water is certain destruction to almost every vegetable.

Local fibration
or exposure is of
great conse-
quence.

After having determined the most suitable soil, we must afterwards strive to give each plant a proper situation. It is well known, that plants from a shady will not thrive well in an open, nor plants from an open in a shady situation. But the necessity of a natural situation is by no plant more evidently illustrated than by the common myrtle (*myrtus communis*), Even at Glenarm, in the latitude 54° 56' N. it grows with great luxuriance contiguous to the sea, and braves our coldest winters; yet all attempts to naturalize it in an inland situation, several degrees farther south, and in a much more genial climate, have hitherto proved unsuccessful. The olive tree

cannot

cannot possibly be cultivated in the interior of Asia or America, though the latitude be in other respects favourable, nor is it fruitful when excluded from the sea breezes*.

The cause of this may be that near the sea the temperature is more regular than within land, and sudden changes are perhaps unfavourable to evergreens: for we always find those with broad leaves grow best in the shade, and those with narrow leaves on elevated places, in both of which situations the temperature is more regular than in open exposures or confined vallies. And we may often observe plants growing on a somewhat elevated situation, if sheltered from strong winds, less hurt in a severe winter, than others in low warm and sheltered places. For vapour being raised in such places during the day, produces a greater degree of cold by condensation and evaporation in the night, than is experienced in other places where the coldness of the air prevented the rise of vapour during the day. From every observation it appears, that those plants which have the least sap in winter, or whose sap is of a resinous or oily nature, suffer least from cold, and that the principal cause of destruction is the vessels being burst by the freezing of the sap. The hoar-frost, which is always most abundant in vales, tends in a great degree to promote this; for being changed into water, part only of this water is evaporated during the day, the rest remains to be converted into ice by the cold of the ensuing night. This icy covering increases the cold, till the vital principle †, and resistance given by the formation of the bark to the entrance of cold, are overcome, the sap frozen, and at the same time the vessels burst by the expansive force of freezing. This gives the reason why plants in a situation where the sun does not shine on them to thaw the hoar-frost, suffer least in severe seasons; and that plants removed in autumn, unless the shoots are completely hardened, will be more liable to be injured by frost than those of the same species, the descent or fixation of whose sap has received no check by transplanting. Miller remarks, that those plants which were removed in the autumn of 1739, were

The myrtle grows well near the sea, because the temperature varies less.
Other remarks.

* See Saint Pierre's *Studies of Nature*, translated by Hunter, Vol. I. page 607. Dublin edition.

† See Smith's *Tracts relating to Natural History*, page 177, and *Philosophical Transactions* for 1788.

mostly killed by the cold of the ensuing winter, while many of the same species escaped uninjured: and the same may be always observed after every severe winter*.

Sunshine and
shade.

Few deciduous shrubs agree with shade; their natural place is the sunny outskirts of the forest; and when otherwise situated, long and slender branches, with large thin leaves, shew their unhealthy state. From these the climbing plants are easily distinguished by their tendency to contortion, or shooting forth roots or tendrils. To the deciduous climbers a slight shade is not hurtful, as it is only there they can find the necessary support; but in the deep recesses of the forest, the evergreen climbers will spread around their tangling branches, and thrive with wild luxuriance,

Where scarce a sunbeam wanders through the gloom.

THOMSON'S SEASONS.

never appearing, if the soil is sufficiently moist in summer, to be hurt by the thickest shade of deciduous trees.

Shade defends
plants from frost.

The shade is also the natural situation for young plants. By the parental shade they are protected from the drought of summer, and the cold of winter. The more a plant is shaded in winter, and the nearer it is to any large body, the less danger it will be in of suffering from frost. For when a plant or water is so situated as to be overtopped by trees, a great part of the hoar or frosty particles, which would fall on it, is intercepted. Under trees we may often observe water unfrozen, and plants unhurt by the severity of cold, and many retaining their leaves; when water at a small distance is frozen, and plants of the same species, but unshaded, lose their leaves and suffer considerably. As large bodies are not easily cooled, the cold is in some degree mitigated by the stems of large trees. That this is the case may be perceived, the twigs and smaller branches being covered with hoarfrost, when the trunk and larger branches remain uncovered.

* The following experiments may throw some light upon the cause of plants remaining unfrozen, when the surrounding water is frozen. Water enclosed in sealed glass globules remains unfrozen, till the thermometer descends to twenty-four; unsealed ones freeze and burst immediately on being cooled down to freezing water. Oil enclosed in the same kind of globules continued unexpanded, and consequently the globules unbroken, when placed in a mixture of snow and sal ammoniac, and cooled below 0,

Dr,

Dr. Wilson of Glasgow observed, that when a great degree of cold prevailed, palisades extending outward from a house, and also from a large pillar, were covered with hoarfrost, in the most regular manner, according to their proximity or distance from the house or pillar, those next the house or pillar remaining free from hoarfrost, while the more distant ones were entirely covered*. This accounts for the fig-tree shoots, mentioned by Miller, being killed when growing out from the wall, at the same time when the other shoots close nailed to it escaped unhurt †. From this circumstance, most people have affixed those plants which they wish to naturalize to the climate against walls. But when put to a wall, care should be taken that they are sheltered from strong winds, which generally injure the leaves and young shoots, thereby destroying the plant if it is not vigorous. On this account the stems of large trees are preferable for climbing plants, and there they must always be more admired as appearing more natural.

But instead of affixing to walls those plants which require no support, we might cultivate them in pots or boxes, which may be placed in their proper exposure during the summer, and, until their hardiness is determined, removed under the fir or other trees in winter, the thickness of whose shade ought to be proportioned to the apparent tenderness of the plant.

These pots or boxes should be always sunk in the earth, and in winter the surface covered with moss. The drier the ground the better, for sunken pots are liable to be too damp.

The best manner of treating ericas is to place them in a proper situation in the spring, and on the approach of severe weather to fix branches of spruce-fir about them, augmenting the covering as the cold encreases. But as the cold seldom becomes suddenly severe, and a slight frost does them little injury, the erica tubiflora, one of the most tender, bearing about 29° of Fahrenheit's thermometer; it is best not to begin covering too soon, lest, as they are plants that require a very small degree of heat, they should be made to shoot, in which case the slightest frost will perhaps destroy them. To protect herbaceous plants from frost, moss is the most proper covering, by remaining alive through the winter; even after being pulled up, it is not liable to heating and putrefaction, as all dead ve-

* Philosophical Transactions, Vol. lxx. p. 471, 2.

† Miller's Dictionary, Article Ficus.

getable substances are, by which they impart to the plant heat and moisture (the two principal agents which cause vegetation) thereby putting the vegetating powers in action, and filling the plant with sap, at an improper season. In our culture of annuals our only care is directed to placing them in proper soils and exposures. For, that no region of the earth should remain uninhabited, with a liberal hand have the annual plants been distributed; from these do men and animals derive their principal support, and of all the vegetable kingdom, they are best adapted for naturalization. By bringing their seed to perfection in a single season, they are capable of cultivation in a greater variety of climates than any other vegetables. And the seed, being equally undestroyed by natural heat and cold, lies dormant, till genial weather calls forth its latent powers, and urges it to vegetation, whether among the frozen snows of Siberia, or the burning sands of Africa. The *reseda odorata* (mignonette), a native of Egypt, and *helianthus annuus* (sunflower), of Mexico and Peru, ripen their seed, and are thereby perpetuated in our northern latitudes. St. Pierre * says, the peasants of Finland cultivate tobacco (*nicotiana tabacum*), with success, beyond the sixty-first degree of latitude; and that barley succeeds in the very bosom of the north. Amidst the rocks of Finland he saw crops of this grain as beautiful as ever the plains of Palestine produced †.

Criteria by which hardy exotics may be distinguished.

When we endeavour to naturalize plants, that we may distinguish those which offer the fairest prospect of success, a comparison of the exotics with the natives of the soil will be our surest guide. Thus we find, that throughout the frosty regions of the north, the trees, shrubs, bulbous and perennials, complete their shoots, and, before the cold of the winter commences, enclose in hybernacule or scaly buds, the embryo for the coming year. And there is every reason to believe that all exotics will cease growing, and form these buds or hybernacule in the open air during the course of our summer, will not suffer from the severity of our winter. In the hot-house many plants complete their shoots that would not probably do so in the open air, the heat not being sufficient to cause them to grow with the vigour necessary for their completion before winter.

* See St. Pierre's Studies of Nature, translated by Hunter, Dublin edition, Vol. I. page 604.

† See the same Work, page 667.

Nevertheless many of these, if not all, might be brought, by enuring them to the open air, to bear our climate. The *camelia japonica*, *thea viridis*, and *calycanthus præcox*, which were formerly kept in the hot-house, then in the green-house, are now sufficiently naturalized to grow in the open air, and are as little injured with the cold of our winters as either the common or Portugal laurels.

Some exceptions to this observation seem to present themselves. The *robinia pseudo acacia* (two-thorned acacia) does not form external hybernacule, nor complete its shoots, yet grows well in our climate; it however, when the frost comes on early, loses a great part of its summer shoots. Several species of the cistus, that cease growing on the approach of winter, but form no hybernacule, live through our mild winters, but suffer greatly in severe frosts. And others, as the *laurustinus* (*viburnum tinus*) continue to shoot and flower, unless the frost is severe throughout the winter, sustained by their vitality, or that principle whose existence preserves plants unhurt by cold before flowering, but which ceases to exist when the parts of fructification have performed their office*.

Exceptions to the rule.

The *laurustinus* is one of those plants that were introduced to Ireland before green-houses were known, consequently planted in the open ground, and experience shews that it is seldom hurt by frost. By it we find that some plants, which to appearance are not fitted for our climate, do yet outlive our winters; and that, without a knowledge of their native stations, we may sometimes suppose plants to be tender which are really hardy: thus the *laurustinus* is unhurt by frost in Ireland until the cold exceeds that of its own climate. The *buddlea globosa* and *fuschia coccinea* are other instances of plants, that without a knowledge of their native climate, Chili, we would not suppose capable of being naturalized to ours. Yet is the *buddlea* seldom injured by our cold, and the *fuschia*, although killed to the ground by the winter's cold, sends forth abundance of shoots which attain the height of three feet in summer, and are decorated with its elegant flowers, which are larger and much more brilliant than ever they are when confined in a house.

On the *laurustinus*, *buddlea globosa* and *fuschia coccinea*.

* See Smith's Tracts, page 177, and Philosophical Transactions for 1788.

And

The plants of elevated tropical regions would endure the cold of the higher latitudes.

And there is a little doubt but many plants of Chili, and even those which grow within the tropics, when found near the elevation of perpetual frost, would bear the cold of Spitzbergen; for on the tops of mountains are found the plants of the plains of more northern latitudes. Thus is the *salix herbacea* of Lapland and Spitzbergen found on the tops of Mourne mountains at about the elevation of 2,500 feet. On the Serra of Madeira, latitude 32° , $38'$, and elevated 5,162 feet, is found the *erica arborea*, of the neighbourhood of Genoa, latitude 44° , $25'$ *. Therefore as the temperature which prevails at the elevation of 5,162 feet, in latitude 32° , is found nearly to correspond with that of 51° north: the *erica arborea*, which grows at that elevation in latitude 32° , will find a climate suited to its nature in latitude 51° †. But as the before mentioned plants have a considerable range of latitude, it may be cultivated farther north when the soil and situation are favourable. At James Holmes's, Esq. on the eastern shore of Carrickfergus Bay, four miles north of Belfast, there is a plant in the greatest vigour at the present time (July 1799) which has now stood uninjured three as severe winters as Ireland ever experienced, viz. 1794, 5, 1797, 8, and 1798, 9 ‡.

The situation is however favourable, being against a western wall facing the sea, and well sheltered by distant trees from strong winds. And in the neighbourhood of the sea I have little doubt but it would grow still farther north.

The sea air is affirmed to be no obstacle to plantations; but that the open exposure is alone pernicious.

The sea air has generally been reckoned a powerful obstacle to have plantations on its shores. But many observations have convinced me that it is the wind alone which prevents the growth of trees on the shores of the sea. And that on a large plain, where the winds are unimpeded in their course, the same difficulty of raising plantations as on the margin of the ocean will be experienced.

* See Sketch of a Tour on the Continent, by J. E. Smith, M. D. F. R. S. &c. page 200, Vol. I.

† On dividing 15,577, the height of perpetual frost at the equator, by the difference of the temperature above and below, it is found that every 299 feet of elevation lessen heat 1° , and on dividing 5,162 feet by 299, we have 17° , which subtracted from 69 mean annual temperature of latitude 32° , give 52, for the Serra of Madeira, corresponding with the latitude 51° (1.)

‡ See Kirwan's table of mean annual temperatures.

In Foster's account of Cook's second voyage, it is mentioned, that the trees on New Zealand were growing so close to the edge of the water, that the ship's masts were entangled among their branches; and in particular situations the same proximity of trees to the sea might be observed in various latitudes. At Fairhead, the most northerly extremity of Ireland, and exposed to the fury of the northern ocean, the *forbus aucuparia* (mountain ash), *betula alba* (birch), *quercus robur* (oak), with other indigenious trees, grow luxuriantly within 15 or 20 yards of high-water-mark. The reason of this appears to be that they grow upon the lower part of very high land, which causes an eddy to be formed about them when the wind blows from the sea; and by the same high land they are protected from the south and southwest winds.

Instances of trees growing close to the sea where sheltered.

On the top of the rocks the wind rages with the greatest fury, even the grass seeming blighted, whereas below the rocks every plant appears in a thriving state, and some houses situated on the lower part never have their thatched roofs disturbed by the storms. In every other part along the coast where land is of the same form it is covered with thriving wood, but where the land is nearly level for a length of way inland no wood appears, and every hedge is seen never to rise higher than the top of the bank which protects it from the wind. Therefore in order to plant near the sea on a low shore, it is necessary to commence the plantations a considerable way inland, and to allow the young trees to have others several feet taller than themselves behind them: these will have the same effect as high land, for by means of the opposition offered by innumerable stems and branches the force of the wind will be greatly lessened; as we may find by standing on the windward side of a thick wood during a storm, where, if the trees are lofty, the wind is much less violent than on an open plain. In water the effect of this kind of opposition is visible, for if into the bed of a swift stream we drive a number of stakes, the water, although it continues to flow, yet has its velocity diminished considerably.

As plantations thrive near the sea when sheltered, it is proper to begin to plant inland, in order that the first trees may protect those near the coast;

Our first plantations in an exposed place ought always to be of such trees as are natives of mountains for these are fitted by nature to bear the rude blasts of winter, and by the stiffness of their leaves, or flexibility of their footstalks, to re-

and the first plantations ought to be natives of mountains

main

main uninjured by a summer storm. Of the first, we have the various race of pines; of the last, the birch, the aspen and the mountain ash*.

Thus by a careful inspection of the operations of nature, is the hand of man enabled to collect the productions of distant countries around his home, cover the arid heath with waving green, and make the lonely wilderness assume a pleasing gloom.

II.

A Method of remedying certain Inconveniences arising from the Inequality of Heat in large Distillation. By SIR A. A. EDEL-
ERAUTZ †.

In distillation the operations are
1. to evaporate,
and 2. to con-
dense.

The heat must be adjusted to the cooling power.

Strong heat wastes fuel and dissipates part of the vapor.

IT is generally known that every distillation consists of two principal operations; the conversion of the matter under distillation into steam by heat, and the condensation of that steam by cold. In order to the speedy accomplishment of these two objects, and without any unnecessary expence of fuel, a perfect equilibrium must be established between the evaporating heat and the condensing cold; that is to say, supposing this last to be constant, as it may be practically made, (a given quantity of water at a fixed temperature passing through the cooler in a given time.) The heat must then be so regulated that the quantity of steam produced shall be neither more or less than can be condensed in the same time, by the cold applied to it. A want of attention to this circumstance produces the two following inconveniences, particularly in the distillation of spirituous liquors.

1st. If the fire be too brisk, a considerable quantity of the confined steam will pass from the worm into the open air, occasioning a loss of the matter under distillation, and a waste of fuel.

* Among the rocks of Agnew's Hill in the county of Antrim, I found the populus tremula (aspen tree) growing luxuriantly on the eastern face, at about the elevation of 1,450. And on the top of Slemish, the sorbus aucuparia (mountain ash) exposed to every storm at the elevation of 1,398 feet.

† Annales de Chimie, XLV. 297.

2nd. If the fire be suffered to slacken too much, the condensation produces a vacuum in the worm and the alembic, which not being supplied in the same proportion by fresh steam, occasions the outer air to enter, which renders the evaporation and condensation more difficult, and when it is, at last, driven out, it takes with it a portion of the steam, thus occasioning a loss of the matter under distillation, as well as a loss of time.

Low heat causes the outer air to enter, which when expelled carries out vapor, &c.

In order to remedy these defects, and at the same time to furnish a simple method of registering the actual heat, I have thought of the following instrument, which is applicable to any distilling apparatus, and is nothing more than an application of known principles of theory to actual practice.

Instruments for remedying these defects.

a, b, c, d, Fig. 1. is a bent tube of copper or glass in separate pieces, with a bulb at *b*; the upper end of the tube *a* may be attached to the worm by means of a screw. The length *b c, c d* is of four feet, and the capacity of the bulb *b*, is a little larger than the whole capacity of the tube *b c d*. The distillation being begun, the condensed steam will pass by *a* and the bulb *b*, into the tube *b c d*; and when its two arms are full, the liquor will run out at *d*, into the vessel intended to receive it.

An inverted syphon of condensed fluid, and a mercurial gage which allows of some latitude of internal or external pressure, without communication from without.

These two arms, therefore, remain full during the whole of the distillation, and in this consists the remedy to the inconvenience above-mentioned; it is evident that if the fire becomes too quick, the uncondensed steam cannot force a passage to the outer air, in order to dissipate itself, till it has driven out all the liquor contained in the tube *b c*, and has overcome the pressure of a column equal in height to *c d*. Again, the outer air cannot enter to fill up the vacuum produced by the slowness of the fire, but by driving back the column *d c*, and overcoming the pressure of the same height. Now this column being four feet, gives range and time sufficient for the operator to regulate the fire accordingly. If the tube *b c d* were made of glass, we should only have to observe the level of the liquor in the two arms, the fall in *b c* would indicate that the heat should be diminished, and in *c d*, that it should be increased: but tubes of this length being subject to accidents, I prefer attaching to *c* a small regulator of glass, *e f e*, the two arms of which, *e f*, each of three inches long, contain mercury, and which rising in them alternately, will indicate the state of the heat and the steam with sufficient accuracy. This regulator may be placed in a bottle or flask, which will

protect

protect it from injury. Between it and the worm is a cock *g*, which at first communicates with the outer air, but after having produced a strong heat, the steam is seen escaping at *g*; by turning it a communication is opened between the worm and the regulator, which then begins its operations. The bulb *b* prevents the liquor pressed by the outer air from rising into *e f e*, and the alembic. It is superfluous to add, that, of whatever form the capital may be, it should be well luted, in order to prevent the access of the outer air.

III.

Account of a Time-Piece for registering the Attendance of Watchmen, and for similar Purposes; constructed by Messrs. BOULTON and WATT, for the Right Honourable the MARQUIS OF EXETER. In a Letter from his Lordship.

To Mr. NICHOLSON.

SIR,

HAVING seen Mr. Day's account of his watchman's regulator in your Journal, dated June 1, 1803, I think it may be proper to inform you that a clock for a similar purpose has been invented by Messrs. Boulton and Watt, Birmingham, which costs no more than thirty shillings. I have had two of them above four years. They go eight days, and have a face like a clock, but do not strike. The dial goes round, and the hour finger is fixed; round the edge of the dial are moveable iron pins, corresponding with the quarters in each hour. A small hammer placed behind the hour finger, when moved downwards, pushes into the dial, one of the pins which happen to be under it at the time, which pin remains so abased until the dial nearly returns to the same place, when by an inclosed plane the pin is raised up into its first position. This gives time to have the machine examined in the morning, to see how many pins have been struck, and at what time they were pushed downwards. The hammer is moved by the pulling of a chain with a handle, like house-door bells, which, by cranks and wires is attached to it. I have one in my library, the handle is out of doors. The other machine is placed in a building

ing at the other end of my premises. I have always two watchmen every night, and they go the round every half hour.

I am,

SIR,

Your obedient humble servant,

EXETER.

Burghles, June 9, 1803.

IV.

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

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

Honey is sometimes of such a nature as to injure the constitution.

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

It always partakes of the nature of the flowers whence it is gathered.

I must observe, that in these hints I do not mean to include among the disagreeable consequences of the eating of honey, the occasional effect of its purging: for although, as I shall presently observe, a purging is one of the common effects of the poisonous honey, yet the most innocent honey will often

Common honey.

* American Transactions, V. 51.

induce

induce the same state of the body, when it is eaten in large quantities, or when it meets with an irritable state of the bowels.

Symptoms produced by deleterious honey.

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

They are seldom fatal.

Death is very seldom the consequence of the eating of this kind of honey ‡. The violent impression which it makes upon the stomach and intestines often induces an early vomiting or purging, which are both favourable to the speedy recovery of the sufferer. The fever which it excites is frequently relieved, in a short time, by the profuse perspiration, and perhaps by the foaming at the mouth. I may add, that as the human constitution resists, to an astonishing degree, the effects of the narcotick and other poisonous vegetables that are best known to us, so we need not wonder, that it also resists the effects of the deleterious honey, which is procured from such vegetables.

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

† The *Kalmia latifolia*.

‡ We shall afterwards see, that not one of Xenophon's men died from the deleterious honey which they had eaten, in large quantities, on the shores of the Euxine-Sea.

It deserves to be mentioned, that the honey which is formed by two different hives of bees in the same tree, or at a little distance from each other, often possesses the most opposite properties. Nay, the honey from the same individual comb is sometimes not less different in taste, in colour, and in its effects. Thus one stratum or portion of it may be eaten without the least inconvenience, whilst that which is immediately adjacent to it shall occasion the several effects which I have just enumerated.

Respecting the external signs of unwholesome or poisonous honey.

I have taken some pains to learn what are the signs by which the deleterious honey may, at first view, be distinguished from innocent honey. I am informed that there is no difficulty in the matter.

The poisonous honey is said, by some, to be of a crimson colour; by others, it is said to be of a reddish-brown colour, and of a thicker consistence than common innocent honey.

These are the signs by which, I am told, the most experienced hunters, in the southern parts of North-America, are enabled to distinguish pernicious from innocent honey.

On a subject such as this, I feel every disposition to pay a good deal of deference to the experience of an American hunter. Even philosophers may obtain much useful information from hunters, however wandering their life, however rude their manners. It is in the power of our hunters to enrich natural history with many important facts. But we ought not, I presume, to confide implicitly in every thing they tell us.

I have good reasons for doubting whether the signs which I have mentioned will enable us, in every instance, to determine whether honey be poisonous or innocent.

The external signs are not very conclusive.

The honey of the bee, undoubtedly sometimes partakes of the colour of the flowers from which it is gathered. The bees gather honey from many flowers of a crimson colour, and from many flowers whose colour is a reddish brown. In these cases, it is probable that the honey will sometimes borrow, in some degree, the colour of the flowers. Yet there are many crimson-coloured and reddish-brown coloured flowers that are perfectly innocent. The honey obtained from them will, I presume, be innocent also. Mr. Bruce says he was surprised to see, at Dixan, in Abyssinia, "the honey red like blood, and nothing," he remarks, "can have

White honey.

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

* Travels to discover the source of the Nile. Vol. V. or Appendix, p. 151. Quarto edition.

† Fauna Suecica.

‡ Burns.

§ Dr. Withering says bees extract a great deal of honey from the flowers of the erica vulgaris, or common heath, and he remarks that "where heath abounds, the honey has a reddish cast." A botanical arrangement of British plants, &c. Vol. 1st.

though

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

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

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

The poisonous wild honey is ſcarcely to be diſtinguiſhed but by cautious trial.

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

Said to be rendered mild by boiling and ſtraining.

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

Hitherto, I have not been able to obtain any certain information concerning the means to be purſued in the treatment of perſons labouring under the effects of the poisonous

Remedies.

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

† Mr. William Bartram.

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

The bees themselves are probably injured by the plants which afford noxious honey.

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

* Hyoscyamus niger.

† Datura stramonium.

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

§ Linnæus, speaking of this plant, says, "Nulla, excepto *Meliantbo*, copiosiori melle scatet planta, quam hæc; sed apes id non colligunt!" *Prælectiones in Ordines Naturales Plantarum*. Edidit Giseke. p. 297. Hamburgi, 1792.

¶ *Nerium oleander*.

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

elm countries they do not thrive; but the truth of which I am yet to learn*.”

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

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

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

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

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

* Silva: or a Discourse on Forest-trees, &c. p. 133 and 134. Dr. Hunter's edition.

† See Transactions of the American Philosophical Society. Vol. III. No. 31.

‡ Flora Caroliniana, p. 138.

About

Narrative of
poisonous honey
from those vege-
tables.

About twenty years since, a party of young men, solicited by the prospect of gain, moved, with a few hives of bees, from Pennsylvania into the Jerseys. They were induced to believe that the savannas of this latter country were very favourable to the encrease of their bees, and, consequently, to the making of honey. They, accordingly, placed their hives in the midst of these savannas, which were finely painted with the flowers of the *kalmia angustifolia*. The bees encreased prodigiously, and it was evident that the principal part of the honey which they made was obtained from the flowers of the plant which I have just mentioned. I cannot learn that there was any thing uncommon in the appearance of the honey: but all the adventurers, who eat of it, became intoxicated to a great degree. From this experiment, they were sensible that it would not be prudent to sell their honey; but, unwilling to lose all their labour, they made the honey into the drink well known by the name metheglin, supposing that the intoxicating quality which had resided in the honey would be lost in the metheglin. In this respect, however, they were mistaken. The drink also intoxicated them, after which they removed their hives.

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

The *kalmia*
latifolia poison-
ous to animals,

II. That the *kalmia latifolia*, known in the United States by the names of laurel, great-laurel, wintergreen, spoon-haunch, spoon-wood, &c. is also a poison. Its leaves, indeed, are eaten, with impunity, by the deer*, and by the round-horned elk †. But they are poisonous to sheep, to horned-cattle and to horses. In the former of these animals, they produce convulsions, foaming at the mouth, and death. Many of General Bradock's horses were destroyed by eating the leaves and the twigs of this shrub, in the month of June 1755, a few days before this unfortunate General's defeat and death. In the severe winter of the years 1790 and 1791, there appeared to be such unequivocal reasons for believing that several persons, in Philadelphia, had died in

* *Cervus Virginianus* of Gmelin.

† *Cervus Wapiti, mibi.*

consequence of their eating our pheasant *, in whose crops and infecting the leaves and buds of the *kalmia latifolia* were found, that their flesh. the mayor of the city thought it prudent and his duty, to warn the people against the use of this bird, by a public proclamation. I know that by many persons, especially by some lovers of pheasant-flesh, the circumstance just mentioned, was supposed to be destitute of foundation. But the foundation was a solid one. This might be shown by several well-authenticated facts. It is sufficient for my present purpose to observe, that the collection of a deleterious honey from the flowers of this species of *kalmia* gives some countenance to the opinion, that the flesh of pheasants that had eaten of the leaves and buds of this plant may have been impregnated with a pernicious quality †.

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

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

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

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

IV. The *andromeda mariana*, or broad leafed moorwort, *Andromeda mariana.* is a very common plant in many parts of North America.

* *Tetrao Cupido* of Linnæus.

† It is not a new suspicion, that the flesh of animals that have eaten of the leaves, &c. of deleterious vegetables is sometimes endued with a poisonous property. Georg. H. Welschius, a very learned German writer, quoted by Dr. Haller, (See *Historia Stirpium Indigenarum Helvetiæ Inchoata*. Tom. I. p. 433.) says, that the flesh of a hare which was fed with the leaves of the *rhododendron ferrugineum* proved mortal to the guests: This species of *rhododendron* is a native of Switzerland, Siberia, and other parts of the old world.

The

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

Plants affording
poisonous honey.

I have already observed, that it is highly probable, that the American poisonous honey is procured from the flowers of a considerable number of the plants of the country. I have mentioned but a few of them. But there are many others which I have some reasons for suspecting are also capable of affording an injurious honey. Indeed, every flower that is poisonous to man, and is capable of affording honey, may produce an honey injurious to man; since the properties of this fluid are so dependant upon the properties of the plants from which it is procured. There is, therefore, more poetry than philosophy in the following lines of Mr. Pope:

“ In the nice bee, what sense so subtly true,

“ From pois'nous herbs extracts the healing dew.”

ESSAY ON MAN, Epistle I. lines 211 & 212:

I have been informed that, in the southern parts of our continent, there is a plant, called hemlock, from the flowers of

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

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

which

which the bees prepare a honey that is poisonous. The ^{Plants affording} flowers are said to be yellow, and the root a deadly poison. ^{poisonous honey.} I do not know what plant this is. Most probably, it is some umbelliferous plant, perhaps a cicuta, an angelica, or a scandix.

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

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

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

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

Plants affording
poisonous honey.

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

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

III. *Datura stramonium*. This plant is known by a variety of names, such as Jamestown-weed, gymfin; stink-weed, French-chefnut. Its active and poisonous properties are now pretty generally known. Children have often been injured by eating the seeds. The tube of the flower contains a considerable quantity of honey. This honey is bitter, and has much of the poisonous smell. Bees quaff it. But admitting that it is of a poisonous nature, it does not follow that our *cultivated* bees (if I may be allowed to use this expression) will

will collect so much of this honey as to prove injurious to those who eat of it. But, in particular places, where this plant has been permitted to increase to a great degree, large quantities of honey may be collected from it: and I cannot help suspecting that the use of this honey may prove injurious*.

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

Pliny has also taken notice of this poisonous honey. "In some years," says the Roman naturalist, "the honey is very dangerous about Heraclea Pontica. It is known to authors from what flowers the bees extract this honey. Here is what we have learned of the matter. In those parts, there is a plant called ægolethron, whose flowers, in a wet spring, acquire a very dangerous quality, when they fade. The honey which the bees make of them is more liquid than usual, more heavy, and redder. Its smell causes sneezing. Those who have eaten of it sweat excessively, lie upon the ground, and call for nothing but cool drinks ‡". He then makes the very remarks which I have quoted from Dioscorides, whose words, indeed, as Mr. Tournefort observes, he seems to have merely translated. The following remark, however, appears to be-

* See the late Dr. Samuel Cooper's inaugural dissertation on the properties and effects of the *Datura Stramonium*. p. 33. Philadelphia, 1797.

† Dioscorides, as quoted by Mr. Tournefort.

‡ C. Plinii Secundi *Naturalis Historiæ*, Lib. XXI, cap. xiii.

Ancient accounts of noxious honey.

long to Pliny: "Upon the same coast of the Pontus, there is found another sort of honey, which is called mœnomenon *, because those who eat of it are rendered mad. It is supposed, that bees collect it from the flowers of the rhododendros, which is common among the forests. The people of those parts, although they pay the Romans a part of their tribute in wax, are very cautious how they offer them their honey †."

The Greeks and the Romans have often described the various plants that were known to them, in such dark and obscure terms, that the botanists of modern times are frequently at a loss to determine, not merely the species but also the genus the ancient writers have mentioned. With respect, however, to the plants which I have just mentioned, the difficulty does not seem to be great. Mr. Tournefort has, I think, shown, in a very satisfactory manner, that the ægolethron of Pliny in the chamæ-rhododendros pontica maxima, Mespili folio, flore luteo of his *Institutiones*, a plant since described by Linnæus, and by other botanists, by the name of azalea pontica. Mr. Tournefort has likewise shown, that the other plant called by Pliny rhododendros in his chamærhododendros pontica maxima, folio laurocerasi, flore cœruleo purpurefcente ‡. This is the rhododendron ponticum of Linnæus. It is considerably allied to the azalea pontica.

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

When the army of the ten thousand had arrived near Trebisond, on the coast of the Euxine or Black Sea, an accident befel the troops which was the cause of great consternation. "As there were a great many bee-hives," say the illustrious general and historian, "the soldiers did not spare the honey. They were taken with a vomiting and purging, attended with a delirium, so that the least affected seemed like men drunk, and others like mad men, or people on the point of death. The earth was strewed with bodies, as after a battle; not a person, however, died, and the disorder ceased the next day, about the same hour that it began. On the third and fourth

* From the Greek verb, *Μαίνωμαι*, insanio.

† Ibid.

‡ *Institutiones*, &c.

days, the soldiers rose, but in a condition people are in after taking a strong potion *.”

Ancient accounts of noxious honey.

The same fact is recorded by Diodorus Siculus.

Mr. Tournefort thinks there is every probability that this poisonous honey was sucked from the flowers of some species of *chamærhododendros*, or *rhododendron*. He observes that all the country about Trebifond is full of the species of this plant, and he remarks that Father Lambert, Theatin missionary, agrees that the honey which the bees extract from a certain shrub in Colchis or Mingrelia, is dangerous, and causes vomiting. Lambert calls this shrub *oleandro giallo*, or the yellow rose-laurel, which Mr. Tournefort says is, without dispute, his *chamærhododendron pontica maxima*, *Mespili folio, flore luteo* †; the *azalea pontica*, already mentioned.

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

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

Neu propius teclis taxum sine. —————

GEORGIC. Lib. IV. l. 47.

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

Sic tua Cyrnæas fugiant examina taxos. l. 30.

The honey of Corsica, as Dr. Martyn strongly expresses it, “infamous for its evil qualities ‡.”

The

* These are nearly the words of Mr. Tournefort’s translation. I am sorry that I have not the original work of Xenophon at hand.

† See Tournefort’s *Voyage into the Levant*. Vol. iii. p. 68. English translation. London, 1741.

‡ See his Translation of the *Georgics* of Virgil, note to line 47, in book IV. Dr. Martyn’s criticisms and annotations always demand attention. I greatly doubt, however, if the *taxus* of Virgil be the common yew, or any species of that genus. Martyn himself allows, that “it does not appear from other writers (beside Virgil), that Corsica abounded in yews.” I have been assured, that the yew is not an indigenous vegetable in that island, and that it is even rare among the foreign vegetables. It may, indeed, be said, perhaps it was common in the time of Virgil. I would observe, that the yew is much less poisonous than has been commonly supposed

Advantages of
a proper atten-
tion to the plants
near bee hives,
&c.

The raising of bees, for the purposes of procuring their honey and their wax, may, at some future period, become an object of great importance to the United States. Surely then, it would be a matter of consequence to attend to the cultivation or preservation of those vegetables which furnish an innocent and a well-flavoured honey, and a good wax. But even in a more limited view of the subject, some knowledge of these vegetables seems to be indispensibly necessary. And in the new settlement, whither the settler has carried his bees, where improvements are still very imperfect, it cannot be deemed a trivial task to have pointed out some of those vegetables from which an injurious honey is obtained.

The ancients, who, in some respects at least, were equal to the moderns, appear to have paid much attention to this subject. Virgil * and Columella have both told us what plants ought to grow about apiaries. It is unnecessary to repeat, in this place, what the two Roman writers have said on the subject. The *Georgics* of the Mantuan poet are in the hands of every man of taste; and the work of Columella † *should* be read, wherever agriculture engages the attention of gentlemen.

The proper management of bees may be considered as a science. It is not sufficient that bees merely make honey and wax. Their honey may be injurious or poisonous, and their wax may be nearly useless. To assist, and to direct the labours of these little insects, the knowledge and the hand of man are

posed. I know not that any modern writer has pretended that the bees procure a pernicious honey from its flowers. These facts give rise to my suspicion, that the taxus of Virgil was not the yew, or taxus of the modern botanists. If not the yew, what vegetable was it? Perhaps, the *buxus virens*, or box. This vegetable abounds in Corsica, where to this day it is known by the name of *taxo*. The gentleman from whom I received this information assured me, that the bees of Corsica are very fond of the flowers of the box, and that the honey from this source is reputed poisonous. The box is, unquestionably, a poisonous vegetable. But there is still a difficulty in the case. Virgil mentions both taxus and buxus. I think there can be no doubt that his buxus (see *Georgic*, lib. II. l. 449.) is the buxus of the modern botanists.

* See *Georgicorum*, lib. IV. l. 30.—32.

† *De Re Rustica*, libri XII.

required. Let, then, this interested being be at least attentive to his own benefits and pleasures. Let him carefully remove from about the habitations of his bees every fetid or poisonous vegetable, however comely its colour or its form. In particular, let him be careful to remove those vegetables which are noxious to himself. In place of these, let him spread the "marjoram and thyme," and other plants, "the love of bees*," and his labours will be rewarded. He may, then, furnish his table with an honey not inferior to that of Mount Hermettus, or of Athens; nor to that of Sicily, to which Virgil has so handsomely alluded in the seventh Eclogue :

*Nerine Galatea, thymo mihi dulcior Hyblæ,
Candidior cygnis, hederá formosior albá.*

L. 37, 38.

V.

On the Composition and Use of Chocolate. By CITIZEN PARMENTIER †.

AMONG the substances with which the conquest of the new continent has enriched the old, must be placed the cocoa or cacao nut. From this fruit, or rather from this seed, it is that the Mexicans have, from time immemorial, prepared their favourite beverage, chocolate: This consists of cocoa roasted and bruised, which they mixed by stirring water, and added the flour of maize to give it consistency, together with pimento to flavour it. The existence of sugar was unknown to them, because the cane, which is indigenous to India beyond the Ganges, was not brought to Saint Domingo by Desficcaca until 1506; and because Balastro was the first who submitted this plant to the operation of the mill, in America ‡.

Preparation of chocolate by the Mexicans.

Importation of the sugar cane into the West Indies and America.

The Spaniards partook of the enthusiasm of the Mexicans in the wonderful properties which they ascribed to chocolate; and its preparation, notwithstanding the little skill it requires, soon became in their hands an object of speculation; they kept

The preparation of chocolate kept secret by the Spaniards.

* Armstrong.

† From the *Annales de Chimie*, No. 134, tom. XLV, 139.

‡ See an excellent sketch of the only history of Sugar, by Dr. Falconer, in our *Journal*, quarto series, II. 136.

it secrete, and fold, and continue to sell to other nations, a simple paste of cocoa roasted, bruised and brought into the form of cylindrical rolls, for chocolate.

Sugar added to it.

The use of sugar having been rendered more common in Europe by the introduction of the cane into our colonies, it soon became the universal condiment, and the Spaniards did not fail to make it an addition to the preparation of chocolate, for the purpose of correcting its unpleasantness to those who were unaccustomed to this beverage; but it was not till some time afterwards that other nations made the discovery that cocoa was its base, sugar its seasoning, and cinnamon and vanilla its aromatic ingredients. This discovery became a source of wealth to a number of individuals, who in their turn made a mystery of it; hence arose the chocolates of Italy, of Portugal, and of Spain, which, compared with those prepared in Paris, and the other towns of France, possess no superiority. Why should we allow those countries to have such an advantage over us? The ingredients which form chocolate are not cultivated in them any more than with us, we all obtain them from the same sources and at the same charges. At Naples, Lisbon, and Madrid, the chocolate of France is in great esteem; but it is an established maxim in all countries that one shall be thought a prophet at home, and this proverb may also be applied to chocolate.

Discovered by other nations.

Extraordinary virtues attributed to it.

I shall not enquire whether chocolate really deserves the encomiums bestowed on it, or whether it be advantageous in all the cases in which its use is recommended. If we credit the writings of the physicians of the two worlds, nature has formed the cocoa as a remedy for all the evils that afflict the human race, and as the means of prolonging life beyond its accustomed limits. But it is difficult to guard against exaggeration, particularly on the subject of productions which frequently have no other value but that arising from their growing at a distance from us, and under another hemisphere. From experience and observation we have learned nothing more than that chocolate is an agreeable aliment, light and of easy digestion; it is for that reason given particularly to convalescents, to persons of delicate habits, and to the aged. In fact, to obtain its good effects uniformly, it is necessary that the ingredients of which it is composed should be properly selected, well prepared, and intimately mixed, so as to form a soft, homogeneous paste.

Whether well founded.

On the other hand, on considering the nature of the substances which form chocolate, and the mode in which their combination is produced, we are forced to acknowledge all the characteristics of a preparation truly pharmaceutical, and to allow that it requires a degree of care and attention, of which ordinary labourers are incapable without superintendance and direction.

It is a pharmaceutical preparation,

The selection of cocoa is not sufficient to give that quality to chocolate which it ought to have: this fruit must be sifted, then roasted by a gentle heat to deprive it of its humidity and to develop the odour and flavour which belongs to it; it must be peeled grain by grain to separate the bark, the germ or radicle and those nuts which appear to be spoiled; the cocoa, thus picked, must be ground for a long time with a certain quantity of the sugar on a stone, gradually heated on a sand-bath; the remainder of the sugar is not to be added until the second bruising, and the aromatic ingredients, which are pounded with the sugar, are not to be put in till near the close of the operation. It is then divided into a mass of a suitable weight, and fashioned in tin moulds, whence it is taken when cold to be wrapped up, care being taken to keep it in a dry cold place: the winter is the most favourable season for this operation.

and requires to be carefully made up.

Since the various species of cocoa which are found in commerce could not, were each treated separately, produce chocolate of a good quality; it is usual to mix them in proportions which are determined by the price intended to be charged, and by the taste or fancy of the consumer; when it is flavoured by cinnamon alone, it is called *chocolat de santé*: it bears the name of chocolate of half, one, two, or three vanilloes, when there are a half, one, two, or three pods of that fruit in a pound of it, because the weight of the pods are unequal.

Preparation of chocolate for commercial purposes.

Chocolate thus composed is infinitely preferable to the crude paste of cocoa which is still prepared in the Antilles, and which the Spaniards continue to send to us, under the pretence that it is more commodious in that state, because the sugar and aromatic ingredient may be added in the requisite proportion when used, but the chocolate thus made does not possess a perfect homogeneity. The butter of cocoa constantly rises to the surface; for when the sugar is bruised with cocoa in the stone, during the mixture, a more intimate combination of all the

Superior to that brought from the Antilles.

principles is effected. In a word, the chocolate thus obtained is more completely dissolved, more miscible in water and consequently more easy of digestion: on the other hand, although cocoa does not become rancid so easily as the fruits which are analogous to it, it may be apprehended that from being exposed too suddenly to the action of the heat necessary to bruise the nut, the oil and the butter which it contains being set free, it may lose part of its mildness and become acrid and heating.

Of the Frauds committed in the Fabrication of Chocolate.

Frauds in the
manufacture.

It is, no doubt, unfortunate, that in the commerce of food, in which the love of humanity, that sentiment so pure and so natural, might seem to banish all want of confidence, and every sordid interest, we nevertheless discover that frauds are multiplied in proportion as the objects pass into different hands to acquire their alimentary or medicinal properties. It is not my intention to attach personal blame to any individual, but the improper conduct of some manufacturers render it necessary to give precautions, which are, no doubt, useless with regard to others, who fulfil the duties of their useful profession with a degree of fidelity worthy of the best of times.

Partly noticed
by others.

Baumé, in his Elements of Pharmacy, and *Demachy*, in his Art of the Liqueur-maker, have discovered part of the abuses which are committed in the sale of chocolate. I shall feel happy if, by adding some observations to what has been already published by those chemists, I should succeed in preserving the just reputation it deserves, and which it has lost in the opinion of some individuals only by the faults of the preparation, or by the addition of materials foreign to its composition.

Instance of the
bad effects arising
from foreign
ingredients.

Among the number of persons I have heard complain against chocolate, I shall instance a lady of a tolerably good constitution, to whom it had been prescribed as a medicine; the ill effects she experienced from it gave me reason to suspect the purity of her chocolate: I examined it, and found that it contained a quantity of farinaceous matter. Now this substance had been expressly forbidden by her physician; I persuaded her not to discontinue the use of chocolate, but to procure it elsewhere: the uneasiness, the oppression, and the acidity which had tormented her, soon disappeared, and her stomach was insensibly restored. Thus the means to which she ought to have been indebted for her cure, were likely to have proved the cause of her destruction.

I thought

I thought it a duty to take this opportunity to examine other chocolates bought indiscriminately from several makers; some I found to be faithfully prepared; but others contained the farina of wheat, others the farina of lentils, of peas and of beans, and lastly, the fecula of potatoes. It will be said that these substances are not injurious to the animal economy; and to that I agree. But in circumstances in which chocolate makes part of a regimen, and is prescribed as a medicinal food, they cannot but be prejudicial to the health: besides, why introduce them into it? they are intirely foreign to the composition of chocolate. These observations apply to all the additions which are mentioned in advertisements, and supposed by some persons to be improvements.

Examination of various kinds.

The added ingredients though not unwholesome may be improper in the medicinal use.

But admitting what is not true, that it is necessary to make chocolate thicker and more substantial, the mixtures here mentioned ought not to be made till the moment of its preparation, and, if I may be allowed the expression, under the eye of the consumer. I must observe that, if it be thought useful to add farinaceous matters, they should always be employed in the state of fecula or starch, because in that state they are deprived of glutinous and extractive matter, and contain the alimentary principle only.

Any addition improper,

The composition of chocolate ought to be distinguished from its preparation, or making up; the latter is in the province of the consumer, who may add at his pleasure the yolks of eggs, for the purpose of giving to his beverage a more saponaceous character, and make use of milk instead of water to increase its nutritive properties. It has even been remarked that many who are unable to take milk without the immediate production of acidity, have succeeded in digesting it by the help of a little chocolate; but I again repeat, that if it should be the design of a physician to prescribe it, it is a medicine on whose effects he cannot rely, while its composition continues arbitrary and uncertain.

unless made by the consumer.

There are other frauds still more injurious to the effects of chocolate, which I have also discovered in my examination; some makers procure, at a low price, the refuse of cocoa paste, from which the butter has been extracted, and replace it by oils or animal fats; others add roasted almonds, gum tragacanth, or gum arabic; and lastly there are some who provide themselves with acrid, bitter, and newly gathered cocoa-nuts, be-

Injurious frauds.

cause these articles, which are always to be had at low prices, are capable of bearing a greater quantity of sugar, which proportionally diminishes the prime cost of the chocolate.

Bad qualities
produced by
want of skill,

We must also observe that, with the best intentions, the chocolate may be of inferior quality, without containing any foreign ingredient, because the substances in its composition may have been ill-selected, or the first operation may have been, in some respects, carelessly prepared: the whole art consists in choosing the quality of the cocoa, and above all, in avoiding either extreme in roasting it; if it be too slightly roasted, it retains a disagreeable taste; if it be burned, it not only acquires bitterness, but the liquid prepared from it is black and wants that unctuousity which is so much admired in it; and lastly, if the germ be not separated from the two lobes of the fruit, its hard and horny state, resisting the action of the bruising and the boiling, it will be found entire at the bottom of the cup of chocolate: its presence is sufficient to show that the first work, which consists in peeling the cocoa grain by grain, has been neglected, and that no more care has been bestowed on the subsequent operations.

or by carelessness
in the fabrica-
tion.

Another observation is that the greater number of the workmen to whom the fabrication of chocolate is confined require much overlooking on the part of the master; they may be unfaithful when they are employed by task-work; they will bruise and work the paste carelessly, and, to save labour and time, they give a degree of heat which is too powerful and particularly injurious to the quality of the chocolate.

Methods of discovering the Frauds.

Methods of dis-
covering frauds.

It is not enough to have pointed out the frauds practised in the fabrication of chocolate, and all the defects of negligence or choice in the qualities of the materials and in the preparation. We shall have only accomplished half our object, if we do not enable the consumer to distinguish them so as not to be mistaken.

Qualities of cho-
colate when ge-
nuine.

Those who are accustomed to pure chocolate can easily judge of its goodness: its fracture ought to show nothing of a granulated appearance. On tasting it, it ought to melt in the mouth, and leave a sort of freshness; and lastly, in making drink of it, it ought to acquire, either with water or with milk, only a moderate consistence.

In all cases when the chocolate leaves a pasty taste in the mouth; when in preparing it, the liquor exhales on the first boiling a smell of glue; and when after its entire cooling it is converted into a species of jelly, we may be certain that the chocolate contains farinaceous matter, in quantity proportioned to the degree of the effects here pointed out: if it deposits at the bottom of the cup, small hard bodies, and an earthy or gravelly sediment, it is a proof that it has not been well picked and that raw sugar, more or less coarse, has been used instead of refined sugar. The smell of cheese discovers the presence of animal fats, and rancidity, or that of emulsive seeds; and the bitter, saline or musty flavour announces that the cocoa employed was too green, too much roasted or decayed.

It cannot be too often repeated that chocolate is not an in-
different preparation; it does not require science, but probity
and care; the makers of chocolate ought to leave the con-
sumers the right to add what they please, when they are de-
sirous either to increase its efficacy or its pleasantness, accord-
ing to their own choice. They are often led by improper con-
fidence, or an economy ill understood, to take it of an inferior
quality; for chocolate in fact possesses a real value, notwith-
standing which, many are unwilling to pay more than half its
fair price, while others pay much too dear for it.

Probity and care
necessary in the
preparation.

The limits of a notice will not permit us to point out the characters which distinguish the cocoas of commerce from each other, nor to ascertain here at what price the chocolate can be obtained by those who fabricate it: these details shall be the subject of a separate memoir.

It results from what I have said, that chocolate is not at pre-
sent what it was when the Spaniards conquered Mexico, at
the beginning of the sixteenth century; that no peculiar me-
thod is required for its preparation; that though the proportions
of the elements which compose it may be varied, the process
for applying and disposing them to form a good compound,
must be constant and invariable; that its quality depends upon
the choice of the ingredients and the care employed in combi-
ning them; that negligence, avarice, and quackery change its
nature so much as to convert it into a heavy, indigestible and
heating fluid; that in order to procure it with all the qualities
which characterise good chocolate, it is necessary to buy of
dealers of good character, and at a fair price; and lastly, that
every

General result.

every manufacturer who admits into the composition of chocolate, materials which ought not to form a part of it, or at least, which the consumer does not require, directly injures his health. He who adulterates an article of general use, ought to be aware that though he conceals himself in the obscurity of his work-shops to introduce low priced ingredients into his chocolate, and to disguise them, yet he cannot escape from a chemical analysis, which can instantly discover his frauds, and denounces his pernicious art and his name to the public notice.

Additional observations by Cadet.

To the observations contained in this notice, I shall add those which citizen *L. C. Cadet* has communicated to me on the chemical analysis of certain foods. I shall use his own words;

“The prefect of the police lately charged me to analyse some chocolate which he suspected to contain some noxious substances. I shall not enter into the detail of the multiplied experiments which this examination required, but shall confine this notice to a single remark, founded on the following fact:

Un-manufactured chocolate was found to contain a metallic precipitate

“To discover whether this chocolate contained any metallic substance, I incinerated it and washed the ashes with very pure nitric acid, which retained every thing that was soluble.

— of iron,

and also lime,

which are not found in the nut but are introduced by the present methods of working.

The filtered lixivium was clear, but I had scarcely poured into it a hidro-sulphuret, when I obtained a very abundant, black, metallic precipitate. This result gave me much uneasiness, because I was still ignorant of the nature of the metal, and it is always distressing to find a pernicious substance in an article of food. I tried the lixivium with prussiate of potash. Immediately a beautiful Prussian blue was formed, which removed my apprehensions. I continued the examination with different re-agents, and the oxalic acid showed me the presence of lime. It was interesting to learn whether the iron and the lime which I had discovered would be met with in all chocolates: I then made an analysis of the Caracca cocoa and of some of the best from the plantations: I did not find in them either lime or metal. I immediately caused some chocolate to be prepared from the same cocoa and very white crystallized sugar; the analysis discovered a pretty large proportion of iron and lime. It is, therefore, the fabrication which introduces these two substances into the chocolate, and the quantity is greater in proportion to the care bestowed on the manipulation. In fact the cocoa is roasted in a cylinder of sheet iron similar to those used in roasting coffee. It is afterwards beat in an iron mortar

and

and finally ground with an iron roller on a calcareous stone, the surface of which is worn by the friction, and affords the lime.

“ If the cocoa was roasted in a vessel of less oxidable metal, or in well baked earthen-ware; if the paste was ground on a granite or a porphyry with a roller of the same materials, the chocolate would contain neither lime nor iron. In manipulations on a large scale, metallic substances are often introduced into the subjects of operation. Hence the extract of tamarinds, and more particularly the extract of liquorice (Spanish juice,) contains so great a quantity of copper, that this metal is frequently visible at the first inspection; for the same reason apothecaries are obliged to purify them to avoid those serious accidents they might occasion.

Proposed alterations.

“ I was curious to know what was the proportion of metal and earth thus introduced into the chocolate. I repeated the experiments with care on quantities accurately weighed, and I ascertained that five hectogrammes (one pound) contained twenty-four decigrammes (forty-eight grains) of lime, and twenty decigrammes (thirty-six grains) of iron; this proportion is the minimum. Thus a man who takes a cup of chocolate daily, at the end of the year has eaten eight hundred and sixty-four decigrammes (three ounces) of lime, and seven hundred and forty decigrammes (two ounces two gros) of iron.

Proportions of iron and lime introduced.

“ As iron is a salubrious metal and as the proportion of the lime in the chocolate is not very considerable, no uneasiness need be formed respecting the use of this aliment; nevertheless, I am of opinion that physicians will perceive the utility of collecting the analyses of the different substances used for the nourishment of mankind. Until our perfect growth, the development and solidification of our bones requires that we should absorb from our food a certain quantity of lime; and we recollect with interest the labours of Citizens Vauquelin and Alexander Brogniart, who found lime in flour, and who calculated that a man who consumes only one pound of bread in a day will have eaten near two pounds of lime at the year's end.

Utility of analysing aliments.

“ When a man has arrived at the summit of his growth, he has no longer occasion for the same quantity of lime; hence it is found abundantly in his urine and excrements; but as this earth, in the different digestive passages meets with different

Physiological remarks.

acids which combine with it and form salts, the greater number of which, such as the phosphate and oxalate are but little soluble, it frequently produces disorders in the most necessary organs of life; it is known that it is the base of most of the urinary calculi and arthritic concretion. It is therefore highly important to the progress of medicine, to examine the aliments which contain this earth, in an ostensible manner. The Citizens Vauquelin and Alexander Brogniart, found it in abundance in bread: I have discovered it in chocolate.

“ Citizen Delaville, in a memoir which made part of the hundred and twenty-second number of the *Annales de Chimie*, says, speaking of the sap of cabbages, “ this sap evaporated yields a considerable quantity of sulphate of lime, the sap of radishes affords results nearly similar.”

“ Here then are four aliments which are frequently employed at the same time, and which carry into the passages of digestion a considerable quantity of lime. Other instances might be cited, such as ciders ameliorated by the merchants with chalk, and I have thought these facts were not unworthy of the attention of physicians. It is the assemblage of such observations, trifling in appearance, which frequently explain the cause of the phenomena belonging to physiology. Nothing should be neglected in animal chemistry, the principal aim of which is the preservation of our health and the annihilation of the most dreadful maladies which afflict human nature.

VI.

On the Causes by which the Origin of the Atmosphere is supplied or renovated. In a Letter from Mr. ROBERT HARRUP.

To Mr. NICHOLSON.

S I R,

IF you think the inclosed paper of sufficient importance to meet the public eye, be so kind as to give it a place in your valuable and interesting Journal.

I am, Sir,

your obedient humble servant,
ROBERT HARRUP.

Chobham, June 22, 1803.

WHEN

WHEN we consider the great importance of oxigen in the system of nature, its almost universal agency and the immense quantities which are every instant consumed on the surface of the globe, the question naturally arises, from what copious sources are we supplied with this wonderful substance in a gaseous state? As neither animal life nor combustion can be maintained one moment without it, the quantity daily destroyed must exceed all calculation. To form some distant idea of this consumption, let us endeavour to ascertain the least possible quantity which must necessarily be decomposed in a given time, in a large populous city. It is proved by experiment, that one person consumes about five cubic feet of atmospheric air in an hour, or, in other words, decomposes the oxigen gas contained in that quantity. Now, if this gas is taken at one fourth of the whole, it will be found that 100 persons decompose 125 cubic feet every hour, and if the population of London be taken at 800,000 persons only, the quantity decomposed every hour, will amount to no less than one million cubic feet. But if this quantity is destroyed by respiration alone, how much more must be consumed by combustion in the same space of time. It would indeed be difficult to ascertain with any degree of precision, the quantity necessary for maintaining a common fire, but if the combustion of a middle sized candle be equal to the consumption of one person by respiration, it is certainly much under-rating the quantity if taken at four times that of the former. However, supposing it to amount to no more, it must follow that the oxigen gas decomposed by respiration and combustion only, in the City of London, amounts to the enormous quantity of five million cubic feet *per* hour.

Great importance and prodigious consumption of oxigen in the system of nature.

Consumption in London 120 millions of cubic feet in a day.

Soon after the discovery of the component parts of atmospheric air, Drs. Priestley and Ingenhouz shewed by a great number of experiments, that oxigen gas was emitted by vegetables when exposed to the rays of the sun. From this fact it was immediately concluded, that the atmosphere was principally, if not altogether, supplied with oxigen gas from this source. This opinion, which still obtains without any attempt to controvert it, appears to me objectionable in several respects. In the first place, it is doubtful if the supply from this quarter be at all adequate to the consumption for Dr. Priestley, likewise found that vegetables during the night, emitted a gas totally

Renovation of the air by vegetables;

supposed to be
insufficient for
reasons stated.

tally unfit for respiration or combustion, so that the oxygen gas formed in the day would not only be consumed in the night by these processes, but a deleterious gas added to the atmosphere. But supposing the quantity of this latter to be so small as to produce no effect, yet it must be admitted, that while the sun is under the horizon, none of the former can be produced; indeed it is only by the direct influence of the solar rays on living vegetables that it is evolved at all. How many circumstances then under the temporary deprivation of light, must concur to obstruct the evolution of this salutary fluid from vegetation. The inhabitants of countries where fogs or clouds obstruct the solar rays for weeks, nay months together, must be indebted for life to the winds waiving oxygen gas from regions far distant from their own, where a perpetual spring and cloudless skies prevail. In the depth of winter, when half the world lies buried under ice and snow, and vegetation suffers a temporary suspension, every living creature in these climates must be indebted to the same friendly winds and regions for health and life.

Oxygen abounds
no less in the
air over seas,
deserts, and
frozen regions,
as in places
where vegeta-
tion and the solar
influence pre-
dominate the
most.

Those portions of the surface of the globe which furnish oxygen gas from vegetation, are not only subject to most of the perpetual interruptions already mentioned, but make but a small part when compared with the whole. The vast tracts of ocean can afford nothing for the preservation of animated beings more than the sandy desert bounded only by the horizon, or the eternally frozen regions of either pole, and the blasts of the north would carry destruction to all at least who inhabit near the arctic circle. It would be only in the torrid zone where we could expect to find, in sufficient quantity, that life supporting fluid, which, unfortunately for the common received opinion, is as abundant in the midst of London at all times, as at Nova Zembla, or on the line. As nature, ever uniform, makes the most ample provision for carrying on her operations throughout her works, it cannot be seriously maintained that she would trust a business of so much importance as the preservation of animal life, to means so precarious as clear or cloudy weather, or the vicissitudes of the seasons. The quantity of oxygen gas contained in the atmosphere is found to be nearly the same at all times and in all places, whether in the depth of winter or middle of summer, whether on land or on water, whether in the crowded city or remote hamlet, but if the

the general opinion were just, it would be subject to perpetual variation. From this single fact we infer, that affinity or attraction is concerned in keeping up a constant and regular supply, and that however unequal at different times the consumption may be, that power will still continue to act in a proportionate degree. Several years ago, I conjectured that the water in a state of solution in the atmosphere, or that which composes the clouds, might be decomposed by the action of light, and consequently furnish a sufficient supply. With this view I made several experiments, by exposing water in glass vessels to the influence of the rays of the sun, but could never succeed in producing a single bubble of any sort of gas, although confined with air of different degrees of density, and in vacuo, and exposed for several months. At that time I attributed the want of success to the difficulty of imitating nature, therefore did not abandon the idea entirely.

The supply is probably by direct chemical affinity;

not perhaps from decomposition of aqueous vapor,

Meditating on the subject some time after, I conceived it extremely probable, whatever may have been alledged to the contrary, that azotic gas has some degree of affinity for oxygen, and that the combined action of this fluid and light might be sufficiently powerful to decompose water. To determine this point, I made the following experiment, which succeeded beyond expectation. Into a small transparent glass retort, filled with fresh drawn pump-water, I introduced 17 ounce measures of pure azotic gas of the temperature of 56° , and inverted it into a bottle filled with water, and sunk to the neck in a pot of sand; the mouth of the retort descended to within about an inch of the bottom of the bottle. After accurately marking that part of the neck of the retort at which the water stood, it was placed on the outside of a window fronting the south, April 22, 1801. On the first three or four days of exposure, a number of very minute bubbles appeared on the sides of the glass and rose to the surface. They after this entirely ceased, and the water became somewhat turbid. In about three weeks, having recovered in a great degree its transparency, an infinite number of small yellowish particles were uniformly diffused through it, and which continued to the end of the experiment. The volume of included gas was perceived to be increased some time after exposure, and after some days of uninterrupted sunshine. Some time in the beginning of October, long after the gas had ceased to gain any additional bulk,

but from the disposition of azote to unite with oxygen.

Experiment. Azote gas was exposed to water in an inverted vessel.

In the course of time the gas was increased,

almost one fourth,

and was better than common air.

There was no hidrogen.

But the oxigen is nevertheless thought to have come from the water.

Qu. If the development of oxigen from water in contact with azote be not the renovating cause in the atmosphere.

bulk, the apparatus was taken in, and being brought to the temperature of 56° , I had the satisfaction to find it was increased considerably. After marking the neck of the retort where the water now stood, in order to know whether any farther change would take place, the whole was placed in a dark closet, near which a constant fire was kept. After standing till November 23, 1802, no increase or diminution had taken place. Upon examination, the increase was found to be somewhat more than *four ounces*, as the whole measured upwards of 21. It now exhibited all the properties of *atmospheric air*. A small taper continued to burn in a given portion the same space of time as in an equal quantity of common air. By several trials with Mr. Davy's eudiometer, it was found to contain a somewhat larger proportion of oxigen gas than the air of a large room where the trials were made. And the test by sulphuret of potash shewed the same. What appeared surprising was, that no hidrogen gas could be found. Its absence is not easily accounted for, I can only conjecture that the hidrogen might have entered into combination with some extraneous substance contained in the water, as the oxigen was evolved particularly, as all that part of the inside of the retort with which the water was in contact, was lined with a pelucid whitish film, which came off in large flakes upon rinsing it. It seems to me extremely difficult if not impossible, to account for the oxigen gas produced in this experiment, otherwise than from the decomposition of the water. It cannot be supposed that this quantity of gas (no less than 4ozs.) was held in an uncombined state in the water, which in all amounted to little more than a pint. That portion of oxigen gas which is found naturally in water, seems to have been extricated at the beginning of the experiment as already mentioned, which altogether could not have amounted to two drams by measure at most, and no more was observed to arise. Neither could it be from putrefaction, as the water was free from smell, and no hidrogen gas produced.

Are we to conclude, then, from these facts, that by the combined action of light and azotic gas water is decomposed, and that when the gas ceases to act, or is saturated, if I may use the expression, no further decomposition takes place till a portion of the oxigen gas is separated? If this be admitted, we can readily account for the immense quantity of oxigen gas

with

with which the atmosphere must be necessarily supplied to keep pace with the consumption, and also why it is only found in one invariable proportion.

VII.

Report on a Memoir sent to la Société Libre des Pharmaciens, of Paris. By Cit. ROBERT, Chief Physician to the Hospital of Humanity, at Rouen. By † Citizens C. L. CADET and BOULLAY.*

CITIZEN ROBERT recounts the different experiments made by Citizens Fourcroy and Vauquelin, on the effects of fur-oxygenated muriate of potash united to certain combustible bodies, and submitted to the action of a blow or friction; these brilliant detonations are now known to all chemists; they are equally well informed of the inflammations which take place when the same bodies are plunged into concentrated sulphuric acid. Cit. Robert has modified the latter experiments in an interesting manner. Instead of throwing the inflammable mix-^{Introductory observations,} tures into the acid, he only touches them with a tube wetted ^{Cit. Robert's experiments,} with this acid. In this manner he inflamed,

1st. Three parts of the fur-oxygenated muriate, and one part of sulphur.

2d. Three parts of the same salt, half a part of charcoal, and as much sulphur.

3d. Equal parts of antimony and fur-oxygenated muriate.

4th. Equal parts of sulphuret of antimony and of the salt.

5th. Equal parts of kermes and golden sulphur (and the salt.)

6th. Equal parts of arsenic and the salt.

7th. Three parts of the muriate and one of sugar.

8th. Three parts of the muriate and one of charcoal.

9th. A part and a half of the muriate and three parts of gunpowder.

* The subject of the Memoir itself is the inflammation of combustible bodies combined with fur-oxygenated muriate of potash, by contact with sulphuric acid. C & B.

† From the *Annales de Chimie*, Frimaire, An. XI.

And

And lastly, pastes made with alcohol, olive-oil, and sur-oxygenated muriate of potash.

repeated with
success;

We have repeated all these experiments which succeeded perfectly, as well as the following, which appeared with reason to Cit. Robert, to deserve attention.

He charged a pistol with common gunpowder, and having primed it with the mixture of powder and muriate mentioned above, he inflamed it with the sulphuric match *, and it went off.

further experi-
ments requisite
to complete the
enquiry.

Cit. Robert observed, that the report, the colour of the flame, and the odour exhaled by the various combustibles are essentially different; but he made no enquiries respecting the gases they formed, or the compounds which remained after the inflammation. We proposed to collect the gaseous products in a hidro-pneumatic apparatus, for the purpose of examining them, but the time appointed by the society for making our report being but a few days, we have postponed these enquiries.

Tho' curiosity alone may appear interested in pursuing such experiments, we have thought it a duty to try, in a similar manner, several simple and compound substances, the inflammation of which had not been before attempted.

New Experiments made by the Commissioners.

New experi-
ments.

The manner of operating being nearly the same, to avoid the continual repetition of the same formula, we shall only point out the substances united with the sur-oxygenated muriate of potash. Phosphorus presented beautiful deflagration. Hydrogen gas took fire. To perform this experiment, we filled a bladder with inflammable air. To its stop-cock we affixed a simple adjutage or copper tube with a single jet. We moistened this tube with sulphuric acid, and touched it with sur-oxygenated muriate of potash, at the same time pressing the bladder to make the current of the gas pass over the salt. At the instant of contact the gas took fire, as if by the electric spark.

Phosphorus.

Hydrogen gas.

Metals.

Gold, silver, zinc and iron, yielded no other phenomenon but the decrepitation of the salt alone. We were not surprised

* The tube and sulphuric acid. Translator.

at this negative result with the two first metals, which are not easily oxidable, but zinc and iron promised an inflammation, because they detonate by a blow.

Brown oxide of copper, the residue of the distillation of ^{Metallic oxides} acetate of copper burnt without flame, with sparks resembling ^{and} those from *gerbes d'artifice* (a kind of firework called the wheat sheaf.)

Metallic sulphurets succeeded pretty well, particularly the sulphurets, sulphuret of tin or aurum musivium, and the black sulphure of mercury. The last yields a very beautiful white flame. Amber, succinic acid, bitumen, did not take fire, but the decrepitation was very considerable.

Among the vegetable substances some inflamed easily, such ^{Vegetables,} as the volatile oils, resin, turpentine, gum copal *, and gum elemi, gum arabic, the dust of lycopodium, soap, camphor, cotton, and sawdust. This last article did not always succeed, but with the addition of a little sulphur it makes an excellent powder of fusion, which from the facility of its inflammation by a re-agent, may be useful to mineralogists.

We tried starch, it inflamed with difficulty, but we succeeded in making it burn. Ether takes fire very quickly. We observed that in this experiment as well as in those with camphor and alcohol, very little of the sur-oxygenated muriate was decomposed, and it did not burn, but was solely of service to favour the inflammation. To ascertain that fact, we mixed sulphur with the residue of the experiment, the sulphuric match produced a second combustion.

We made a paste with the sur-oxygenated muriate and honey; this mixture caught fire, swelling up and emitting an odour of boiling sugar mixed with a very penetrating acid, which one of us believed to be acetic acid.

Crystallized benzoic acid diffuses a considerable reddish flame; tartareous acid and acidule burn also very well; tartrite of potash offers a beautiful whitish flame; tartrite of soda presents neither inflammation nor light. Antimoniated tartrite of potash gives beautiful sparks without flame.

Oxalic acid with the oxygenated muriate sparkles without

* The gum resin, copal, still but little known, has been assimilated by some chemists with amber. This experiment establishes a very remarkable difference between these two substances.

inflammation. Acetic acid produces a considerable deflagration and a beautiful blueish flame. The acetates of potash and soda take fire with crackling.

Animal matters. These results induced us to examine what animal matters were susceptible of burning by the same processes; we tried dried gluten and hartshorn shavings without success.

The yolk of an egg, wax, butter, tallow and grease, burnt like oil, but with less crackling; wool and a piece of rabbit's skin with its fur on, strongly impregnated with fur-oxygenated muriate of potash, were inflamed and continued to burn until their total incineration.

Singular results. Among those experiments which presented us remarkable singularities, we must mention the fulminating powder which we could never set fire to, though it was decomposed with a considerable disengagement of gas and heat. Three other mixtures with metallic bases surprised us greatly by their terrifying detonation, by their rapid inflammation, and by their strong effect in fire-arms*. Cit. Robert has no doubt tried them, but he makes no observation on them. We shall copy his reserve, and only allow ourselves two remarks. The arms are strongly oxidized and soon destroyed by these mixtures, which are expensive and more difficult to prepare than gunpowder; we think they are not capable of being granulated. Their inflammability would render their carriage and their use too dangerous, since a blow or friction produces detonation. We shall conceal their names, because malevolence too often abuses the fatal secrets made known by chemists. The remembrance of the calamities at Essone, still more the public safety makes it our duty to be silent, and not to give a new resource to the cruel art of destroying mankind.

* They take fire like powder by the spark from a gun flint.

VIII.

Description of a Telegraph used in Sweden. Constructed by Sir A. N. EDELCRANTZ, Counsellor of the Chancery, and private Secretary to the King of Sweden, Archivist of the Orders of His Majesty, and One of the Eighteen Members of the Swedish Academy. From the Copy of a Treatise on Telegraphs, communicated by the Author.

AFTER numerous observations on the various descriptions of signals, and an historical detail of the use of them, from the most remote antiquity to the present time, the author states the principal qualities of a good telegraph to consist in:

Qualities requisite in a telegraph;

1. Perfection in the principle of the machine, and in the nature of the signals.

2. Perfection in the means employed to work the machine.

The first of these requires: 1. a sufficient number of signals to express not only letters, but also such words and phrases as are in general use: 2. that the signals shall be so apparent, that any one may see them very distinctly: 3. that the application of the signals to their respective significations shall be easily understood and retained.

The second requires: 1. the least possible weight and friction of the parts: 2. quickness in the movements and changes of the signals: 3. certainty to prevent mistakes and ambiguous results.

The credit of uniting all these qualities in one machine, are ascribed to M. Chappe, the inventor of the French telegraph, of which he gives a description. The number of signals it is capable of, allowing the angle of inclination of its respective arms to be 45° , is 256; by diminishing that angle to 30° they may be increased to 864, and in like manner the number may be still further augmented by every diminution of the angle, but the signals would be proportionally less distinct.

first united by M. Chappe.

When this discovery was announced, the learned of all countries instituted enquiries into its principles and construction. Among others, the Chevalier made various machines, some resembling the French telegraph, and others totally different, and after many attempts he succeeded in constructing that which is the principal subject of his treatise, and to which he

Attempts to discover the construction and principles of the French telegraph.

Advantages of that discovered by the author.

gave the preference, because it occupied less space, was more rapid and easy in its movements, more certain and unequivocal to the sight, and, to all its other advantages, added that of being useful by night.

Description of the Machine.

Description.

a, b, c, d, e, f, (Fig. 1, Pl. XII.) is a frame of wood, in the openings of which are placed ten shutters at equal distances from each other, in three vertical ranks, that in the middle consisting of four shutters. These shutters, which should be as thin as possible, may be made of wood, iron or copper, and are fixed on an axis *on*, the extremities of which move in holes made in the frame. On the axis *on*, perpendicular to its direction, but with an angle of 45° to the surface of the shutter is a small arm *ms*, from one extremity of which a string descends to the bottom of the frame, by means of this and of the arm *ms*, the shutter receives at pleasure a vertical or horizontal position. Fig. 1 represents all the shutters in a vertical position, and Fig. 4 shows the profile of the machine, the shutters being horizontal. It will be easily seen, that when the shutters which are very thin, are observed from a distance and horizontally, they will be scarcely perceptible; but they become visible, if, by drawing the string *sk*, a vertical position is given to them. When the machine is at rest, the shutters are kept in a horizontal position by a weight *q* attached to them at *l*, and are supported by the pin *pl*. In the other position they are kept upright by a piece of wood *p* placed behind them, but which is shown in front in the plate. To prevent the strings from getting entangled, the arms *ms* are fixed at unequal distances from their respective shutters. The strings pass through holes made in the cross plank *ur*, or *uuu*, (Fig 2.) which shows the plan. They may then be made to pass through the plank *ww*, and be secured to ten rings *tt*, each corresponding to one finger, by which means a small machine may be conveniently worked.

Number of its signals.

According to this construction, 1024 combinations or distinct signals may be formed by raising or lowering the shutters. The better to distinguish them they are expressed by numbers on the following principle. Leaving out the upper shutter *A*, the machine will consist of three vertical lines, with three shutters on each. To the upper shutter of each line give the value

of 1, to the second 2, and to the last 4, and from the value of the single shutters form that of their combinations. Thus when the first and second shutters on a line are visible, they will signify $3 = 1 + 2$, the first and third $5 = 1 + 4$, the second and third $6 = 2 + 4$, and the three together $7 = 1 + 2 + 4$, and in the same manner for each vertical line. If in any of the lines no shutter is visible, the vacancy is expressed by 0. These three lines give 512 different signals, and the number is doubled by the addition of the shutter A. Thus in expressing the number of the signal, suppose the first and second shutters of the first line to be seen, all three on the second line, and none on the third, the value must be written 370, if to these the shutter A is added, the number must be expressed by A 370, and so of every other combination. On the left side of the machine is placed an arm H, the use of which is partly to direct the arrangement of the lines, beginning from that side; and partly to point out the middle shutter in each line. It may be made moveable on hinges, and can then be concealed or made visible at pleasure. It will be easily conceived that the method of valuing the lines may be varied by agreement, so as to be reckoned from right to left, or horizontally, in either direction: thus the position of the telegraph as indicated above, may be expressed by 370 — 670 — 073 — 076 — 662 — 266 — 332 or 233, but the method first described seems to be the most natural.

By the mode of reckoning, the value of the shutters as described above, it will be seen that they are capable of expressing every number of three places, from 000 to 777, which does not contain an eight or nine. To make use of these numbers, a table must be constructed on the following plan,

Table of
cyphers.

000	100	200 and fo on	to 700
000 - - - 777	100 - - - 677	200 - - - 577	
001 - - - 776	101 - - - 676	201 - - - 576	
002 - - - 775	102 - - - 675	202 - - - 575	
003 - - - 774	103 - - - 674	203 - - - 574	
004 - - - 773	104 - - - 673	204 - - - 573	
005 - - - 772	105 - - - 672	205 - - - 572	
006 - - - 771	106 - - - 671	206 - - - 571	
007 - - - 770	107 - - - 670	207 - - - 570	
010 - - - 767	110 - - - 667	210 - - - 567	
011 - - - 766	111 - - - 666	211 - - - 566	
012 - - - 765	112 - - - 665	212 - - - 565	
013 - - - 764	113 - - - 664	213 - - - 564	
014 - - - 763	114 - - - 663	214 - - - 563	
015 - - - 762	115 - - - 662	215 - - - 562	
016 - - - 761	116 - - - 661	216 - - - 561	
017 - - - 760	117 - - - 660	217 - - - 560	
020 - - - 757	120 - - - 657	220 - - - 557	
021 - - - 756	121 - - - 656	221 - - - 556	
022 - - - 755	122 - - - 655	222 - - - 555	
023 - - - 754	123 - - - 654	223 - - - 554	
024 - - - 753	124 - - - 653	224 - - - 553	
025 - - - 752	125 - - - 652	225 - - - 552	
026 - - - 751	126 - - - 651	226 - - - 551	
027 - - - 750	127 - - - 650	227 - - - 550	

completing the series in each direction until the whole number of combinations are expressed, and filling up the vacancies between the double row of numbers with such letters, figures, words or sentences, as may have been previously agreed upon. its use by night. The two rows of numbers are only necessary when the telegraph is intended to be used by night as well as by day, in which case it must be obvious, that when the concerted signal is made by raising the shutters, those lights which are placed behind them, as will be described hereafter, become hid, and the value of the signal is reversed in consequence of the open spaces being visible instead of the closed ones.

A second table must also be formed to contain the same series with the letter A prefixed as described above.

The correspond-
ence may be
secret.

When the correspondence requires secrecy, the significations of these numbers, and the methods of estimating their value may be varied almost to infinity by any one versed in the knowledge

knowledge of cryptography. Several plans of this kind are inserted by the Chevalier, the details of which would extend the limits of this paper too far.

Methods of working the Machine.

It has been already noticed, that a small machine may be worked by the rings *tt*, but as the strength of the fingers are unequal to the weight and resistance of a large machine, more powerful means must be had recourse to. With this view the author proposes to replace the rings with metal rods affixed to the strings, and having a button at the lower extremity of each. To work the machine so prepared, he proposes to have an instrument of iron nearly in the form of a T, at the bottom of which shall be an opening or stirrup to receive the foot. The cross arm of it is to have ten notches corresponding to the ends of the rods, and is to be kept on a level with them by a weight behind. When a signal is to be made, the ends of the rods hanging to the shutters which are to be closed, are slipped into their respective notches, and the machine is worked by pressing down the instrument with the foot. When the signal has been observed the foot is withdrawn, and the instrument restored to its original position, by the weight which has been described. The rods are then disengaged, and the whole is in a state of preparation for the next signal. This may be more expeditiously performed by another contrivance formed of ten plates of metal moveable on a common axis, and each having a hollow in its outer edge. When the machine is at rest, these plates are in a vertical position, and the box containing them is placed before the lower ends of the rods. To make a signal, the plates corresponding with the rods intended to be acted on, are pushed with the fingers into an horizontal position, and the box slid along in two grooves made for the purpose, by which means all those rods are pressed into their respective notches at once, and a considerable space of time is saved, while the rods whose corresponding plates retain their vertical position are unmoved. If a still greater force is required to work the machine than can be obtained by the foot, a winch with rackwork, or with a pulley, instead of the upright bar of the instrument, may be applied to the cross arm.

In order to afford more perspicuity in making a signal, the Chevalier also proposes to form the combinations of each respective

Each signal may be made by drawing one rod on each line.

spective line by means of united strings, thus to represent 1, a string attached to that shutter is fastened to a rod numbered 1; for 2, a string is brought from that shutter to a rod numbered 2; but to produce 3, a string from each of the shutters 1 and 2 is affixed to the rod numbered 3, and in a similar manner 5, 6 and 7, are each produced by combining the strings before they are united to the rods. In this method the number of the rods are increased to 22, but as only one of each line can be used at once, and they are all numbered, there is less probability of a mistake, and the quickness of the operation is increased.

Requires only one or two persons to work it;

According to the construction above described, it will be seen that one person is sufficient to work the largest telegraph at the extremities of the line of stations, provided the telescope is so placed that he can look-out and work the shutters at the same time; but in the intermediate stations, where the observations must be made with two telescopes, in contrary directions, there should always be two persons who may relieve each other. It is not requisite that they should be men above the common rank; all that is wanted of them is to be able to write down the numbers, and to combine 1, 2, 3, 4. In the attempts made by the Chevalier, he, during a year and half, employed only children, and a very few hours were sufficient to give them a complete knowledge of what was required of them. It was for this reason he was induced to sacrifice something of the simplicity of the mechanism to simplicity in the execution. When those employed are not well skilled in the use of the machine, it is important not to load their memory or their conception with too many details, and it is probably to this as much as to rapidity of execution that the chief merit of this telegraph is owing.

who need not be men of science.

Children employed.

The observatory.

The observatory should always be placed below the telegraph, and should enclose the telescopes and machinery with which the telegraph is worked. It ought to be as much darkened as possible, and painted black within, in order that all foreign light may be excluded, by which means the impression of an object is rendered much more distinct to the eye.

Methods of using the Telegraph by Night.

Methods of using the telegraph by night.

For the purpose of rendering the telegraph useful in the night, the following plans are adopted.

In

In the first it is proposed to place a lamp behind each shutter at such a distance as not to obstruct the movements, and rather above the axis. In this method, by closing the shutters according to the plan indicated above, the lamps corresponding to the shutters which are raised will disappear, and those which remain visible will form a signal the reverse of that intended, the value of which will be found on the right side of the table of cyphers described before; hence the day signal will be before the signification, and the night signal after it. But as this plan was found to be inconvenient in practice, or at great distances the following was substituted for it, *a, b, c, d,* Second plan.

(Fig. 3.) is a tin lanthorn having only two openings *e*, to permit the passage of the light; placed on opposite sides, and covered with talc or muscovy glass. Between these two openings a good lamp is situated so as to give light in both directions. *g, f, k* is a quadrant of tin adapted to each side of the lanthorn, and moveable on its axis *h*, in such a manner, that by means of a string attached to the arm *i*, it may be raised before the openings of the lanthorn, but falls again by its own weight when the string is let go. Ten of these lamps are to be fixed on a frame in a similar manner to the ten shutters used by day, the strings depending from each are united at the foot of the machine, according to the same principles and the same combinations as in the day telegraph; but as the weight and friction to be overcome is much less, this machine may be worked at all times with the hands alone. One advantage which this establishment possesses over the day telegraph is, that it does not require to be placed above the horizon, it is even better to be below it. Besides, in every point of view it is more easily worked and is much superior in utility to the telegraph.

It is known that the stars may be seen in the middle of the day from the bottom of a deep well; it remains to be proved, if, by a similar effect the lamps can be seen in the day-time with the assistance of very long tubes: should this be practicable it will be highly advantageous in many respects, particularly in requiring only one establishment for both purposes. By means of the equatorial telescopes of Short or Ramsden, the stars may be observed at noon even close to the sun, hence it is probable that if all foreign light is excluded, the lamps will be visible on a dark ground. Telescopes which magnify the most will be preferable, because they darken the field of view

Probable visibility of the lamps by day.

view more than those of smaller power, without diminishing the impression of the light on the eye in any considerable degree.

Usefulness of
telegraphs,

Opinions are divided on the utility of telegraphs, and some of their opponents found their arguments on the alterations to which the transparency of the air is liable, and which impede their use. But the art of seeing through fogs and darkness will perhaps never be discovered. It will scarcely be contended that navigation is a useless science, because winds and tempests sometimes impede it. Others consider the expense of telegraphs as too great in proportion to their utility.

to governments.

But independent of the advantages arising to every government, in being enabled to transmit its orders to the extremities of the kingdom in less than a quarter of an hour, and to be no less expeditiously informed of what is passing on the frontiers, it is in time of war, when events occur on the knowledge of which the safety of thousands may depend, and which require precautions as prompt as vigorous; it is in such a period that the value of telegraphs is incalculable, and their cost bears no comparison with their utility, particularly if the expense of couriers saved by their means is taken into the account. It may be added, that the use of these machines in France during the late war furnished incontestable proofs of their utility.

There are two ways in which this machine may be useful; the first, when a quick communication between the two places is required; and the second, when this communication is not practicable in any other way. The post goes from Sweden to Finland in five days, but if the distance was divided into telegraphic stations, intelligence might be conveyed in three or four minutes. Should the post be retarded or interrupted either by the obstacles of the season, by the insecurity of the lakes, or by contrary winds or tempests, the communication may be kept up with the customary expedition of the post, by forming stations in those places where the principal obstructions are met with, such for example as the sea of Aland; or with the greatest rapidity, by placing them along the whole road.

to science.

From every appearance it is expected that natural philosophy will acquire many important benefits from these establishments. Every telegraph is a real observatory, which, with the assistance of some instrument, and the requisite information

in those who govern it, might enrich the science with new discoveries. An acquaintance with the effects of the refraction of the air might be disseminated by daily experiments; meteorology, which is the least cultivated, and perhaps the most difficult branch of natural philosophy, might be stored with discoveries on the influence of heat and cold, humidity, storms, changes of weather, fogs, snow and rain, as well as that produced by several other meteors on the transparency of the air; a daily comparison of the indications given by the barometer, the hygrometer, the anemometer, and the electrometer, with those of the diaphanometer of M. de Saussure *, and of the cyanometer †, might, in time, lead to very unexpected results.

At the conclusion of the work, the chevalier notices the English telegraph erected on our admiralty, which appears to have been an invention subsequent to his, and to be less complete. His telegraphs were established in Sweden in 1794, ours was not erected until 1796, and the number of its signals does not exceed 64, unless those are reckoned which depend on the order in which the shutters are shown, and which are liable to great ambiguity.

IX.

Additional Observations on the Probability that the Eruptions of Lunar Volcanos may sometimes reach the Earth. By a Correspondent.

To Mr. NICHOLSON.

S I R,

SOME time ago, I addressed a letter to you, on the probability that the eruptions of lunar volcanos may sometimes reach the earth; which was inserted in your Journal for December last. I will now beg leave to trouble you with a few additional observations on the hypothesis, I there ventured to advance.

* *Essay sur l'Hygrometrie*, § 371.

† See a description of this instrument in observations sur la physique et sur l'Histoire Naturelle, par Rosier, 1791, mars, p. 199.

In

More particular illustration of the probability of projectiles falling from the earth to the moon.

Terrestrial volcanos do produce the requisite velocity.

In Fig. 4. Plate X. supposing E and M to be the centers of the earth and moon; P Q K to be a path which a body must describe, so as to be equally attracted by both; it will appear evident from a very easy calculation, that, P Q K is a circle whose radius C P is equal to about $24\frac{1}{2}$ semidiameters of the moon, and the distance of whose center C, from the center of the moon, is equal $2\frac{1}{3}$ semidiameters (nearly). If therefore, the lunar volcanos in any part of the hemisphere of that planet, which is visible to us, should project bodies with a force sufficient to carry them through 24 of her semidiameters; i. e. with a velocity of about * 7000 feet in a second, they must necessarily throw them within the sphere of the earth's attraction. It may be said, however, that the atmosphere of the moon, although rarer and of less altitude than that of the earth, may yet be considerable enough to afford some resistance to the motion of bodies; allowing, therefore, (the utmost that can be allowed) that a body projected from a lunar volcano, has a resistance equivalent to that of two miles of an atmosphere of equal density with ours, and supposing the velocity of projection to be 12000 feet per second, and the body to be a sphere whose diameter is one foot, and specific gravity 10,000 times that of the atmosphere; it would lose in its passage less than $\frac{1}{3}$ of its first velocity, and would still retain more than sufficient force to carry it within the sphere of the earth's attraction. That the volcanos of the earth throw out heavy bodies with a force at least equal to this, seems easily proved; for a body such as we have supposed, if projected upwards with the above-mentioned velocity, would rise to the height of about nine miles from the earth's surface, in a medium of equal resistance with our atmosphere. It may be objected, that the atmosphere becomes considerably rarer at that height, so as to render the calculation of the effect of resistance greater than it would be in reality; this, however, may be more than counterbalanced by the effect of the elasticity of the atmosphere, and of the vacuum produced by bodies moving with such very great velocity. That volcanos frequently during an eruption, throw up heavy bodies to the height of eight or nine miles, will, I believe, be acknow-

* It was by mistake that I stated in my last letter, the velocity necessary at 12,000 feet per second.

ledged. I will now conclude with an attempt to answer your objection, from the bodies being in a state of ignition when they fall to the earth. As the space between the earth and moon must be either nearly, or altogether a vacuum, it must be almost, if not quite, a non-conductor of heat; so that it may easily be conceived, that a body passing through it, may retain during its passage of about four or five days, nearly the same degree of heat with which it set out, especially as no change of texture takes place, by which its heat can become latent.

I remain,

Your's, &c.

Cambridge, May, 24th, 1803.

J. B.

P. S. For the principles of the estimations, which I have made of the resistance of the atmosphere. See *Atwood on Rectilinear Motion*, sect. 5th.

X.

Description of an Ancient Lock of Combination. W. N.

THE lock delineated in Plate IX. is fixed in the lid of an old brass money box for the pocket; from which Mr. Latham of King Street, Soho, who deals in articles of curiosity, permitted me to make a drawing.

Fig. 1. is a perspective view of the box, which measures about three inches in diameter. Fig. 2. shews the outside of the lid, at one edge of which is a hinge, and upon the outside are figures of the sun, the moon, and two clock faces with hands. The figure of the moon can be slid nearer or farther from the center, and carries a bolt placed within the lid. The other three parts, namely, the sun and the hands of the clock, can be moved round their center pieces, and left in any situation at pleasure. Fig. 3. shews the inside of the lid. The bolt which is moveable along by means of the figure of the moon, is here seen with three tails, pointing to three circular pieces, in each of which pieces there is a notch to receive its correspondent tail. These pieces move round by means of the three parts on the other side, and it will thence be easily understood that the bolt cannot be slid back, nor the

box

box opened, unless one particular point of the sun, and each of the hands be duly placed from a knowledge of the secret. And as there are twelve points to the sun, and twelve marks for the hours on each dial, we may (without considering the probability of intermediate positions) reckon the chances as eleven to one, against setting the sun to the proper position; and eleven times still more against setting the sun and face of the dials right at the same time; and again eleven times more against all these being duly set at once. That is to say, the chances are 11 times 11 = 121, taken 11 times or = 1331. And consequently the lock is very secure.

The secret of the lock is secure as 1331 to 1.

Cardan's lock.

Secret locks and locks of combination.

The present lock was easily opened.

As I hope soon to give a short essay on locks, I shall only make a few remarks on the present machine. In the first place, it may be noticed that this, like the lock of Cardan*, which consists of four or more rollers, is a secret lock, and not a lock of combination; this last term being usually applied to locks in which a secret can be changed through a great number of varieties, at the pleasure of the owner. And, secondly, that it has a practical defect very common to all secret locks; namely, that a skilful examiner may discover the secret, if the workmanship be in the smallest degree inaccurate. In this manner I opened the present lock, at the very first trial. If the three notched pieces be not truly circular, and the motion of the bolt extremely precise, and the lengths of the tails accurately adjusted, one of these tails will bear before the other. I therefore pressed the bolt back, by urging the figure of the moon towards that of the sun, and then turned the three pieces round one after the other. Two of them moved freely round, and the third gave notice when the bolt fell in, and consequently told its secret. Of the remaining two pieces, one moved freely, and the other hung and shewed its position. And lastly, the third then required only to be moved till the bolt took its notch, and went quite back.

But it may be made safe by a very slight change.

This lock could be easily made safe, by adding short teeth to the inside pieces all round, and making the tails of the bolt terminate in small projections adapted to take between those teeth. By this contrivance, the tentative process of turning the pieces would be rendered impracticable.

* The secret padlock of four rollers is sold at our ironmongers. I bought one at Downer's in Fleet Street, for sixteen pence.

It would also be easy to make it into a true lock of combination; by making each of the circular pieces to consist of two equal pieces, locking into each other by contrate teeth, and screwed together by a nut. And if one of these pieces were permanently fixed to the axis, and the other were to carry the edge-teeth, and notch, as mentioned in the last paragraph, the lock would not only be secure from opening by the method I used; but it would also be in the choice of the possessor to vary the secret of the positions by unscrewing the nut, and changing the application of the contrate teeth, after which he would secure them together as before.

And also extended in its combinations.

XI.

Analysis of a Pulmonary Calculus, by PHILIP CRAMPTON, M. D. Member of the Royal College of Surgeons in Ireland. Communicated by the HONOURABLE GEORGE KNOX. F. R. S. M. R. I. A. &c.*

SOME apology seems necessary for offering the analysis of a single pulmonary concretion, but the difficulty of procuring those substances, and the importance of every fact, however insulated, which relates to the changes that take place in the bodies of living animals must plead my excuse.

Difficulty of obtaining pulmonary concretions.

I have no where been able to find a detailed analysis of pulmonary calculi; Mr. Fourcroy mentions them in a general way, as being similar to lachrymal, pineal and salivary concretions, all of which he says are composed, as might be expected *a-priori*, of phosphate of lime (either from a superabundance of that substance in the fluids of surfaces, or in the blood)

They have not been examined.

But as the calculus in question neither agrees with any of those in its external characters, nor in its chemical composition, without in the least calling in question the accuracy of Mr. Fourcroy, I shall briefly state the result of my own examination of it.

The calculus which is the subject of this paper, was taken from the lungs of a deceased soldier, in the year 1796, and

Description and history of a pulmonary calculus.

* Read before the Royal Society in August 1802.

was deposited in the museum of the Royal College of Surgeons in Ireland, It was irregularly spheroidal and measured about $6\frac{1}{2}$ inches in circumference. It appeared to be formed of short thick branches, proceeding from a solid center; its fracture was laminated, of a pure white colour, small communicating canals appeared to intersect its substance in every direction*. The saw cuts it with difficulty; 10 grains afforded an analysis.

It was carbonate
of lime.

grs.
3.7 carbonic acid.
4.5 lime.
1.8 albumen and water.

10.0.

The analysis.

I shall not trespass on your time by detailing the process of so simple an analysis, suffice it to say, that it dissolved with effervescence in the sulphuric, nitric, muriatic, and acetic acids, leaving some floculi of coagulated animal matter (probably albumen) floating in the liquor of solution, the gas which was extricated precipitated lime from lime water, and the quantity of carbonic acid was ascertained, by adding a known quantity of muriatic acid to a certain quantity of the calculus, and taking the loss after effervescence for the weight of the carbonic acid which was extricated. A portion of the calculus exposed to a low heat assumed a blackish hue, but when subjected to a white heat for 20 minutes, it became of a brilliant white colour. The residuum was found to consist of pure lime. Morgani Epist. 17 to 19 upon the subject of pulmonary calculi, says, that he has seen many of these substances coughed up, that they were light and porous like pumice stone, and varied from the size of a millet-feed, to that of a pea; but he once saw a calculus which had been taken from the lungs of a person who had died of *Thisis Pulmonalis*, which weighed 20 grains, was "*hard and heavy like marble.*" Dodenius saw a pulmonary calculus taken after death from the lungs of a nobleman, which affected the shape of the bronchiæ, having short thick branches proceeding from it, and canals which intersected it in every direction. Morton saw smooth cretaceous stones taken from the lungs, Morgani Epist. XV. 23.

Other calculi
referred to.

* Apparently formed by the deposition of calculous matter upon the bronchial vessels.

It would appear thence that there are at least two kinds of pulmonary concretions: those which Morgani observed to be thrown up by coughing, answer the description of the calculi, examined by M. Fourcroy, and those it has been stated were composed of phosphat of lime, while the concretions which were hard and heavy like marble, it is probable agree in their chemical composition, as well as in their external characters with the calculus, which is the subject of this paper; this it has been proved was composed of carbonate of lime. A question then naturally arises, as to the formation of carbonate of lime in the lungs. Was it deposited as such from the blood or formed in the Bronchiæ by the chemical combination of its principles? We have certainly not hitherto been able to detect carbonate of lime in the blood, and we have no reason to suppose that the phosphat of lime which exists in it, can be decomposed by the carbonic acid which is formed in the lungs, to avoid these alternatives, the chemical physiologist might be inclined to look to an external cause for the formation of this extraordinary substance, and with this view, he would enquire into the previous occupation of the person from whose lungs it had been taken; but he would be checked in his enquiries by the recollection that the same chemical affinities which can form carbonate of lime in the teeth and bones, may by an accidental coincidence deposit it in the lungs.

Probability that there may be two kinds; viz. the carbonate and the phosphate of lime.

XII.

Of the State of Vapour subsisting in the Atmosphere. By RICHARD KIRWAN, Esq. LL.D. F.R. S. and P. R. I. A. *

VAPOUR or moisture in the atmosphere may subsist in dense air, or in air highly rarified: that it is found in the former is well known, and that it may subsist in the latter appears by the observations of Bouguer, for he saw clouds three or four hundred toises above Chimboracho, and consequently at the height of twenty-two thousand five hundred and twenty-eight English feet, or 4,3 miles over the level of the sea; a height

The substance of vapour in dense and in highly rarefied air.

* From his essay on the variations of the atmosphere; In the Irish transactions, Vol. viii.

at which in the temperature of 32° a barometer would stand at 12,7 inches. At such heights, and at much inferior, since evaporation proceeds much more quickly, it is not to be supposed that all the vapour so rapidly produced is dissolved in the ambient air, but part rises uncombined as it does under an exhausted or half exhausted receiver, and in this case Mr. De Luc's system is admissible. This emission of pure vapour seems to begin at heights at which the density of the air is 25 (that is at heights at which the barometer would stand at twenty-five inches, and thus I shall in future express the various densities of air,) at least it is very considerable where the density is twenty, as already seen, p. 309. This leads me to treat of the properties and state of pure invisible vapour, namely, its specific heat, elasticity, and specific gravity.

Latent Heat of
Steam.

The immortal Doctor Black, the father of all discoveries of this kind, informed me that the vapour of water, boiling at 212° , that is at 180° above the freezing point, and possessing the same sensible heat as the water, contains nine hundred and forty times more latent heat than an equal weight of water does heated to 212° , or 5,222 times more latent heat than it does of sensible heat, counting from the freezing point, for $180 \times 5,222 = 940$ nearly. In this case the pressure or density of the atmosphere is thirty, the barometer standing at the height of thirty inches; and with Doctor Black's account the experiments of Mr. Schmidt of Giessen very nearly agree, for according to him the latent heat of the vapour of water, barometer 29,84 inches, and the heat 212° , is 5,33 times greater than its sensible heat above the freezing point, now $180 \times 5,33 = 959,4$ *. The difference or excess in his experiment proceeds from the pressure of the atmosphere being somewhat lower, as Mr. Watt's experiments prove.

— is less the
greater its
temperature.

Mr. Watt discovered that the latent heat of steam diminished in proportion as its sensible heat increased, Phil. Transf. 1784, p. 335. Now the sensible heat of steam exceeds 180° above the freezing point when the barometer stands above thirty inches, and is less than 180° when the barometer stands lower than thirty inches, as Mr. De Luc first discovered, and may be seen in Sir George Shuckburgh's, and Mr. De Luc's tables, Phil. Transf. 1779. p. 375. From these I have deduced the following table:

* 4 Gren's Phys. Journal, p. 315.

Heat of boiling water.	
Bar.	Heat.
30	212°
29	210,28
28	208,52
27	206,73
26	204,91
25	203,06
24	201,18
23	199,27
22	197,33
21	195,36
20	193,36
19	191,06
18	188,46
17	185,56
16	184,36
15	180,86
14	176,70

The accuracy of this table even in the lower part of the scale is sufficiently apparent by the result of the experiments of Saussure on ebullition on Mount Blanc; for on that enormous mountain, the barometer standing at 16 French inches or 17,05 English, he found water to boil at the heat of $68^{\circ},993$ of Reaumur, a degree which on Geneva thermometers is equal to $185^{\circ},56$ English. Hence we see that distillation may be more advantageously effected on mountains than on plains, and at low barometrical heights than at the greater, yet within certain limits; for at heights that surpass eight or ten thousand feet, the fuel, by reason of the rarity of the air, is more slowly consumed. Hence also from the knowledge of the degree of the heat of ebullition to two or more decimal places, the state of the barometer above or below 212° may infer to one or more decimal places.—The reason of this rapid diminution of the heat of ebullition below 25 inches is evidently the diminution of resistance, from the diminished weight of the atmosphere, which then is very sensible; but as the cold continually produced by evaporation is then also very considerable, the *time* necessary to procure ebullition is longer as Saussure remarked on Mount Blanc.

Table of the temperatures of boiling water at different stations of the barometer, from 30 inches to 14 inches.

vol. vii. in 8vo. § 2011, p. 328. He found the heat of ebullition barometer 16 to be $68^{\circ},993$ degrees, or in English measures barometer 17,05. $185^{\circ},5$ of Fahr. (counting one of Reaumur at Geneva = $2,225$ of Fahr.)

Hence since, according to Mr. Watt, the sensible heats of the vapours of boiling water at different barometrical heights are as the barometrical heights reciprocally, and the specific heats of the vapours of water boiling are as the sensible heats reciprocally, it being known, that the specific heat of the vapour of water heated to 180 degrees above the freezing point is 940. The specific or latent heat of the vapour of boiling water, whose sensible heat is known, (and it may be known by the barometrical height as shewn in the above table and the notes) may also be discovered.

Hence the specific heat of steam at different temperatures.

Thus the sensible heat of the vapour of boiling water barometer 30 being 180° above the freezing point ($212^{\circ} - 32^{\circ} = 180^{\circ}$) and the specific or latent heat of vapour, whose sensible

fible heat is $208^{\circ},56$ (that is $176,56$ above 32°) as it is when the barometer stands at 28 inches, is 958 for $\div 176,56$. $180 :: 940.958^*$.

Unto combined
vapour consider-
ed not to exist.

As pure invisible vapour does not in my opinion (of which I have already stated the grounds) exist in the atmosphere when its density is higher than 25, as it is in most of the inhabited parts of the globe, but is always in this case united to air, an enquiry into its latent heat at different temperatures below ebullition were superfluous. But as it does exist in air whose density is 25 or less, since it is found in air whose density is 12,5, it becomes necessary to examine its latent heat in such cases, in all temperatures inferior to that of ebullition. Now, by analogy, I apprehend, this latent heat in all inferior temperatures may thus be determined :

Determination of
the latent heat of
pure vapour.

As the *sensible* heat of ebullition, when the barometer is at 25 or below 25 is to the *latent* heat of the vapour at ebullition, so is the *sensible* heat of water heated to any inferior degree above 32° to the latent heat of its vapour, multiplied by 5,222. Thus the sensible heat of water in ebullition barometer 25 being $171^{\circ},4$ ($= 203^{\circ},4 - 32^{\circ}$) its specific heat is 987 ($= \frac{169206}{171,4}$) the latent heat of the vapour of water at 22° above congelation (that is 52 on Fahr. scale) is 657 for $\div 171^{\circ},4$. $987 :: 22^{\circ}. 126 \times 5,22 = 657$. The latent heat of vapour in such cases cannot be determined by experiment on account of the admixture of atmospheric air, we must therefore resort to analogy, which in this case is perfect.

The latent heat of pure vapour at greater heights is more considerable: thus at heights, at which the barometer stands at 20 inches, the latent heat of vapour whose temperature is 22° above 32° , as in the last case, is 730; for the heat of ebullition is $194^{\circ},8$, per table, $= 162^{\circ},8$ above 32° ; and the latent heat of the vapour at ebullition is 1039. Now $\div 162,8$. $1039 :: 2^{\circ}. 140$ and $140 \times 5,22 = 730$.

As air is cooled by the reception of moisture dissolved in it, we must infer that its capacity for containing heat is increased, and hence moist air is more difficultly heated or cooled than dry air of the same temperature. (For the cold proceeds from the absorption and not from the expulsion of caloric.)

* Hence 169206, being the product of $180^{\circ} \times 940$, is the common dividend of all sensible heats *below* 180° . when the latent heat of the vapour is sought at barometrical heights below 30 inches.

The elasticity or expansive force of pure vapour has been examined at every fifth degree of Reaumur above 0 to 110° by Mr. Betancourt, and may be seen in an excellent work of Prony's, his *Architecture Hydraulique*, he has by a most ingenious calculation interpolated the expansions answering to the intermediate degrees. But Mr. Schmidt seems to have determined this expansive force still more exactly than Betancourt. Hence I here insert his table, adding Farenheit's for Reaumur's degrees, and distinguishing the expansions interpolated by calculation from those actually observed by *I*. The forces are measured by the elevation of a mercurial column in inches and hundreds of a French inch *.

Reaumur.	Farenheit.	Expansive Force.	Reaumur.	Farenheit.	Expansive Force.
1	34°,25	,01 <i>I</i>	21	79°,25	1,01 <i>I</i>
2	36,5	,03 <i>I</i>	22	81,5	1,01
3	38,75	,05 <i>I</i>	23	83,75	1,19 <i>I</i>
4	41,	,07 <i>I</i>	24	86,	1,29 <i>I</i>
5	43,25	,11	25	88,25	1,30
6	45,5	,15	26	90,5	1,38 <i>I</i>
7	47,75	,16 <i>I</i>	27	92,75	1,42
8	50,	,20 <i>I</i>	28	95,	1,60 <i>I</i>
9	52,25	,25 <i>I</i>	29	97,25	1,80 <i>I</i>
10	54,5	,28	30	99,5	1,93
11	56,75	,34 <i>I</i>	31	101,75	1,02 <i>I</i>
12	59,	,38	32	104,	1,12 <i>I</i>
13	61,25	,44	33	106,25	2,23
14	63,5	,50 <i>I</i>	34	108,5	2,40 <i>I</i>
15	65,75	,55	35	110,75	2,68
16	68,	,61	36	113,	2,80 <i>I</i>
17	70,25	,69 <i>I</i>	37	115,25	3,20
18	72,5	,76	38	117,5	
19	74,75	,84 <i>I</i>	39	119,75	3,40
20	77,	,90	40	122,	3,64
			80	212,	28,

* The Paris cubic inch = 1,21 English. Now the English cubic inch of mercury when its specific gravity is 13,6 weights 3443,7 English grains, therefore the Paris inch weighs 4186 English grains, and $\frac{1}{10}$ of this inch = 418,6 grs: and $\frac{1}{100}$ of this inch 41,86 grains.

Note.—1st. Most of the interpolations from the 88th degree to the 122nd I have myself inserted as those calculated by Schmidt erred too widely by his own account. 4 Gren. Phys. Journ. 273.

2d. Mr. Pictet has also made a set of curious experiments on the elasticity of pure vapour in low temperature. *Essais de Physique*, p. 157. He found that a grain of warm water in *vacuo* evaporates in forty minutes in the temperature of 38° Fahr. under a receiver containing 1452 English cubic inches,* but that it did not diffuse itself equally in less than six hours, and then raised the hygrometer from 17° to 60° that is 43°, and during this whole time the cold under the receiver was constantly decreasing, though slowly, which decrease undoubtedly contributed to the diffusion of the vapour.

Mr. Schmidt has also made a series of experiments upon the dilatibility of air, made as *dry* as possible by exposure to hot tartarin, an object of great importance, that had never before been examined. This table I here insert, converting Reaumur's degrees into those of Fahr. and adding from his formula the degrees he omitted.

Expansions of dry Air:

Table of expansions of dry air.

	Reaumur.	Fahr.	Expansion of one Inch at 32	Reaumur.	Fahr.	Expansions of one Inch at 32°.
1		34°,25	,004467		79°,25	,0938175
		36,5	,008935		81,5	,0982850
		38,75	,0134025		83,75	,1027525
4		41,	,0178700	24	86,	,1072200
		43,25	,0223375		88,25	,11116875
		45,5	,0268050		90,5	,1116155
8		47,75	,0312726		92,75	,1206225
		50,	,035740	28	95,	,1250909
		52,25	,0402075		97,25	,1295557
12		54,5	,0446750		99,5	,1340250
		56,75	,0491425		101,75	,1384925
		59,	,0536100	32	104,	,1429600
16		61,25	,0580775		106,25	,1474275
		63,5	,0625450		108,5	,1518950
		65,75	,0670125		110,75	,1563625
20		68,	,0714800	36	113,	,1608300
		70,25	,0759475		115,25	,1652975
		72,5	,0804150		117,5	,1697650
20		74,75	,0848825		119,75	,1742325
		77,	,0893500	40	122,	,1787000
					212,	,3574000

* Ibid. page 91.

Note.

Note.—1. Hence we see that 1000 inches or measures of dry air at 32° would become 1004,4675 at 34,25 Fahr. and at 50°, would become 1017,87. Hence 1000 measures of dry air gain 1,985555, &c. by each degree of Fahr. above 32° (or more compendiously 1,9856 which is true to two decimal places) or nearly two.

2. We see the source of the discordant results of D'Amontons, De Luc, Lambert, Schuckburg, Roy, Berthollet, and Monge, &c. for they all operated upon air more impregnated with various degrees of moisture; besides taking the boiling point at different barometrical heights; in the present experiments it was taken at 29,841 English inches.

3. It appears that the expansions are as the differences of heat above 32° as D'Amontons, Lambert, and Schuckburg also noticed, though their experiments, not being made on perfectly dry air, could not be very exact.

The dilatation of the moisture contained in air has been separately examined by Mr. Schmidt, and he has shewn how from it the volume of air saturated with moisture, saturated I say at every degree of Reaumur, may be discovered; the result of his experiments appear in the following table of the volume which 1000 measures at 32° of air would acquire if *saturated with moisture* at each degree of Reaumur above 32° expressed on Fahrenheit's scale *.

Dilatations of air saturated with moisture.

Reaumur.	Fahr.	Expansive Force.	Reaumur.	Fahr.	Expansive Force.
1°	34°,25	1010,56		70,25	1122,68
	36,5	1010,78		72,5	1132,25
	38,75	1016,45		74,75	1142,53
	41,	1022,21	20	77,	1152,83
5	43,25	1028,58		79,25	1164,02
	45,5	1034,97		81,5	1175,23
	47,75	1040,41		83,75	1186,52
8	50,	1048,52	24	86,	1198,59
	52,25	1056,26	25	88,25	1211,44
10	54,5	1064,72		90,5	1223,65
	56,75	1071,28		92,75	1279,62
12	59,	1078,52	28	95,	1377,09
	61,25	1087,11		97,25	1494,02
	63,5	1095,76	30	99,5	1610,02
15	65,75	1104,46		101,75	1725,49
16	68,	1113,21	32	104,	1849,96
			33	106,25	1983,42

* To prevent mistakes, it must be noted that this table is not meant

Remarks.

Air saturated with moisture expands more than dry air.

Note.—1. Hence we see that air saturated with moisture at high heats is much more expanded than dry air of the same temperature, as De Luc and General Roy have also observed. but in temperatures below $36^{\circ},5$ dry air is more dilatible which probably induced Saussure to conclude it was so at higher temperatures. At $54^{\circ},5$ the difference is very perceptible for 1000 parts dry air at 32° are expanded at $54^{\circ},5$ that is by $22^{\circ},5$ above the freezing point to 1044,67, whereas 1000 parts of air saturated with moisture, are extended to 1064,72 and in higher heats, the differences of expansion are incomparably greater.

Moist air more suffocating than dry.

2. Hence it is plain why moist air, such as that of the West Indies is much more suffocating than dry air of the same temperature. For 1000 cubic inches of air saturated with moisture at 86° of Fahr. contain nearly 76 inches of moisture which is useless to respiration.

Gen. Roy's experiments.

3. These experiments agree with those of general Roy, in which steam was introduced at hazard, for the general found that from 32° to 52° each degree gave at a mean 2,588, and consequently these 20° would expand 1000 inches to 1051,76, and by Schmidt's experiments much more accurately made, we have 1050,33.

Irregularity of the expansions of moist air,

4. Schmidt also observed a peculiarity in the expansion of moist air, previously noticed by Roy, for Schmidt found that the expansibility of air, saturated with moisture, was smaller than the expansibility of pure vapour, until the 167^{th} degree of Fahr. but in higher degrees they constantly approached nearer to each other. And the general observed that the mean rate of expansion, which from 152° to 172° of Fahr. was 12 for each degree, did from the 172° to the 192° increase to 17,88 for each degree, and increased still more after the 192° to the boiling point. The sluggishness of expansion of air, saturated with moisture at about 32° , was also noticed by the general, and he hence concludes the mean rate of expansion from 0 to 32° of Fahr. to be 2,27 for each degree, which is smaller than that of drier air.

meant to express the dilatations of air saturated at any particular degree of heat it would acquire at other superior degrees, but only the bulk that 1000 parts dry air at 32° would acquire by saturation at each higher degree.

These

These variations of the rates of expansibility of moist air, saturated at different temperatures, Schmidt very justly attributes to the variation of the degrees of affinity or adherence of air and vapour to each other at different temperatures. At 32° Fahr. it is very strong, and also below that degree; and hence the strong solvent power of air, colder than the water it acts upon, remarked by Richman; but if both are equally cold very little moisture will be taken up by the air, as already mentioned; and hence I have said that air dissolves vapour when this is in a nascent state. But in heats above 167° or 170° air and vapour are disposed to separate.

5. Hence we may deduce the impossibility of discovering a coefficient universally applicable to express the rate of expansion of air in every state of moisture, (as Tremley has well noticed. See 2 Saussure voy. aux Alpes 4to). This must vary with the mean state of hygrometers above and below the heights to be measured; and experiments of this kind have not yet been made. De Luc's coefficient answers tolerably well for very dry air, that is whose saturability is greatest, Sir George Schuckburg's for air much moister, and general Roy's for air still more moist, that is whose saturability is smallest. Hence each succeeds in certain cases, and fails in others. The dilatation or contraction, which air saturated with moisture at any one given degree of temperature receives without the addition of any more moisture, at any higher or lower degree of temperature, has not as yet been discovered; for Schmidt, who alone has attempted it, is justly dissident of the correctness of the table he has given of it, and in fact it is not grounded on the indication of any known hygrometer, and improperly supposes the L. degree to indicate the mean betwixt the lowest and saturation. Whereas the LXVth degree on Saussure's indicates that mean; and XCVIII. and not C. indicates saturation.

According to Mr. Watt (as stated by De Luc, 3 Meteorolog: p. 145) the specific gravity of pure vapour is to that of air as 4 to 9. I suppose he compares it with air at the usual density of 30 or 29, and at some particular temperature which is not mentioned; for at high temperatures the difference must be much greater, as appears by the foregoing tables.

(To be concluded in our next.)

XIII.

Notices concerning some Philosophical Apparatus.—Electrical Battery of Talc.—Complex Horseshoe Magnet.—Mr. DAVY'S Apparatus, &c.

Battery of talc.

SEVERAL years ago (in 1788), when I was busied in electrical researches, I was induced to construct a battery of talc, in consequence of the very great capacity of that substance which I found (conformably to the determination of Mr. Cavendish with glass), to be inversely as its thickness. As our galvanic researches now seem to give additional interest to apparatus of great capacity and low intensity, I have thought it might be acceptable to give a sketch of it.

Estimate of the electrical capacity of talc. It is as the thickness inversely; or eight times that of glass.

The full charge of two square inches of talc gives a shock sufficiently severe to be felt above the elbows, giving a spark of above one tenth of an inch long, and requiring the friction of more than one foot of glass to charge it. I compared this talc with a large jar of 351 square inches, and 0.082 inch thick, and the talc was 0.01125 inch thick. The communication from plus to minus was made by one single wire, and Lane's electrometer was set so that the talc exploded regularly at each turn of a seven inch cylinder. The jar was then placed in the circuit, and exploded at twenty-one turns. Their absolute capacities were therefore as 1 to 21. But the surfaces are as 1 to 175, whence the capacities of equal surfaces were as 1 to $\frac{175}{21}$, or about 1 to 8. And the thicknesses are inversely 11 and 82, whence it appears that the actual capacities proved rather in favour of the talc, if taken from the inverted ratio of the thicknesses. This, however, may be only apparent, because the talc was more rapidly charged, and had an insulated rim of only 0.4 inch, whereas the glass had a bare rim of 4.0 inches. In addition to which it may be observed, that the uncertainty of the result is not quoted, than the unavoidable errors from the measurements of the thicknesses and the unsteadiness of excitation.

Drawing and description of a talc battery two inches square, and equivalent to seven square feet of glass.

Plate XI. Fig. 1. represents a battery composed of 12 pieces of talc, each measuring 2.6 inches in the side, and coated on a surface of two square inches. The thickness of each piece was 0.0025 inch, and consequently its capacity may be taken at about 20 times that of thin window glass of one-twentieth

of an inch thick. The whole surface was 48 square inches, which multiplied by 20, answers to 960 square inches of glass in capacity, or almost seven square feet. It is necessary that a card should be placed between each plate of talc and its neighbour, and hence by an easy computation, it is found that the talcs and cards (of $\frac{1}{30}$ of an inch thick), requisite to answer to 100 feet of glass would be about $3\frac{1}{2}$ inches thick.

This instrument for the purposes of low intensities would be incomparably cheaper and more manageable than glass. Indeed a battery answering to twenty thousand square feet would be very portable, and might be put into a box of one foot square and two feet deep. But it must be remembered that the intensity cannot be greater than to give an explosive spark of one thirteenth of an inch, or less, and consequently that this apparatus is limited to experiments requiring no greater intensity.

The construction is that described by Beccaria, in his artificial electricity. Fig. 2. represents the arrangement of the plates of talc with square coatings of tinfoil between them. From each coating alternately succeeds a tail, so that all the positive sides unite above, and all the negative below. In Fig. 1. the wire *c* communicates with all the coatings of one kind, and the wire *a* in like manner unites all the coatings from the opposite sides; and at *b* is an insulated screw wire forming the electrometer of Lane. The pillar supporting the screw *c* may either be of glass or other insulating matter, or else a piece of amber, *d* may be placed between the battery and *c*. In my apparatus, the talcs were confined between two squares of glass, and on the outside of these were two smaller pieces of wood as in the figure.

I did not make many experiments with this battery, because the spontaneous explosions of one plate usually broke all the rest, and it was my intention to have them somewhat thicker, and to have interposed cards between plate and plate. But other pursuits have hitherto prevented my resuming this object.

Complex Horseshoe Magnet.

Fig. 3. in the same Plate XI. was given me by the late George Adams of Fleet Street. It was wrapped in a paper on which the workman who made it had written the following words.

“ The

“ The outside bit taken off, but drops when the inside one sticks, but drops when the outside one is put on. Apply the magnet as directed, the strokes to be north to north strait across the inside, very difficult to gain, but may be otherwise varied at pleasure.”

has four poles and four neutral points, which are changed in their power by the conductors.

When I first had it, the middle piece would not drop off when the outer was put on, but on the contrary, the outer was disengaged by application of the inner piece, and this is the case at present. I could not obtain any explanation of the obscure writing I have copied. The sketch shows where the poles were at first and still are placed, being four in number, so that it seems to be two horseshoe magnets joined by a short bar nearly strait. It barely held its own weight by the outer piece, and much less by the inner; but now, whether by the course of time in which it has been carelessly thrown in a drawer among other metallic matters for sixteen years, or whether from any other cause, its attraction is strongest at the inner extremities though not sufficient to bear its weight.

Galvanic Apparatus.

Davy's apparatus for galvanizing the gases.

Plate X. Fig. 2 and 3. represents the apparatus of Mr. Davy for taking the galvanic spark in the gases*.

Fig. 1. represents the apparatus for taking the spark. A is a tube graduated to grain measures. C is a platina wire hermetically sealed into the tube and having a piece of charcoal attached to its top. B is a moveable platina wire, having charcoal at the top; the effect is produced by making the contact between the piece of charcoal. In cases where the fluids are very imperfect conductors, the wires may be used without the charcoal.

Fig. 2. represents the apparatus for taking the spark in gases, it is used over mercury. A and B are the communicating platina wires to which the charcoal is fastened, and C is the graduated tube in which the gas is acted upon.

* Royal Institution, I. 214.

XIV.

Experiment to determine the relative Quantities of Light afforded by Candles of different Dimensions, in a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

THOUGH your ingenious correspondent Mr. Ezekiel Walker, asserts with considerable decision, that the light afforded by candles, is proportioned to the quantity of material consumed, yet as he has not given the detail of his experiments, but seems in some measure to have deduced this result by argument, from the supposed nature of the subject; and as the quantity of light does undoubtedly follow a very different ratio in candles which require snuffing, accordingly as that operation is either performed or neglected. I think it very desirable, that a course of experiments should be instituted for the purpose of affording the results you have yourself enumerated in your annotation upon one of that gentleman's papers. In the mean time, I present you with a single experiment upon his two wax candles, which were taken by me in preference, because they require no snuffing.

One of these candles was a taper of sixteen to the pound, and measured in length twelve inches; the other was a thicker candle of six to the pound, and nine inches long. It is not necessary to state the diameters, because they are not required in the present estimate, and might if necessary, be deduced with greater precision from the weight and length, than from actual admeasurement; and with regard to the wicks, I have only to remark, that they were well adapted to the candles—though it was impossible for me to ascertain their weight or fineness in the manufactured state.

These two candles were lighted at the same time, and when they were in the state of full and perfect combustion, their lengths were respectively measured and noted down. The intensities of their lights, were compared by the method of shadows. The distance between the two candles was twenty-four inches; and when the shadows were of equal intensity, the distance of the screen from the nearest candle was seventy-

two

Prefatory remarks on Mr. E. Walker's experiments on light, &c.

Experiment with a thin and a thick wax candle.

Experiment with
a thin and a
thick wax
candle.

two inches. The shadow into which the large candle threw its light, was evidently ruddy or brownish, while the other shadow illuminated by the small flame, seemed to have a bluish tinge. The flames themselves appeared to differ in their aspect; in the same manner as their shadows: now the lights emitted being inversely as the square of the distances, will be as 36 to 64, or as 9 to 16; and when both were extinguished, it was found that three inches of the large candle had been burned, and 5.8 inches of the smaller: but of the larger candle, fifty-four inches in length make one pound, and of the smaller 192 inches make the pound; and if we divide 54 inches by three, the quotient will be 18, which shews, that the whole pound of large candles would have required eighteen times our period for its consumption: in like manner, by dividing 192 by 5.8, we have a quotient of 33.1, or the time of burning a pound of the small candles. And lastly, if we multiply the quantity of light afforded by each candle, by the time required to consume a pound of each, we shall have the proportionate quantities of light afforded by equal expenditures of the combustible, that is to say, 18 multiplied by 16, produce 288, or the light afforded by the larger candle, and 33.1 multiplied by 9, gives 297.9 for the smaller: which is one part in 29 in favour of the small candle.

I consider this difference as a greater quantity than the errors of experiment, though upon the whole, it seems to confirm Mr. Walker's doctrine.

SCIENTIFIC NEWS.

On the Nutriment to be obtained from Bones.

Results as detailed by Proust.

IN the first volume of the octavo series of this Journal, page 100, notice is taken of the experiments on this subject, detailed by professor Proust of Madrid, in his inquiry into the means of improving the subsistence of the soldier. He states the quantity of jelly obtained from bones to be very variable, according to the nature of those employed, viz. from nine pints to thirteen quarts, from five pounds of bones; those from the rump and spine yielding the most and best; and those of the legs and thighs the least and most unpalatable.

Sinc

Since his work a memoir on the gelatine of bones has been published at Paris by Cadet de Vaux, extracts from which have been circulated through France by order of the minister of the interior, in which this method of making a wholesome and pleasant food is strongly recommended. His experiments are said to have afforded results nearly similar to those of Proust, but on being repeated by Dr. Young seem to have completely failed. His experiment which is inserted in the fifteenth number of the Journals of the Royal Institution, is as follows:

Three quarters of a pound of bones were taken from a piece of beef which had been roasted; they were principally the vertebral and spinal processes; they were reduced to a smooth paste, by pounding them for twenty minutes, with a little water, in a large mortar. They were kept on the fire in an earthen pipkin, simmering or slowly boiling, for forty hours, with the addition of proper vegetables, so as to make a quart of broth. The broth shewed no tendency to jelly when cold; it tasted only of the vegetables, and had neither the appearance, nor the flavour of a highly nutritious substance: there was nothing disagreeable in it except its insipidity, but a few ounces of meat would probably have made a better soup.

By Cadet de Vaux.
Dr. Young's experiment.

Method of preparing Muriatic Ether with simple Acid; by
M. BOSSA, Apothecary at Hameln*.

Preparation of
muriatic ether
by Basse.

MELT marine salt in a crucible and keep it in fusion an hour, or till the whole of the water of crystallization is dissipated. Put twenty ounces of this salt into a tubulated retort, adapt a curved tube to it, and plunge the tube to the bottom of a bottle with two necks, into which have been poured ten ounces of alcohol, marking 100 on Richter's alcoholimeter. (This alcohol must be prepared by mixing three parts of highly rectified spirit of wine with one part of potash, melted and pulverized whilst hot, and distilling it in a retort till it be diminished one half.) When the whole is well luted, pour into the retort, in quantities of half a penny weight at a time,

* From Scherer's Journal de Chemie.

ten ounces of highly concentrated sulphuric acid. After each introduction of acid, close the tubulure carefully, and put in no more fresh acid till the salt has quite ceased bubbling. The cork of the other neck of the bottle must be taken out from time to time, to suffer the air condensed above the alcohol to escape.

After all the acid is introduced, place the retort on a sand-bath, and heat it gradually till all the muriatic acid be expelled. During this part of the operation, care must be taken frequently to cool the bottle containing the alcohol, by wrapping a wet cloth round it.

The alcohol, thus charged with acid, is then put into a retort and distilled to one half. Shake the distilled liquor with a sufficient quantity of alkaline ley to carry off the acid, decant the supernatant portion, which is ether, and keep it in bottles well corked. From the above mentioned quantities, two ounces and a half of ether are usually obtained.

M. Bossa asserts, that by operating with thermoxy dated muriatic acid, light alcoholic ether can never be obtained, but only a heavy oleaginous ether, which, instead of floating on water, falls to the bottom of that liquid.

On the Effects produced by the Decortication of Trees.

Decortication
of trees.

SEVERAL papers have lately appeared in the American Philosophical Transactions, and other scientific works, recommending the depriving trees of their bark as a means of exterminating noxious insects which find shelter in the crevices of the bark, and between it and the body of the tree. From the effects produced on two apple-trees, as related in the Monthly Magazine for last month, though the source from which their information is derived does not appear, it would seem that this practice is destructive of the tree itself. The relator asserts, that, one of the trees died in May following the operation, and the other before the neighbouring trees had shed their leaves. He also states that the denuded trees were affected by several long fissures, which he attributes to the alterations of heat and cold, and considers as the immediate cause of their death. He concludes by a query, whether

whether sheltering the trunks by matting, or other effectual means might cause the success of the scheme, and recommends the experiment.

The society of agriculture, commerce, sciences and arts at Chalons, in the department of the Marne, have repropoed the following prize question, to which no satisfactory answer was received last year.

“ What description of useful plants of any species, can be made to grow on the most barren soils, such as those of the ancient Champagne (or department of the Marne) which presents little or no vegetable earth, above a chalky or sandy tufa ?”

Prize question.
Plants for
barren soils.

In consequence of the importance of the question, the society have increased the value of the gold medal, from 1000 to 1500 grammes of silver, or 300 livres ; it is to be decreed at the public sitting of the Society, on the 1st of Vendemiaire in the 12th year.

ACCOUNT OF NEW BOOKS.

Researches into the Properties of Spring Water with Medical Cautions (illustrated by Cases) against the Use of Lead in the Construction of Pumps, Water Pipes, Cisterns, &c. By WILLIAM LAMBE, M. D. late Fellow of St. John's College Cambridge, pp. 204. London, 1803.

Lambe on Spring
water.

THE object of this work is an investigation of the qualities accidentally acquired by water, which render it in many instances injurious to health. Of these, lead is a principal cause, which in whatever form it is introduced into the human body, is equally deleterious and fatal. In treating of this poisonous metal as a receptacle or conductor of water, he shows how it is acted on by it and air, so as to be capable of suspension in the fluid, by which means it is received into the alimentary canals, and conveyed to every part of the system. Hence arise diseases, which from their cause being unsuspected, and consequently suffered to continue its effects, are unconquerable by medical skill. Many instances of this description are given, some of which were removed by change of residence, and recurred on the return of the patient, nor were

were they effectually subdued, until the discovery of the cause, and the discontinuance of the use of the water which produced them.

Mr. Lambe has shewn a persevering attention to the subject he has undertaken to investigate, and has very clearly proved the noxious effects arising from the unguarded use of lead in domestic economy.

Sheldrake on
ruptures.

Useful Hints to those who are afflicted with Ruptures, on the Nature, Cure, and Consequences of the Disease; and on the Empirical Practices of the present Day. By T. SHELDRAKE, Truss-Maker to the Westminster-Hospital, pp. 180. London, 1803.

A
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AUGUST, 1803.

ARTICLE I.

On the Production of Sulphate of Magnesia from the Ashes of Pit-Coal, with Remarks on the Efflorescence of the same Salt, observed by Dr. Bostock. In a Letter from the Rev. WILLIAM GREGOR.

To Mr. NICHOLSON.

SIR,

IN the number of your Philosophical Journal for December, 1802*, I read a letter from Dr. Bostock of Liverpool, in which he gives an account of an unexpected production of the sulphate of magnesia; which brought to my recollection a fact, which fell under my notice, about two years ago, and which may possibly tend to throw some light upon the phænomenon observed by your correspondent.—Having accidentally discovered a saline crust adhering to the wood-work of a frame on which I sift my coal-ashes, for economical and agricultural purposes, I scraped off a sufficient quantity of this substance with a view of ascertaining its nature. I dissolved it in distilled water, and separated all impurities from it, by the filter. This solution tried by the usual tests, gave indications of the presence of sulphuric acid, of lime, and of some other earth. I evaporated the solution, and some gypsum was separated; when this salt ceased to appear I poured off the fluid, which had assumed

Efflorescence from ashes of pit coal, which afforded gypsum and sulphate of magnesia.

* Vol. III. p. 238.

a brownish hue. I gradually evaporated it still further, and set it aside in a cool place: It shot into a beautifully crystallized salt, which agreed in form and properties with the purest sulphate of magnesia, except as to colour, which inclined towards a brownish tint, which circumstance probably arose from the extractive matter of the decayed wood, which this salt had penetrated. The coals, which had been sifted in this place, were those from Liverpool and Wales. The place was open to the air and rain.

Liverpool coal treated more carefully afforded a purer result.

In order to narrow my field of conjecture as to the origin of the above-mentioned salts, and to exclude, as far as possible, the concurrence of unknown circumstances, I collected a sufficient quantity of ashes from my own fire in my study, where I had burnt Liverpool coals only. On repeating the same process, the result was the same: I obtained gypsum and a still purer sulphate of magnesia: The quantity of the latter exceeded that of the former. I examined also the ashes produced by Welch coal, and I detected gypsum and the sulphate of magnesia in them also. They contained, however, a much larger quantity of gypsum than of the latter salt.

Welch coal also.

The exact quantities of these salts, which might be obtained from a given weight of the ashes of these different coals, I did not ascertain.

These ashes were a good manure.

The ashes obtained from the above-mentioned coals were from time to time spread, (or to use our common and provincial term) *skeaded* * over grass, and with apparently good effects, notwithstanding the sulphate of magnesia, which, I was well assured that they contained.

Mr. Tennant has proved that magnesia and its carbonate are inimical to vegetation, but the sulphate may be good.

Mr. Tennant's discovery of the unfriendly nature of magnesia to vegetation is to be ranked amongst the few prominent and well established facts in agriculture, which modern sagacity has brought to light. Communications of facts and discoveries in this highly important science are, indeed, abundantly supplied unto us by ingenious and experienced agriculturists, but the truth and the utility of the greater number of them must, from the nature of the subject, depend upon such a variety of variable circumstances, that the application of them to particular cases can be only considered as a matter of experiment. But Mr. Tennant's observations, contained in his essay on the

* Etymologists may probably recognize in this word the Greek verb $\Sigma\kappa\acute{\iota}\delta\alpha\omega$.

different sorts of lime used in agriculture, seem to stand, in a great measure, independent of local and contingent circumstances, and may be safely recorded amongst the certainties of the science. But in this, as in other subjects, we must be careful, that our conclusions do not wander beyond the limits which our premises warrant. Magnesia and carbonate of magnesia may be inimical to vegetation: But it by no means follows from the admission of this fact, that magnesia combined with sulphuric acid possesses the same destructive properties.

I conjectured that the origin of these salts in coal ashes was to be ascribed to the decomposition of schistus and pyrites, both which substances are frequently found amongst pit coal. Origin of these sulphates.

In order to form a judgment respecting the probability of my conjecture, I mixed some pounded pyrites with some steatite reduced to powder in the proportion of about 2 : 1. and exposed this mixture in a crucible for about an hour and a half to the heat of a brisk fire in a common grate. There remained at the bottom of the crucible a dark red powder, which was exposed to the air for a considerable time before I examined it.—I threw it upon a filtre and poured distilled water upon it, which tasted exceedingly bitter, and on further examination, I found it to contain sulphate of magnesia. Prussiate of potash did not betray the least trace of iron held in solution. Whether the magnesian salt was in a state of sulphite at first I cannot tell. Pyrites and steatite ignited and then exposed to the atmosphere afforded sulphate of magnesia.

In places where steatite and pyrites are plentiful, this process for obtaining sulphate of magnesia would be neither difficult nor expensive.—In the case which Dr. Bostock records, might there not have been some coal ashes mixed with the clay used for making the brick, on which he discovered the saline efflorescence? This artificial process may perhaps be profitable.

As I have never seen the foregoing facts noticed by any person, I have been induced to transmit an account of them to you. If you think them worthy to be recorded, they are at your service.

I am,
SIR,

Your obedient humble servant,
WILLIAM GREGOR.

Creed, near Grampound, Cornwall,
July 6th, 1803.

II.

The Method of making Artificial Yeast in Germany and Sweden.*
 Communicated by SIR A. N. EDELCRANTZ.

Artificial yeast.
 One hundred
 pounds of malt
 is brewed with
 hops nearly in
 the common
 way;—evapo-
 rated to half;—
 fermented with
 yeast;—and
 agitated with
 fifty or more
 pounds of meal.

TO one hundred pounds of the best malt, consisting of one part of malted wheat and two parts of malted barley, dried in the open air and well ground or bruised; add ten pounds of good hops, and brew the mixture with three hundred and fifty pounds of water to form wort, in the common way. After a short boiling you separate the grains and hops from the wort, which last by continued boiling may be reduced to the half or one hundred and seventy-five pounds. Cool it down as soon as possible to 70° fahrenheit. and mix it then with thirty-two pounds of good yeast, which this first time may be common brewers yeast, but in every subsequent operation of the artificial. The wort will very soon ferment, and after four or five hours it will be covered with a thick, white, yeasty froth. When this appears, the whole mass must be strongly agitated, and at the same time mix it well with from fifty to seventy-five pounds of fine ground meal of wheat or barley, either malted or unmalted. By keeping it constantly in a cool place, it will continue in a good state for ten or fifteen days in summer, and in winter from four to six weeks, and still longer if stirred once or twice daily. This yeast, when employed in distilleries, breweries, or for making cider and vinegar, as also by bakers and pastry-cooks, is asserted to be as useful and effectual as the best common yeast.

III.

Remarkable Fact of the Disappearance of a Mixture of Oxygen and Hydrogen, at the common Temperature over Water; inducing the Probability that Water may be formed at low Heats. In a Letter from Mr. B. HOOKE.

To Mr. NICHOLSON.

S I R,

IF you think the following fact which occurred to me in the summer of 1800 (and which seems rather to prove the forma-

* This process was communicated to me by my learned friend, the celebrated chemist, Mr. Westrumb in Hameln, and differs very little, though more perfect, from the method used in Sweden.

tion of water from its elements at common temperatures) worthy a place in your very interesting Journal, the insertion will oblige, Sir,

your's, &c.

B. HOOKE.

Fleet Street, July 12, 1803.

HAVING been accustomed to keep (for the purpose of deflagrating) a mixture of oxygen and hydrogen gases in the proportion as nearly as I could guess that would form water, I was much surpris'd upon inverting a quart bottle of such a mixture in a pneumatic tub, to find the water immediately rise up and fill it; as I could only account for it by supposing water had been formed from its constituent parts, or that the gases had escap'd from the bottle and left a vacuum. Either supposition appears to be attended with considerable difficulties; for what nice play of the affinities could occasion the abstraction of that portion of caloric which is essential to the aëri-form state of the gases, and must be before water could possibly be formed; and on the other hand, if the gases had made their escapé at the cork, what prevented the atmospherical air from entering? The bottle containing the gases had been left inverted in a common bottle rack with a small quantity of water in its neck for about three months.

Oxygen and hydrogen mixed and confined in an inverted bottle with a little water disappeared during three months and left a vacuum.

IV.

Experiments on the Quantity of Gases absorbed by Water, at different Temperatures, and under different Pressures. By Mr.

WILLIAM HENRY*.

THOUGH the solubility of an individual gas in water forms, generally, a part of its chemical history, yet this property has been overlooked, in the examination of several species of the class of aëri-form substances. The carbonic acid, indeed, is the only gas whose relation to water has been an object of much attention; and, at a very early period of its history, Mr. Cavendish, in the course of inquiries, the results of which were the groundwork of the most important subsequent discoveries, ascertained, with peculiar care, the proportion of carbonic acid

Solubility of gases in water hitherto little examined,

except with regard to carbonic gas.

* From the Philosophical Transactions for 1803.

gas condensible in water, at the temperature of 55° of Fahrenheit. Dr. Priestley also, about the same period, directed his attention to the saturation of water with fixed air, and contrived a simple and effectual mode of obtaining this impregnation. His apparatus, afterwards, gave way to the more manageable one of Dr. Nooth; and this, in its turn, has been superseded by the improved mode of condensing, into water, many times its bulk of various gases, invented and practised by several chemical artists, (as well as by myself,) both in this country and abroad.

Pressure.

The influence of pressure, in accomplishing this strong impregnation, was first, I believe, suggested by Dr. Priestley, "In an exhausted receiver," that most ingenious philosopher observes, "Pymont water will actually boil, by the copious discharge of its air; and I do not doubt, therefore, that by means of a condensing engine, water might be much more highly impregnated with the virtues of the Pymont spring*."

Before describing my experiments on the effects of additional pressure, in saturating water with gases, it will be necessary to state the results of others, that were previously expedient, to determine the quantity of each gas combinable with water, at a given temperature, and under the ordinary weight of the atmosphere. In a few instances, also, it was deemed proper to ascertain the influence of different temperatures, over the condensation of gases in water.

SECTION I.

ON THE QUANTITY OF GASES ABSORBED BY WATER,
UNDER THE USUAL PRESSURE OF THE ATMOSPHERE.

Apparatus for
shewing the
proportion of gas
absorbed by
water.

In order to attain considerable minuteness in observing the proportion of gases absorbed by water, an apparatus was employed, of which the following is a description.

The vessel A (Plate I. Fig. 1.) is of glass, about 2 inches diameter, and $4\frac{1}{2}$ inches long. It is graduated into cubical inches, and quarter inches; and furnished at the top with a brass cap, into which a cock *a* is screwed. To the lower aperture, a copper tube C is cemented, which is bent at a right angle, the leg nearest the vessel being carried downwards, and

* Experiments on Air, arranged and methodized, Vol. I. p. 51.
furnished

furnished with a cock *b*. B is a glass tube, of about $\frac{1}{4}$ inch bore, bent at a right angle, and graduated, from a given point, into hundredth parts of a cubical inch. It is attached to the copper pipe, by a tube of Indian rubber D, over which is a covering of leather, forming a joint, which admits of the vessel A being briskly agitated. When the apparatus is used, it is first filled with quicksilver; a transfer bottle of elastic gum, furnished with a cock, and containing water of a known temperature, is screwed on; and a communication is opened, through the cocks, between the bottle and the glass vessel. The lower cock *b* is then opened, through which the mercury runs out, while its place is supplied by a quantity of water from above, measurable by the scale on A. This transfer is removed, and another containing gas being substituted, a measured quantity of gas is admitted in a similar manner. Strong agitation is now applied, by means of the joint D; and mercury is poured into the tube B, to supply the descent occasioned by the absorption in A; its level being exactly preserved in both legs of the syphon, both at the commencement and close of the experiment. The quantity of mercury required for this purpose, indicates precisely the amount of the gas absorbed.

The only advantage of this apparatus over a cylindrical jar, inverted in the usual way over mercury, is, that by means of the tube B, very minute degrees of absorption may be measured, which would scarcely be perceived in a wide vessel.

For the more absorbable gases, I found this instrument to answer perfectly well, but, for ascertaining the solubility of those which are taken up by water in only small proportion, I preferred one of different construction. It consisted simply of a glass vessel, of the capacity of $57\frac{1}{2}$ cubical inches, and shaped as in Fig. 2. At *a* was cemented a cock, provided with a screw; and the lower cock *b* was of glass, accurately ground in. The vessel was then filled with water which had been long boiled; a lifting valve was screwed on *a*, the cock being open, and the vessel was placed under the receiver of an air pump, where it was kept for some time, the pump being occasionally worked, as long as any air bubbles could be seen to arise. The gas under examination was next admitted from an elastic bottle, the cock *b* being opened, and a measured quantity of water let out. The gas and water were then violently agitated

Another apparatus for gases sparingly absorbed by water.

agitated together; and the cock *b* opened under mercury, which ascended into the vessel. The agitation was still continued, observing to preserve the same level of mercury without as within the vessel; and, when it rose no higher, the ascent was noted by means of the graduated scale. The quantity of mercury that had entered the vessel, indicated the amount of absorption that had ensued.

Another apparatus, being simply a glass globe with a long neck.

It might, however, be objected, that the water would acquire air again, while poured into the vessel; and I therefore sometimes used large glass globes, having long necks, accurately graduated. These globes, being of very thin glass, were filled with boiling water, and inverted instantly in a trough of quicksilver. When the water became cold, the mercury was, of course, found to have risen partly into the vessel. This portion was displaced by a measured quantity of gas; and the absorption was denoted by the ascent of the mercury in the graduated neck.

General proceedings and cautions.

The water employed in these experiments was boiled, during several hours, in a tin vessel having an aperture barely sufficient to allow the egress of the steam, and poured, while boiling hot, into glass vessels, which were corked, and tightly tied over with bladder. An equable temperature was produced in the water, mercury, and gas, except when above 85° , by regulating that of the room in which the experiments were made; and the glass vessel, during agitation, was carefully guarded from the warmth of the hand. The agitation was continued, till it appeared, by the scale, to produce no further effect; and, in the absorption of difficultly condensible gases, was repeated at intervals, during a space of from twelve to twenty-four hours. Alterations of the barometer were always observed; and the residuary gas measured, or estimated, at a pressure of $29\frac{1}{2}$ inches.

1. Absorption of Carbonic Acid Gas by Water.

Carbonic acid gas less absorbable by water at higher temperatures.

That the temperature of water influences the proportion of carbonic acid which it is capable of absorbing, is already known as a general fact; * but the exact amount of this influence has not, I believe, been hitherto ascertained. In the course of a

* See Mr. Cavendish's experiments in the Phil. Transf. Vol. LVI. p. 163; and Fourcroy's *Système*, 4to. Tom. I. p. 215.

series of experiments to determine it with precision, I was surpris'd by obtaining results which differed considerably from each other, at the same temperature of the gas and water; when both were, in different experiments, of like purity; and when the barometer had the same elevation. Of the cause of these variations I was not aware, till my friend Mr. Dalton suggested, that they probably depended on the variable amount of the residues; and, on repeating the experiments, with different proportions between the gas and the water, this suggestion was fully confirmed. Thus, when two measures of carbonic acid gas were agitated with one measure of water, the absorption was considerably greater than when, to the same quantity of water, a less proportion of gas was used. The cause of this diminished absorption, seems to be connected with the proportion of common air contained in the unabsorbed residuum; for, besides the unavoidable contamination of the gas employed, with a minute portion of the air of the vessel used for its extrication, a small quantity will always be liberated from the water, whatever pains have been taken to deprive it of air, by previous long boiling, exposure under the air pump, or both in succession. That this is the true explanation, appears also, from the result of adding to the gas a proportion of common air. Thus, when, at the temperature of 55°, 20 measures of carbonic acid are agitated with 10 of water, at least 10 measures of gas are taken up; but, from a mixture of 20 measures of carbonic acid with 10 of common air, 10 parts of water take only 6 of carbonic acid, or 4 less than in the former instance.

The results at like temperatures and pressures are not constant, unless like quantities of gas be presented;

because the smaller residues will contain a greater proportion of common air which retains gas along with it.

An analogous fact was observed by Dr. Brownrigg*, who remarked that gas does not escape from the water which it impregnates, unless the water be in contact with air: for, when the Pouhon water was excluded from air, but, at the same time, liberty was given for its gas to arise into an empty bladder, the gas did not spontaneously separate from the water; but, on the contrary, remained united with it, when exposed to the greatest heat of our climate. When the impregnated water, he observes, is thus excluded from air, the gas will escape very slowly, at any temperature less than 110° of Fahrenheit, al-

Reverse fact. That gas escapes much more readily from water when it can issue into common air.

* See Dr. Brownrigg's Paper on the Pouhon Water. Phil. Trans. Vol. LXIV.

though

though such heat be sufficient for the distillation of water; nor can it be wholly expelled by a heat of 160° or 170° , continued two hours. But it is well known, that water saturated with carbonic acid gives up its gas rapidly, when freely exposed to the atmosphere.

In fixing the proportion of carbonic acid absorbed, it is therefore necessary to note the quantity of residuum, as is done in the following table.

Tabulated results of carbonic acid absorbed by water.	Experiment.	Temperature.	Measures of water.	Measures of gas.	Quantity absorbed.	Residue.	Absorbed by 100 inches of water.
	1	55	13	32	14	18	108
	2	85	13	32	11	21	84
	3	55	13	24	14	10	108
	4	55	10	15	10	5	100
	5	55	20	20	18	2	90
	6	55	19	19	16	3	84
	7	85	19	19	13	6	70
	8	110	10	20	6	14	60
	9	110	20	20	9	11	45

Since the above table was drawn up, I have been gratified by remarking that, in the experiments of Mr. Cavendish, similar variations in the quantities absorbed, were produced by the variable amount of the residua; as will appear from the following deductions from his experiments.

At the temperature of 55° .

Mr. Cavendish's results.	1. When the gas absorbed was to the residue as 100 to 164, 100 cubical inches of water took up	- - -	116
	2. When the absorbed gas was to the residue as 100 to 16, 100 inches of water took up	- - -	107
	3. The absorbed gas being to the residue as 100 to 10, 100 parts of water absorbed	- - -	$102\frac{1}{2}$
	4. The absorption being to the residue as 100 to $1\frac{1}{2}$, 100 parts of water took up	- - -	$95\frac{1}{2}$

The quality of the residuum, I only ascertained in experiments 5 and 6 of the preceding table. In experiment 5, the residuary two measures contained $7\frac{1}{2}$ per cent. of common air, or 0.15 of a measure. But, of those, .13 existed previously in the 20 measures of carbonic acid gas; and the 20 measures of

of water had, therefore, only given up .02 of a measure, or about $\frac{1}{1400}$ of its bulk. I apprehend, however, that the whole of the common air was not, even thus, extricated from the water. In experiment 6, the 3 residuary measures contained $\frac{1}{2}$ of common air.

To judge of the influence of temperature, it is essential that the experiments compared should be on similar proportions of gas and water. Thus, from a comparison of experiment 1 and 2, it appears, that about $\frac{1}{14}$ of the whole bulk absorbable at 55° , is the diminution of the quantity of absorption produced by each elevation of 10° of temperature; and the same inference follows from various other experiments, the results of which I have thought it needless to state *.

2. Sulphuretted Hydrogen Gas.

One hundred parts of water, at 55° of temperature, absorb 86 parts of this gas, obtained from sulphuret of iron and dilute sulphuric acid, a residue being left, equal in bulk to the gas absorbed. At 85° , under similar circumstances, the same quantity absorbs 78.

3. Nitrous Oxide.

At 45° , 100 cubic inches of water take up 50 of nitrous oxide; and, at 70° , the same quantity takes up only 44. According to Mr. Davy, in whose experiments, from his intimate knowledge of this gas, and skill in its preparation, I place more confidence than in my own, 100 inches of water at 45° , take up 54 of nitrous oxide, the residuum being about one half the volume of the gas absorbed.

4. Less absorbable Gases.

The experiments with those gases which are absorbed only in sparing proportion by water, I could not conveniently make at more than one temperature; nor, indeed, did the object

* During the absorption of carbonic acid by water, the gas and water having previously the same temperature, there is an extrication of caloric, sufficient to raise the temperature of the water between $\frac{1}{2}$ and $\frac{1}{4}$ of a degree of Fahrenheit. The same effect is produced by the condensation of sulphuretted hydrogen, and nitrous oxide gases, though less apparently. To perceive this phenomenon, considerable quantities of gas and water should be used.

appear

appear to me worthy of the time and attention which such a repetition of them would have required. Of the accuracy of the following, however, I satisfied myself, by repeating each two or three times; and with gases of the greatest attainable purity.*

100 cubic inches of water, at 60°, absorb,

These numbers are corrected in the appendix.	Of nitrous gas	-	-	-	-	5	inches.
	Oxygenous gas	-	-	-	-	2.63	
	Phosphuretted hydrogen ditto	-	-	-	-	2.14	
	Gaseous oxide of carbon	-	-	-	-	2.01	
	Carburetted hydrogen gas	-	-	-	-	1.40	
	Azotic gas	-	-	-	-	1.20	
	Hydrogen gas	-	-	-	-	1.08	

The solubility of atmospherical air cannot easily be ascertained; for, as I shall hereafter shew, in a memoir on the expulsion of gases from water by each other, air is decomposed by agitation with boiled water, its oxygenous portion being absorbed in preference.

Former statements of air separable from water inaccurate from the portion still left in the fluid.

From the statements given by various philosophers, (the Abbé Nollet, Drs. Hales, Priestley, and Pearson,) of the quantity of air separable from water of different kinds, by heat or a diminished pressure, I expected that a much larger proportion of the gases constituting the atmosphere would have been absorbed by water, than the above numbers assign. It is to be recollected, however, that no method hitherto discovered detaches from water all its air; and the unknown quantity remaining in it, after these modes of separation have been employed, is to be added to that with which a given volume of water can be artificially impregnated. Dr. Pearson, in his enquiries into the nature of the gas obtained by passing electric discharges through water, was at great pains to purify the subject of his experiments from air, by boiling and a powerful air pump; but he always found, that after the full effect of both these methods, electricity liberated a further, and not an inconsiderable, portion of air*.

Expulsion of gas from spring water.

Common spring water may, I think, be fairly taken as a specimen of water full charged with atmospherical air; and, with the view of determining the quantity and kind of gases extricated from it, I made the following experiment. A glass

globe, of the capacity of $117\frac{1}{2}$ cubical inches, was filled with water fresh from the well. To its mouth was adapted a curved and stoppered tube, which held $\frac{3}{4}$ of an inch; and this was also filled with water. The globe was then placed in a vessel of brine, which was kept boiling between six and seven hours; and the gases were received over mercury. Their quantity and quality were as follows.

No.	Cub. inches.	Consisted of		Proportion of Oxygen gas in the residuary air.	Its nature and quantity.
		Carbonic acid.	Air.		
1	1.25	0.50	0.75	0.20	
2	1.25	0.85	0.40	0.16	
3	1.63	1.23	0.40	0.16	
4	0.50	0.49	0.01	lost by acci- dent,	
	<u>4.63</u>	<u>3.07</u>	<u>1.56</u>		
Air remaining in the bent tube. } 0.75					

5.38, total gas from $117\frac{1}{2}$ inches of water.

But, $4\frac{1}{2}$ inches of water were expelled, owing to the expansion by heat. Therefore $117\frac{1}{2} - 4\frac{1}{2} = 113$ inches of water, gave 5.38 inches of gas; and 100 inches, consequently, gave 4.76, of which 3.38 were carbonic acid, and 1.38 atmospherical air. Hence, the water afforded about $\frac{1}{75}$ its bulk of atmospherical air, and $\frac{1}{25}$ of a mixture of gases. In this estimate, the gas remaining in the tube is reckoned as carbonic acid, which may be allowed, since the portion last obtained held only $\frac{1}{35}$ its bulk of common air.

SECTION II.

ON THE INFLUENCE OF PRESSURE IN PROMOTING THE ABSORPTION OF GASES; AND THE DESCRIPTION OF AN APPARATUS FOR EXHIBITING THIS PHENOMENON.

For the purpose of determining the ratio between the addition of pressure and the increased absorption of gases by water, I employed the apparatus, with some addition, which has been already described. The tube B was lengthened at pleasure, with the view of obtaining, by a column of mercury, any additional pressure that might be required. The vessel A, Fig. 1, was

Method of operating when the gases were absorbed under pressure.

was

was then filled completely with mercury, which rose to its corresponding level in the tube B. A given quantity of water, of a known temperature, and afterwards a measured volume of gas, were transferred into the vessel, in the mode already described; and, as the mercury, by opening the cock *b*, was brought to the same level in both legs of the syphon, the gas, it is evident, must have been under the ordinary weight of the atmosphere. A quantity of mercury was next poured into the leg B, sufficient to form a column 28 inches higher than the level of the mercury in A, after this addition; and the bulk of the gas was again noted. This was found to be, pretty exactly, $\frac{1}{2}$, $\frac{1}{3}$, &c. of the space occupied before, when one, two, or more additional atmospheres were applied. Brisk agitation was now used, as long as any absorption took place; and, into the tube B an assistant poured mercury, so as to preserve in it the excess of 28 inches above the level of the mercury in A. The degree of absorption was known by the scale on A, or, more accurately, by the quantity of mercury required to support the elevation of 28 inches in B.

By lengthening the column in B to 56 inches, the pressure of two additional atmospheres was obtained; and this was the utmost extent to which the addition of weight could be carried, without forcing the joint at D.

When the cock *b* was opened, and the column in each leg thus suddenly fell to the same level, the water, which had been previously charged with gas, under a pressure of three atmospheres, effervesced violently; but some time elapsed before the additional gas, forced in by compression, was wholly evolved. These appearances are very striking and amusing; and are well calculated for exhibition in a chemical lecture. The apparatus, however, I have no doubt, may be greatly improved; but, at the distance of nearly 200 miles from the metropolis, I was under the necessity of using such an one as could be constructed by my own hands.

Improved apparatus.

A considerable improvement in the construction of the apparatus, which would obviate the expediency of the flexible tube D, would be the following. To the lower neck of the vessel A, Fig. 1, let a cap and cock, with a female screw, be cemented; and let the upper end of the pipe C be terminated by a cock with a male screw. Introduce the gas and water,

in

in the manner already described; apply the increased pressure; and, having shut the two additional cocks, unscrew them from each other. The vessel A will thus be detached, and agitation may be easily applied; after which, again screw it into its former place, and, on opening the two cocks, the mercury will rise in the vessel A. Supply the descent in B by fresh mercury, and proceed as before, repeating alternately the pressure and agitation, as long as any further absorption takes place.

A further amendment of the apparatus, would consist in the substitution of cocks of some other metal than brass, which, however perfect at first, are always injured by the repeated action of the mercury. If cocks of glass could be ground sufficiently tight, metal caps with screws might be cemented to them.

For observing the increased absorption of less condensible gases, I found it necessary to substitute a vessel of larger size than A, and of the capacity of at least 50 cubical inches. It is represented by the dotted lines in Fig. 1, and was furnished with a cock and screw at *c*. As it would have been troublesome to have filled so large a vessel entirely with quicksilver, it was filled with boiled water, with the exception of a quantity of quicksilver rather exceeding the bulk of the gas employed. The gas was admitted, as usual, from a transfer bottle, the mercury which it replaced escaping through the cock *b*. The increased pressure was next applied; and the experiment conducted as before, except that the agitation was much longer continued.

The results of a series of at least fifty experiments, on carbonic acid, sulphuretted hydrogen gas, nitrous oxide, oxygenous and azotic gases, with the above apparatus, establish the following general law: *that, under equal circumstances of temperature, water takes up, in all cases, the same volume of condensed gas as of gas under ordinary pressure.* But, as the spaces occupied by every gas are inversely as the compressing force, it follows, *that water takes up, of gas condensed by one, two, or more additional atmospheres, a quantity which, ordinarily compressed, would be equal to twice, thrice, &c. the volume absorbed under the common pressure of the atmosphere.* By frequent repetition of the experiments, I obtained results differing a little from

Operation under pressure with the less absorbable gases.

General law. Water at like temperatures always absorbs the same bulk of gas, however dense or rare,

and consequently a mass or quantity proportional to the pressure.

from the general principle above stated; but, for all practical purposes, I apprehend, the law has been announced with sufficient accuracy*.

Increased temperature of gas by condensation.

In place of the cock *a*, I cemented, in one experiment, a very sensible thermometer. The vessel was next filled with mercury through the cock *b*; and the tube B being also filled, the cock *b* was shut, and a bottle of carbonic acid gas screwed on. The cock *b* being then opened, the mercury descended, and a measured quantity of carbonic acid arose into the vessel A. In the same way, a measured quantity of water was introduced. When the density of the air was suddenly doubled by a column of quicksilver, the mercury in the thermometer, whose bulb was still surrounded by the condensed gas, rose about $1\frac{1}{2}$ degree. On agitating the vessel, till the water encompassed the bulb of the thermometer, an elevation of barely $\frac{1}{2}$ a degree ensued in the temperature of the water. This ascent would probably have been greater, if the evolved heat had not been carried off by the mercury on which the water floated.

Manchester, Dec. 8th, 1802.

APPENDIX.—(Since read.)

Corrections of the numbers in the foregoing paper.

SINCE my paper was finished I have found that the numbers assigned in it, as indicating the quantities taken up by water of some of the more absorbable, and of all of the less absorbable gases, are rather below the truth. The accuracy of these numbers I was led to doubt by a suspicion that due attention had not always been paid, in my former experiments, to the quality of the unabsorbed residuum. For, the theory which Mr. Dalton has suggested to me on this subject, and which appears to be confirmed by my experiments, is that the absorption of gases by water is purely a mechanical effect, and that its amount is exactly proportional to the density of the gas, considered abstractedly from any other gas with which it may accidentally be mixed. Conformably to this theory, if the residuary gas contain $\frac{1}{2}$, $\frac{1}{10}$, or any other proportion of foreign gas, the quantity absorbed by water will be $\frac{1}{2}$, $\frac{1}{10}$, &c. short of the maximum. The proof of these propositions would lead me into a minuteness of detail not suited to the present occasion, I therefore hasten to communicate the results of my latest experiments.

Absorption of gases is purely mechanical.

The

The report which I have already given of the quantity of carbonic acid gas absorbed under the ordinary pressure of the atmosphere I find no reason to correct, but of sulphurated hydrogen gas I have effected a larger absorption than the one before stated, and have repeatedly observed its amount to be 106 or 108 by 100 measures of water, at 60° of Fahrenheit, which temperature is to be understood in all the following experiments.

Of several experiments on the absorption of nitrous oxide, I take the following as a fair example of the whole. I agitated at three several times, 1175 measures of nitrous oxide, with 1320 measures of water; 1025 parts of gas disappeared, and the unabsorbed remainder 150 contained 15 of foreign admixtures. It follows that 100 parts of water had taken up 77.6 of nitrous oxide; and after adding to these the diminution of absorption occasioned by the impurity of the residuum, it may be inferred that 100 parts of water would absorb 86 of absolutely pure nitrous oxide.

With respect to the remaining gases I have been prevented by urgent professional engagements, from examining the quantity of each absorbable under similar circumstances, except in the instances of oxygenous, azotic and hydrogenous gases. The results of these experiments are comprised in the following table. The first column shows the quantity of gas which 100 parts of water, at 60°, have actually absorbed; the second the quantity which ought to be taken up, provided the residue were in a state of absolute purity. In the example of nitrous gas alone, the estimated is less than the actual absorption; because a small portion of this gas loses its aerial form, by union with the oxygenous gas from which water cannot be entirely freed.

A table shewing the quantity of each gas absorbed by 100 measures of water at 60°. Table of absorptions of gases.

	Actual Absorption.	Inferred Absorption.
Nitrous gas - - -	5.	5.
Oxygenous gas - - -	3.55	3.7
Phosphuretted hydrogen gas	2.14	-
Gaseous oxide of carbon -	2.01	-
Carburetted hydrogen gas	1.40	-
Azotic gas - - -	1.47	1.53
Hydrogenous gas - - -	1.53	1.61

V.

Of the State of Vapour subsisting in the Atmosphere. By RICHARD KIRWAN, Esq. LL.D. F. R. S. and P. R. I. A*.

(Concluded from Page 215.)

MR. SAUSSURE, *Hygrometer*, p. 284, has given us the specific gravity, not indeed of pure vapour, but of vapour dissolved in air, with more precision, for he tells us, 1. That a cubic foot of perfectly dry air has its volume augmented by $\frac{1}{34}$ when saturated with ten grains of moisture at about 65° Fahr. of heat, and barometer 28,77 inches (English).

Specific gravity of dry air and of moist air.

2. That a cubic foot of *pure* or perfectly dry air of that density and at that temperature weighs 751 grains (French), and after dissolving 10 grains of moisture, by which it is dilated $\frac{1}{34}$, this new volume weighs $751 + 10 = 761$ grains; but a cubic foot of *pure* air augmented by an accession of $\frac{1}{34}$ of its bulk of pure air would weigh $751 + \frac{1}{34} 751 = 765$ grains, that is 14 grains more. Hence he infers that in this case the specific gravity of the dissolved moisture is to that of dry air as 10 to 14, for $\frac{1}{34}$ of a cubic foot in the one case weighs 10 grains, and in the other 14 grains nearly.

Suspected errors in the original experiment.

But I strongly suspect that the original experiment, on which this calculation is founded, is erroneous, chiefly by reason of the strong adherence of moisture to cold glass, as will hereafter be seen in treating of dew. From Schmidt's experiments, it may be inferred, that the specific gravity of vapour dissolved in air at this temperature is much lower with respect to that of pure air than Saussure has stated, for he tells us that about 1066 measures of dry air in temperature 65° would, if saturated with moisture at that temperature, occupy the space of about 1100 measures, and consequently receive an augmentation amounting to about $\frac{1}{32}$ of their bulk; now, transferring this ratio to the cubic foot in Saussure's experiment, it appears that $\frac{1}{32}$ of a cubic foot thus added to the cubic foot of dry air weighs 10 grains; but a cubic foot of dry air, augmented by an accession of $\frac{1}{32}$ of similar air, would weigh $751 + 23,46$ grains, which approaches nearly to Mr. Watt's ratio, therefore the specific gravity of vapour dissolved in air at this temperature is to that of perfectly dry air as 10 to 23,5 nearly. It should however

be

be recollected that Mr. Sauffure found that a cubic foot of dry air in reality took up 11,069 grains of moisture when saturated at this temperature, and that it was only by way of concession to those against whom he argued, that he stated the weight taken up at 10 grains; then we should have of 11,069 to 21,195, or in round numbers as 11 to 21 or 10 to 19. And it should farther be remarked, that the temperature is given very loosely, for it is stated to be from 14 to 15 or 16 degrees of Reaumur. See *Hygrometer*, p. 104 and 284.

Sauffure has given us a table, by the help of which the absolute quantity of vapour at any barometrical height, in a cubic foot of air being known, the proportion and absolute quantity in a cubic foot, at another barometrical height, 3,6 inches lower, may be known from the mercurial height 28,77 to that of three inches and one-half, nearly.

Table of the quantities of vapour in air of different densities but like temperature.

This table I here give, adapting it to our measures.

Barometer	Ratio.
28,77	1,0000
25,17	0,9528
21,57	0,8899
17,97	0,8264
14,37	0,7629
10,77	0,6887
7,17	0,6230
3,57	0,4311

Thus supposing the absolute quantity of dissolved vapour at any temperature, and barometer 28,77 to be 10 grains per cubic foot, then the quantity of vapour at a height at which a barometer would stand at 25,17 inches would be $10 \times 0,9528 = 9,528$ and at the height at which a barometer would stand at seven inches, the quantity in a cubic foot would be only $10 \times ,6230 = 6,23$. But still

it is supposed that at those great heights, at which barometers would stand so low, that the air is of the same temperature as the original experiment is made at, namely in this case, as it is found at barometer 28,77 inches; but since in reality air at great heights is generally much colder than below, to ascertain the real proportion of vapour at those heights it will be necessary to find the quantity of vapour which a cubic foot of air is capable of holding at that temperature barometer 28,77, and the ratio which the quantity or weight of vapour actually found bears to the complement at that temperature. Then, 2. to find the complement of a cubic foot of air at the temperature which prevails at the given barometrical height, and diminish it in the same ratio in which it was found diminished below, and finally diminish it still farther in the ratio which that barometrical height demands. An example will fully explain this rule.

— correction for
different tem-
peratures.

Thus Sauffure found, barometer 28,77 and thermometer 82° of Fahr. a cubic foot of air contained about 10 grains of moisture at Geneva. Now the complement of 82° is nearly 15 grains, and the ratio of 10 to 15 is $\frac{2}{3}$. Then at Mount Blanc, on the same hour, the barometer stood at 16° and the thermometer at 26°,8; the complement of a cubic foot of air at this temperature is 5,3 grains, which diminished in the ratio of 2 to 3 becomes 3,5, and this, farther diminished by the ratio which the barometrical height of 16 inches demands, namely $,78 = 3,5 \times ,78 = 2,7$ grains, by observation it was found to be 1,7; the difference is only one grain. *Voy. aux Alpes*, § 2007. How the temperature which prevails at those great heights may be found, will be shewn in the sequel.

System of Lam-
bert respecting
the quantities of
vapour in air of
different den-
sities.

The celebrated Lambert, of Berlin, Mem. Berlin, 1772, has also given an estimate of the proportion of vapour which prevails in the atmosphere at different barometrical heights, deduced from calculations founded on many fictions, such as that of an homogeneous atmosphere, of pure air distinct from common air, and an erroneous system of the ascent of heat; yet as it is much easier in its application and in the instance just quoted, approaches very near the truth, I have calculated the results of his system, which is nothing more than that the quantity of vapour at different barometrical heights above the earth is in the ratios of the squares of those heights. By an homogeneous atmosphere it is probable he meant such a state of the atmosphere as prevails in serene unclouded weather, and it is certainly only in such an atmosphere that any calculation can be instituted.

Table of the Ratios of the Quantities of Vapour at different barometrical Heights, the Quantity of the Surface of the Earth being given.

Barometer.	Ratio of Vapour.	Barometer.	Ratio of Vapour.	Barometer.	Ratio of Vapour
30,	900	24,	576	12	144
29,5	870	23,	529	11	121
29,	841	22	484	10	100
28,5	812	21	441	9	81
28,	784	20	400	8	64
27,5	756	19	361	7	49
27,	729	18	324	6	36
26,5	702	17	289	5	25
26,	676	16	256	4	16
25,5	650	15	225	3	9
25,	625	14	196	2	4
24,5	600	13	169	1	1

Thus

Thus in the example last quoted, the quantity of vapour in a cubic foot at Geneva being 10 grains, barometer 28,77, the quantity on Mount Blanc, barometer 16, should be ,309, for as $827,7 (= 28,77^2)$ is to $256 (= 16^2)$ so is 10 to 0,309, which differs from the truth by only 0,391 of a grain.

As vapours unite to air, partly through the agency of heat, and partly through that of affinity and of electricity, so they separate from it, sometimes from a diminution of that degree of heat which they possessed in their nascent state, sometimes from a diminution of affinity, and sometimes from an alteration in their electrical state.

Separation of vapour from air by change of heat and of electricity.

In their first degree of coalescence when separated from air, they form aggregates of exceeding minute particles, separated from air by the diminution of affinity, and also from each other by electrical atmospheres; these aggregates are of equal, and often lower, specific gravity, than the air in which they are formed, and yet are visible by reason of their opacity; when near the earth they are called *foggs, mists or haze*, (which differ only in density) and when at greater heights, *clouds*.

First coalescence of vapour, mists or clouds.

Vapours issuing from water or moisture warmer than the air to which they unite, are soon cooled by it and thence in great measure dismissed; hence the morning mists observed in summer and the winter mists of the colder regions; evening mists on the contrary proceed from the supersaturation of air with vapours previously dissolved, arising from the supervening decreased temperature. The inferior strata of the atmosphere are scarce ever supersaturated by vapours arising from water or moisture warmer than the air into which they ascend; for before the point of saturation can be attained, their affinity to the portion of air to which they are united is weakened, and thence exceeded by the unincumbered affinity of the superior strata, and this happens successively on to the higher regions; but with diminished activity, by reason of the diminished density of the higher strata, until their ulterior progress is checked by saturation; but as they are still continually recruited from below, their quantity is at last so far increased that they coalesce into clouds. Here the process recommences, for from the surface of these clouds a fresh evaporation often takes place, which, after some progress, is again checked in its turn, and clouds are formed at a superior height; these again give room to a further evaporation, and a new stage of clouds is formed, until the

Statement of the manner in which clouds are formed.

the

the process is at last arrested by the intense cold of the superior regions. But the mere cold of congelation is not sufficient to arrest it, for Bouguer informs us that clouds are formed 2500 feet above the lower line of congelation, and that ice itself evaporates, though cooled, several degrees below the freezing point, is well known. The distance of the particles, both of air and vapour, from each other, when so far rarified as they must be in the superior regions of the atmosphere prevents their coalescence in any but the extreme degrees of cold.

Clouds in strata
above each
other.

Hence we see that in the warmer latitudes and seasons, various strata of clouds may be formed one above the other; Muschenbrook attests, that even in Holland, in August 1748, he distinctly discerned three. These distinct strata, variously electrified and otherwise circumstanced, give occasion to various phenomena, the detail of which would here be misplaced.

Clouds near
mountain tops.

The clouds which commonly crown the summits even of low mountains, and often announce rain, are caused by the near approach to saturation, at those elevations, and its actual attainment through the evaporation from those summits. But the summits of the loftiest mountains ever crowned with snow, are generally shrouded in clouds from the cold they impart to the air in contact with them, and the loss of electricity conducted away from the vapours contained in that air, by the mountain.

Heights at which
clouds are
formed.

The heights at which the lowest clouds are formed are various in various latitudes and seasons; greater in the warmer and smaller in the colder. In latitude 54° in Cumberland, Mr. Crosthwaite observed none lower than 2700 feet, and none higher than 3150 in the course of several years*. But this country being mountainous they are probably lower than in others under the same parallel. Lambert, in Berlin, latitude 52° 32', in the month of July 1773, found their height 7792 feet, thermometer 65°, and the barometer somewhat below its mean height †. Schuckburg also remarks, that clouds frequently rest below the summit of *Saleve*, whose height is 2831 feet. Phil Trans. 1777, p. 538, and Gentil at Pondicherry, latitude 12°, observed some at the height of 10240 feet. 2 Voy. p. 79.

* D'Alton's Meteorological Observations, p. 41.

† Mem. Berlin, 1773, p. 44.

The weight of clouds, Saussure estimates at one-third or one-fourth of that of the cubic foot of air in which they subsist. Hygrometer, p. 270. When the barometer rises, clouds are partly dissolved, as dense air is a better solvent than rarer air, and partly rise higher in consequence of the increased specific gravity of the inferior air; when the barometer falls the contrary takes place.

VI.

Experiments and Observations in support of that Theory of Ventriiloquism which is founded on the Reflection of Sound. In a Letter from Mr. JOHN GOUGH.

To Mr. NICHOLSON.

S I R,

YOU have given a short but comprehensive view of my theory of ventriiloquism, at page 202, of the fourth volume of your Journal; after which, you observe, that the manifest difficulty of managing the echos of a room renders my explanation of the phenomenon doubtful. In order to examine the force of this objection experimentally, I proceeded in the following manner: perhaps the instruments which were used on the occasion, may be deemed childish by many of your readers; but a genuine friend to physical inquiry looks for the event of an experiment, and does not regard the appearance of the apparatus.

Exp. 1. I took the shell of a cocoa-nut, one end of which had been cut off, so as to make it an aperture, something more than two inches in diameter: after lining this with woollen cloth, I put a watch into it, and surrounded the whole with a pillow in a manner, that left no part of the shell in contact with the atmosphere, except the open end. When things were thus prepared, the pillow was placed upon my knees, having the mouth of the nut turned from me: I then listened to the sound of the watch, sitting in an erect posture, and observed it to be weak, but distinct as to direction: a circular metal tray, one foot in diameter, was held up before my face, in the next place, and at the distance of two feet from it; upon which, the clicking of the watch grew much stronger,

Whether the phenomenon of ventriiloquism be referable to echo.

Exp. 1. The beats of a watch made to issue chiefly in one direction. This strongest sound was then reflected by a metallic surface and was referred to its last direction.

stronger, and proceeded from the tray. The certainty of the latter circumstance was ascertained by changing the place of the reflector, so as to make the sound fall upon my right and left ears alternately.

Exp. 2. A small metallic rattle was substituted instead of the watch; and other reflectors tried: with similar results.

Exp. 2. The result of the last experiment convinced me of the practicability of turning the echos of a room to the use of a ventriloquist, seeing I had employed those of my own lap to a similar purpose; nevertheless I desired to give an additional proof of the same thing, by the assistance of a stronger sound. With this view, I took a small metal rattle, belonging to a child's toy; it weighed 25 grains, and consisted of two thin convex shells of brass, something less than one inch broad: a pellet was lodged in the cavity formed by the junction of these hollow plates; the motion of which produced a sound, such as I wanted. This rattle being put into the same-cocoa nut prepared as in the last experiment, I gave a gentle motion to it by my finger, which was introduced at the aperture; and I soon perceived, that the sound might be transferred in this case also, from the shell to the tray, but not with equal certainty: for frequently a slight change, given to the inclination of the reflector, rendered the place of the sound evident, which was indistinct before; and the contrary. The reason of this uncertainty will be investigated hereafter; and I may add at present, that a thin quarto as well as a fan were substituted with success, in lieu of the tray; but bodies of three or four inches in diameter, did not produce the effect. The preceding experiments have been related with an attention to circumstances, which cannot fail of instructing any one to repeat them accurately; and a few trials will, without doubt, convince him who will take the trouble, that the echos of a room are subject to external influence, as well as those of a valley.

Observations on the facts. The reflected sound was made to predominate over, and obliterate the direct sound, &c.

We come in the next place to examine the peculiarities that gave rise to these counterfeit sounds; that is the unusual circumstances of my experiments are to be reviewed, and their effects ascertained. The shell was encompassed by a bad conductor of sound on all sides, the mouth excepted; it was from this aperture then, that the pulses entered the atmosphere with their wonted freedom. Now we know from experience, that confined sound does not diverge from the place of its egress, as it were from a centre: for should any one attend to

a noise coming from an open window, he always finds it strongest, in front of the passage; sounds therefore resemble the properties of a river entering a lake; the current of which affects its former direction, after it is freed from the restraints of a channel. My ear was placed behind the apparatus, in relation to the shell's mouth; consequently it did not lay in the track of the direct pulses; therefore the sound, heard by me in the absence of the reflector, shall be supposed at present to have consisted wholly of lateral pulses, reflected from the circumference of the aperture; but when a greater mass of sound was forced on the ear, from a different quarter, the weaker became imperceptible; which is a thing, that happens invariably in similar cases; this change in the direction of the prevailing pulses necessarily transferred the seat of the sound from the shell to the reflector. But if the reflected lateral pulses can be silenced in one case, by diverting those of the centre towards the same parts; a like rule might be expected to obtain in all cases: because the powers of reflected sounds may be supposed to have a fixed relation to their primitives; but the rule fails in a number of instances; consequently my artificial ventriloquist possesses another source of sound; which is the next subject of inquiry.

Exp. 3. When I let the small rattle fall from the height of an inch, upon the cloth which lined the shell the stroke was heard in the nut; but the noise, occasioned by the rebounding of the pellet, came from the reflector; in this instance then we have two sounds, proceeding from different places, though they evidently issued from the same spot; and the cause, that gave them opposite directions, may be investigated in the following manner. The stroke of the rattle gave vibrations to the shell, which were stronger than those imparted by the pellet; the former set of vibrations could not be suppressed by the pillow on account of their strength, they therefore produced a sound, conjointly with the lateral pulses, which proved an overmatch for the echo of the reflector; while the feebler force of the second set was overpowered by the pulses, reflected from the tray. In reality the art of suppressing the direct sound appears to constitute the secret of ventriloquism; for the more completely this sound is smothered, the easier it is to place the reflector properly: this appears to be the reason, why the experiment succeeds with a watch more frequently than

The same effect appears universally producible.

Exp. 3. The rattle was let fall in its confining receptacle. The stroke on the receptacle gave a direct sound; the rattle in its rebound gave the reflex sound.

The secret of ventriloquism is to suppress the direct sound.

than with the rattle ; because the impulse of the former is less than that of the latter. In fact, if the force of the sounding body be augmented, while the power of the muffle remains unchanged, the reflector soon becomes useless ; as I found by using a small bell, the sound of which was disguised with the greatest difficulty in my rude apparatus ; and every precaution, that could be taken made it a hard task to place the reflector properly. The best position of this body is apparently that which brings the ear in the line, constituting the path of the strongest pulses, after they are reflected ; which line and their course before reflection must make equal angles at the reflecting surface, with a perpendicular to it ; which must also lay in the same plane with them. I have now gone through the demonstration of a proposition, which forms the foundation of my theory of ventriloquism ; should you think proper to repeat the preceding experiments, I only ask the indulgence of a little patience on your part : treat them in other respects with critical accuracy ; it was my intention to scrutinize your theory freely ; but too much of the present sheet is occupied already.

JOHN GOUGH.

Middleshaw, July 16, 1803.

REPLY. W. N.

I never meant to have expressed a doubt as to the interesting facts respecting sound, which Mr. Gough has stated and commented upon, in his paper at page 125 of our second volume ; nor can it be disputed that many curious effects are produced by the reflection and transmission of sound. I am very far from wishing to advance any theory of an art concerning which I have witnessed so little ; and I take it for granted that the ingenious author, when he speaks of his theory and mine, in the last paragraph of the preceding communication, does not mean to support the position that the effects of what is called ventriloquism are exclusively produced by reflection. The narrative of Fitz James's performance at page 202. vol. 4. of our Journal, will at least prove that the causes which I have considered rather as moral than physical, must be admitted in many cases, if not in all. Possibly there may be ventriloquists who operate more by the reflection of sound, than by the art

The ventriloquism of Fitz James was an imitation of sounds with appropriate action and not reflection.

of imitation assisted by theatrical action; as Fitz James evidently does, to a scattered audience, who cannot all, or even many of them, be supposed to be duly placed for receiving one and the same echo: and upon a careful recollection of the positions, I am much inclined to think none of them were so placed. The reflection of sound by a simple echo,—its more complicated echo from a curved surface, or whispering gallery, or speaking trumpet,—its yet unexplained transmission along a smooth wall or the surface of water,—its transmission through or along the ground, so well known to militarymen, and to which the string of Dr. Moyse *, the speaking apparatus of Ezekiel Walker †, and other similar effects, are referable. These and other modes of operation, unknown to or unthought of by me, may surely be used in producing delusions and other useful or remarkable effects, and may constitute part of the art of ventriloquism. I can only express my opinion, that the art of the professor I have spoken of was exquisite mimicry, and nothing more.

Many phenomena of sound may produce delusion; but how far ventriloquism?

VII.

Examination of the so called Salt of Bitumen, the Bit-Nobin of the Hindoos. By FREDERICK ACCUM, Practical Chemist and Teacher of Chemistry. From the Author.

IN the LV Number of Mr. Tilloch's Philosophical Magazine, p. 206, we find some observations concerning a saline compound lately imported from India, under the name of *Salt of Bitumen* or *Bit-Nobin*. From the experiments which the author of that paper (Mr. Henderson), made in order to become acquainted with the nature of that salt, it appeared to him to be chiefly composed of common salt and sulphurated hydrogen gas. But as the experiments of this gentleman do not appear to have been made with a view of affording an analysis of this saline substance, I was called upon by some medical practitioners of eminence to examine chemically the nature of this curious compound. I shall therefore confine myself merely to this examination, which was conducted in the following manner.

Account of Bit-Nobin, in the Philosophical Magazine,

supposed to be common salt and sulphurated hydrogen.

* Philos. Journal.

† Ibid.

Alcohol by digestion dissolved a portion;

Upon 480 grains of the salt reduced to a fine powder, 3ozs. of highly rectified alcohol were poured; the mixture was frequently agitated in a stopped-vial, during the course of one week, and the alcohol afterwards separated by filtration; the salt which remained on the filtre was treated in a similar manner, with fresh quantities of alcohol repeatedly. The different portions of spirit employed which were all colourless, had on being mingled together a disagreeable odour and bitterish taste: they were evaporated to dryness.

which afforded after evap. and deliques. a white salt and yellowish liquid.

On leaving the product obtained in this manner unobserved to the air for some weeks, it was found to have attracted moisture so as to be separated into two distinct saline substances, namely, a yellowish fluid, and a crystalline white salt.

The yellow fluid being separated by alcohol was diluted with water and the alcohol driven off Carbonate of soda threw down chalk; and,

To ascertain the nature of these saline bodies, alcohol was affused upon it in order to separate the liquid part from the crystallized salt. The fluid which was taken up by the alcohol, I mingled with a like quantity of water, heated the mixture to ebullition, and then dropt into it a solution of carbonate of soda. A cloudiness appeared and a precipitate was deposited, which after being washed and dried was found to be carbonate of lime. The residuary fluid from which this precipitate had been separated yielded nothing but muriate of soda.

hence the salt contains muriate of lime;

I learned from these experiments that the salt under examination contained muriate of lime.

the muriatic acid of which gave a precip. with solution of silver, from the weight of which the quantity of muriate of lime was determined:

To determine the quantity of it contained in the above portion of salt, I judged it necessary to ascertain the quantity of one of its constituent parts; for the composition of this salt being known, the real quantity of the compound might be thence found. The artificial muriate of soda produced before was therefore dissolved in water, and its solution decomposed by the addition of nitrate of silver: the muriate of silver produced when perfectly dried, weighed 40 grains, which indicate nine grains of muriatic acid, answering to 12 grains of muriate of lime. This therefore was the true quantity of muriate of lime contained in the above quantity of salt.

The solid salt left by the alcohol was dissolved in water. It gave odour of sulphurated hydrogen, became green, and afforded a black precipitate.

Upon the salt which had been repeatedly extracted by alcohol, distilled water was poured in order to dissolve it. On shaking the mixture, the whole acquired instantly a dark olive green colour, the peculiar odour of sulphurated hydrogen gas was evolved and a black pulverulent precipitate became diffused

fused through the solution. To collect this precipitate the solution was filtered as expeditiously as possible: the precipitate washed and dried weighed six grains.

This precipitate to which the cause of the green colour of this was iron, the solution was owing, after having been slowly dried, on exposure to air, had the appearance of a dark brown powder. It was soluble in muriatic, sulphuric and nitric acids. Tincture of galls and prussiate of potash proved it to be an oxide of iron. In order to be positive in this respect, a like quantity of the same precipitate obtained in a similar manner, was dissolved in sulphuric acid, and the solution again decomposed by carbonate of soda. The carbonate of iron was re-dissolved in dilute nitric acid, and repeatedly boiled to dryness in that fluid, till it became completely insoluble, and acquired all the properties of red oxide of iron.

The saline solution from which this oxide had been separated, had a strong odour of sulphurated hydrogen gas. Its taste greatly resembled Harrowgate water. Mercury agitated in contact with it lost its brilliancy, and became covered with a black pellicle. Arsenious acid immersed into it became yellow; leaf silver tarnished instantly; nitrous acid, holding much nitrous gas in solution, occasioned a white precipitate; nitrate of silver, acetite of lead, nitrate of bismuth, occasioned black precipitates; muriate of barytes rendered it milky, and a precipitate was deposited which was insoluble in muriatic acid; prussiate of potash and tincture of galls had no effect upon it. All these experiments (the latter three excepted) sufficiently evinced the presence of sulphurated hydrogen gas.

In order to ascertain the contents of the water it was evaporated to dryness: the salt obtained after having been examined in the usual manner, consisted entirely of muriate of soda, contaminated with a small quantity of sulphuric acid; its weight amounted to 444 grains. To ascertain the quantity of sulphuric acid, the salt was redissolved in water, and muriate of barytes was added till no further precipitate ensued. The sulphate of barytes produced when dry, weighed $51\frac{1}{2}$ grains, which are equivalent to 14 grains of sulphur. It is perhaps needless to remark that, the sulphuric acid thus found did not previously exist in the salt: its production was evidently owing to the decomposition of the produced sulphurated hydrogen gas, for no vestige of such acid could be detected by any other agent, or under other circumstances.

The fluid contained sulphurated hydrogen.

The saline portion was muriate of soda with a small portion of sulphuric acid.

The quantity of this last acid was found by the precipitate by muriate of barytes.

The Sulphuric acid was not originally such, but was produced from sulphur;

which existed
in a sulphuret of
iron in the salt.

Sulphur was therefore the compound which must have been present; and if we are permitted to reason from the phenomena which result from the action of water upon this compound, it becomes obvious that the sulphur is joined to the iron, detected before, in the form of a sulphuret of iron. Hence it becomes evident that while the salt continues in a dry state, the equilibrium of the affinities of its constituent parts remains unaltered;

The equilibrium
of affinity is
broken by the
addition of water.

but the moment we bring this compound in contact with water, a new arrangement of principles takes place; the water is decomposed, the oxide of iron is reduced to a state of *minimum* of oxidation, and appears under the form of a black powder diffused through the fluid, sulphuric acid is formed by the decomposition of the sulphurated hydrogen gas, and thus a set of bodies appears which did not previously exist in the state wherein they present themselves to our view.

Component parts
of bit-nobin
about 1 iron;
3 sulphur; 2½
muriate of lime;
92½ common
salt.

It is evident therefore that 480 grains of this salt yielded by this analysis.

Black oxide of iron	-	-	-	6 grains.
Sulphur	-	-	-	14
Muriate of lime	-	-	-	12
Muriate of soda	-	-	-	444
				<hr/>
				476
Loss	-	-	-	4
				<hr/>
				480

How to deter-
mine the sulphu-
rated hydrogen
if required.

I have taken no notice in this analysis of the quantity of sulphurated hydrogen gas evolved during the solution of a given quantity of salt in a limited quantity of water, this being foreign to my subject. I merely remark that, if this is wanted to be ascertained, it may easily be found by introducing it into a glass tube closed at the top, and furnished at the other extremity with a stopper, a weighed quantity of salt, and then adding to it a measured quantity of water, taking care to fill the tube with this fluid no higher than about $\frac{2}{3}$. Having done this let the tube be stopped, and agitate it till all the salt is dissolved, or at least till no more gas is extricated. Then let the tube be immersed into warm water, and let it stand undisturbed: remove the stopper carefully so as to get rid of the deposited oxide of iron, and introduce into the saline solution tube, nitrous gas, in small quantities, till no further diminution of the gas in the upper part of the tube takes place, or till no more
red

red vapours are produced by a new addition of a small quantity of the gas. The saline solution will now be turbid: leave it therefore undisturbed, and a precipitate will be deposited, which is sulphur, from the quantity of sulphur so produced we may learn the quantity of sulphurated hydrogen gas which was contained in the solution, for one grain of it when perfectly dry, are equivalent to 3.33 cubic inches of sulphurated hydrogen gas.

Old Compton Street, Soho,

July 20, 1803.

VIII.

Experiments on the Invisible Rays of the Solar Spectrum. By M. RITTER, of Jena. Communicated by M. VICKTRED, Doctor in the University of Copenhagen, to the Editors of the Bulletin des Sciences at Paris, No. 73.*

THESE enquiries form a sequel to the experiments by which Herschell discovered the existence of invisible calorific rays beyond the limits of the solar spectrum. The experiments of M. Ritter present a very simple method of proving the existence of these rays by the exhibition of a very curious property which he says is peculiar to them.

Ritter's experiments on invisible rays.

He placed muriate of silver without the solar spectrum and next to the violet rays. This oxide became blackened in a short time, it became still deeper in the violet rays, still more in the blue, and so on.

The rays in the confine of the violet reduce or blacken the muriate of silver.

On the contrary, placing muriate of silver a little blackened next to the red rays, and without the spectrum it quickly became white, that is to say, it was disoxygenated.

those near the red render it white or dis-oxide it.

According to M. Ritter, these experiments may be performed very well with phosphorus: by letting fall on it the invisible ray nearest to the red, it instantly emits white vapours; but the invisible rays nearest the violet are thrown on the same phosphorus, it instantly becomes extinguished.

Combustion and extinction of phosphorus by the same means.

From these facts, M. Ritter concludes, that there exists without the spectrum, and at its two extremities, invisible rays which possess the property of assisting oxygenation and disoxygenation.

* From the Bulletin des Sciences, Germinal, An. XI.

Negative galvanism gives the sensation of red; positive of blue.

The same philosopher has also observed a singular coincidence between these effects and those of galvanism. He finds that when the eye is placed in contact with the negative conductor of the pile, it sees every object red, but if placed against the positive conductor it sees them blue: whence there appears to result an analogy between the action of the negative electricity and that of the red light, and of the positive and the violet light.

IX.

An Account of some Experiments and Observations on the Constituent Parts of certain Astringent Vegetables; and on their Operation in Tanning. By HUMPHRY DAVY, Esq. Professor of Chemistry in the Royal Institution.*

Seguin's discovery of the tanning matter;

THE discovery made by M. Seguin, of a peculiar vegetable matter which is essential to the tanning of skin, and which is possessed of the property of precipitating gelatine from its solutions, has added considerably to our knowledge of the constituent parts of astringent vegetables.

extended by Proust;

Mr. Proust has investigated many of the properties of this substance; but, though his labours, and those of other chemists, have led to various interesting observations, yet they are far from having exhausted the subject. The affinities of tannin have been hitherto very little examined; and the manner in which its action upon animal matters is modified by combination with other substances, has been scarcely at all studied.

pursued by the author at the desire of the Royal Institution.

At the desire of the Managers of the Royal Institution, I began, in September 1801, a series of experiments on the substances employed in the process of tanning, and on the chemical agencies concerned in it. These experiments have occupied, ever since, a considerable portion of my leisure hours; and I now presume to lay before the Royal Society an account of their general results. My chief design was, to attempt to elucidate the practical part of the art; but in pursuing it, I was necessarily led to general chemical inquiries concerning the analysis of the different vegetable substances containing tannin, and their peculiar properties.

* From the Philosophical Transactions, 1803.

I. OBSERVATIONS ON THE ANALYSIS OF ASTRINGENT
VEGETABLE INFUSIONS.

The substances that have been supposed to exist most generally in astringent infusions are, tannin, gallic acid, and extractive matter.

Astringents contain tannin, gallic acid and extractive.

The presence of tannin in an infusion, is denoted by the precipitate it forms with the solution of glue, or of isinglass. And, when this principle is wholly separated, if the remaining liquor gives a dark colour with the oxygenated salts of iron, and an immediate precipitate with the solutions of alum and of muriate of tin, it is believed to contain gallic acid, and extractive matter.

Tannin shewn by glue, gallic acid, and extract, by salts of iron, and by alum and muriate of tin.

The experiments of MM. Fourcroy, Vauquelin, and Seguin, have shown that many astringent solutions undergo a change by exposure to the atmosphere; an insoluble matter being precipitated from them. A precipitation is likewise occasioned in them by the action of heat; and these circumstances render it extremely difficult to ascertain, with any degree of precision, the quantities of their constituent parts, as they exist in the primitive combination.

Astringent solutions very apt to change.

After trying several experiments on different methods of ascertaining the quantity of tannin in astringent infusions, I was induced to employ the common process of precipitation by gelatine, as being the most accurate.

This process, however, requires many precautions. The tanning principle in different vegetables, as will be seen hereafter, demands for its saturation different proportions of gelatine; and the quantity of the precipitate obtained by filtration,

The Precip. of tannin by gelatine (a) varies in different vegetables;

is not always exactly proportional to the quantities of tannin and gelatine in solutions, but is influenced by the degree of their concentration. Thus, I found that 10 grains of dry isinglass, dissolved in two ounces of distilled water, gave, with solution of galls in excess, a precipitate weighing, when dry, 17 grains; whilst the same quantity, dissolved in six ounces of water, produced, all other circumstances being similar, not quite 15 grains. With more diluted solutions, the loss was still greater; and analogous effects took place, when equal portions of the same solution of isinglass were acted on by equal portions of the same infusion of galls diluted in different de-

(b) and by the concentration of the solutions.

grees with water; the least quantity of precipitate being always produced by the least concentrated liquor. In all cases, when the weak solutions were used, it was observed, that the residual fluid, though passed two or three times through the filtre, still remained more or less turbid and opaque; so that it is most likely that the deficiency arose from the continued suspension of some of the minutely divided solid matter in the liquid mass.

The solutions of gelatine must be quite fresh and very strong;

The solutions of gelatine, for the purposes of analysis, should be employed only when quite fresh, and in as high a state of saturation as is compatible with their perfect fluidity. I have observed, that in cases when they approach towards the state of jelly, their power of acting upon tannin is materially altered, and they produce only a very slight precipitation. As the degree of fluidity of solutions of gelatine is influenced by their temperature, I have found it expedient, in all comparative experiments, to bring them and the astringent infusions on which they are designed to act, as nearly as possible to a common degree of heat. My standard temperature has been between 60 and 70° Fahrenheit; and the solutions of gelatine that I have used, were made by dissolving 120 grains of isinglass in 20 ounces of water.

at the temperature 60°.

If an excess of gelatine be added some of the tannin will be redissolved.

In ascertaining the proportions of tannin in astringent infusions, great care must be taken to prevent the presence of any excess of gelatine; for, when this excess exists, I have found that a small portion of the solid compound formed is redissolved, and the results of the experiment otherwise affected. It is not difficult to discover the precise point of saturation, if the solution of isinglass be added only in small quantities at a time, and if portions of the clear liquor be passed through a filtre at different periods of the process. The properties of these portions will indicate the quantities of the solution of gelatine required for the completion of the experiment.

The comparisons of the quantities of precipitant and precipitate made after drying at 150°.

That the composition of any precipitate containing tannin and gelatine may be known with a tolerable degree of precision, it is necessary that the isinglass employed in the solution, and the new compound formed, be brought as nearly as possible to the same degree of dryness. For this purpose, I have generally exposed them, for an equal time, upon the lower plate of a sand-bath, which was seldom heated to more than

150°. This method I have found much better than that of drying at the temperatures of the atmosphere, as the different state of the air, with regard to moisture, materially influence the results.

Mr. Hatchett has noticed, in his excellent Paper on Zoophytes, &c.* that isinglass is almost wholly composed of gelatine. I have found, that 100 grains of good and dry isinglass contain rather more than 98 grains of matter soluble in water. So that, when the quantity of isinglass, in any solution employed for acting upon astringent infusion, is compared with the quantity of the precipitate obtained, the difference between them will indicate the proportion of tannin, as it exists in the combination.

After the tannin has been separated from an astringent infusion, for the purpose of ascertaining its other component parts, I have been accustomed to evaporate the residual liquor very slowly, at a temperature below 200°.† In this process, if it contains extractive matter, that substance is in part rendered insoluble, so as to fall to the bottom of the vessel. When the fluid is reduced to a thick consistence, I pour alcohol upon it. If any gallic acid or soluble extractive matter be present, they will be dissolved, after a little agitation, in the alcohol; whilst the mucilage, if any exist, will remain unaltered, and may be separated from the insoluble extract, by lixiviation with water.

I have made many experiments, with the hope of discovering a method by which the respective quantities of gallic acid and extractive matter, when they exist in solution in the alcohol, may be ascertained; but without obtaining success in the results. It is impossible to render the whole of any quantity of extractive matter insoluble by exposure to heat and air, without at the same time decomposing a portion of the gallic

Isinglass is gelatine nearly pure.

After precipitating the tannin, the fluid is partly evaporated, and the gallic acid and soluble extract taken up by spirit.

It is difficult to ascertain the proportions of gallic acid and extract in the alcohol.

* Phil. Transf. for 1800, page 327.

† M. Deyeux has shewn, (*Annales de Chimie*. Tome XVII. page 36,) that in the process of evaporating solutions of galls, no gallic acid is carried over by the water, at a temperature below that of ebullition. Many astringent infusions, however, lose a portion of their aromatic principle, even in cases when they are not made to boil; but this substance, though evident to the smell, in the water that comes over, cannot be detected by chemical reagents.

acid. That acid cannot be sublimed, without being in part destroyed; and, at the temperature of its sublimation, extractive matter is wholly converted into new products.

Ether takes the former in preference, but has little effect as to perfect separation.

Ether dissolves gallic acid; but it has comparatively little action upon extractive matter. I have been able, in examining solutions of galls, to separate a portion of gallic acid by means of ether. But, when the extractive matter is in large quantities, this method does not succeed, as, in consequence of that affinity which is connected with mass*, the greatest part of the acid continues to adhere to the extract.

Alumine attracts extract, but does not separate it perfectly from gallic acid.

Alumine has a strong attraction for extractive matter; but comparatively a weak one for gallic acid †. When carbonate of alumine is boiled for some time with a solution containing extractive matter, the extractive matter is wholly taken up by the earth, with which it forms an insoluble compound; but, into this compound, some of the gallic acid appears likewise to enter; and the portion remaining dissolved in the solution is always combined with alumine.

This problem not yet perfectly solved.

I have not, in any instance, been able to separate gallic acid and extractive matter perfectly from each other; but I have generally endeavoured to form some judgment concerning their relative proportions, by means of the action of the salts of alumine, and the oxygenated salts of iron. Muriate of alumine precipitates much of the extractive matter from solutions, without acting materially upon gallic acid; and, after this precipitation, some idea may be formed concerning the quantity of the gallic acid, by the colour it gives with the oxygenated sulphate of iron. In this process, however, great care must be taken not to add the solution of the sulphate of iron in excess; for, in this case, the black precipitate formed with the gallic acid will be redissolved, and a clear olive-coloured fluid only will be obtained.

Approximation.

The saline parts of astringent infusions are also scarcely to be separated.

The saline matters in astringent infusions, adhere so strongly to the vegetable principles, that it is impossible to ascertain their nature with any degree of accuracy, by means of common re-

* See Berthollet, *Récherches sur les Lois de l'Affinité*. *Mém. de l'Institut National*. Tome III. p. 5.

† See Fiedler, *Journal de Chimie*, par J. B. Van-mons, Tome I. page 85.

agents. By incineration of the products obtained from the evaporation of astringent infusions, I have usually procured carbonate of lime and carbonate of potash.

In the different analyses, as will be seen from the results given in the following sections, I have attended chiefly to the proportions of the tanning principle, and of the principles precipitable by the salts of iron, as being most connected with practical applications.

The practical results are by precipitation with gelatine and with salts of iron.

With regard to the knowledge of the nature of the different substances, as they exist in the primitive astringent infusion, we can gain, by our artificial methods of examination, only very imperfect approximations. In acting upon them by reagents, we probably, in many cases alter their nature; and very few of them only can be obtained in an uncombined state. The comparison, however, of the products of different experiments with each other, is always connected with some useful conclusions; and the accumulation of facts with regard to the subject, must finally tend to elucidate this obscure but most interesting part of chemistry.

Though strict analysis is here very difficult, yet the experiments lead to useful conclusions.

II. EXPERIMENTS ON THE INFUSIONS OF GALLS.

I have been very much assisted in my inquiries concerning the properties of the infusions of galls, by the able Memoir of M. Deyeux, on galls.*

The strongest infusion of galls that I could obtain, at 56° Fahrenheit, by repeatedly pouring distilled water upon the best Aleppo galls broken into small pieces, and suffering it to remain in contact with them till the saturation was complete, was of the specific gravity 1.068. Four hundred grains of it produced, by evaporation at a temperature below 200°, fifty-three grains of solid matter; which, as well as I could estimate, by the methods of analysis that have been just described, consisted of about $\frac{2}{5}$ of tannin, or matter precipitable by gelatine, and $\frac{1}{5}$ of gallic acid, united to a minute portion of extractive matter.

Cold Infusion of 400 grains of galls gave 53 grains of solid matter, which was nine parts tannin and one gallic acid.

100 grains of the solid matter obtained from the infusion, left, after incineration, nearly $4\frac{1}{2}$ grains of ashes; which were chiefly calcareous matter, mixed with a small portion of fixed

The infusion reddened litmus. Its solid matter left by incineration at the 1-20th chiefly calcareous with some alkali.

* *Annales de Chimie*, Tome XVII. page 1.

alkali. The infusion strongly reddened paper tinged with litmus. It was semitransparent, and of a yellowish-brown colour. Its taste was highly astringent.

Sulph. acid precipitates the solution. When sulphuric acid was poured into the infusion, a dense whitish precipitate was produced; and this effect was constant, whatever quantity of the acid was used. The residual liquor, when passed through the filtre, was found of a shade of colour deeper than before. It precipitated gelatine, and gave a dark colour with the oxygenated sulphate of iron.

This precip. seemed to be the acid with tannin and gallic acid and some extract. The solid matter remaining on the filtre, slightly reddened vegetable blues; and, when dissolved in warm water, copiously precipitated the solutions of isinglass. M. Proust,* who first paid attention to its properties, supposes that it is a compound of the acid with tannin: but I suspect that it also contains gallic acid, and probably a small portion of extractive matter. This last substance, as is well known, is thrown down from its solutions by sulphuric acid; and I found, in distilling the precipitate from galls by sulphuric acid, at a heat above 212°, that a fluid came over, of a light yellow colour, which was rendered black by oxygenated sulphate of iron; but which was not altered by gelatine.

Muriatic acid threw down a precip. of tannin and acid, and a similar compound with excess of acid continued dissolved. Muriatic acid produced, in the infusion, effects analogous to those produced by sulphuric acid; and two compounds of the acid and the vegetable substances were formed: the one united to excess of acid, which remained in solution; the other containing a considerable quantity of tannin, which was precipitated in the solid form.

Nitric acid destroyed both principles and formed a kind of extract. When concentrated nitric acid was made to act upon the infusion, it was rendered turbid; but the solid matter formed was immediately dissolved with effervescence, and the liquor then became clear, and of an orange colour. On examining it, it was found that both the tannin and the gallic acid were destroyed; for it gave no precipitate, either with gelatine or the salts of iron, even after the residual nitric acid was saturated by an alkali. By evaporation of a portion of the fluid, a soft substance was obtained, of a yellowish brown colour, and of a slightly sourish taste. It was soluble in water, and precipitated the nitro-muriate of tin, and the nitrate of alu-

* The fact of the precipitation of solution of galls by acids, was noticed by M. Dizé. See *Annales de Chimie*, Tome XXXV. p. 37. mine;

mine; so that its properties approached to those of extractive matter; and it probably contained oxalic acid, as it rendered turbid a solution of muriate of lime.

When a very weak solution of nitric acid was mixed with the infusion, a permanent precipitate was formed, and the residual liquor, examined by the solution of gelatine, was found to contain tannin.

Weak nitric acid acted more like the other acids.

A solution of pure potash was poured into a portion of the infusion. At first, a faint turbid appearance was perceived; but, by agitation, the fluid became clear, and its colour changed from yellow brown to brown red; and this last tint was most vivid on the surface, where the solution was exposed to the atmosphere. The solution of isinglass did not act upon the infusion modified by the alkali, till an acid was added in excess, when a copious precipitation was occasioned.

Pure potash seemed to have combined with the tannin, which did not then separate by gelatine till an acid had taken the alkali.

The compound of potash and solution of galls, when evaporated, appeared in the form of an olive-coloured mass, which had a faint alkaline taste, and which slowly deliquesced when exposed to the air.

The alkaline compound separated by evaporation.

Soda acted upon the infusion in the same manner as potash; and a fluid was formed, of a red-brown colour, which gave no precipitate to gelatine.

Soda acted like potash.

Solution of ammonia produced the same colour as potash and soda, and formed so perfect an union with the tannin of the infusion, that it was not acted upon by gelatine. When the compound liquor was exposed to the heat of boiling water, a part of the ammonia flew off, and another part reacted upon the infusion, so as to effect a material change in its properties. A considerable quantity of insoluble matter was formed; and the remaining liquor contained little tannin and gallic acid, but a considerable portion of a substance that precipitated muriate of tin, and the salts of alumine.

Ammonia.

When the experiment on the ebullition of the compound of the infusion and ammonia was made in close vessels, the liquor that came over was strongly impregnated with ammonia; its colour was light yellow, and, when saturated with an acid, it was very little altered by the salts of iron. The residual fluid, after the process had been continued for some time, as in the other case, precipitated gelatine slightly, but the salts of alumine copiously; and it gave a tinge of red to litmus paper.

When

Lime, frontia, or barytes, in excess, threw down nearly the whole contents. When solution of lime, of frontia, or of barytes, was poured in excess into a portion of the infusion, a copious olive-coloured precipitate was formed, and the solution became almost clear, and of a reddish tint. In this case, the tannin, the gallic acid, and the extractive matter, seemed to be almost wholly carried down in the precipitates; as the residual fluids, when saturated by an acid, gave no precipitate to gelatine, and only a very slight tint of purple to oxygenated sulphate of iron.

Effect of smaller doses. When the solutions of the alkaline earths were used only in small quantities, the infusion being in excess, a smaller quantity of precipitate was formed, and the residual liquor was of an olive-green colour; the tint being darkest in the experiment with the barytes, and lightest in that with the lime. This fluid, when examined, was found to hold in solution a compound of gallic acid and alkaline earth. It became turbid when acted on by a little sulphuric acid; and, after being filtrated, gave a black colour with the solutions of iron, but was not acted upon by gelatine.

Effect of lime more particularly stated, When a large proportion of lime was heated for some time with the infusion, it combined with all its constituent principles, and gave, by washing, a fluid which had the taste of lime-water, and which held in solution only a very small quantity of vegetable matter. Its colour was pale yellow; and, when saturated with muriatic acid, it did not precipitate gelatine, and gave only a slight purple tinge to the solutions of the salts of iron. The lime in combination with the solid matter of the infusion, was of a fawn colour. It became green at its surface, where it was exposed to the air; and, when washed with large quantities of water, it continued to give, even to the last portions, a pale yellow tinge.

also of magnesia. Magnesia was boiled in one portion of the infusion for a few hours; and mixed in excess with another portion, which was suffered to remain cold. In both cases, a deep green fluid was obtained, which precipitated the salts of iron, but not the solutions of gelatine; and the magnesia had acquired a grayish-green tint. Water poured upon it became green, and acquired the properties of the fluid at first obtained. After long washing, the colour of the magnesia changed to dirty yellow; and the last portions of water made to act upon it were pale yellow, and altered very little the solutions of iron.

When the magnesia was dissolved in muriatic acid, a brownish and turbid fluid was obtained, which precipitated gelatine and the oxygenated salts of iron. So that there is every reason to believe, that the earth, in acting on the astringent infusion, had formed two combinations; one containing chiefly gallic acid, which was easily soluble in water; the other containing chiefly tannin, which was very difficultly soluble.

Muriatic solution of magnesia.

Alumine boiled with the infusion became yellowish-gray, and gave a clear white fluid, which produced only a tinge of light purple in the solutions of iron. When the earth * was employed in very small quantity, however, it formed an insoluble compound only with the tannin and the extract; and the residual liquor was found to contain a gallate of alumine with excess of acid.

Alumine,

The oxides of tin and of zinc, obtained by nitric acid, were boiled with separate portions of the infusion for two hours. In both cases, a clear fluid, which appeared to be pure water, was obtained; and the oxides gained a tint of dull yellow. A part of each of them was dissolved in muriatic acid. The solution obtained was yellow: it copiously precipitated gelatine; and gave a dense black with the salts of iron. Mr. Proust †, who first observed the action of oxide of tin upon astringent infusions, supposes that portions of tannin and gallic acid are decomposed in the process, or converted, by the oxygen of the oxide, into new substances. These experiments do not, however, appear to confirm the supposition.

Effect of the oxides of tin and zinc on the infusion. They seized the dissolved matter, and a muriatic solution of the compound precipitated gelatine and gave a black with iron.

M. Deyeux observed, that a copious precipitation was occasioned in infusion of galls, by solutions of the alkalis combined with carbonic acid. Mr. Proust has supposed that the solid matter formed is pure tannin, separated from its solution by the stronger affinity of the alkali for water; and he recommends the process, as a method of obtaining tannin.

The precipitate by alkalis from inf. galls is not pure tannin as Proust supposes;

In examining the precipitate obtained by carbonate of potash fully combined with carbonic acid, and used to saturation, I have not been able to recognize in it the properties which are usually ascribed to tannin: it is not possessed of the astringent taste; and it is but slightly soluble in cold water, or in alcohol.

for it is not astringent, nor well soluble in cold water or alcohol, nor acts on gelatine, nor tans skins.

* Mr. Fiedler, I believe, first observed the action of alumine upon tannin. Van-Mons's Journal, Vol. I. p. 86.

† *Annales de Chimie*, Tome XLII. p. 69.

Its solution acts very little upon gelatine, till it is saturated with an acid; and it is not possessed of the property of tanning skin.

In various cases, in which the greatest care was taken to use no excess, either of the astringent infusion or of the alkaline solution, I have found the solid matter obtained possessed of analogous properties: and it has always given, by incineration, a considerable portion of carbonate of potash, and a small quantity of carbonate of lime.

It contains
alkali.

The remaining
fluid containing
alkali,

The fluid remaining after the separation of the precipitate, was of a dark-brown colour, and became green at the surface, when it was exposed to the air. It gave no precipitate to solution of gelatine; and afforded only an olive-coloured precipitate with the salts of iron.

exhibited tannin
when the alkali
was saturated
with mur. acid.

When muriatic acid was poured into the clear fluid, a violent effervescence was produced; the fluid became turbid; a precipitate was deposited; and the residual liquor acted upon gelatine and the salts of iron, in a manner similar to the primitive infusion.

Crystals of gallic
acid by distill.
from the pre-
cipitate.

M. Deyeux, in distilling the precipitate from infusion of galls by carbonate of potash, obtained crystals of gallic acid. In following his process, I had similar results; and a fluid came over, which reddened litmus-paper, and precipitated the salts of iron black, but did not act upon gelatine.

The precip. was
partly soluble in
warm water:

When the precipitate by carbonate of potash was acted upon by warm water, applied in large quantities, a considerable portion of it was dissolved; but a part remained, which could not in any way be made to enter into solution; and its properties were very different from those of the entire precipitate. It was not at all affected by alcohol: it was acted on by muriatic acid, and partially dissolved; and the solution precipitated gelatine and the salts of iron. It afforded, by incineration, a considerable portion of lime, but no alkali.

Hence the
precip. was tannin
and gallic acid
with a little
alkali, and also
the same with
lime; both car-
bonated.

In comparing these facts, it would seem, that the precipitate from infusion of galls, consists partly of tannin and gallic acid united to a small quantity of alkali, and partly of these vegetable matters combined with calcareous earth; and it will appear probable, when the facts hereafter detailed are examined, that both the potash and the lime are contained in these compounds in a state of union with carbonic acid.

The

The solutions of carbonate of soda and of carbonate of ammonia, both precipitated the infusion of galls in a manner similar to the carbonate of potash; and each of the precipitates, when acted on by boiling water, left a small quantity of insoluble matter, which seemed to consist chiefly of tannin and carbonate of lime.

Habitudes of carb. of soda and ammonia nearly as those of potash.

The entire precipitate by carbonate of soda produced, when incinerated, carbonate of soda and carbonate of lime. The precipitate by carbonate of ammonia, when exposed to a heat sufficient to boil water, in a retort having a receiver attached to it, gave out carbonate of ammonia, (which was condensed in small crystals in the neck of the retort,) and a yellowish fluid, which had the strong smell and taste of this volatile salt. After the process of distillation, the solid matter remaining was found of a dark brown colour; a part of it readily dissolved in cold water, and the solution acted on gelatine.

Particular detail. The precipitate :

The residual fluid of the portions of the infusion which had been acted on by the carbonates of soda and of ammonia, as in the instance of the carbonate of potash, gave no precipitate with gelatine, till they were saturated with an acid; so that, in all these cases, the changes are strictly analogous.

The residual fluid.

The infusion of galls, as appears from the analysis, contains in its primitive state calcareous matter. By the action of the mild alkalis, this substance is precipitated in union with a portion of the vegetable matter, in the form of an insoluble compound. The alkalis themselves, at the same time, enter into actual combination with the remaining tannin and gallic acid; and a part of the compound formed is precipitated, whilst another part remains in solution.

Infusion of galls contains calcareous matter.

When the artificial carbonates of lime, magnesia, and barytes, were separately boiled with portions of the infusion of galls for some hours, they combined with the tannin contained in it, so as to form with it insoluble compounds; and, in each case, a deep green fluid was obtained, which gave no precipitate to gelatine, even when an acid was added, but which produced a deep black colour in the solutions of the salts of iron.

Carbonates of alkaline earths gave insol. precip.

The fluid shewed no tannin.

Sulphate of lime, when finely divided, whether natural or artificial, after having been long heated with a small quantity of the infusion, was found to have combined with the tannin of it, and to have gained a faint tinge of light brown. The liquid

Sulphate of lime seized the tannin by boiling.

became

became of a blue-green colour, and acted upon the salts of iron, but not upon gelatine; and there is every reason to suppose, that it held in solution a triple compound, of gallic acid, sulphuric acid, and lime.

Solutions of neutral salts precip. Inf. galls, but it is not pure tannin.

We owe to Mr. Proust, the discovery that different solutions of the neutral salts precipitate the infusion of galls; and he supposes, that the precipitation is owing to their combining with a portion of the water which held the vegetable matter in solution. In examining the solid matters thrown down from the infusion, by sulphate of alumine, nitrate of potash, acetite of potash, muriate of soda, and muriate of barytes, I found them soluble, to a certain extent, in water, and possessed of the power of acting upon gelatine. From the products given by their incineration, and by their distillation, I am however inclined to believe that they contain, besides tannin, a portion of gallic acid and extractive matter, and a quantity of the salt employed in the primitive solution.

Metallic solutions give dense precip.

It is well known, that many of the metallic solutions occasion dense precipitates in the infusion of galls; and it has been generally supposed, that these precipitates are composed of tannin and extractive matter, or of those two substances and gallic acid, united to the metallic oxide; but, from the observation of different processes of this kind, in which the salts of iron and of tin were employed, I am inclined to believe, that they contain also a portion of the acid of the saline compound.

probably containing some of the acid.

Muriate of tin.

When the muriate of tin was made to act upon a portion of the infusion, till no more precipitation could be produced in it, the fluid that passed through the filtre still acted upon gelatine and seemed to contain no excess of acid; for it gave a precipitate to carbonate of potash, without producing effervescence. The solid compound, when decomposed by sulphuretted hydrogen, after the manner recommended by Mr. Proust, was found strongly to redden litmus-paper, and it copiously precipitated nitrate of silver: whereas, the primitive infusion only rendered it slightly turbid; so that there is every reason to believe, that the precipitate contained muriatic acid,

Oxygenated sulphate of iron.

By passing the black and turbid fluid, procured by the action of solution of oxygenated sulphate of iron in excess upon a portion of the infusion, through finely-divided pure flint, contained

tained in four folds of filtrating paper, I obtained a light olive-green fluid, in which there was no excess of sulphuric acid, and which I am inclined to suppose was a solution of the compound of gallic acid and sulphate of iron, with superabundance of metallic salt. I have already mentioned that gallic acid, when in very small proportion, does not precipitate the oxygenated salts of iron; and Mr. Proust, in his ingenious paper upon the difference of the salts of iron, has supposed that, in the formation of ink, a portion of the oxide of iron in union with gallic acid is dissolved by the sulphuric acid of the sulphate. This comes near to the opinion that they form a triple compound: and, in reasoning upon the general phenomena, it seems fair to conclude, that, in the case of the precipitation of tannin by the salts of tin and of iron, compounds are formed, of tannin and the salts; and that, of these compounds, such as contain tin are slightly soluble in water, whilst those that contain iron are almost wholly insoluble.

In examining the action of animal substances upon the infusion of galls, with the view of ascertaining the composition of the compounds of gelatine, and of skin; with tannin, I found that a saturated solution of gelatine, which contained the soluble matter of 50 grains of dry isinglass, produced from the infusion a precipitate that weighed nearly 91 grains; and, in another instance, a solution containing 30 grains of isinglass, gave about 56 grains; so that, taking the mean of the two experiments, and allowing for the small quantity of insoluble matter in isinglass, we may conclude, that 100 grains of the compound of gelatine and tannin, formed by precipitation from saturated solutions, contain about 54 grains of gelatine, and 46 of tannin.

A piece of dry calf-skin, perfectly free from extraneous matter, that weighed 180 grains, after being prepared for tanning by long immersion in water, was tanned in a portion of the infusion, being exposed to it for three weeks. When dry, the leather weighed 295 grains: so that, considering this experiment as accurate, leather quickly tanned by means of an infusion of galls, consists of about 61 grains of skin, and 39 of vegetable matter, in 100 grains.

After depriving a portion of the infusion of all its tanning matter, by repeatedly exposing it to the action of pieces of skin, I found that it gave a much slighter colour to oxygenated sulphate

Animal matters
and the infusion
of galls.

Precipitate by
gelatine contains
54 gelatine and
46 tannin nearly.

A skin quickly
tanned contains
about three parts
skin and one
vegetable matter.

The exhausted
infusion contain
ed less gallic
acid; probably
from decomposition
by exposure.

fulphate of iron, than an equal portion of a similar infusion which had been immediately precipitated by solution of isinglass; but I am inclined to attribute this effect, not to any absorption of gallic acid by the skin, but rather to the decomposition of it by the long continued action of the atmosphere; for much insoluble matter had been precipitated, during the process of tanning, and the residuum contained a small portion of acetous acid.

Component parts of Aleppo galls. In ascertaining the quantity of tannin in galls, I found that 500 grains of good Aleppo galls gave, by lixiviation with pure water till their soluble parts were taken up, and subsequent slow evaporation, 185 grains of solid matter. And this matter, examined by analysis, appeared to consist,

Solid matter.	Of tannin	- - - - -	130 grains.
	Of mucilage, and matter rendered insoluble by evaporation	- - - - -	12
	Of gallic acid, with a little extractive matter		31
	Remainder, calcareous earth and saline matter		12

Residual fluid. The fluid obtained by the last lixiviation of galls, as M. Deyeux observed, is pale green; and I am inclined to believe, that it is chiefly a weak solution of gallate of lime. The ashes of galls, deprived of soluble matter, furnish a very considerable quantity of calcareous earth. And the property which M. Deyeux discovered in the liquor of the last lixiviations, of becoming red by the action of acids, and of regaining the green colour by means of alkalis, I have observed, more or less, in all the soluble compounds containing gallic acid and the alkaline earths.

III. EXPERIMENTS AND OBSERVATIONS ON CATECHU OR TERRA JAPONICA.

Catechu or terra japonica. The extract called catechu is said to be obtained from the wood of a species of the Mimosa *, which is found abundantly in India, by decoction and subsequent evaporation.

History and external characters from Bombay and Bengal. There are two kinds of this extract; one is sent from Bombay, the other from Bengal; and they differ from each other more in their external appearance than in their chemical composition. The extract from Bombay is of an uniform texture, and of a red-brown tint, its specific gravity being generally

See Kerr. Medical Observations, Vol. V. p. 155.

about 1.39. The extract from Bengal is more friable, and less consistent; its colour is like that of chocolate externally, but, when broken, its fracture presents streaks of chocolate and of red-brown. Its specific gravity is about 1.28. Their tastes are precisely similar, being astringent, but leaving in the mouth a sensation of sweetness. They do not deliquesce, or apparently change, by exposure to the air.

The discovery of the tanning powers of catechu, is owing to Sir J. Banks the President of the Royal Society, who, concluding from its sensible properties that it contained tannin, furnished me, in December, 1801, with a quantity for chemical examination.

In my first experiments, I found that the solutions of catechu copiously precipitated gelatine, and speedily tanned skin; and in consequence, I began a particular investigation of their properties.

The strongest infusions and decoctions of the two different kinds of catechu, do not sensibly differ in their nature, or in their composition. Their colour is deep red-brown, and they communicate this tinge to paper; they slightly redden litmus-paper; their taste is highly astringent, and they have no perceptible smell.

The strongest infusions that I could obtain from the two kinds of catechu, at 48° Fahrenheit, were of the same specific gravity, 1.057. But, by long decoction, I procured solutions of 1.102, which gave, by evaporation, more than $\frac{1}{2}$ of their weight of solid matter.

Five hundred grains of the strongest infusion of catechu from Bombay, furnished only 41 grains of solid matter; which, from analysis, appeared to consist of 34 grains of tannin, or matter precipitable by gelatine, and 7 grains that were chiefly a peculiar extractive matter, the properties of which will be hereafter described. The quantity of solid matter given by the strongest infusion of the Bengal catechu, was the same, and there was no sensible difference in its composition. Portions of these solid matters, when incinerated, left a residuum which seemed to be calcareous; but it was too small in quantity to be accurately examined, and it could not have amounted to more than $\frac{1}{200}$ of their original weights.

The strongest infusions of catechu acted upon the acids and pure alkalis in a manner analagous to the infusion of galls. With the concentrated sulphuric and muriatic acids, they gave dense

Sir J. Banks first concluded that it contains tannin.

Experiments shew the truth of this conclusion.

Both kinds agree in their nature, &c.

The strongest infusions of catechu

afforded 41 solid matter; viz. 34 grains tannin and 7 peculiar extract.

The infusion examined by acids and alkalis;

dense light fawn coloured precipitates. With strong nitrous acid they effervesced; and lost their power of precipitating the solutions of isinglass, and the salts of iron. The pure alkalis entered into union with their tannin, so as to prevent it from being acted upon by gelatine.

— with the soluble earths.

When the solutions of lime, of strontia, or of barytes, were poured into the infusions, copious precipitates, of a shade of light brown, were formed; and the residual fluid assumed a paler tint of red, and was found to have lost its power of precipitating gelatine.

Lime, magnesia, carbonate of magnesia.

After lime had been boiled for some time with a portion of the infusion, it assumed a dull red colour. The liquor that passed from it through the filtre had only a faint tint of red, did not act upon gelatine, and seemed to contain only a very small portion of vegetable matter. Pure magnesia, when heated with the infusion, acted upon it in an analogous manner; the magnesia became light red, and the residual fluid had only a very slight tinge of that colour. With carbonate of magnesia, the infusion became deeper in colour, and lost its power of precipitating gelatine; though it still gave, with oxygenated sulphate of iron, a light olive precipitate.

Carbonates of potash, soda, ammonia.

The carbonates of potash, of soda, and of ammonia, in their concentrated solutions, produced only a slight degree of turbidness in the infusion of catechu; they communicated to them a darker colour, and deprived them of the power of acting upon gelatine; though this power was restored by the addition of an acid.

Exposure to the air.

After the mixture of the solution of carbonate of potash and the infusions had been exposed to the atmosphere for some hours, a brown crust was found to have formed upon its surface, and a slight precipitation had taken place

Aluminous salts on infusion of catechu.

The salts of alumine precipitated the infusions, but less copiously than they precipitate the infusion of galls. A similar effect was produced by nitrate of potash, sulphate of magnesia, prussiate of potash, and many other neutral salts.

Nitrate or acetate of lead.

The nitrate, or acetate, of lead, in concentrated solution, when poured into the infusion, produced in it a dense light brown precipitate, which gave to the fluid a gelatinous appearance. After this effect, there was no free acid found in it; and both the tannin and the extractive matter seemed to have been carried down, in union with a portion of the metallic salt.

The

The solution of muriate of tin, acted upon the infusion of Muriate of tin, catechu in a manner similar to that in which it acts upon the infusion of galls.

The least oxygenated sulphate of iron produced no change in the infusion. With the most oxygenated sulphate it gave a dense black precipitate, which, when diffused upon paper, appeared rather more inclined to olive than the precipitate from galls.

The infusions were precipitated by the solution of albumen.

The precipitates by gelatine had all a pale tint of red-brown, which became deeper when they were exposed to the air. The compound of gelatine and the tannin of the strongest infusions of catechu appeared, by estimation of the quantity of isinglass in the solutions used for their precipitation, to consist of about 41 parts of tannin, and 59 of gelatine.

Of two pieces of calf-skin which weighed, when dry, 132 grains each, and which had been prepared for tanning, one was immersed in a large quantity of the infusion of catechu from Bengal, and the other in an equal portion of the infusion of that from Bombay. In less than a month they were found converted into leather. When freed from moisture, by long exposure in the sunshine, they were weighed. The first piece had gained about 34 grains; and the second piece 35½ grains. The leather was of a much deeper colour than that tanned with galls, and on the upper surface was red-brown. It was not acted on by hot or cold water; and its apparent strength was the same as that of similar leather tanned in the usual manner.

In examining the remainder of the infusions of catechu, in which skin had been converted into leather, I found in them much less extractive matter than I had reason to expect, from the comparative analysis of equal portions of the unaltered infusions made by solutions of gelatine. At first, I was inclined to suppose that the deficiency arose from the action of the atmosphere upon the extractive matter, by which a part of it was rendered insoluble. But, on considering that there had been very little precipitation in the process, I was led to adopt the supposition, that it had entered into union with the skin, at the same time with the tannin; and this supposition was confirmed by new experiments.

Both kinds of catechu are almost wholly soluble in large quantities of water; and, to form a complete solution, about 18

Sulphates of iron.

Precipitates from infusion of catechu by gelatine 41 tannin and 59 gelatine.

Calf skin (quickly) tanned by catechu afforded good leather. The increase of weight was rather less than one fifth of the leather.

Little extractive matter left.

It had in part combined with the skin.

Water dissolves nearly the whole of catechu.

ounces of water, at 52°, are required to 100 grains of extract. The residuum seldom amounts to $\frac{1}{4}$ of the original weight of the catechu; and, in most cases, it is found to consist chiefly of calcareous and aluminous earths, and of fine sand, which, by accident or design, had probably been mixed with the primitive infusion at the time of its evaporation.

Alcohol dissolves much; and leaves mucilage. A considerable portion of both kinds of catechu is soluble in alcohol; but, after the action of alcohol upon it, a substance remains of a gelatinous appearance and a light brown colour, which is soluble in water, and is analogous in its properties to gum or mucilage.

The peculiar extract of catechu is much less soluble than tannin in water. The peculiar extractive matter in the catechu, is much less soluble in water than the tanning principle; and, when a small quantity of water is used to a large quantity of catechu, the quantity of tannin taken up, as appears from the nature of the strongest infusion, is very much greater than that of the extractive matter.

And is more soluble by heat. The extractive matter is much more soluble in warm water than in cold water; and when saturated solutions of catechu are made in boiling water, a considerable quantity of extractive matter, in its pure state, falls down, as the liquor becomes cool.

Repeated lixiviations of catechu in water, leave the peculiar extract. The peculiar extractive matter of the catechu may be likewise obtained, by repeatedly lixiviating the catechu, when in fine powder, till the fluids obtained cease to precipitate gelatine; the residual solid will then be found to be the substance in question.

Its sensible qualities. The pure extractive matter, whether procured from the Bombay or Bengal catechu, is pale, with a faint tinge of red-brown. It has no perceptible smell; its taste is slightly astringent; but it leaves in the mouth, for some time, a sensation of sweetness, stronger than given by the catechu itself.

Solution in water and in alcohol. Its solution in water is at first yellow-brown; but it gains a tint of red by exposure to the air. Its solution in alcohol does not materially change colour in the atmosphere; and it is of an uniform dull brown.

Habitude with tests; The extractive matter, whether solid or in solution, was not found to produce any change of colour upon vegetable blues.

— with alkalis. It became of a brighter colour by the action of the alkalis; but it was not precipitated from its solution in water by these bodies, nor by the alkaline earths.

The aqueous solution of it, when mixed with solutions of nitrate of alumine and of muriate of tin, became slightly turbid: — with other bodies.

To nitrate of lead, it gave a dense light brown precipitate.

It was not perceptibly acted upon by solution of gelatine; but, when solution of sulphate of alumine was added to the mixture of the two fluids, a considerable quantity of solid matter, of a light brown colour, was immediately deposited.

To the solution of oxygenated sulphate of iron, it communicated a fine grass-green tint; and green precipitate was deposited, which became black by exposure to the air.

It was not precipitated by the mineral acids.

Linen, by being boiled in the strongest solution of the extractive matter, acquired a light red-brown tint. The liquor Linen abstracts its tinge. became almost colourless; and, after this, produced very little change in the solution of oxygenated sulphate of iron.

Raw skin, prepared for tanning by being immersed in the strong solution, soon acquired the same kind of tint as the linen. — as does raw skin. It united itself to a part of the extractive matter; but it was not rendered by it insoluble in boiling water.

The solid extractive matter, when exposed to heat, softened, and became darker in its colour, but did not enter into fusion. At a temperature below that of ignition, it was decomposed. The volatile products of its decomposition were, carbonic acid, hydrocarbonate, and water holding in solution acetous acid and a little unaltered extractive matter. There remained a light and very porous charcoal. The solid extract softens but does not fuse by heat. Destructive decomposition.

In considering the manner in which the catechu is prepared, it would be reasonable to conclude, that different specimens of that substance must differ in some measure in their composition, even in their pure states; and, for the purposes of commerce, they are often adulterated to a considerable extent, with sand and earthy matter*.

In attempting to estimate the composition of the purest catechu, I selected pieces from different specimens, with which I was supplied by the president, and reduced them together into powder; mixing, however, only those pieces which were from catechu of the same kind. Component parts of catechu.

* One specimen that I examined, of the terra japonica of commerce, furnished, by incineration, $\frac{1}{7}$ of sand and earthy matter; and another specimen, nearly $\frac{1}{2}$.

Two hundred grains of the powder procured in this way, from the catechu of Bombay, afforded by analysis.

	Grains.
Bombay catechu. Tannin - - - - -	109
Peculiar extractive matter - - - - -	68
Mucilage - - - - -	13
Residual matter; chiefly sand and calcareous earth	10

The powder of the Bengal catechu gave, by similar methods of analysis, in 200 grains,

	Grains.
Bengal catechu. Tannin - - - - -	97
Peculiar extractive matter - - - - -	73
Mucilage - - - - -	16
Residual matter; sand, with a small quantity of calcareous and aluminous earths - - - - -	14

In the solid samples the tannin and mucilage are imperfectly mingled. In examining those parts of the catechu from Bengal which were differently coloured, I found the largest proportion of tannin in the darkest part of the substance; and most extractive matter in the lightest part. It is probable that the inequality of composition in this catechu, is owing to its being evaporated and formed without much agitation; in consequence of which, the constituent parts of it that are least soluble, being first precipitated, appear in some measure distinct from the more soluble parts, which assume the solid form at a later period of the process.

Pale catechu most in request. It contains more of the extract. From the observations of Mr. Kerr*, it would appear, that the pale catechu is that most sought after in India; and it is evidently that which contains most extractive matter. The extractive matter seems to be the substance that gives to the catechu the peculiar sweetness of taste which follows the impression of astringency; and it is probably this sweetness of taste which renders it so agreeable to the Hindoos, for the purpose of chewing with the betle-nut.

(To be continued.)

* Medical Observations, Vol. V. page 155.

X.

Memoir on the Nature and the new Properties of the Prussic Acid.

By C. IT. CURAUDAU, corresponding Member of La Société libre des Pharmaciens de Paris*.

THE Prussic colouring matter has been long known in chemistry, but we have yet no correct knowledge of its nature and different modifications; chemists are not even agreed as to the formation of this singular substance. Some admit that oxygen forms a part of its composition, and others assert it to be an acid without oxygen. Indeed this composition performs the functions of an acid in many instances; but in certain others, and particularly in its simple state, it ceases to exhibit the properties common to other acids. This diversity of opinions on a substance which we can compose and decompose, reduce and combine at pleasure, seems to contradict our chemical knowledge, and to carry us from that perfection towards which perfect science ought continually to tend.

Uncertainties still existing as to the theory of Prussic acid.

In fact, if in the class of acids it were proved that there were one without oxygen; might we not conclude that the bases might, as well as oxygen, contain the acidifying principle, and would it not follow that we have gratuitously ascribed to vital air the property of generating acid?

Whether an acid can exist without oxygen

It is of importance then, under these circumstances, to fix the opinions of the chemists on this point, and to determine what may be the concurring causes that give the prussic colouring matter the characteristic properties of acids, or to separate it from their class. This is the problem which I have undertaken to resolve. If I have not fulfilled the task, I dare at least flatter myself that my observations will prove useful to the progress of science, and that they may deserve the attention of the chemical world.

The present research is to determine the causes by which Prussic colouring matter is acidified, and the contrary.

In order to avoid the continual repetition of Prussic acid, Prussic colouring matter, &c. I shall in future describe this acid under the name of "*Prussire*," its combinations without oxygen "*Prussiurets*," and its oxygenated combinations "*Prussiates*." This correction of Nomenclature becomes necessary

Adjustment of terms.

* From Les Annales de Chimie, No. 137. Vol. XLVI. p. 148.

not only to shorten the expressions, but to explain in proper terms such notions as indicate the real state of the substance under description.

The gaseous prussic acid has less oxygen than when in combination.

It cannot exist with more oxygen: For this last will acidify its carbon, while the azote and hydrogen form ammonia; and hence carbonate of ammonia will be had.

Supposed incongruity of the volatility (or gaseous form) of prussic, and its original fixity.

The fixity of prussic, as shewn by the great heat it supports during calcination does not arise from oxygen.

Chemists have never distinguished the Prussic acid, saturating a metallic substance from the gaseous *Prussire*. They have always believed on the contrary that this last, extracted from any Prussiate whatever, was the same as in its state of combination. But on a nearer examination, we find it impossible to obtain simple Prussic acid; that it can only exist in a combined state; that its combinations can only take place by virtue of the oxygen afforded by the base to which it is united: lastly, that the simple prussire cannot exist with the presence of oxygen without an immediate destruction of the affinity which held its three component principles together. The carbon in this radical is particularly disposed to unite with oxygen, and at the instant when the subtraction of this principle is made, the azote and the hydrogen become liberated, and immediately form ammonia, which is also saturated by the carbonic acid at the moment of their mutual production. Water is the oxygenated compound which has the strongest effect in producing the decomposition of prussire, without separating its elements; that is to say, the result of this decomposition is always carbonic acid and ammonia.

The fixity of prussire shewn by the high temperature at which it is formed, does not appear to me to agree with what is already known relative to its extreme volatility, and of its little disposition to unite directly with saline bases. I thought it necessary to direct my enquiries to this object, in order to ascertain, if possible, the cause which produced such opposite properties in the same substance, and which, as I apprehend, must arise from facts not yet known and understood.

I could not attribute the fixity of prussire to the oxygen, since, by the experiments which I published five years ago *, I have shewn that this principle always opposes its formation, and even destroys it when already formed. Thus, from the moment it was proved that the calcinations intended to form the prussire is ineffectual wherever oxygen is present, I could no longer admit that this compound, when formed by calcination, with an alkali, can derive its fixity from oxygen. It be-

* Journal de Pharmacie, cahier de Germinal, An 6.

came necessary, therefore, for me to direct my enquiries according to the indications afforded by my own experiments, and to continue them till I should become acquainted with the cause of fixity in a substance which immediately on its formation, possessed properties quite opposite to those which it seemed to possess during the action of calcination.

Perseverance in varying my enquiries on this object, gave me an opportunity of making many important observations, and to ascertain, by decisive experiments, that the Prussic calcination gives nothing more than the carbonated azote of potash, and that the hydrogen which enters into the composition of the prussire, is always the result of a combination which takes place subsequent to the calcination. Hence I saw that it was this circumstance which so long produced the chemical illusion, and from which we still confound this production with that contained in the crucible. But it is a phænomenon worthy of remark, and of which the explanation is very proper to throw light on the nature and formation of prussire, that the carbonated azote of potash possesses the property of instantly decomposing water; and at the moment the water is decomposed, the prussire is formed, together with ammonia and carbonic acid. Though this immediate production of three new compounds is the result of very complicated affinities, which are difficult to be well made out; we may nevertheless suspect that the potash, which is always found in excess in prussic calcinations, is the substance which in this case preponderates in the affinities, and that this substance, by a predisposing attraction, promotes the oxygenation of the carbon at the expence of the oxygen of the water; the hydrogen, which being set at liberty by this subtraction of oxygen, unites at the same time with the carbonated azote of potash. One part of the hydrogen is thus employed in forming ammonia, while the other serves to form the prussire, which was not prussire for want of the hydrogen, its third element. It is still farther in favour of this theory that the potash in its progress to saturation with carbonic acid may, by long ebullition, successively convert the greater part of the prussire, into ammonia and carbonic acid; this decomposition usually continues till the potash is completely saturated with the carbonic acid. Hence it is that a very good prussic lixivium may be partly decomposed by simple ebullition, and that in all prussic calcinations, those

The calcination affords only carbonated azote of potash. The hydrogen is not given till water is applied:

— which fluid is instantly decomposed.

Explanation of the effect of the affinities.

The prussic lixivium may be decomposed by water and loss incurred.

which

which are most strongly heated always form lixivium saturated with the carbonic acid; this can only happen at the expence of the oxygen of the water and the carbon of the carbonated azote of potash: The potash, therefore, as I have already observed, contributes in part, by a predisposing attraction, to the successive developement of all these new combinations.

After having proved that carbonated azote of potash instantly decomposes water, and that water also successively decomposes the prussire; which is always by a diminution of its quantity; I shall now shew the means which must be used to prevent the destruction of this substance, and shew that they perfectly agree with the theory I have here laid down.

Remedy against the loss by using water with the prussic calcined matter.

Sulphate of iron used instead.

The prussire can only be oxygenated by a metallic oxide.

Inference.

The destructibility of prussire of potash being highly prejudicial in large operations, I directed my researches to the means of preventing that loss. The result of my observations is, that, in order to stop this decomposition, it is an indispensable condition, never to wet a prussic calcination but with a solution of sulphate of iron at the maximum of oxygenation; this method is intitled to the most decided preference; for in this case the prussire, which is formed at the moment of the mixture, being in a state favourable to the saturation of potash, it becomes fixed at the expence of the oxygen of the oxide of iron, and in this state it is prussic acid. I must observe that this oxygenated combination of prussire cannot take place but by the concurrence of metallic substances already oxygenated; and that it is only then that an equilibrium of affinities is established between the potash, the prussire, and the metallic oxide; which produces a complex and energetive combination, that I propose to call prussiate of potash by the oxide of iron; which method of nomenclature I propose to apply to each several metallic oxide which enters into the composition of prussiate of potash. All these combinations crystallize easily by evaporation, and by cooling the lixivium we may obtain very regularly formed crystals.

The consequences we should naturally draw from these latter observations shew therefore irrevocably that the prussire owes its acid or neutralising property to the oxygen of the oxide of iron, and that as the quantity of water decomposed in this case does not exceed what is necessary to supply the hydrogen in the prussire, we must accordingly obtain more abundant products than in any other case.

I may

I may observe, in support of what I have here advanced, that M. Berthollet, whose sagacity is well known, has proved, more than fifteen years ago, by a very accurate analysis, that what was called the prussic acid, does not contain oxygen. It is true indeed, that there are many chemists no less celebrated, who have not coincided in this opinion, and have considered what M. Berthollet had advanced as very far from complete proof. I must, nevertheless, insist that the labours which that learned chemist has made known on this object, are a prelude, in some sort, to the discoveries which remained to be made, in order to complete the history of the properties of the prussic acid.

It remains then very evidently demonstrated from these results, as well as those obtained by M. Berthollet, how far this theory is satisfactory, and how much it enables us to explain facts, respecting which we have hitherto had no very correct notions. Lastly, from all these new applications, we may, with a knowledge of the causes, fabricate in the large way, crystals of prussiate of potash, by oxide of iron. It is now a considerable time since I first prepared similar crystals, but without being able to establish a theory sufficiently clear to be communicated.

It is to be remarked that Prussian blue obtained with the prussiate of potash by the oxide of iron, gives a blue of the most exquisite beauty. Its interior fracture is smooth and of a copperish hue, similar to that seen in indigo of the best quality. An observation which equally deserves mention is, that the prussiate of potash by oxide of iron has the property of forming with a hot solution of sulphate of alumine, a magma analogous to that of boiled fecula. If the mixture be diluted with water to favour the separation of the precipitate it is then of a sky blue colour. This precipitate washed and dried becomes of a deep blue, approaching to black; its fracture is smooth and has a resinous appearance; it is the prussiate of alumine by the oxide of iron.

Recapitulation.

It results from all that has been here said :

1. That the prussic calcination contains but two of the elements of prussure, azote and carbon; and that the third element necessary to the composition of the acid, is the result of a combination formed after the calcination.

2. That

Berthollet first asserted that prussic gas contains no oxygen.

The theory is satisfactory and the results beneficial.

The Prussian blue greatly improved.

Prussiate of alumine.

Recapitulation.
1. Prussic calcination gives only azote and carbon along with the alkali.

2. which is a new compound,

2. That the carbonated azote of potash is a new compound in chemistry, from our acquaintance with which we may succeed in decompositions which have hitherto been attempted without success.

3. capable of instantly decomposing water.

3. That the property which carbonated azote of potash has of instantly decomposing water produces the prussic acid, and that it is the potash particularly which assists the oxigation of the carbon and the combination of the hydrogen with the azote and the carbon.

4. proposed nomenclature of its compounds,

4. That we must now call the supposed prussic acid by the name of prussire, because its properties are analogous to those of all the radicals. Its combinations without oxygen will be called prussiurets, and those which are oxigated, prussiates; with respect to the latter, when they are not directly combined with a metallic substance, it will be necessary to mention along with the direct base, the metallic oxide which may have given them the properties common to the acids. For example, the combination of prussire with potash will be called prussiuret of potash; with oxide of iron, prussiate of iron, and with potash and oxide of iron, prussiate of potash by oxide of iron. With respect to those metallic prussiates which are capable of passing successively into different degrees of oxigation they may be described by their colour, which will sufficiently indicate the state of the prussiate.

5. water decomposes prussiuret and occasions loss,

5. That the continued action of water upon the prussiuret of potash is able to change it completely into carbonic acid and ammonia. This must shew that the hydrogen in the prussire is not combined with the azote in the proportion necessary to the composition of ammonia, and that it must be by the loss of hydrogen in the water that the prussire passes successively into the ammonial state.

6. Which may be prevented by using sulphate of iron,

6. That the decomposition of part of the prussire may be prevented, by never moistening a prussic calcination, but with a solution of sulphate of iron at the maximum of oxigation.

7. the prussic acid gas is a mere radical, which acquires its acid properties by oxygen from an oxide.

7. Lastly: What is called prussic acid gas, is not an acid, but only the prussic radical, and that it acquires the acid or neutralizing property only by means of the oxygen which is imparted to it by a metallic oxide, of which the concurrence is necessary to form with the saline basis a strong and durable compound.

XI.

Description of the Subterranean Graphometer invented by M. KOMARZEWSKI, F. R. S. and presented to the National Institute of France.*

THIS instrument is a simple construction of the common theodolite, and is calculated to answer the author's intention in operations like those of mining, where great accuracy is not required.

Description of a
subterranean
graphometer.

The roughest kind of surveys are made by taking angles with the compass, and these are still used in mines, and in the districts of unsettled countries. Our author has taken the next process, and obtains considerably more accuracy, without deviating from simplicity and cheapness.

This subterranean graphometer is composed of three principal parts, *viz.* a circular horizontal plate, a circular vertical plate, and a pedestal, which are respectively represented in perspective, (Figs. 2, 3 and 5, Plate XV.)

The horizontal Plate A. Fig. 2. is divided into 360° doubly numbered, the subdivisions are carried to $15'$, this is considered sufficient for mines where there is seldom light enough to observe a more minute division: for operations on the surface of the earth a Vernier's scale may be added.

Within the line of divisions above mentioned, is that of the magnetic compass: which gives the reduction of hours into degrees, and that of degrees into hours.

This plate measures the horizontal angles which the miners call the *direction*.

Through its centre passes the cylinder *a*, on which turns the hollow cylinder, which forms the centre of the horizontal index *bb*, and carries the vertical plate B, as represented in Fig. 1.

The horizontal index is one inch in breadth, and is increased at the centre by two circular segments *cc* cast with it, in order to give the vertical plate a more firm bearing on the horizontal plate at the top of the cylinder *a*, a screw *a* is inserted to confine the cylinder.

* From a folio publication in English, entitled, *Memoir on a Subterranean Graphometer, &c.* published at Paris by Pougens, 1803.

Description of a
subterraneous
graphometer.

The hook attached to one extremity of the horizontal index is intended for the suspension of a plumb line.

The vertical plate B, Fig. 3, is only six inches in diameter, in order that the extremities of the horizontal index may be left perfectly clear.

Two indexes *dd*, are moveable round its centre *c*, which measure the angles of inclination. They are somewhat narrower than the horizontal index. Their extremities are likewise furnished with threads to mark the quantity of the angles to the extremities of these indexes, and in the direction of their axes are fixed two hooks in order to receive the cords which put the indexes in motion.

The vertical plate is divided on each side by two lines passing through its centre, one of which is horizontal and the other vertical, the one mark the diameter, and the other the radius. The upper part of the plate is likewise divided on each side into twice 90. This division commences at the horizontal diameter, and proceeds to the vertical radius, the lower part which is but a segment of the circle, contains only on each side 30°, this is sufficient for a lode or gallery in mines, more considerable inclinations being measured like the depth of a shaft.

These two plates with their indexes form the graphometer, the other parts belong to the pedestal, on which it may be moved as occasion requires. Its dimensions depend upon the space in which the graphometer is to be employed. The pedestal is represented at Fig. 5, as follows,

A board *EE* about 18 inches long, six inches wide, and one in thickness, receives in a socket made its centre, the brass ball *K*, surmounted by the cylinder *H* cast with the ball, the board is bored towards its extremities, to receive brass screws, by which it is fixed on some wooden bar in the mines.

The ball *K* is covered by a square plate *g*, which is concave and perforated in its centre, and is fixed to the board *E* by the four screws *h*: it is by loosening or tightening them that the graphometer is fixed or levelled.

On the cylinder *H* turns the hollow cylinder *F*, which is cast with the small plate *D*, this hollow cylinder is furnished with a tightening screw *G*, to fix the graphometer when placed upon the plate *D*: the graphometer is fixed to this plate by two
staples

staples *ff* foldered to the horizontal plate A, which enter two corresponding holes in the plate D, and are confined by the bolts *ff* sliding in grooves foldered beneath the same plate. Description of a
subterraneous
graphometer.

In the centre of the plate D as well as in the centre of the inferior part of the ball K, is a female screw *i*, to receive the solid cylinder L, Fig. 4, the use of which will be seen in the solution of the problems. This cylinder is furnished with a moveable collar *m*, having a hook in its centre, to which the other extremity of the cord may be attached.

Fig. 1. is a perspective view of the graphometer, in which A is the horizontal plate, B the vertical plate, *aa* the centre of the vertical plate, &c. *bb* the horizontal index, *dd* the vertical indexes.

From this disposition it results that, when one direction is known, which for the first operation may be determined by the magnetic compass, or in mining still better by a meridian line passing through one of the principal shafts, the instrument may with facility be directed to any given point, and all plans be executed which are necessary in operations of mining, without making use of the magnetic needle, and that the directions and inclinations may be taken at the same time, since in all these operations it is only necessary to observe the angles indicated by the instrument, to measure the length of the line, and to calculate right angled triangles, of which three parts are known.

The pedestal is intended to be placed at the point of the angle sought, and the cords stretched to it from the graphometer, the cord is fixed to the hook of the graphometer by a noose, and its other extremity to the cylinder L of the pedestal, by means of the scale and pin represented at Fig. 6.

XII.

Experiments and Observations on the various Alloys, on the Specific Gravity, and on the comparative Wear of Gold. Abstracted from the Memoir of CHARLES HATCHETT, Esq. F. R. S.*

INTRODUCTION.

Appointment of H. Cavendish Esq. and C. Hatchett Esq. to make experiments on the wear of gold;

THE Lords of the Committee of his Majesty's most honourable Privy Council, appointed by his Majesty, on the 10th of February, 1798, to take into consideration the state of the coins of this kingdom, having among other circumstances remarked the considerable loss which the gold coin appeared to have sustained by wear within certain periods, and being desirous to ascertain whether this loss was occasioned by any defect, either in the quality of the standard gold or in the figure or impression of the coins, were pleased to request that Henry Cavendish, Esq. F. R. S. and the author of the present memoir would examine, by such experiments as should be deemed requisite, whether any of these defects really existed.

Two questions were to be principally decided,

different in composition;

1st. Whether very soft and ductile gold, or gold made as hard as is compatible with the process of coining, suffers the most by wear, under the various circumstances of friction to which coin is subjected in the course of circulation?

or in figure.

2dly. Whether coin with a flat, smooth, and broad surface, wears less than coin which has certain protuberant parts raised above the ground or general level of the pieces?

The research is new.

The great value of the material, had hitherto prevented private individuals from ascertaining these facts by experiment; and, as a public concern; this subject of investigation, although so important to political economy and to science, does not appear to have been noticed by any European government, until the Right Honourable and enlightened Members of the above-mentioned Committee proposed the inquiry, and furnished the requisite means for making the experiments †.

* In the Philosophical Transactions for 1803.

† These experiments were begun in the latter end of 1798, and were completed in April, 1801.

SECTION I.

ON THE VARIOUS ALLOYS OF GOLD.

The wear of coin is subject to be modified by certain physical Plan of experi-
ments. properties, such as ductility and hardness, which vary in degree, according to the chemical effects produced by different metallic substances, when employed in certain proportions as alloys. From these considerations, it appears proper,

First, to examine the effects which the various metals pro- Effects of alloys. duce upon gold, when combined with it in given proportions, beginning with $\frac{1}{2}$, which is the standard proportion of alloy, and in certain cases gradually decreasing to $\frac{1}{4}$ of a grain in the ounce of troy, or $\frac{1}{1920}$ part of the mass.

Secondly, to examine the specific gravity of gold dif- Specific gra-
vities. ferently alloyed, and the causes of certain variations to which it is liable.

And, thirdly, to ascertain the effects of friction variously Wear. modified.

GOLD ALLOYED WITH ARSENIC.

Experiment I.

To 5307 grains of gold in fusion, 453 pure arsenic were Gold and
arsenic. added. Only six grains remained in the ingot, which was brittle.

Experiment II.

To 442 grs. gold were added 19 grs. copper, and on perfect Gold, copper,
and arsenic. fusion, 19 grs. pure arsenic, and the whole quickly poured out. None of the arsenic remained.

Experiment III.

480 grains of gold in a small crucible was inclosed within Gold and arse-
nic in close
vessels. two larger crucibles, well luted together. 480 grains of pure arsenic were included in the outer vessel. Strong and continued heat, (a) raised some arsenic in the white oxide, (b) and also a small portion of the gold, (c) and the gold was brittle, with a coarse gray fracture, an increase of 1,5 grains in weight. (d) It was difficult to expell the arsenic by heat.

Experiment IV.

A plate of gold was suspended within two crucibles ground Gold exposed to
the vapor of
arsenic at a red
heat. and luted together, pure arsenic being placed in the lower.
By

By a full red heat, the arsenic rose and combined with the surface of the plate, producing a very fusible compound, which ran down into the lower vessel. The plate, which had been $\frac{1}{30}$ of an inch thick, was become as thin as paper; but was still pure gold. The fused compound was extremely brittle, and gray within. It contained a very large quantity of arsenic, but how much is not stated.

Experiment V.

Variation of
Exp. 4.

Variation of the preceding experiment. The arsenic was introduced through a hole after the ignition, and the heat not kept up. The surface of the plate was impregnated with arsenic; but the heat not being sufficient to cause it to flow down, it adhered to the plate, and rendered it less flexible.

GOLD ALLOYED WITH ANTIMONY.

Experiment I. to VIII.

Gold and anti-
mony.

Golds in various proportions and of different fineness were combined with pure antimony by heat. Small proportions of the latter, produced a pale, dull, unmetallic colour, and rendered the compound exceedingly brittle. Minute quantities still impaired the metallic brilliancy, and caused brittleness. Less than one quarter of a grain, in the ounce of gold, were sufficient to produce these effects. The vapors of antimony readily combine with gold in open as well as close vessels, and afford the same changes.

GOLD ALLOYED WITH ZINC.

Experiment I. to VI.

Gold and zinc.

By these experiments, it was shewn, that zinc is highly injurious to the ductility of gold; that a portion of it is easily separated from gold by heat; that, when a large quantity of gold is alloyed with the standard proportion of zinc, only part of the latter is speedily volatilized, but, when small quantities are treated, the whole of the zinc becomes separated, and the gold remains pure; that, if zinc is previously combined with copper in the state of brass, it is not so easily separated by heat, when added to melted gold; and, lastly, that gold in fusion absorbs and retains a portion of zinc, when exposed to the latter metal in a volatilized state, even in open vessels.

GOLD ALLOYED WITH COBALT.

Experiment I. to IV.

This addition debased the colour of the gold, and rendered it brittle. When to an alloy of about 10 parts gold, and 1 part copper, nearly half a quarter part of cobalt were added, the metal began to be ductile. Gold and cobalt.

GOLD ALLOYED WITH NICKEL.

Experiment I. to IV.

The colour and ductility of gold were injured by nickel, but less than by any other of the brittle metals. When to about eleven parts of gold, and one of copper, one fourth of a part of nickel was added, the compound was scarcely brittle. Gold and nickel.

GOLD ALLOYED WITH MANGANESE.

Experiment I. to III.

The black oxide of manganese strongly heated with gold did not affect it. But when olive oil had been previously burned on the oxide, this powder strongly and permanently heated with gold, rendered it pale and brittle. This alloy is less fusible than pure gold; it does not change by long exposure to the air; and the manganese is defended from the usual action of acids. It may be separated by cupellation, or better by quartation and nitric acid. Gold and manganese.

GOLD ALLOYED WITH BISMUTH.

Experiment I. to VI.

Bismuth added to pure, or to alloyed gold, is exceedingly injurious to its colour and ductility. It produces these effects when the quantity is no more than $\frac{1}{2000}$ part of the mass; and the combination takes place by the vapor of bismuth, in open as well as close vessels. Gold and bismuth.

GOLD ALLOYED WITH LEAD.

Experiment I. to IX.

Lead is nearly as injurious to gold as bismuth; one quarter of a grain in the ounce producing complete brittleness. Its vapor in close vessels greatly contaminates gold, but in open vessels the effect was inconsiderable. Gold and lead.

GOLD ALLOYED WITH TIN.

Experiment I. to IV.

Gold and tin. A considerable portion, as $\frac{1}{10}$ of tin rendered gold paler and less ductile: small quantities as $\frac{1}{30}$ did not material injure it. Mr. Bingley, in consequence of Tillet's experiments, related in the Acad. Par. 1790, and in the Quarto Series of our Journal II. 140, 179, made experiments on an alloy of gold and tin, containing eight grains in the ounce of the latter metal. The facts are, 1. the earlier chemists, probably as Mr. Hatchett observes, misled by a tin containing bismuth, lead, antimony or zinc, did affirm that the smallest portion of that metal renders gold brittle; 2. Mr. Alchorne shewed the fallacy of that general position, by experiments on such alloys. 3. Mr. Tillet shewed that tho' gold alloyed with tin, may be hammered and laminated cold, yet it falls to pieces by the annealing heat, or cherry red; and lastly, Mr. Bingley finds that an annealing by five degrees of Wedgwood's pyrometer, or red visible by day light, does not render the gold unfit for working; but that the cherry red or ten degrees blisters the surface, warps the bar, and causes it to fall in pieces, by the fusion of the most fusible metal; a general property on which the process of eliquation depends.

GOLD ALLOYED WITH IRON.

Experiment. I. to IV.

Gold and iron. Gold, with and without copper, was alloyed with about $\frac{1}{11}$ part of iron, and in other experiments of cast iron, cast steel, and iron wire, gave a metal of a pale yellowish gray, approaching to a dull white, which was very ductile and laminable, and was stamped with great ease without annealing; though it was harder than gold.

EXPERIMENTS ON EMERY AND GOLD.

Emery and gold:

Gold was heated strongly, and for a long time, with emery in fine powder, and in some trials previously heated with oil. No change was produced.

GOLD ALLOYED WITH PLATINA.

Gold and platina.

Fine gold, and also standard gold, were alloyed with platina. The compound was yellowish-white, and very ductile.

It was not thought necessary to extend the experiments, because these metals have been much examined by other chemists*.

GOLD ALLOYED WITH COPPER.

From many experiments it appears, that the varieties of copper, in commerce, although similar in aspect, and other obvious properties, are far from being uniform in quality; so that many of them are by no means sufficiently pure to be employed as an alloy for gold; but render it brittle.

Gold and copper. Many kinds of copper render gold brittle.

And the different effects produced by the moulds of iron and those of sand, are such as fully prove, that copper which is not perfectly pure, and which has a tendency to render gold brittle, acts more powerfully, in this respect, when the alloyed mass is cast in sand than when it is cast in iron; and, all things being considered, there is reason to conclude, that moulds of iron are much to be preferred to those of sand †.

The ores of antimony and of lead frequently accompany those of copper; and it has already been proved, that $\frac{1}{1920}$ of either of the former metals is sufficient to destroy the ductility of gold. It may therefore be suspected, that the brittle quality which certain kinds of copper communicate to gold, proceeds from those metals; for, though other metallic substances produce the same effect, yet, as the former especially are so commonly present with the ores of copper, it is highly probable that antimony, or lead, may remain combined with the smelted copper, in a proportion too small to affect the general and more obvious properties of that metal, yet still sufficient to destroy the ductility of gold, when such copper is employed as an alloy.

— most probably from antimony or lead.

To ascertain how far copper might be alloyed with lead, or antimony, without any very apparent change in its obvious properties, the following experiments were made:

Copper may contain a notable portion of lead or antimony, and be very merchantable.

* Gold made standard by platina, is not only very ductile, but also (when hammered) tolerably elastic. perhaps it might be advantageously employed for the springs of watches, &c. C. H.

† Bars of alloyed gold (particularly those which are alloyed with copper) are generally discoloured on the surface, when cast in moulds of sand; but not so when cast in iron. It may be suspected, that the alloy is superficially oxidized when sand is employed, in consequence of the air which is lodged in the interstices, together, perhaps, with some degree of moisture. C. H.

To 476 grains of fine malleable copper, in fusion, four grains of antimony were added, and, being well mixed, the whole was poured into a mould.

The colour of this copper, when filed and polished, was such as not easily to be distinguished from that which had not been thus alloyed.

It was also hammered and rolled, without shewing any signs of brittleness. The specific gravity was 8,354*.

The like quantity of copper was alloyed with four grains of lead.

This also was ductile, and did not suffer any apparent change of colour.

The specific gravity was 8,472.

The same experiment was repeated with four grains of bismuth; but the copper thus alloyed was exceedingly spongy and brittle.

It appears, therefore, that four grains of antimony, or of lead, may be present in one ounce, or 480 grains of copper, without producing any very apparent change in colour or ductility, and but little in specific gravity; such copper may, therefore, without suspicion, be occasionally employed to alloy gold; then, however, the antimony or lead will produce a powerful effect; for it has been proved, that $\frac{1}{1920}$ of either of these will destroy the ductility of gold. But, supposing one ounce troy of copper which contains four grains of antimony, or of lead, to be employed to alloy eleven ounces of gold, 24 carats fine, there would then be four grains of the above-mentioned metals in the 12 ounces or troy pound; and therefore the quantity of these would be considerably more than is required to destroy the ductility of gold. For the troy pound contains 5760 grains; and 4 is to 5760 as 1 to 1440; consequently, this proportion much exceeds the quantity which is capable of producing the above-mentioned effect.

It often contains a very considerable proportion.

But the copper of commerce often contains a much greater proportion of one or other of these metals; and, although it then appears more pale than common, yet it has, without suspicion, been purchased by those who, from their profession, are supposed to be competent judges, and who especially

* The finest Swedish copper was employed in these experiments; the specific gravity of it was 8,895.

require copper to be as pure as possible. Persons of this description, however, are liable to be deceived; for, in 1791, Mr. Roitier, Director of the Mint at Paris, purchased a quantity of copper from the mines of Poullaoen in Britany; but he soon discovered, from the effects which it produced, when employed as an alloy, that it was not pure, and therefore requested Mr. Sage to examine it. By the latter, it was analysed, and was found to contain one forty-eighth of antimony*.

Allowing, therefore, that other metallic substances may at times be present in copper, and may contribute to affect gold which is alloyed with it, yet, for the reasons above related, the author is inclined to attribute, most frequently, this effect to antimony or lead.

Copper which is pure, is uniform in its effects, and does not injure the ductility of gold; it would therefore be proper in all cases when copper is to be purchased for the purpose of alloying gold, to make a previous trial of it on a small quantity, as this would answer every purpose of a tedious and expensive analysis.

Since the above was written, he made various experiments in the humid way, on the different kinds of copper, which are known in commerce, especially on the following:

- | | | |
|--------|----------------------------------|------------------|
| No. 1. | Finest granulated Swedish copper | sp. grav. 8,895. |
| 2. | — Swedish dollar copper | sp. grav. 8,799. |
| 3. | — sheet British copper | sp. grav. 8,785. |
| 4. | Fine granulated British copper | sp. grav. 8,607. |

480 grains of the first, only afforded a few particles of sulphate of lead, which could not be estimated.

The second contained both lead and antimony, of which the lead was in the largest proportion, as it amounted to nearly one grain of metallic lead, whilst the antimony did not exceed half a grain.

The sheet British copper yielded some lead, with scarcely any antimony; and, on the contrary, the granulated British copper contained antimony with but very little lead. We may therefore conclude, that the varieties of copper known in commerce, are seldom, if ever, absolutely free from lead or antimony; and that the brittle quality, so frequently communicated to gold by an alloy of copper, arises from the presence of one

Antimony is most frequently the noxious addition.

Farther examination of copper in the market.

* *Journal de Physique*, 1792, Tome XL. p. 273.

or both of these metals, which, even in the proportion of $\frac{1}{1000}$ part of the mass, was already proved to be capable of destroying the ductility of gold.

Facts respecting Swedish copper.

Mr. H. lately made some farther inquiries concerning the varieties of Swedish copper, and was informed, that the fine granulated copper is made in this country from the Swedish cake-copper, merely by the ordinary process of granulation; and, as the quality even of this copper has been found variable, the Deputy Master of the Mint has of late employed British copper, which has been refined expressly for the purpose, and seems to answer perfectly well. Respecting the variable and occasional very bad quality of the copper dollars, Mr. Swedenstierna, a learned Swedish gentleman at present in London, has favoured him with some particulars, in a letter, of which the following is an extract*.

Copper dollars.

“ But with regard to dollars, I should be much surpris'd if they had ever been perfectly pure; because, as far as I know, they have always been made of the copper of Fahlun, of which the ores have always been more or less mixed with sulphurated lead, and perhaps antimony. However, as these dollars were originally struck under the Reign of *Frederic* king of Sweden, and it is possible that the ores at Fahlun might have been then purer, it is probable that these dollars may have been purer at first than those which have since been counterfeited; for these dollars having been called for in India, and ordered more particularly in considerable quantity, by the Asiatic Company of Copenhagen, others have been struck since repeatedly, and of all kinds of copper without distinction. This fact, at least, explains the cause of their inequality of composition.”

“ It remains to be shewn whether any of these dollars are perfectly pure. For my part, I am most inclined to think, that the demand in India has arisen from the convenience of form, and that prejudice has given them credit and circulation as money. For several years of late, the Danes, having found this copper too dear, have themselves counterfeited the mark, and struck them at Rocraas in Norway; and this must afford another variety of this copper.”

* The Extract is in French, of which I give the translation. *W. N.*

GOLD ALLOYED WITH SILVER.

Gold alloyed with pure silver, in standard proportion, is so generally known, that it would be needless here to say more, than that it approaches the nearest to the ductility of fine gold, and that the specific gravity of this mixture differs but very little from that which, according to calculation, would result from the relative proportions of the two metals.

From the foregoing experiments it is evident, that many of the metallic substances with which gold may be alloyed, are more or less liable to be separated from it during fusion, in consequence of their relative affinities with caloric, with oxygen, or with both; and that these affinities become modified, by those which prevail between the various metallic substances and gold. Moreover, it is evident, that even the most oxidable metals have this property much diminished or checked by being united with gold, which appears so to envelope and retain their particles, as to impede the usual influence of heat, as well as the natural exertion of their affinities with the oxygen of the atmosphere. The following experiment was therefore made, to ascertain the comparative loss caused by the volatilization, or by the oxidizement of various metallic substances, when added to gold during a given period of fusion, and under similar circumstances.

Experiment.

Ten four-inch crucibles, which had been previously made red-hot, were put into as many twelve-inch crucibles, which were placed in wind furnaces of similar construction, and heated as equally as possible. Each of the small crucibles contained five ounces ten pennyweights and fourteen grains of gold, 23 car. $3\frac{1}{2}$ grs. fine, which being completely melted, nine pennyweights and ten grains of the several metals were added, and mixed in the usual manner, after which, the fusion was continued in the open vessels during one hour.

The different masses, when cold, were weighed; but, previous to this, the scoria or glass which had been formed on some of them was gently removed.

It appears, that fine gold, gold alloyed with silver, gold alloyed with copper, and gold alloyed with tin, did not suffer any loss during the experiment.

More-

Gold and silver.

General remarks
on alloys of gold.

Experiment on
the loss by heat-
ing the differ-
ent alloys.

Results of loss
by strong heat.

Moreover, that gold alloyed with lead only lost three grains, chiefly by vitrification.

That gold alloyed with iron lost 12 grains, which formed scoria.

That gold alloyed with bismuth also lost 12 grains chiefly by vitrification.

That gold alloyed with antimony lost the same quantity, partly by volatilization, and partly by vitrification.

That gold alloyed with zinc lost one pennyweight, by volatilization. And,

That gold alloyed with arsenic, not only lost the whole quantity of alloy, but also two grains of the gold, which were carried off in consequence of the rapid volatilization of the arsenic.

Lewis, (Phil. Comm. of Arts, p. 88,) however, asserts that "gold is more volatilized by antimony than by arsenic or zinc; but to produce this effect the fire must be vehement, the crucible shallow, and the air strongly impelled." These circumstances, according to their variations, must undoubtedly very much influence the results of such experiments; and therefore, although the reverse was found to take place in the experiments here stated, it does not follow that certain changes should not be produced by different degrees of heat, by the figure of the vessels, and by a current of air more or less strong.

Silver and copper only are fit for the alloy of gold.

The whole of the experiments of this section tend to prove, that (agreeably to general practice and opinion) only two of the metals are proper for the alloy of gold coin, namely, silver and copper; as all the others either considerably alter the colour, or diminish the ductility of gold. In respect to the latter quality, the different metallic substances which have been employed in the present experiments, appear to affect gold nearly in the following decreasing order.

Order of effect as to ductility.

1. Bismuth. 2. Lead. 3. Antimony. (These are nearly equal in effect.)
4. Arsenic. 5. Zinc. 6. Cobalt. 7. Manganese. 8. Nickel. 9. Tin. 10. Iron. 11. Platina*.
12. Copper. 13. Silver.

* Had the platina been quite pure, the compound metal would probably have possessed more ductility; I cannot therefore take upon me to assert positively, that the place here assigned to platina, is precisely that which it ought to occupy.

SECTION III.*

ON THE COMPARATIVE WEAR OF GOLD, WHEN ALLOYED
BY VARIOUS METALS.

Gold, when in the form of coin, appears to be generally exposed to three varieties of friction, *viz.*

1st. Friction between pieces of gold coin of a similar or of a different quality.

2d. Friction of gold coin against coin of other metals, such as silver and copper.

3d. The friction which gold coins of various qualities suffer, when exposed to the action of certain substances, such as the particles or filings of metals, gritty powders, &c.

The consideration of these different modes of wear, points out the best method to be pursued in an experimental investigation.

The whole of the experiments which compose this section may therefore be divided into three subordinate series; the two first of which have been directed to the consideration of that part of the diminution of the coin which arises from the rubbing of one piece of metal against another; while,

The third of these subordinate series was intended to show the comparative power of gold, differently alloyed, to resist abrasion from sand or other gritty powders.

In the first set of experiments, 28 pieces of coin were fixed to a frame, and over each of them was placed another piece of coin, which was pressed against it by a weight. These upper pieces were all attached to a second frame, so that, by means of the motion communicated thereto by cranks, each upper piece was made to move about $\frac{3}{8}$ of an inch backwards and forwards on the lower one. This mode of experiment afforded an opportunity of trying the comparative diminution of gold differently alloyed, both when rubbed against pieces of the same and of a different alloy; and also of examining the difference of wear between pieces with plain and with stamped faces.

* The facts detailed in this important memoir having extended my abridgement beyond my first estimate, I find myself under the necessity either of transposing the 2d and 3d sections *on the Specific Gravity and the comparative Wear of Gold*, or else of separating Plate XIV. from its description.

In

many pieces in a box. In the second series, 200 pieces of gold, differently alloyed, were inclosed in a wooden box, of a cubic figure, which was kept constantly turning round, till, by the repeated rubbing and striking of the pieces against each other, and against the sides of the box, they were found to be sensibly diminished. This, like the experiments of the first set, was intended to show the comparative diminution of gold differently alloyed; but, whereas that shewed the effect of rubbing only, this shewed the joint effect of rubbing and striking, and was intended to imitate (although in a more violent degree) the effect produced upon coin by pouring it out of one bag or drawer into another.

Coin rubbed on a flat surface. The experiments of the third set were made by pressing the pieces to be examined against the rim of a flat horizontal wheel, by means of equal weights, so that, by turning the wheel round, they all suffered an equal degree of friction. That part of the wheel against which the pieces rubbed, was sprinkled or coated with some kind of powder, which was occasionally varied.

The above statement will convey a general idea of the manner of making the experiments; but, that the whole may be more fully comprehended, the following description of the instruments has been added by Mr. Cavendish.*

DESCRIPTION OF THE INSTRUMENTS.

Description of machinery for determining the effects of friction on coin. In the first series of experiments, 28 pieces of coin were fixed to a frame, and over each of them was placed another piece, which was pressed against it by a weight; and these upper pieces were all connected to a second frame, so that, in consequence of the motion communicated thereto by cranks, each upper piece was rubbed backwards and forwards upon that which was under it.

Fig. 1, (Plate XIV.) represents a plan of this instrument; and Fig. 2 is a vertical section of it, drawn parallel to the line AB.

The upper frame, or that to which the upper pieces of coin are connected, is of brass, and consists of four bars, Fig. 1, AB, Bb, ba, and aA, with three cross bars Cc, Cc, Cc.

* The instruments were made by Mr. Cuthbertson, of Poland-street, who also had the care of them during the experiments which were made at his house.

The

The lower frame consists of a board, placed immediately under the upper frame, and is expressed in Fig. 2, by the letters LL.

Description of machinery for determining the effects of friction on coin.

The upper frame is supported by two vertical boards, extending the whole length of the sides *Bb* and *Aa*, so that the ends of them are seen in Fig. 2, and are denoted by the letters DD, DD. These boards are fastened to the upper frame, and to the table upon which the apparatus stands, by hinges, so that the upper frame can move freely in the direction BA, but can have no motion in the direction perpendicular thereto. These vertical boards are omitted in Fig. 1; for, as the intention of this description is not to give a detail of all the parts of the instruments, but only to explain their manner of acting, I have taken the liberty to omit such parts as tended to produce an intricacy in the figures, without being necessary to this object.

The disposition of the pieces of coin on the frames, is represented in Fig. 1. *Nmn* denote one of the connecting pieces, by which the upper pieces of coin are connected to the upper frame, and in which the small circle represents the position of the coin; the large circle is the part which supports the weight, and *m* the part by which it is connected to the upper frame.

To avoid confusion, neither these connecting pieces nor the pieces of coin are represented in Fig. 2; but, instead thereof, a section of one of these pieces is given in Fig. 3, upon a larger scale.

In this figure, LL is the lower frame, and C one of the bars of the upper frame; *y* is one of the lower pieces of coin, which is bedded and fixed firmly in a brass socket *x*, fastened to the lower frame; *u* is the piece of coin to be rubbed against it, which, in like manner, is fixed in another brass socket *w*; *Nn* is the connecting piece, by which this socket is connected to the bar C of the upper frame. This piece turns on pivots, in two studs *n*, fixed to the bar C, so that it can turn freely on those pivots in a vertical direction, but cannot be perceptibly shaken horizontally.

Z is the weight by which this connecting piece is pressed down; it is round, and is placed with its centre exactly over that of the socket *w*.

It must be observed that, in the construction of this machine, three things principally demanded attention.

1st. That

Description of machinery for determining the effects of friction on coin.

1st. That the pieces of coin should all move equally.

2dly. That they should all be pressed against the lower pieces by the same weight. And,

3dly. That they should bear flat against them.

As to the first requisite, it is evident that the pieces must all move alike, excepting so far as proceeded from the springing of the parts of the machine, or from the shake in its joints, both of which were very small.

Secondly, as the connecting pieces move freely in a vertical direction, it is clear that the force with which the upper piece of coin is pressed against the lower one, depends only on its own weight, on that of the socket w , on that of the connecting piece Nn , and on the weight Z by which it is loaded; so that the second requisite is thus easily obtained.

Thirdly, The connecting piece Nn bears against the socket w only by the pin p , which enters into a hole in the centre of the socket, so that the two pieces must necessarily bear flat against each other; but, as this pin alone would not have prevented the socket from turning round on its centre, two other pins $\Pi \Pi$ were fixed into the connecting piece, and entered into flits made in the socket near its circumference, allowing no more shake than was necessary to prevent it from sticking; and thus the motion round the centre was effectually prevented.

It may be observed, that the pieces might have been made to bear flat against each other by fixing the sockets w in gimbals; but, as the method above described was effectual, and much easier made, it was preferred.

It may be also remarked, that the breadth of the bars Cc , as represented in Fig. 1, is not sufficient to prevent them from springing considerably; for this reason, a method of strengthening them was employed, which answered the purpose perfectly well, but is omitted in the drawing, as it could not be easily represented.

It was at first intended, that the lower frame should have remained fixed, and that only the upper one should have moved; but, in a previous trial, in which two pieces of metal were rubbed backwards and forwards upon each other in the same line, with a view to discover what weight would be necessary to make the pieces wear tolerably fast, it was found that for a time they diminished slowly, but that little furrows or gullies were soon worn in them, and that then the diminution was rapid

rapid. It was also observed, that the gullies in the upper pieces corresponded to those in the lower ones; so that it was impossible that the pieces of metal should touch each other in those places where the diminution was most rapid, and consequently the gullies must have been formed by the particles of metal which had been abraded, and which subsequently had become accumulated.

Description of
machinery for
determining the
effects of fric-
tion on coin.

It seemed to Mr. C. that the most probable way to prevent the little furrows or gullies from being thus formed, would be, to construct the instrument in such a manner, that the direction in which the pieces rubbed upon each other should continually vary. The following contrivance was therefore adopted, by which the pieces were prevented from rubbing together twice in the same direction.

In this method, the lower frame, as well as the upper, is supported on two moveable vertical boards; but, whereas the boards supporting the upper frame are placed parallel to *Bb*, in consequence of which the frame can move only in the direction *BA*, these are placed parallel to *BA*, so that the frame can move only in the direction *Bb*.

EE is the axis by which the upper frame is moved: this turns in fixed sockets at *SS*, and is turned at each end into the form of an eccentric circle, which acts as a crank; so that, by means of the levers *EK*, which at one end turn on these eccentric circles, and at the other end turn on joints fixed to the upper frame, this frame is made to move $\frac{1}{4}$ of an inch, in the direction *BA*, during one half of the revolution of the axis, and as much in the contrary direction, during the other half revolution.

ee is an axis of the same kind, serving to move the lower frame. *HH* is a windlass, which turns these two axes by means of the toothed wheels *F, f*, which work in the toothed wheels *G, g*, fastened to the axes *EE* and *ee*. *TTTTT* is the table upon which the apparatus stands.

The wheel *F* has 90 teeth, *f* has 75, and *G, g*, have each 20; so that the axis *EE* makes six revolutions while *ee* makes five; and, at a medium, these axes make about four revolutions to one of the windlasses. A counter is placed so as to show the number of revolutions of the windlasses.

If the two frames had performed their vibrations in the same time, no advantage would have been gained, for the pieces of
coin

Description of machinery for determining the effects of friction on coin.

coin would still have moved upon each other always in the same line; but, as their vibrations are performed in different times, Mr. C. shews, that the effect must be quite different.

This contrivance, therefore, effectually prevented the pieces from moving upon each other always in the same line; and it seems also to have much diminished the disposition which they had to wear in gullies, but not intirely; for, from the following experiments it appears, that still some few particles would become occasionally collected, and then acted as a grinding powder, which accelerated the wear of the pieces. This was observed particularly to happen to the pieces of gold alloyed with an equal proportion of copper, and to the pieces of copper, which were also more frequently worn in furrows or gullies, than the other pieces of more ductile metal.

The motion of the pieces of coin upon each other, is greater than it would have been if only one frame had been made to move, nearly in the proportion of 3 to 2; so that the whole motion of the pieces, in each semi-revolution of the axes *EE* or *ee*, is about $\frac{3}{8}$ of an inch, and therefore it is about three inches in each revolution of the windlafs.

The instrument employed in the second series of experiments, is so simple as not to require any drawing. It consisted only of a cubical box of oak, which measured eight inches each way, within side. This box was moved by the axis *EE* of the former instrument, which was passed through the middle of two opposite sides, and was fixed in that position.

Fig. 4, represents a plan of the instrument used in the third series of experiments. *aaa* is a horizontal table, turning upon a vertical axis; and *BBBbbb* is a fixed frame surrounding it.

The pieces of coin are fastened to this fixed frame, by the same connecting pieces which were formerly employed, and are pressed down also by similar weights. The diameter of that part of the wheel against which the centres of the pieces of coin are pressed, is 29 inches; so that, while this wheel makes one revolution, the pieces are rubbed against it through the whole circumference of this circle, that is, through $91\frac{1}{8}$ inches.

A shallow groove *ggg* is cut in this wheel, in that part against which the pieces are pressed, in order to confine the powders employed in the experiments; and the number of revolutions of the wheel are marked by a counter.

By the help of the instruments above described, it was proposed to determine, as accurately as possible,

Description of machinery for determining the effects of friction on coin.

1st. The comparative wear of soft and of hard gold.

2dly. Whether coins with flat or with raised surfaces suffer the greatest loss by friction, when subjected to it under similar circumstances*.

It is scarcely necessary to observe, that rigorously exact results could not be expected in all the minutiae of experiments like the present; for, many circumstances, apparently but trivial, produced almost unsurmountable obstacles; but, nevertheless, these did not impede the essential objects from being investigated, and determined, in a manner sufficiently satisfactory.

Before the experiments are described, it will be proper to add, that, to obviate the irregular effects which would be produced by the inequality of the impressions usually employed for coins, Mr. Cavendish suggested a die, which was executed by Mr. Pingo, and which consisted of round prominencies regularly disposed over the surface, so that the effects which this impression produced, during friction, were uniformly the same in every direction.

The first experiments were intended to ascertain the different wear of gold made standard by various metals; and the pieces were rubbed against each other by means of the first-described apparatus, which the author calls No. 1.

Some preparatory experiments were also made, to try the effects of this machine, as well as to determine, in some measure, the comparative wear of gold made standard by copper, of a mixture of gold and copper in equal proportions, and, lastly, of copper.

* Although coins with protuberances on their surfaces, have been generally supposed to suffer more by friction than those which are flat, yet, as this opinion has been questioned, and as several objections have been made to it by intelligent persons, it was thought expedient that the decision of the question should form part of the present investigation.

(To be continued.)

XIII.

*Observations of the Transit of Mercury over the Disk of the Sun ; to which is added, an Investigation of the Causes which often prevent the proper Action of Mirrors. By WILLIAM HERSCHEL, LL. D. F. R. S. **

Transit of Mercury.

THE following observations were made with a view to attend particularly to every phenomenon that might occur during the passage of the planet Mercury over the sun's body. My solar apparatus, on account of the numerous observations I have lately been in the habit of making, was in great order for viewing the sun in the highest perfection ; and, very fortunately, the weather proved to be as favourable as I could possibly have wished it.

The time at which the observations were made, not being an object of my investigation, is only to be considered as denoting the order of their succession.

Mercury seen on the sun's disk, perfectly round ;

November 9, 1802. About 40' after seven o'clock in the morning, I directed a telescope, with a glass mirror of seven feet focal length, and 6,3 inches in diameter, to the sun ; and perceived the planet Mercury. It was easily to be distinguished from the openings in the luminous clouds, generally called spots, of which there were more than forty in number. Its perfect roundness would have been sufficient to point it out, had I not already known where to look for it.

10^h 0'. When the sun was come to a sufficient altitude to show objects on its surface with distinctness, I directed my attention to the contour of the mercurial disk, and found its termination perfectly sharp.

* From the Philosophical Transactions for 1803.

(To be continued.)

Mr. John Webb's Gun Lock for preventing Accidents in the use of Fire Arms

Fig. 1.

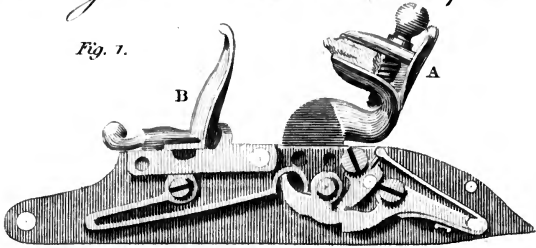


Fig. 2.

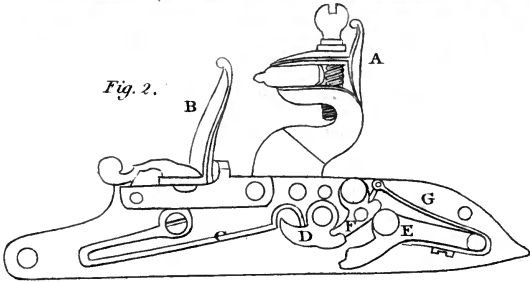


Fig. 3.

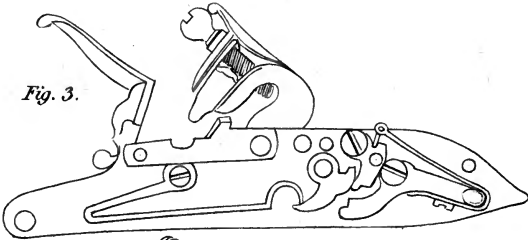


Fig. 4.

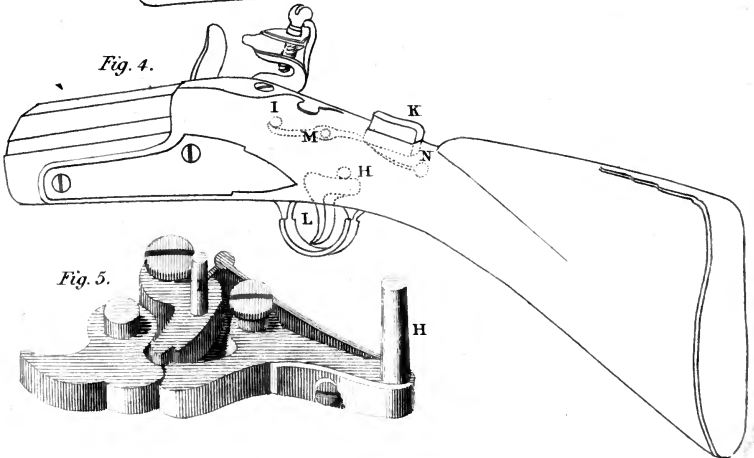
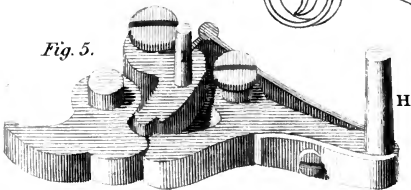


Fig. 5.





Mr. Knight's Apparatus for blasting Wood.

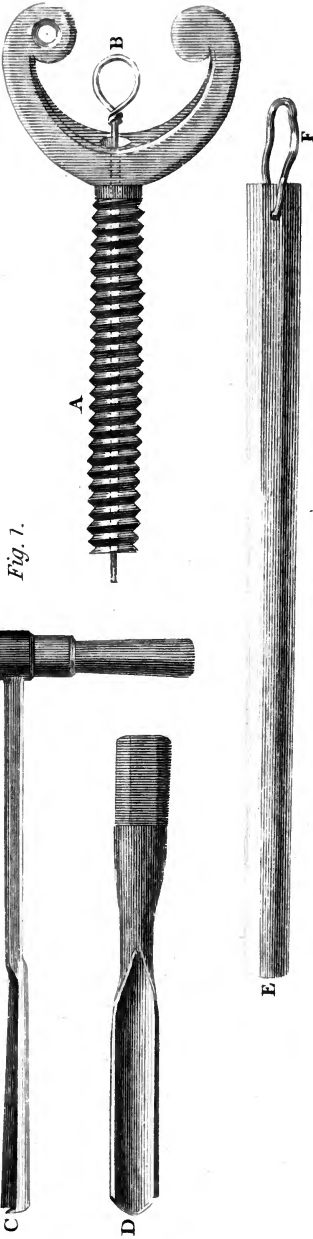


Fig. 1.

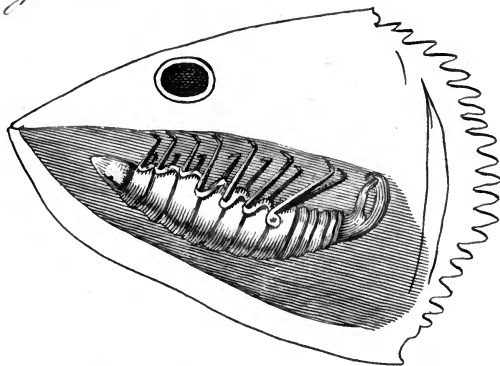
Mr. Cooleston's Peat-Borer.



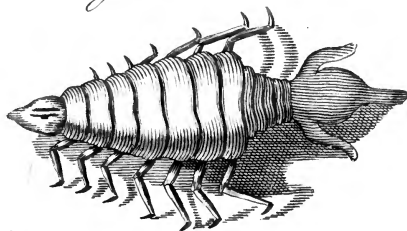
Fig. 2.



The Insect, as it places itself in the mouth of the
Clupea tyrannus.



The *Oniscus praegustator*, drawn to its natural size,
by measurements



Leaches found upon the Insect.

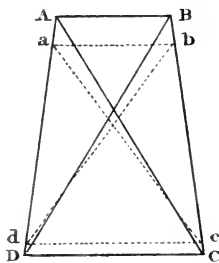
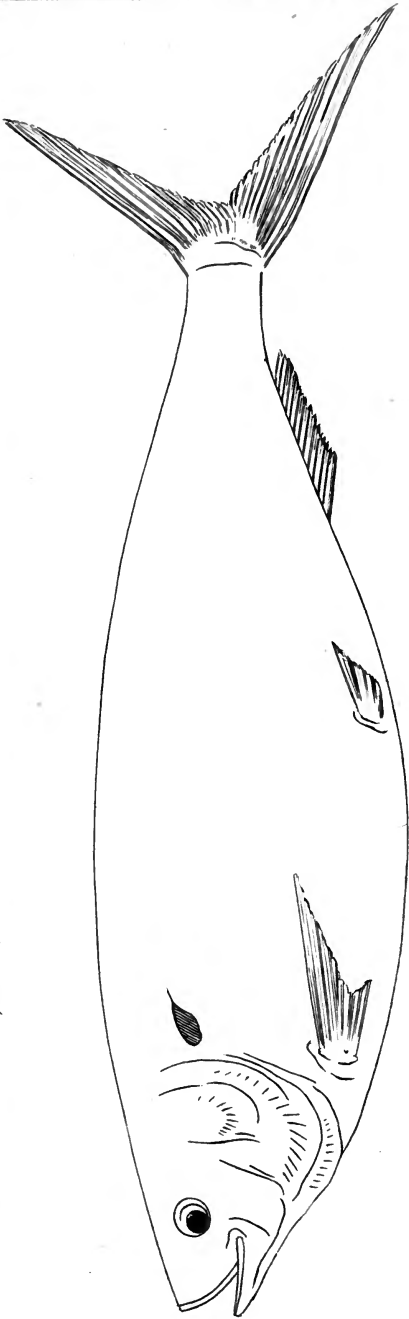


Fig. 3.



Outline of the Clupea byrninus, correctly drawn to two thirds of its natural size.





Dr. Herschel on the Construction of the Heavens

Fig. 1.

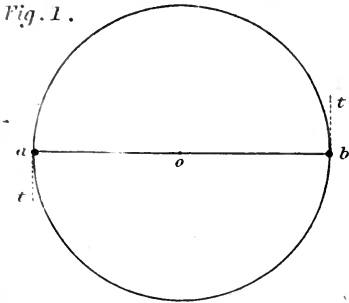


Fig. 2.

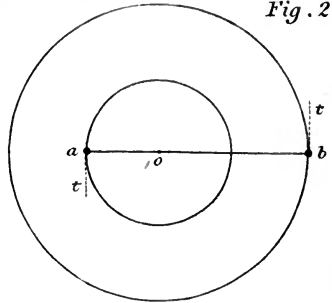


Fig. 3.

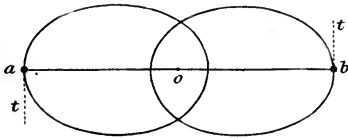


Fig. 4.

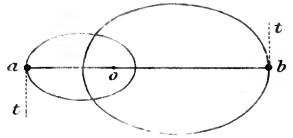


Fig. 5.

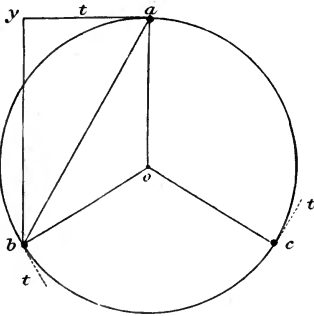


Fig. 6.

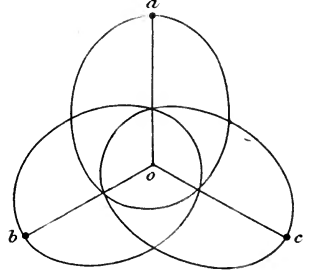


Fig. 7.

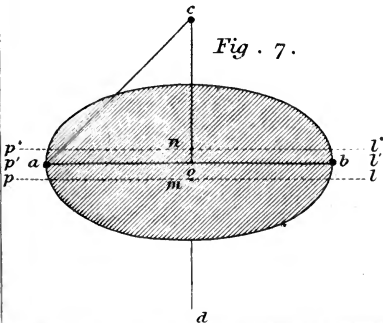


Fig. 8.

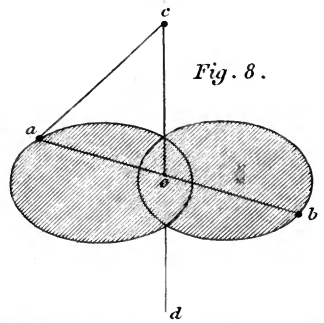




Fig. 9.

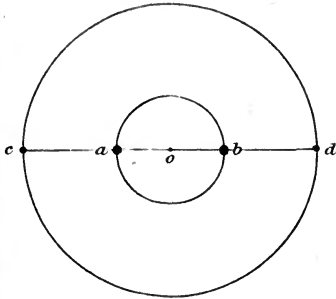


Fig. 10.

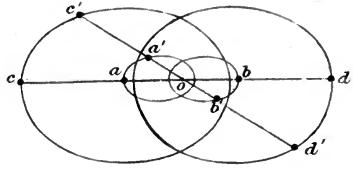


Fig. 12.

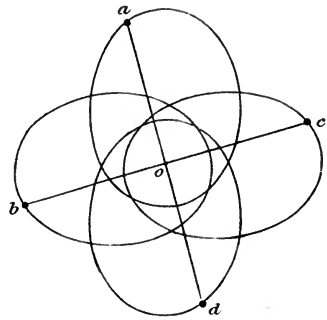


Fig. 11.

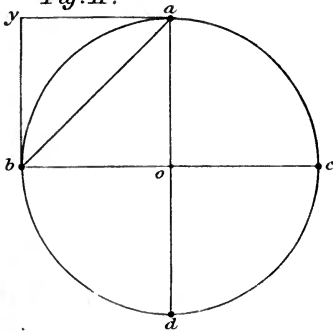


Fig. 14.

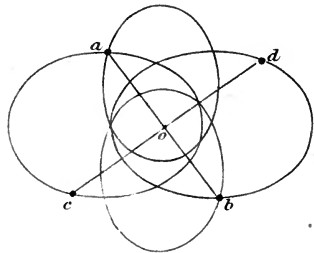


Fig. 13.

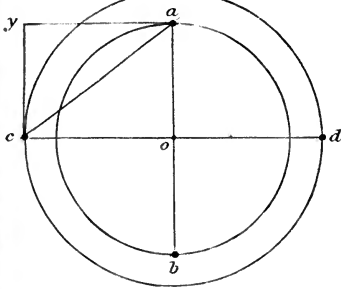


Fig. 16.

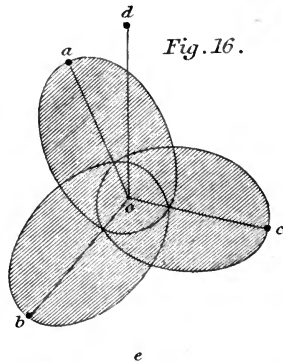
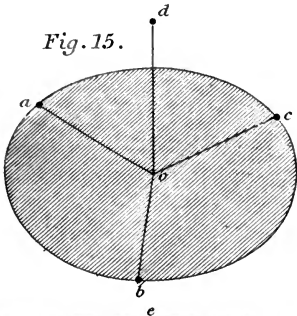
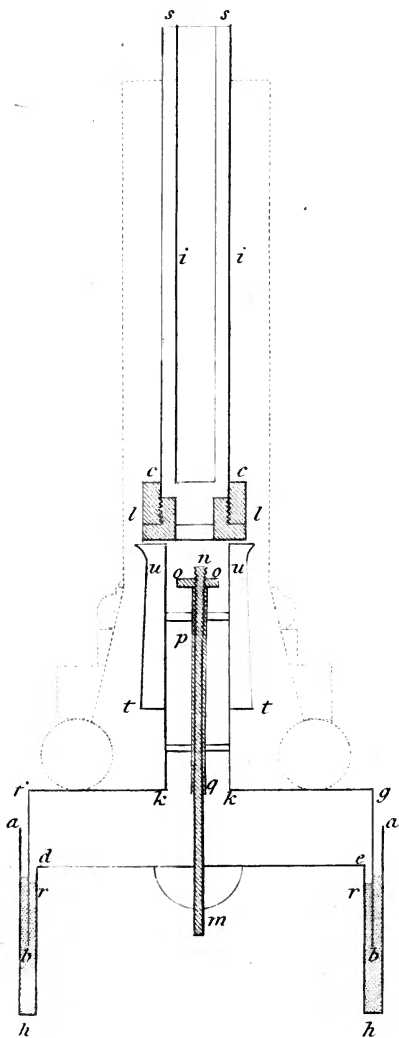


Fig. 15.





New Statical Lamp!
by Sir A. H. Edelcrantz!





Antient Magazine Gun.

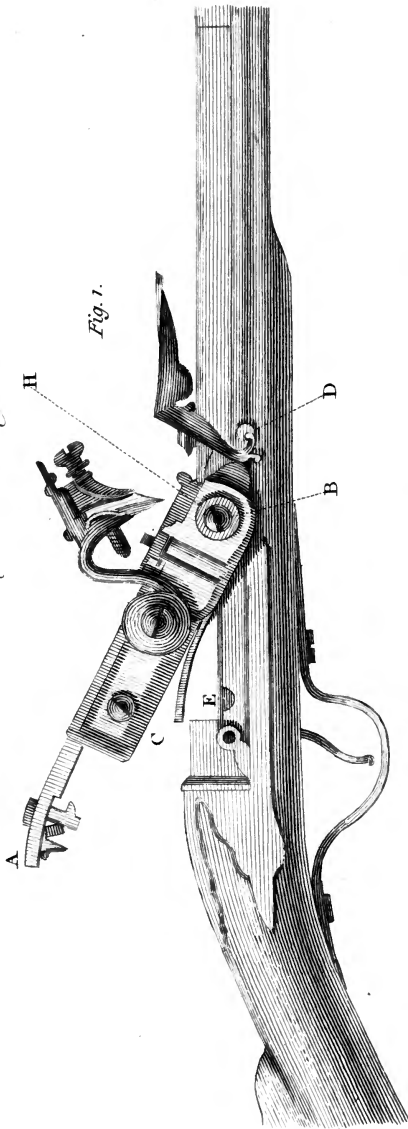


Fig. 1.



Fig. 3.

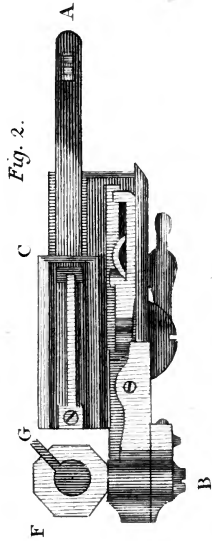


Fig. 2.



Ancient Lock.

Fig. 1 .

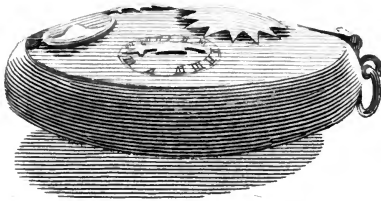


Fig. 2 .

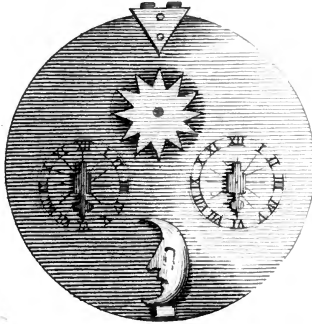
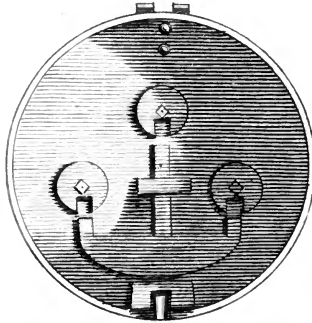


Fig. 3 .





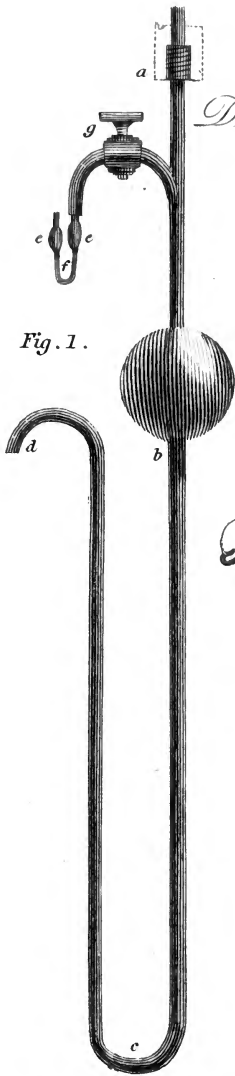


Fig. 1.

Distillation

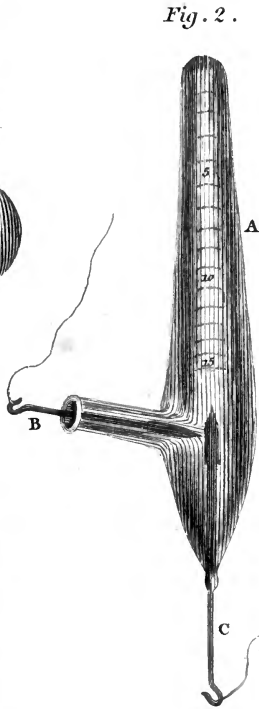
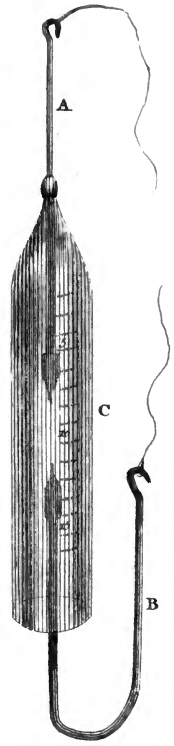


Fig. 2.

Fig. 3.



Galvanic Apparatus

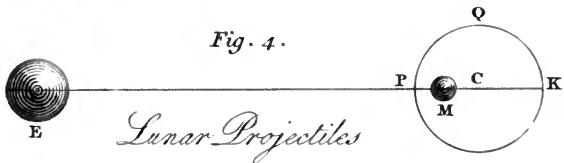


Fig. 4.

Lunar Projectiles

Fal. Battery

Fig. 1.

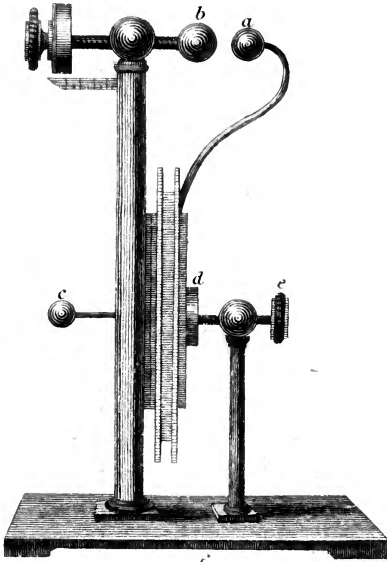


Fig. 2.

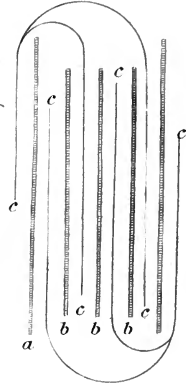
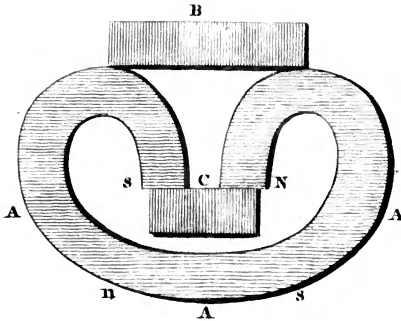


Fig 3



Artificial Magnet



*Telegraph used in Sweden
Invented by Mr. A. V. Edlerantz.*

Fig. 4.

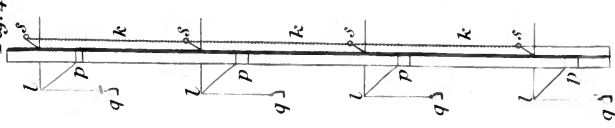


Fig. 1.

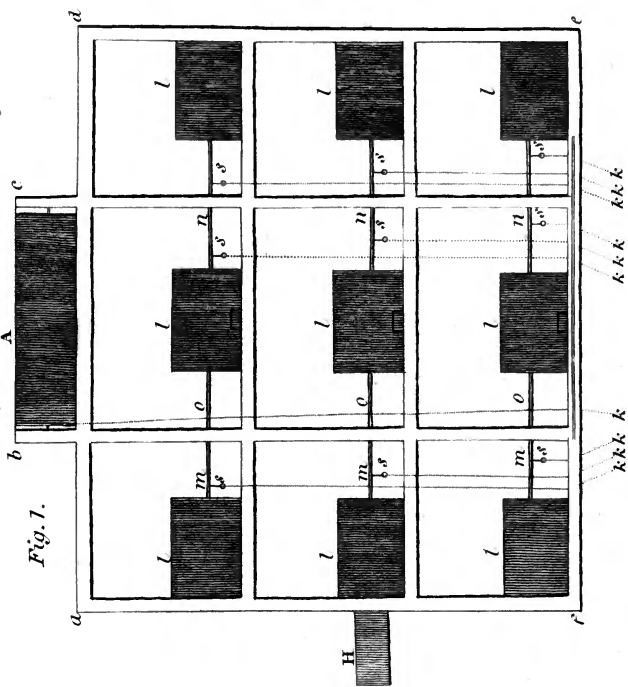


Fig 2

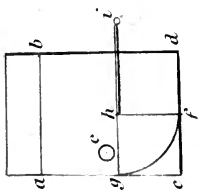
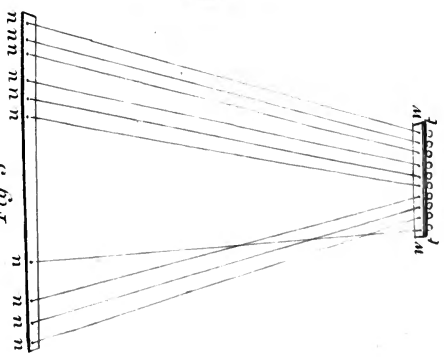
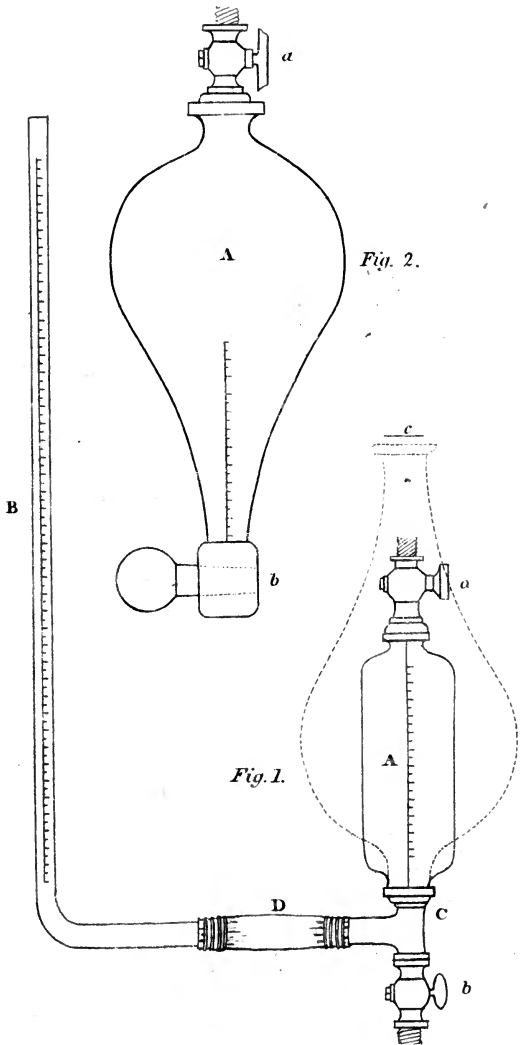


Fig 3





*M^r Henry's Apparatus for measuring the
Absorption of Gases.*





Apparatus for determining the comparative wear of gold, by the Hon. W. M. Carew & Co.

Fig. 1.

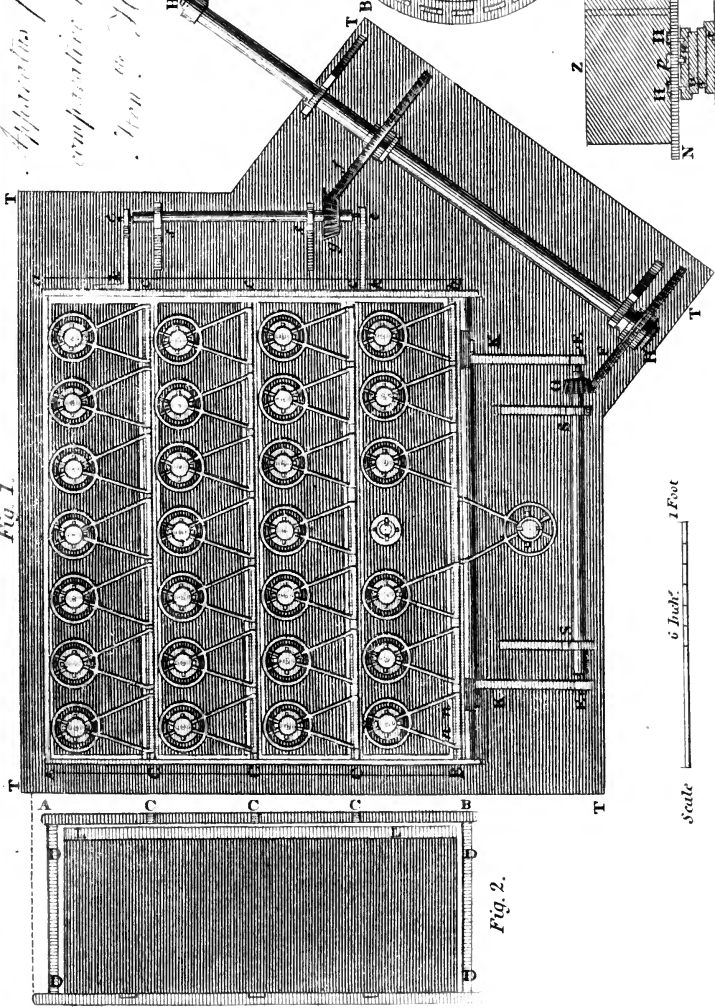


Fig. 2.

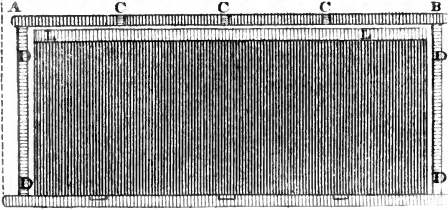


Fig. 3.

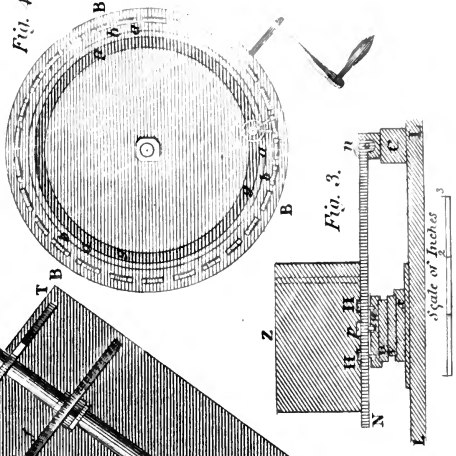
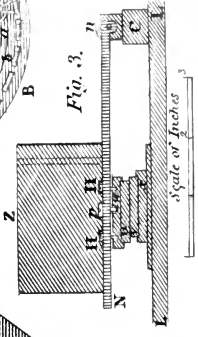


Fig. 4.



Scale



*Subterraneous Graphometer,
by M. Kōmarzewski?*

Fig. 2.

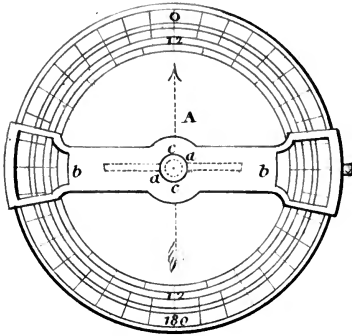


Fig. 1.

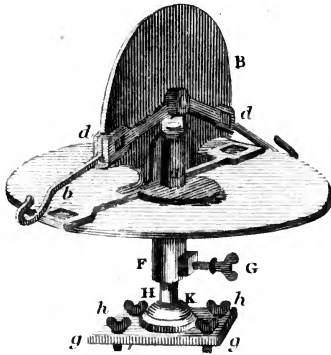
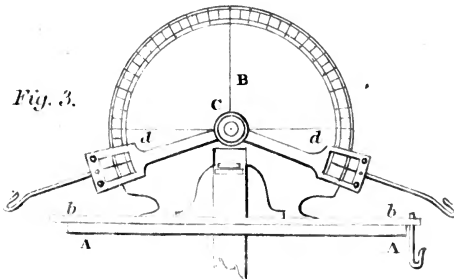


Fig. 3.





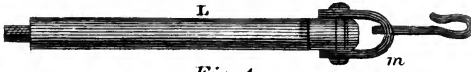


Fig. 4.

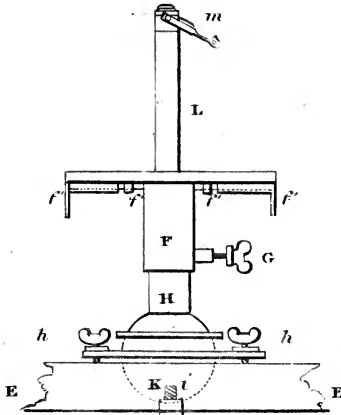


Fig. 5.

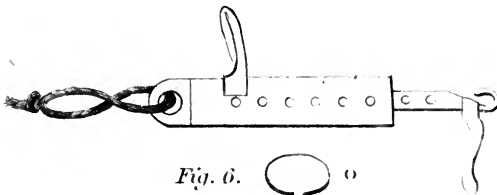


Fig. 6.



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