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ILLUSTRATED WITH ENGRAVINGS.

BY WILLIAM NICHOLSON.

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THE ARTS.

APRIL 1801.

ARTICLE I.

Some Obſervations on different Hydrocarbonates and Combinations of Carbone with Oxygen, &c. in reply to ſome of Dr. Prieſtley's late Objections to the New Syſtem of Chemiſtry. By W. CRUICKSHANK, *Woolwich.*

ON peruſing Dr. Prieſtley's late publication, in which he ſupports the doctrine of phlogiſton, I was much ſtruck with ſome of the experiments there related, more eſpecially with the quantity and nature of the gaſes obtained from the grey oxide of iron and charcoal, and the ſame oxide with the carbonate of barytes.

Although I had not the leaſt doubt of the accuracy of theſe experiments, yet I was determined to repeat, and extend them, and to pay particular attention to the nature of the different gaſes obtained.

Accordingly a quantity of the grey oxide, or ſcales of iron ſeparated in forging, were introduced into a crucible, and expoſed to a red heat for upwards of half an hour; at the ſame time a quantity of powdered charcoal was introduced into another crucible, which had a cover luted on, and likewiſe expoſed to a ſtrong red heat; theſe were mixed while hot, and introduced into a ſmall coated glaſs retort. The retort was then placed in a furnace, its neck being plunged under water, in a pneumato-chemical tub, and heat gradually applied.

By the time that the retort became red, or rather ſooner, gas was produced in great abundance, and continued to come over for two hours or upwards; this gas was received in different veſſels, ſo that the portions obtained at different periods of the proceſs might be examined ſeparately: the whole amounted to 150 oz. meaſures (troy.)

The firſt portion conſiſted of carbonic acid gas, one part, and inflammable gas four parts.

The ſecond portion of carbonic acid gas, one part, and inflammable gas five parts.

The third was exactly ſimilar to the ſecond, and then to the end of the proceſs; it conſiſted pretty uniformly of carbonic acid gas, one part, and inflammable gas, ſix parts. I repeated this experiment, and employed an iron retort, in order that a much greater degree of heat might be applied. In this caſe the gaſes obtained were exactly ſimilar, and the very laſt portion was found to conſiſt of carbonic acid gas one part, and inflammable gas ſix parts; but the quantity in all, although the oxide of iron and charcoal together did not exceed two ounces, was prodigious, and muſt have amounted to many gallons, by meaſure.

Suſpecting that ſome of the other metallic oxides might yield ſimilar reſults, if treated with charcoal in the ſame manner; I expoſed a quantity of the ſublimed oxide of zinc to a red heat for nearly an hour, and then mixed it with charcoal, which had likewiſe been previouſly heated; theſe were introduced into a coated glaſs retort, and ſubjected to a heat gradually raiſed as in the former experiments; before the retort became red, a conſiderable quantity of gas was diſengaged, but upon increaſing the heat it came over in torrents.

The firſt portion conſiſted of carbonic acid gas, and inflammable gas, in the proportion of 1 to 9. The ſecond, of the ſame gaſes, in the proportion of 1 of the former, to 26 of the latter, and what was received afterwards, was purely inflammable; the whole amounted to 90 ounce meaſures. At the end of the proceſs a quantity of metallic zinc was found in the neck of the retort.

I next ſubjected the red oxide of copper and charcoal to a ſimilar proceſs, and the quantity of gas collected from an ounce of this mixture, amounted to 64 ounce meaſures.

The firſt portion conſiſted of the carbonic acid, and inflammable gaſes, in the proportion of 10 of the former to one of the latter. The ſecond of the ſame gaſes, in the proportion of 15 to 55; and the third was purely inflammable gas, without any ſenſible quantity of carbonic acid. At the end of the proceſs the copper was found in ſmall globules in its metallic ſtate, mixed with the charcoal.

The next ſubject of experiment was litharge, which was dried as much as poſſible, mixed with hot charcoal, then introduced into a coated glaſs retort, and expoſed to heat in the ſame manner as the other mixtures.

The firſt portion of gas conſiſted of carbonic acid, and inflammable gas very nearly in equal bulks. The ſecond, of the ſame gaſes in the proportion of 13 of the former to 39 of the latter, and what came over afterwards was almoſt purely inflammable, the whole amounting to about 40 ounces. The lead was found revived and mixed with the charcoal in the form of ſmall globules.

The laſt metallic oxide which I ſubjected to this proceſs, was that of manganeſe; about half an ounce of the black ore of this ſemi-metal was expoſed for an hour to a ſtrong red heat in a crucible, it was then mixed with hot charcoal, and introduced into a coated glaſs retort; heat being applied and gradually increaſed, a quantity of gas, amounting in all to 38 ounces, was procured. The firſt portion was chiefly carbonic acid; the ſecond a mixture of this with inflammable gas, in nearly equal proportions; and the laſt almoſt purely inflammable.

From theſe experiments we may draw the following concluſions: 1ſt. That all metallic oxides capable of enduring a red heat, will, when mixed with charcoal, not only yield carbonic acid, but alſo a very conſiderable quantity of an inflammable gas. 2dly, That thoſe oxides which retain their oxigen moſt obſtinately, yield the greateſt quantity of inflammable gas; and on the contrary, that thoſe which part with it readily, afford the greateſt proportion of carbonic acid. And 3dly, That the carbonic acid gas was chiefly diſengaged at the beginning of the proceſs, and the pureſt and greateſt quantity of inflammable gas towards the concluſion of it.

Having thus aſcertained the relative proportions of theſe gaſes, I next endeavoured to inveſtigate the properties of the inflammable gas, which from the peculiar circumſtances under which it was procured, I had little doubt muſt be different from any of the known hydrocarbonates.

Accordingly I took a quantity of that obtained from the grey oxide of iron, and waſhed it repeatedly with lime water, until the whole of the carbonic acid was completely ſeparated. Its ſpecific gravity was then aſcertained, and it was found to be but a very little lighter than common air, that is to ſay, in the proportion of about 22 to 23, a circumſtance which proved it at once to be different from the common hydrocarbonates, all of which are much lighter than common air.

I next endeavoured to eſtimate the quantity of pure oxygen neceſſary to ſaturate it, and likewiſe the products of the combuſtion. After ſeveral trials I found, that when four meaſures of the waſhed gas, mixed with two of oxygen, were introduced into a ſtrong glaſs jar over mercury, and fired by the electric ſpark, they were reduced to $3\frac{1}{5}$ meaſures, and that when this reſiduum was agitated with lime water, there only remained about $\frac{1}{4}$ of a meaſure, which was found by the nitrous teſt to be pure oxygen; hence it would appear, that eight meaſures of this gas require $3\frac{1}{2}$ meaſures of pure oxygen gas to ſaturate them, and when combined produce nearly ſix meaſures of carbonic acid gas, with a little water. Now if we ſuppoſe a meaſure equal to a cubic inch, and that a cubic inch of the inflammable gas, at the temperature of 55°, and under a mean preſſure of the atmoſphere, weighs .3 of a grain, a cubic inch of oxygen gas, .34 of a grain, and a cubic inch of carbonic acid gas .47 of a grain; then the weight of eight meaſures of the inflammable, with $3\frac{1}{2}$ of oxygen gas, will be 3.6 grains very nearly; but the weight of the carbonic acid gas generated, is only 2.8 grains; hence there remains a deficiency of .8 of a grain to be accounted for, and which muſt ariſe from the water produced. The chief circumſtance, however, which diſtinguiſhes this gas from other inflammable gaſes, is the great proportion of carbonic acid which it yields when united with oxygen. In the above experiment we find ſix meaſures of carbonic acid gas generated; now this quantity would require at leaſt ſeven meaſures of pure oxygen, whereas the quantity employed was only 3½ meaſures; we muſt therefore infer, that the remaining 3½ meaſures muſt have been originally combined with the inflammable gas, conſtituting what may be called a gaſeous oxide. Hence it would appear, that this gas bears the ſame relation to carbonic acid, that nitrous gas does to the nitrous acid. In future, for the ſake of diſtinction, we ſhall call it the gaſeous oxide of carbone. The gaſes obtained from the other metallic oxides and charcoal, were perfectly ſimilar in their properties; all of them when mixed with common air and ignited, burned with a lambent blue flame, without any ſenſible exploſion, and much carbonic acid, with a little water, was produced. When mixed with nitrous gas no diminution or ſenſible charge could be perceived, which proves that their oxygen is in a combined, and not in a diſengaged ſtate.

Dr. Prieſtley has likewiſe found, that when the carbonate of barytes was mixed with the grey oxide of iron, and expoſed to a ſtrong heat in a retort, that not only carbonic acid, but alſo inflammable gas was obtained. In order to ſatisfy myſelf with regard to this fact, I mixed a quantity of the carbonate of barytes and oxide of iron, both having been ſeparately made red hot for ſome time, and introduced them into a dry coated glaſs retort. Heat being gradually applied, a quantity of gas, which at firſt was nothing more than carbonic acid mixed with a little azote, eſcaped; but when the retort had been red for a few minutes, the gas diſengaged conſiſted of carbonic acid 25 parts, and gaſeous oxide of carbone ten parts; the whole amounting to 30 ounce meaſures. This experiment was repeated, and an iron retort employed, in order that a much greater degree of heat might be given; in this caſe, not leſs than 90 ounce meaſures of a ſimilar mixture of gaſes were obtained.

Conceiving that in theſe experiments the gaſeous oxide muſt proceed from the partial decompoſitions of the carbonic acid by the iron, when raiſed to a high temperature, I thought I ſhould ſucceed better by employing iron filings in place of the grey oxide, as theſe would have a greater affinity for oxigen. Accordingly I took a quantity of common chalk, and expoſed it to a low red heat for 10 minutes; it was them mixed with an equal quantity of clean iron filings made as dry as poſſible; the mixture being introduced into an iron retort, heat was gradually applied. As ſoon as the retort became red, gas began to be diſengaged in great abundance, and continued to come over for a conſiderable length of time, ſo that the whole muſt have amounted to many gallons. Portions of this being examined at different periods, it was found to conſiſt on a medium of one part of carbonic acid gas, and four or five parts of gaſeous oxide of carbone.

From this experiment we perceive, that a much greater proportion of gaſeous oxide is obtained, and more carbonic acid decompoſed, where iron filings are employed in place of the grey calx, which proves that it is the acid which is decompoſed by the iron. In another

other inſtance I ſubſtituted lime for the carbonate of lime ; here too the gaſeous oxide was obtained, but in much leſs quantity, and had the lime been perfectly pure, it is probable that very little, or none, would have been procured ; this experiment likewiſe points out the origin of the gas. In inveſtigating the properties of this inflammable gas, I found that when perfectly free from carbonic acid, it was lighter than common air, in the proportion of 22 to 23.

That when mixed with common air and ignited, it did not explode, but burnt ſlowly with a lambent blue flame : that when the products of this combuſtion were collected, by burning a conſiderable quantity in a large receiver containing common air or oxygen gas, nothing but carbonic acid could be collected, nor was there any perceptible quantity of water condenſed againſt the ſides of the veſſel. That when 20 parts of this gas were mixed with eight parts of oxygen, and fired over mercury by the electric ſpark, the whole was reduced to 18 or 19 parts, which were found to be carbonic acid gas, being totally abſorbed by lime water. Hence we ſee, that this gas contains rather more oxygen than that produced from charcoal and finery cinder, as 20 parts of it, with eight of oxygen, produce no leſs than 18 or 19 parts of carbonic acid gas ; upon the whole, however, they appear to be very ſimilar, more particularly in their ſpecific gravities ; but I remarked, nevertheleſs, that when a conſiderable quantity of the gaſeous oxide from charcoal, and the different calces was burned in a large receiver, ſome moiſture was depoſited on its ſides ; there can be little doubt, therefore, that this difference in the reſults depends on a portion of hydrogen, which enters more or leſs into the compoſition of all common charcoal.

The ſtatement at page 7, taken from the foregoing and other experiments, will ſhew more correctly the difference between theſe gaſes, and ſome of the beſt known hydrocarbonates ; from theſe calculations it will likewiſe appear, that the weight of the gaſeous oxide obtained from the decompoſition of the carbonic acid, added to that of the pure air neceſſary for its ſaturation, is very nearly equal to that of the carbonic acid gas produced ; for 100 cubic inches of this gas weighs 30 grains, and 40 cubic inches of oxygen gas 13.6 grains, making together 43.6 grains. Now 92 cubic inches of carbonic acid gas weighs 43.2 grains ; a greater degree of correctneſs cannot well be expected from experiments of this nature. But by making a ſimilar calculation with the products obtained from the exploſion of the gaſeous oxides from charcoal and the metallic calces, it will be found, that the weight of the gaſeous oxide, added to that of the pure air, is greater than the weight of the carbonic acid produced ; a circumſtance which would ſeem to prove the formation of water, more eſpecially when compared with the other experiments already related. From theſe facts, therefore, we would infer, that the pureſt gaſeous oxide is to be obtained from the decompoſition of carbonic acid ; for in this caſe it appears to contain neither water, nor the baſis of water.

Dr. Prieſtley, in his obſervations on theſe gaſes remarks, that in their properties they are very ſimilar to the inflammable air obtained by heat from moiſtened charcoal ; this, however, upon a more particular inveſtigation, I did not find to be the caſe. A quantity of of moiſtened charcoal being introduced into a coated glaſs retort, heat was applied, and gradually increaſed until the retort became red. The firſt portions of gas collected conſiſted of nine parts of carbonic acid gas, and 57 hydrocarbonate; what came over about the middle of the proceſs was three parts of carbonic acid, with 55 hydrocarbonate, and the laſt portions were nearly pure hydrocarbonate, the whole amounting to ſeveral gallons.

Having ſeparated the carbonic acid from this gas by repeated agitation with lime water, I found that it was ſpecifically lighter than common air, in the proportion of 11 to 23; in this reſpect, therefore, it differs eſſentially from the gaſeous oxides, being not more than half their weight; I likewiſe found, that when burned in a receiver containing pure or common air, a very conſiderable quantity of water was produced, and condenſed againſt the ſides of the veſſel; the receiver likewiſe contained much carbonic acid gas. I next endeavoured to aſcertain the proportion of oxygenous gas neceſſary to ſaturate it. After ſeveral trials I found, when ſix meaſures of this gas well waſhed, were mixed with four of pure oxygen, and exploded over mercury by the electric ſpark, that the reſiduary gas amounted to $2\frac{1}{2}$ meaſures nearly, which was entirely carbonic acid. Here again we find a remarkable difference between this hydrocarbonate and the gaſeous oxides, for ſix meaſures of oxygenous gas produce only $2\frac{1}{2}$ of carbonic acid, when combined with the former; whereas with the latter, the ſame quantity would produce no leſs than 14 meaſures of the ſame acid gas. The production of water from the two gaſes is likewiſe very different, being little or none from the oxides, but very copious from the hydrocarbonate. Being deſirous to aſcertain if any of the known hydrocarbonates were ſimilar in their properties to the gaſeous oxides, I made comparative experiments with moſt of them, but there were none which appeared to contain oxygen, at leaſt in any quantity. The pureſt hydrocarbonates are obtained from camphor, by making its vapour paſs through a red hot tube; from ether by the ſame proceſs; from animal ſubſtances, and ſome vegetables, by deſtructive diſtillation; and where it was not expected, from the gas of marſhes, for I had always ſuppoſed that this gas was ſimilar to that obtained from moiſtened charcoal by diſtillation. All theſe pure hydrocarbonates have exactly the ſame properties; when well freed from carbonic acid gas, they are lighter than common air in the proportion of 15.5 to 23.5, or 2 to 3 nearly; two parts by meaſure require no leſs than $3\frac{1}{2}$ of pure oxygen to ſaturate them, the products being $2\frac{1}{4}$ parts carbonic acid with ſome water (ſee the table) One of their moſt remarkable properties however, is, that when mixed with $\frac{2}{3}$ their bulk of pure oxygen, or thereabout, and fired in a jar by the electric ſpark, inſtead of a diminution, there is a conſiderable increaſe of volume, notwithſtanding the production of carbonic acid. The following may be conſidered as the mean reſult of ſeveral experiments made in this way.

Six meaſures of hydrocarbonate, mixed with $4\frac{1}{2}$ of pure oxygen, were introduced into a ſtrong glaſs jar over mercury, being fired by the electric ſpark, the mixture after the exploſion, which was violent, was augmented to $12\frac{3}{4}$ meaſures, or there was an increaſe of $2\frac{1}{2}$ meaſures nearly. Lime water being admitted, it ſoon became turbid, and there was an abſorption of ſomewhat better than two meaſures. The remaining gas amounting to $10\frac{1}{2}$ meaſures, was found by the teſt of nitrous gas to contain no ſenſible quantity of diſengaged oxygen. Two meaſures of this reſiduary gas being fired with one of oxygen, were reduced to one meaſure, which was found to be carbonic acid gas; hence the whole reſiduary gas would require about $5\frac{1}{4}$ meaſures of oxygen to ſaturate it, and produce five meaſures of carbonic acid. It would appear, therefore, that notwithſtanding the apparent change which the hydrocarbonate underwent in the firſt exploſion, yet the final reſults are not ſenſibly varied, for by comparing the two experiments together, it will be found that the proportions of hydrocarbonate, oxygen, and carbonic acid produced, are very nearly as three to one, which they ſhould be where no change is effected. I varied theſe experiments in different ways, but conſtantly found the ſame reſults.

Alcohol may likewiſe be converted into a ſpecies of hydrocarbonate, by making its vapour paſs through a red hot tube; and it is rather ſingular that this gas ſhould be ſo different in its properties from that obtained from æther in the ſame way; for the latter, as has been already obſerved, is a pure hydrocarbonate.

Having obtained a large quantity of this hydrocarbonate from the decompoſition of alcohol, it was well agitated with lime water, but little or no diminution took place, a circumſtance which would ſeem to prove, that pure ſpirit does not contain oxygen. I next took its ſpecific gravity, and found that it was lighter than common air in the proportion of 12 to 23, whereas that from æther is lighter only in the proportion of 15 to 23. When burned in a receiver containing pure or common air, much carbonic acid, and a conſiderable proportion of water, were produced. After ſeveral trials I found, that four parts of this gas require $4\frac{2}{3}$ of oxygen to ſaturate them, the products being three parts of carbonic acid, with water. But four parts of hydrocarbonate from æther require ſeven of oxygen to ſaturate them, and $4\frac{1}{2}$ meaſures of carbonic acid with water are produced. Hence the difference between theſe gaſes muſt be manifeſt; it likewiſe appears from theſe experiments, that æther contains leſs carbone and more hydrogen than alcohol; for it will be found by calculation, that the proportion of carbone to hydrogen in æther is very nearly as 3 to 1, but in alcohol as 4 to 1; indeed this circumſtance ſeems to be confirmed by the depoſition of charcoal which takes place in the proceſs for obtaining æther.

From the foregoing experiments, therefore, we perceive, that none of the hydrocarbonates at preſent known, are ſimilar in their properties to the gaſeous oxides of carbone, being much lighter, and yielding a far leſs proportion of carbonic acid when combined with a given quantity of oxygen.

After acquiring a knowledge of the nature and properties of theſe gaſes, we are no longer at a loſs to account for their production; we know that the common hydrocarbonates conſiſt of carbone united with, or held in ſolution by hydrogen, ſo there can be little doubt that the gaſeous oxides are nothing more than carbone united with, or held in ſolution by oxygen, and then converted into the gaſeous ſtate by caloric; that they contain oxygen, is manifeſt from the very ſmall additional quantity which is found ſufficient to convert them into pure carbonic acid, from their being procured by a mixture of metallic calces and charcoal in the drieſt poſſible ſtate, in which proceſs the metallic calx is found to be revived, having parted with its oxygen to form the gaſeous oxide and acid; and from their being obtained by the partial decompoſition of this ſame acid, as in the experiments with chalk and iron filings, &c. From this laſt circumſtance it would alſo appear, that carbonic acid when raiſed to a high temperature may be decompoſed by any fixed ſubſtance which has a conſiderable affinity for oxygen. Upon the whole, therefore, we conſider the gaſeous oxide of carbone as holding the ſame relation to pure hydrocarbonate and carbonic acid; which nitrous gas, or the gaſeous oxide of azote, does to pure azote and nitrous acid. We likewiſe conceive, that the objections to the new ſyſtem of chemiſtry advanced on this head, by Dr. Prieſtley, are ſufficiently anſwered. We find that theſe gaſeous oxides are very different from what the Doctor conceived them to be, and that the preſence of water does not appear to be neceſſary to their production. We have no more reaſon therefore to ſuppoſe that the grey oxide of iron ſhould contain water, than that the other oxides, which give the very ſame products when mixed with charcoal and diſtilled, ſhould contain it likewiſe.

A Table ſhewing the Analyſis, &c. of the different Species of Hydrocarbonates, and the gaſeous Oxide of Carbone.

Gaſes, and the ſubſtances from which the gaſes are obtained, &c. &c.	Weight of 100 cubic inches or meaſures	Proportion of oxygen neceſſary to ſaturate 100 meaſures of the gas		Products when combined with oxygen				Hence the gaſes conſiſt of			
				Carbonic acid		Water produc.	Water held in ſolution by the gas	Oxygen	Carbone	Hydrogen	Water
				in volume	in quant.						
	Grains	Meaſures	Quantity Grains	Meaſures	Grains	Grains	Grains				
Pure hydrocarbonate from camphor, &c.	21.	176.	59.8	116.	54.5	18.	8. or 9.	none	11.	2+	8. or 9.
Ditto from æther	20.	170.	58.	108.	50.5	18.	9.	none	9.	3.	8.
Hydrogen from Alcohol	16.	118.	40.	75.	36.	13.	7.	none	7.	1.9	7.
Ditto from wet charcoal	14.5	66.	22.4	40.	19.	9.	9.	none	4 nearly	1.3	9.
Gaſeous oxides from charcoal and metallic oxides	30.	44.	15.	76.	35.5	about 8.	probably none	about 15.	7.	1+	uncertain
from iron filings and carbonate of lime, or barytes.	30.	40.	13.6	92.	43.2	none	none	21+	8.6	none	none

In the preceding table, the weight of one 100 cubic inches of common air, under the mean preſſure of the atmoſphere, and at the temperature of 55° feet, is eſtimated at 31 grains; the quantity of pure carbone in carbonic acid at $\frac{1}{5}$ of the whole nearly, and the proportion of oxygen to hydrogen in water as 85 to 15.

The quantity of oxygen in the gaſeous oxides was thus eſtimated. In the laſt line, for example, we find that 30 grains of the oxide, produced, when combined with 13.6 grains of of oxygen, 43.3 grains of carbonic acid. Now this acid conſiſts of 8.6 grains of carbone, and 34.6 of oxygen nearly; but the quantity of oxygen employed was only 13.6 grains, hence the difference between 34.6 and 13.6, or 21 grains, muſt have been originally contained in the gas. It would therefore appear, that the gaſeous oxides in their pureſt ſtate conſiſt of oxygen and carbone in the proportion of 21 to 9 nearly, combined with caloric.

Woolwich, March 19, 1801.

II.

On the Power of penetrating into Space by Teleſcopes; with a comparative Determination of the Extent of that Power in natural Viſion, and in Teleſcopes of various Sizes and Conſtructions; illuſtrated by ſelect Obſervations. By WILLIAM HERSCHEL, LL. D. F. R. S.

(Concluded from Vol. IV. page 554.)

JULY 12, 1784. I viewed the ſame nebula with a large 20-feet Newtonian reflector; power 157.

“ A moſt beautiful extenſive cluſter of ſtars, of various magnitudes, very compreſſed in “ the middle, and about 8′ in diameter, beſides the ſcattered ones, which do more than “ fill the extent of the field of view*: the large ſtars are red; the ſmall ones are pale red. “ RA $18^h\ 23'\ 39''$; PD 114° 7′.”

The penetrating power of the firſt inſtrument was 39, that of the latter 61; but, from the obſervations, it is plain how much ſuperior the effect of the latter was to that of the former, notwithſtanding the magnifying power was ſo much in favour of the inſtrument with the ſmall penetrating power.

July 30, 1783. With a ſmall 20-feet Newtonian reflector, I viewed the nebula in the hand of Serpentarius, diſcovered by Mr. Meſſier, in 1764.

“ With a power of 200, I ſee it conſiſts of ſtars. They are better viſible with 300. “ With 600, they are too obſcure to be diſtinguiſhed, though the appearance of ſtars is ſtill “ preſerved. This ſeems to be one of the moſt difficult objects to be reſolved. With me, “ there is not a doubt remaining; but another perſon, in order to form a judgment, ought “ previouſly to go through all the ſeveral gradations of nebulæ which I have reſolved into “ ſtars.”

May 25, 1791. I viewed the ſame nebula with a 20-feet reflector of my conſtruction, having a penetrating power of $\frac{\sqrt{\overline{,64 \times 188}|^2}}{2} = 75{,}08$.

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* This field, by the paſſage of an equatorial ſtar, was 15′ 3″.

"With a magnifying power of 157, it appears extremely bright, round, and eaſily re-
"ſolvable. With 300, I can ſee the ſtars. It reſembled the cluſter of ſtars taken at 16^h
"43′ 40″ *, which probably would put on the ſame appearance as this, if it were at a diſ-
"tance half as far again as it is. R A 17^h 26′ 19″; PD 93° 10′."

Here we may compare two obſervations; one taken with the penetrating power of 39, the other with 75; and, although the former inſtrument had far the advantage in magnifying power, the latter certainly gave a more complete view of the object.

The 20-feet reflector having been changed from the Newtonian form to my preſent one, I had a very ſtriking inſtance of the great advantage of the increaſed penetrating power, in the diſcovery of the Georgian ſatellites. The improvement, by laying aſide the ſmall mirror, was from 61 to 75: and, whereas the former was not ſufficient to reach theſe faint objects, the latter ſhewed them perfectly well.

March 14, 1798. I viewed the Georgian planet with a new 25-feet reflector. Its penetrating power is $\frac{\sqrt{,64 \times \overline{240}^{2}}}{2} = 95,85$; and, having juſt before alſo viewed it with my 20-feet inſtrument, I found, that with an equal magnifying power of 300, the 25-feet teleſcope had conſiderably the advantage of the former.

Feb. 24, 1786. I viewed the nebula near Flamſteed's 5th Serpentis, which has been mentioned before, with my 20-feet reflector; magnifying power 157.

"The moſt beautiful extremely compreſſed cluſter of ſmall ſtars; the greateſt part of
"them gathered together into one brilliant nucleus, evidently conſiſting of ſtars, ſur-
"rounded with many detached gathering ſtars of the ſame ſize and colour. R A 15^h 7′
"12″; P D 87° 8′."

May 27, 1791. I viewed the ſame object with my 40-feet teleſcope; penetrating power $\frac{\sqrt{,64 \times \overline{480}^{2}}}{2} = 191,69$; magnifying power 370.

"A beautiful cluſter of ſtars. I counted about 200 of them. The middle of it is ſo
"compreſſed that it is impoſſible to diſtinguiſh the ſtars."

Here it appears, that the ſuperior penetrating power of the 40-feet teleſcope enabled me even to count the ſtars of this nebula. It is alſo to be noticed, that the object did not ſtrike me as uncommonly beautiful; becauſe, with much more than double the penetrating, and alſo more than double the magnifying power, the ſtars could not appear ſo compreſſed and ſmall as in the 20-feet inſtrument: this, very naturally, muſt give it more the reſemblance of a coarſer cluſter of ſtars, ſuch as I had been in the habit of ſeeing frequently.

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* The object referred to is No. 10. of the *Connoiſſance des Temps* for 1783, called "*Nebuleuſe ſans* "*etoiles*." My deſcription of it is, "A very beautiful, and extremely compreſſed, cluſter of ſtars: the "moſt compreſſed part about 3 or 4′ in diameter. R A 16^h 46′ 2″; P D 93° 46′."

The 40-feet teleſcope was originally intended to have been of the Newtonian conſtruction; but, in the year 1787, when I was experimentally aſſured of the vaſt importance of a power to penetrate into ſpace, I laid aſide the work of the ſmall mirror, which was then in hand, and completed the inſtrument in its preſent form.

"Oct. 10, 1791. I ſaw the 4th ſatellite and the ring of Saturn, in the 40-feet ſpecu-
"lum, without an eye glaſs."

The magnifying power on that occaſion could not exceed 60 or 70; but the great penetrating power made full amends for the lowneſs of the former; notwithſtanding the greateſt part of it muſt have been loſt for want of a greater opening of the iris, which could not take in the whole pencil of rays, for this could not be leſs than 7 or 8 tenths of an inch.

Among other inſtances of the ſuperior effects of penetration into ſpace, I ſhould mention the diſcovery of an additional 6th ſatellite of Saturn, on the 28th of Auguſt, 1789; and of a 7th, on the 11th of September, in the ſame year; which were firſt pointed out by this inſtrument. It is true that both ſatellites are within the reach of the 20-feet teleſcope: but it ſhould be remembered, that when an object is once diſcovered by a ſuperior power, an inferior one will ſuffice to ſee it afterwards. I need not add, that neither the 7 nor 10-feet teleſcopes will reach them; their powers, 20 and 29, are not ſufficient to penetrate to ſuch diſtant objects, when the brightneſs of them is not more than that of theſe ſatellites. It is alſo evident, that the failure in theſe latter inſtruments, ariſes not from want of magnifying power; as either of them has much more than ſufficient for the purpoſe.

Nov. 5, 1791. I viewed Saturn with the 20 and 40-feet teleſcopes.

"20-feet. The 5th ſatellite of Saturn is very ſmall. The 1ſt, 2d, 3d, 4th, 5th, and
"the new 6th ſatellite, are in their calculated places."

"40-feet. I ſee the new 6th ſatellite much better with this inſtrument than with the
"20-feet. The 5th is alſo much larger here than in the 20-feet; in which it was nearly
"the ſame ſize as a ſmall fixed ſtar, but here it is conſiderably larger than that ſtar."

Here the ſuperior penetrating power of the 40-feet teleſcope ſhewed itſelf on the 6th ſatellite of Saturn, which is a very faint object; as it had alſo a conſiderable advantage in magnifying power, the diſk of the 5th ſatellite appeared larger than in the 20-feet. But the ſmall ſtar, which may be ſaid to be beyond the reach of magnifying power, could only profit by the ſuperiority of the other power.

Nov. 21, 1791. 40-feet reflector; power 370.

"The black diviſion upon the ring is as dark as the heavens about Saturn, and of the
"ſame colour."

"The ſhadow of the body of Saturn is viſible upon the ring, on the following ſide; its
"colour is very different from that of the dark diviſion. The 5th ſatellite is leſs than the
"3d; it is even leſs than the 2d."

20-feet reflector; power 300.

" The 3d ſatellite ſeems to be ſmaller than it was the laſt night but one. The 4th ſa-
" tellite ſeems to be larger than it was the 19th. This teleſcope ſhews the ſatellites not
" nearly ſo well as the 40-feet.

Here, the magnifying power being nearly alike, the ſuperiority of the 40-feet teleſcope muſt be aſcribed to its penetrating power.

The different nature of the two powers above mentioned being thus evidently eſtabliſhed, I muſt now remark, that, in ſome reſpects, they even interfere with each other; a few inſtances of which I ſhall give.

Auguſt 24, 1783. I viewed the nebula north preceding Flamſteed's 1 Trianguli, diſcovered by Mr. Meſſier, in 1764.

" 7-feet reflector; power 57. There is a ſuſpicion that the nebula conſiſts of exceed-
" ingly ſmall ſtars. With this low power it has a nebulous appearance; and it vaniſhes
" when I put on the higher magnifying powers of 278 and 460."

Oct. 28, 1794. I viewed the ſame nebula with a 7-feet reflector.

" It is large, but very faint. With 120, it ſeems to be compoſed of ſtars, and I think I
" ſee ſeveral of them; but it will bear no magnifying power."

In this experiment, magnifying power was evidently injurious to penetrating power. I do not account for this upon the principle that by magnifying we make an object leſs bright; for, when opticians have alſo demonſtrated that brightneſs is diminiſhed by magnifying, it muſt again be underſtood as relating only to the *intrinſic* brightneſs of the magnified picture; its abſolute brightneſs, which is the only one that concerns us at preſent, muſt always remain the ſame *. The real explanation of the fact, I take to be, that while the light collected is employed in magnifying the object, it cannot be exerted in giving penetrating power.

June 18, 1799. I viewed the planet Venus with a 10-feet reflector.

" Its light is ſo vivid that it does not require, nor will it bear, a penetrating power of
" 29, neither with a low nor with a high magnifying power."

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* This may be proved thus. The mean intrinſic brightneſs, or rather illumination, of a point of the picture on the retina, will be *all the light that falls on the picture, divided by the number of its points*; or $C = \frac{l}{N}$. Now, ſince with a greater magnifying power m, the number of points N increaſes as the ſquares of the power, the expreſſion for the intrinſic brightneſs $\frac{l}{N}$, will decreaſe in the ſame ratio; and it will conſequently be in general $N \propto m^2$, and $\frac{l}{N}$ or $C \propto \frac{1}{m^2}$; that is, by compounding $CN \propto \frac{m^2}{m^2} \doteq l = 1$; or abſolute brightneſs a given quantity. M. Bouguer has carefully diſtinguiſhed intrinſic and abſolute brightneſs, when he ſpeaks of the quantity of light reflected from a wall, at different diſtances. *Traité d'Optique*, page 39 and 40.

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This is not owing to the leaſt imperfection in the mirror, which is truly parabolical, and ſhews, with all its aperture open, and a magnifying power of 600, the double ſtar γ Leonis in the greateſt perfection.

“ It ſhewed Venus, perfectly well defined, with a penetrating power as low as 14, and “ a magnifying power of 400, or 600.”

Here, penetrating power was injurious to magnifying power; and that it neceſſarily muſt be ſo, when carried to a high pitch, is evident; for, by enlarging the aperture of the teleſcope, we increaſe the evil that attends magnifying, which is, that we cannot magnify the object without magnifying the medium. Now, ſince the air is very ſeldom of ſo homogeneous a diſpoſition as to admit to be magnified highly, it follows that we muſt meet with impurities and obſtructions, in proportion to its quantity. But the contents of the columns of air through which we look at the heavens by teleſcopes, being of equal lengths, muſt be as their baſes, that is, as the ſquares of the apertures of the teleſcopes; and this is in a much higher ratio than that of the increaſe of the power of penetrating into ſpace. From my long experience in theſe matters, I am led to apprehend, that the higheſt power of magnifying may poſſibly not exceed the reach of a 20 or 25-feet teleſcope; or may even lie in a leſs compaſs than either. However, in beautiful nights, when the outſide of our teleſcopes is dropping with moiſture diſcharged from the atmoſphere, there are now and then favourable hours, in which it is hardly poſſible to put a limit to magnifying power. But ſuch valuable opportunities are extremely ſcarce; and, with large inſtruments, it will always be loſt labour to obſerve at other times.

As I have hinted at the natural limits of magnifying power, I ſhall venture alſo to extend my ſurmiſes to thoſe of penetrating power. There ſeems to be room for a conſiderable increaſe in this branch of the teleſcope; and, as the penetrating power of my 40-feet reflector already goes to 191,69, there can hardly be any doubt but that it might be carried to 500, and probably not much farther. The natural limit ſeems to be an equation between the fainteſt ſtar that can be made viſible, by any means, and the united brilliancy of ſtar-light. For, as the light of the heavens, in clear nights, is already very conſiderable in my large teleſcope, it muſt in the end be ſo increaſed, by enlarging the penetrating power, as to become a balance to the light of all objects that are ſo remote as not to exceed in brightneſs the general light of the heavens. Now, if P be put for penetrating power, we have $\sqrt{\frac{P^2 a^2}{x}} = A =$ 10 feet 5,2 inches for an aperture of a reflector, on my conſtruction, that would have ſuch a power of 500.

But, to return to our ſubject; from what has been ſaid before, we may conclude, that objects are viewed in their greateſt perfection, when, in penetrating ſpace, the magnifying power is ſo low as only to be ſufficient to ſhew the object well; and when, in magnifying objects, by way of examining them minutely, the ſpace-penetrating power is no higher than what will ſuffice for the purpoſe; for, in the uſe of either power, the injudicious overcharge of the other, will prove hurtful to perfect viſion.

It is remarkable that, from very different principles, I have formerly determined the length of the viſual ray of my 20-feet teleſcope upon the ſtars of the milky way, ſo as to agree nearly with the calculations that have been given *. The extent of what I then figuratively called my ſounding line, and what now appears to anſwer to the power of penetrating into ſpace, was ſhewn to be not leſs than 415, 461, and 497 times the diſtance of Sirius from the ſun. We now have calculated that my teleſcope, in the Newtonian form, at the time when the paper on the Conſtruction of the Heavens was written, poſſeſſed a power of penetration, which exceeded that of natural viſion 61,18 times; and, as we have alſo ſhewn, that ſtars at 8, 9, or at moſt 10 times the diſtance of Sirius, muſt become inviſible to the eye, we may ſafely conclude, that no ſingle ſtar, above 489,551, or at moſt 612 times as far as Sirius, can any longer be ſeen in this teleſcope. Now, the greateſt length of the former viſual ray, 497, agrees nearly with the loweſt of theſe preſent numbers, 489; and the higher ones are all in favour of the former computation; for that ray, though taken from what was perhaps not far from its greateſt extent, might poſſibly have reached to ſome diſtance beyond the apparent bounds of the milky way: but, if there had been any conſiderable difference in theſe determinations, we ſhould remember that ſome of the data by which I have now calculated are only aſſumed. For inſtance, if the opening of the iris, when we look at a ſtar of the 7th magnitude, ſhould be only one-tenth of an inch and a half, inſtead of two, then a, in our formula, will be $= 1,5$; which, when reſolved, will give a penetrating power of 81,58; and therefore, on this ſuppoſition, our teleſcope would eaſily have ſhewn ſtars 571 times as far from us as Sirius; and only thoſe at 653,734, or 816 times the ſame diſtance, would have been beyond its reach. My reaſon for fixing upon two-tenths, rather than a lower quantity, was, that I might not run a riſk of over-rating the powers of my inſtruments. I have it however in contemplation, to determine this quantity experimentally, and perceive already, that the difficulties which attend this ſubject may be overcome.

It now only remains to ſhew, how far the penetrating power, 192, of my large reflector, will really reach into ſpace. Then, ſince this number has been calculated to be in proportion to the ſtandard of natural viſion, it follows, that if we admit a ſtar of the 7th magnitude to be viſible to the unaſſiſted eye, this teleſcope will ſhew ſtars of the one thouſand three hundred and forty-ſecond magnitude.

But, as we did not ſtop at the ſingle ſtars above mentioned, when the penetration of the natural eye was to be aſcertained, ſo we muſt now alſo call the united luſtre of ſidereal ſyſtems to our aid in ſtretching forwards into ſpace. Suppoſe therefore, a cluſter of 5000 ſtars to be at one of thoſe immenſe diſtances to which only a 40-feet reflector can reach, and our formula will give us the means of calculating what that may be. For, putting S

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* Phil. Tranſ. Vol. LXXV. page 247, 248.

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for the number of ſtars in the cluſter, and D for its diſtance, we have $\frac{\sqrt{x A^2 S}}{a} = D$[*]; which, on computation, comes out to be above 11¾ millions of millions of millions of miles! a number which exceeds the diſtance of the neareſt fixed ſtar, at leaſt three hundred thouſand times.

From the above conſiderations it follows, that the range for obſerving, with a teleſcope ſuch as my 40-feet reflector, is indeed very extenſive. We have the inſide of a ſphere to examine, the radius of which is the immenſe diſtance juſt now aſſigned to be within the reach of the penetration of our inſtruments, and of which all the celeſtial objects viſible to the eye, put together, form as it were but the kernel, while all the immenſity of its thick ſhell is reſerved for the teleſcope.

It follows, in the next place, that much time muſt be required for going through ſo extenſive a range. The method of examining the heavens, by ſweeping over ſpace, inſtead of looking merely at places that are known to contain objects, is the only one that can be uſeful for diſcoveries.

In order therefore to calculate how long a time it muſt take to ſweep the heavens, as far as they are within the reach of my 40-feet teleſcope, charged with a magnifying power of 1000, I have had recourſe to my journals, to find how many favourable hours we may annually hope for in this climate. It is to be noticed, that the nights muſt be very clear; the moon abſent; no twilight; no hazineſs; no violent wind; and no ſudden change of temperature; then alſo, ſhort intervals for filling up broken ſweeps will occaſion delays; and, under all theſe circumſtances, it appears that a year which will afford 90, or at moſt 100 hours, is to be called very productive.

In the equator, with my 20-feet teleſcope, I have ſwept over zones of two degrees, with a power of 157: but, an allowance of 10 minutes in polar diſtance muſt be made, for lapping the ſweeps over one another where they join.

As the breadth of the zones may be increaſed towards the poles, the northern hemiſphere may be ſwept in about 40 zones; to theſe we muſt add 19 ſouthern zones; then, 59 zones, which, on account of the ſweeps lapping over one another about 5′ of time in right aſcenſion, we muſt reckon of 25 hours each, will give 1475 hours. And, allowing 100 hours per year, we find that, with the 20-feet teleſcope, the heavens may be ſwept in about 14 years and ¾.

Now, the time of ſweeping with different magnifying powers will be as the ſquares of the powers; and, putting p and t for the power and time in the 20-feet teleſcope, and $P = 1000$ for the power in the 40, we ſhall have $p^2 : t :: P^2 : \frac{t P^2}{p^2} = 59840$. Then, ma-

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[*] $D = 11765475948678678679$ miles.

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king the ſame allowance of 100 hours per year, it appears that it will require not leſs than 598 years, to look with the 40-feet reflector, charged with the abovementioned power, only one ſingle moment into each part of ſpace; and, even then, ſo much of the ſouthern hemiſphere will remain unexplored, as will take up 213 years more to examine.

Slough, near Windſor, June 20, 1799.

III.

Enquiries relative to the Laws of Affinity. By CITIZEN BERTHOLLET, *Member of the National Inſtitute, &c. Abridged by* CIT. BOUILLON LAGRANGE.*

THE publication of the work of Cit. Berthollet on the affinities, has long been expected by chemiſts. Their anxiety was doubtleſs very natural; for an object ſo important could only be completed by that celebrated individual, whoſe diſcoveries have been ſo highly advantageous to ſociety.

The work here announced cannot be abridged without difficulty; for by preſenting the reader with a mere analyſis of each article, much information would be loſt. Hence we ſhall frequently ſuffer the author to ſpeak for himſelf, as his work is a collection of facts, upon which the theory is eſtabliſhed.

Cit. Berthollet has divided his memoir into fifteen articles:

In the firſt of theſe, the author makes a panegyric upon Bergmann, who devoted his attention to this ſubject with the greateſt ſucceſs. Cit. Berthollet has principally attended to the work of the Swediſh chemiſt, when treating on this ſubject. He then ſtates the object of his memoir: "It is my intention" ſays he "to prove that the elective affinities do not act like abſolute forces, by which one ſubſtance might be diſplaced by another in a combination; but that in all the compoſitions and decompoſitions which are owing to elective affinity, a diviſion is made of the object of the combination between the ſubſtances whoſe actions are oppoſed; and that the proportions of this diviſion are determined not only by the energies of affinity of theſe ſubſtances, but alſo by the quantity with which they act; ſo that the quantity may compenſate for a want of the affinity, and produce a ſimilar degree of ſaturation.

If, continues the author, I eſtabliſh that the quantity of a ſubſtance can ſupply the want of force of its affinity, it will follow that its action will be proportionate to the quantity neceſſary to produce a determinate degree of ſaturation. I denominate *maſs*, that quantity which is the *meaſure* of the capacity of the ſaturation of different ſubſtances.

When therefore I compare the affinities of ſubſtances, I ſhall direct my attention to the ponderable quantity, which in this compariſon ought to be equal; but, when I compare

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* Annales de Chimie, XXXVI. 302.

their action, which is compoſed of both their affinity and their proportion, I muſt then conſider the maſs.

The author next obſerves, that in the following diſcuſſions, he ſhall principally make uſe of acids and alkalis, among which he compriſes thoſe earths which act as ſuch; becauſe they exerciſe great powers, which may diſpel the influence of ſlight cauſes; and becauſe they often produce comparable degrees of ſaturation, and afford reſults which may be eaſily obſerved. But the conſequences drawn from their properties by Cit. Berthollet, are applicable to all other combinations. It is proved by many examples, that the principle eſtabliſhed by the author extends to the whole chemical action of bodies.

After having proved by direct experiments, that the chemical action of ſubſtances, of which the forces are oppoſed, does not depend upon their affinity alone, but likewiſe on their quantity, the author ſtates that he ſhall make choice of obſervations on different kinds of combinations which will confirm this principle, and prove its extent. I ſhall examine, he continues, the circumſtances which modify it, or thoſe affections of bodies which favour or diminiſh their chemical action, and which cauſe the proportions of the combinations which they are capable of forming to vary: I ſhall apply theſe conſiderations to the complex affinities, and to thoſe of compound bodies; and laſtly, I ſhall endeavour to determine the baſis on which the general and particular theories of chemical phenomena ought to be eſtabliſhed.

ARTICLE II.

Experiments which prove that in the Elective Affinities, the oppoſing Subſtances divide that Body which is the Subject of the Combination.

To demonſtrate the truth of this propoſition, Cit. Berthollet deſcribes ſeveral experiments, from which we ſhall quote the following: I kept, ſays he, in a ſtate of ebullition in a ſmall quantity of water, an equal weight of pot-aſh, purified by alcohol, and of ſulphate of barytes. The operation was made in a retort, and conſequently without the contact of air, and continued till the mixture was evaporated to dryneſs: the reſiduum was treated with alcohol, which diſſolved the pot-aſh, and afterwards with water. The latter effected a ſolution which alſo exhibited alkaline properties. This alkali was ſaturated with acetous acid, after which there was formed, by evaporation, a conſiderable quantity of ſmall cryſtals, which poſſeſſed all the characters of ſulphate of pot-aſh; ſo that the ſulphate of barytes was partly decompoſed by the pot-aſh, and the ſulphuric acid was divided between the two baſes.

The other experiments were made,

1. With ſulphate of pot-aſh and lime: 2. With oxalate of lime and pot-aſh: 3. With oxalate of lime and nitric acid: 4. With phoſphate of lime and pot-aſh: 5. With pot-aſh and carbonate of lime: 6. With ſoda and ſulphate of pot-aſh.

In the whole of theſe experiments it is ſeen that thoſe baſes, which are admitted as forming, with the acids, the ſtrongeſt and moſt permanent combinations, were partly extricated by a baſis to which a weaker affinity is attributed; ſo that the acid was divided between the two baſes. It is likewiſe obſerved that the acids are partly diſengaged from their baſe by others, the affinity of which is conſidered as inferior, ſo that the baſe is divided between two acids.

The author premiſes, that if a ſmall quantity only of the decompoſing ſubſtance be employed, the effect will not be perceptible, and he concludes, that when any ſubſtance acts upon a combination, that which is the ſubject of the combination is divided between the two other ſubſtances, not only according to the reſpective energies of their affinity, but likewiſe according to their quantity.

ARTICLE III.

Obſervations which confirm the Principle of the Chemical Action in Proportion to the Maſs.

The author makes obſervations on different kinds of combination expoſed to an elective affinity, and examines whether the principle of chemical action, in proportion to the maſs, be not preciſely applicable to their explanation.

If, as he obſerves, we treat the carbonate of pot-aſh with lime, we cannot deprive the pot-aſh of all its carbonic acid, even by making ſucceſſive operations with freſh lime; and if we evaporate the fluid, the reſiduum is ſtill capable of effervеſcence, on being ſaturated by the acids, becauſe the pot-aſh which remained preſent with the lime oppoſed its action; and, the greater the quantity of carbonic acid which has been before abſorbed by the lime, the more powerful is the pot-aſh to maintain its combination with this acid.

When the equilibrium is eſtabliſhed between the action of the lime and the reſiſtance of the pot-aſh, if the liquid be filtered and evaporated, we may take away, by a weak affinity, that portion of alkali which is ſuperabundant to the conſtitution of carbonate of pot-aſh, that is to ſay, the whole portion which is not defended by a conſiderable maſs of carbonic acid. Alcohol poſſeſſes this property; the ſeparation is effected by this fluid. The carbonate of pot-aſh remains in ſolution in a ſmall quantity of water, while the alcohol of pot-aſh floats above. The ſeparated carbonate may again be treated with lime; and by this ſecond operation it may be reduced to an inconſiderable quantity.

Cit. Berthollet alſo quotes ſeveral other experiments relative to the chemical action in proportion to the maſs.

If a carbonate which contains an exceſs of pot-aſh be treated with alcohol, it can only be deprived of a part of its exceſs by ſuch treatment.

The other neutral ſalts likewiſe poſſeſs the property of retaining a part of that pot-aſh which may be contained in them ſuperabundant to their neutralization.

It is alſo known that phoſphate of lime cannot be entirely decompoſed by the ſulphuric acid; though it has been aſſerted that the ſulphuric acid has a greater affinity to lime than the phoſphoric acid.

The

The ſame happens when the ſulphate of alumine is decompoſed by ammonia: the precipitate always contains a portion of ſulphuric acid.

If the magneſia of the ſulphate of alumine (qu. magneſia?) be precipitated by pot-aſh, the magneſia will alſo contain a portion of ſulphuric acid; for when the magneſia is ſubmitted to ſtrong heat, it is afterwards found to poſſeſs rather a ſtrong taſte of ſulphate.

All theſe experiments prove, that in chemical analyſes we are liable to error, by taking that for the real weight, of alumine or magneſia, which may have been contained in a compound ſubſtance, and is merely the precipitate formed by elective affinity.

From the preceding obſervations, and many others which cannot here be given, it appears that in operations of elective affinity the ſubject of the combination is divided between the two ſubſtances which act upon it, in conſequence of the forces they are capable of oppoſing to each other.

A circumſtance worthy of attention, and which clearly proves that the chemical action depends as much on the quantity as on the affinity of ſubſtances, is, that to produce oppoſite effects, it is only neceſſary to make the requiſite alteration in the quantities.

ARTICLE IV.

On thoſe Modifications of Chemical Action, which ariſe from the Inſolubility of the Subſtances.

The author ſucceſſively examines thoſe affections of bodies which may diſguiſe or modify the applications of the principle eſtabliſhed in the preceding articles. He proves by a number of experiments; 1. The manner in which an inſoluble ſubſtance acts when oppoſed to a combination. 2. If a ſubſtance poſſeſſes ſome degree of ſolubility, its action is compoſed of that of the diſſolved part, and of that which remains ſolid. 3. If an inſoluble combination be attacked by a liquid ſubſtance, the inconveniencies of its inſolubility ſpeedily diſappear, in the caſe when it is only neceſſary that the inſoluble ſubſtance ſhould loſe a part of its conſtituent principles, to become liquid.

The author next proceeds to examine the difference of ſpecific gravities of the inſoluble ſubſtance, and the liquid. According to him, this has an influence on their reſpective actions, even when agitation and heat are employed; becauſe it continually tends to ſeparate the inſoluble ſubſtance, and to withdraw from the action oppoſed to it. Hence there is a difference on this account between the ſulphates of barytes and of alumine.

Laſtly, as the inſolubility prevents thoſe proportions of diviſion from taking place, which might elſe have been eſtabliſhed by the oppoſing forces, ſo likewiſe it is found to retard the operation of thoſe which can take place; ſo that it may eaſily produce deception by the appearances which it affords at the commencement of an operation. For example, when we mix ſome concentrated ſulphuric acid with a ſolution of ſulphate of pot-aſh, or any other ſalt which requires a large proportion of water to diſſolve it, the acid ſoon combines with the water; and the ſalt which loſes its liquidity is precipitated; but by continuing the experiment, and multiplying the contacts, the ſalt becomes diſſolved, and enters into combination with the liquid.

ARTICLE V.

On Coheſion and Cryſtallization.

The coheſion of the particles of a body is owing to the mutual affinity of thoſe particles: it is a force which muſt be overcome by the action of the ſubſtance which has a tendency to combine with thoſe parts, or to decompoſe their combination. It is known that clay, the particles of which have formed a ſtrong coheſion by deſiccation, is no longer attacked by an acid which would be capable of diſſolving it when in another ſtate.

The mutual affinity of the ſaline particles is the cauſe of cryſtallization; and this modifies the chemical action in a manner which deſerves attention. The author makes ſome remarks on this ſubject, as well as on the force which produces cryſtallization in a ſaline ſolution. It eſtabliſhes a limit to which the degree of ſaturation of the ſalt in water can arrive; ſo that if the water does not diſſolve a greater quantity of it, this is not in conſequence of its affinity being ſatisfied, but becauſe it is no longer ſufficiently powerful to overcome the reſiſtance of the cryſtallization. From theſe and numerous other conſiderations, Cit. Berthollet concludes that the force of coheſion, which has hitherto been conſidered merely as an obſtacle to ſolution, determines the quantities of ſubſtances which may be brought into action in a liquid; and thus modifies the conditions of chemical action. It is this alſo which cauſes thoſe ſeparations that take place, either by cryſtallization or precipitation, and determines the proportions of the combinations which are formed by ſeparation from the liquid, when the ſolubility or inſolubility depends upon theſe proportions.

ARTICLE VI.

On the Elaſticity of Subſtances which exert a Chemical Action.

When any ſubſtance eſcapes in a gaſeous ſtate, in proportion as it is diſengaged from an intimate combination, the whole of that part which aſſumes the elaſtic ſtate, ceaſes to afford any reſiſtance; ſo that the ſubſtance no longer acts in the ratio of its maſs. The ſubſtance preſented may in this caſe render the decompoſition complete; and it will be ſufficient to uſe ſimply that quantity which would have been requiſite for immediately forming the combination in which it is to enter, or at leaſt a ſmall exceſs only will be neceſſary.

This happens, as Cit. Berthollet obſerves, with the carbonic acid, when it forms a carbonate, and another acid is preſented to it. This laſt acid, which acts in proportion to its maſs, may, even with an affinity inferior to that of the carbonic acid, ſucceſſively expel it from the combination, even till none ſhall remain, provided it be employed at firſt in a quantity rather greater than would have been neceſſary for immediately forming its combination with the baſis.

From all theſe facts preſented by the author, it follows that elaſticity produces effects analogous to thoſe of the force of coheſion, by modifying, in an oppoſite direction, the effects of the affinity proper to each ſubſtance.

ARTICLE VII.

Of the Action of Caloric.

In this chapter Cit. Berthollet examines the action of caloric upon bodies, and the phenomena it produces in the various combinations or decompoſitions. Articles VIII. and IX. contain a great number of experiments on efflorеſcence and on ſolvents.

What Cit. Berthollet underſtands by efflorеſcence, is the property of riſing above the maſs, and of ſeparating from it by chemical action.

In order that ſoda may riſe in efflorеſcence, he obſerves, it muſt neceſſarily have combined with carbonic acid, which it can attract from the air of the atmoſphere; but the action of the carbonic acid, which is very ſmall in quantity, and in an elaſtic ſtate, cannot ſenſibly add to the force which has produced the ſeparation of the ſoda, from the combination in which it is found; it only ſeparates the eliminated part, and hinders it from continuing to act upon the combination.

With reſpect to ſolvents, the author is of opinion that their uſe is to overcome the reſiſtance which ariſes from the coheſion of the parts intended to be brought into action, or from their elaſticity, and to increaſe their mutual contact.

Solvents act upon ſubſtances, which they diſſolve by their affinity and their quantity, like all ſubſtances which have a tendency to combination; and to theſe is applicable all the doctrine of the combination of bodies. The author takes water for his example, which is moſt frequently employed as a ſolvent.

Cit. Berthollet then examines the circumſtances in which this action may change or ſenſibly modify the reſults; and this leads him to offer a ſeries of experiments; on which he eſtabliſhes a luminous theory.

This article is concluded by ſome remarks upon caloric. The author conceives it to act upon bodies which are not equally dilatable in a manner ſimilar to ſolvents, by attacking the force of coheſion, and thus enabling the particles to exerciſe their mutual action. Its action concurs with thoſe of the ſolvents, in oppoſing the force of coheſion: whence it happens, that the ſolution of a ſalt by water varies according to the degrees of temperature.

When caloric acts upon ſubſtances not equally dilatable, it produces ſeparations and new combinations, independent of the affinity peculiar to ſubſtances; in the ſame manner as the ſolvents when they act upon ſubſtances not equally ſoluble.

(To be continued.)

IV.

Supplementary Letter on the Syphon, to the Communication at page 547 of the laſt Volume.

By Mr. WILLIAM CLOSE.

To Mr. NICHOLSON.

SIR,

VALVES covered with leather may be adapted to the ends of the ſmall pipes, and they may be opened by drawing a wire after the external valve has cloſed its tube. For this purpoſe a long ſmall pipe muſt be fixed into the bottom of A W, (Pl. XXIV. Fig. 1.) muſt riſe to near the top of that veſſel, and its lower end be immerſed in a veſſel of water below the loweſt part of the ſyphon. The wire muſt paſs through this pipe, and be fixed to one lever within the veſſel for raiſing the valves, and to another below the bottom of the pipe, connected with the moveable veſſels above. When the ſecond and fourth veſſels aſcend, the valves will open, unleſs they are prevented by the weight of water in the ſyphon, and water will riſe in the ſmall wire pipe, but cannot enter the veſſel A W. The aſcent, however, will extend through a ſpace nearly equal to the length of the deſcending column in the ſyphon.

A very eaſy and extenſive method of rendering active the preſſure of the atmoſphere is here preſented to our notice. The veſſel A W may be filled with water through a ſmall vertical pipe, without receiving any from the ſyphon, to which it need only be connected by an air pipe.

By the ſame principle water may be raiſed to a conſiderable height above the ſyphon. To return, however, to our former ſubject, the water in the pipe will completely prevent the admiſſion of air into the receptacle of raiſed water: it ſerves the purpoſe of packing or leathering.

The water in the veſſel A W, will never reach the higher end of this pipe, becauſe it cannot riſe above the air pipe connected with the ſyphon.

If ſuch a long pipe ſhould be inconvenient, a much ſhorter one may be uſed, if its lower end be immerſed in mercury inſtead of water; it may then alſo be conſiderably wider than the other, and an iron rod of a rather leſs diameter may be placed within it to puſh up the valves. If the valve of the water tube be made light, it will open and ſhut without any aſſiſtance.

The veſſel A W. ſhould not be large; the top part of it ſhould be fixed on with ſcrews, to allow the valves to be eaſily taken out and repaired. Two cocks in the ſmall pipes oppoſite A and W will then be uſeful to prevent the depreſſion of the columns in the branches of the ſyphon.

It will be always adviſeable to make the length of the deſcending branch of the ſyphon exceed that of the aſcending one, as much as circumſtances will admit, and to let the loweſt part of it be made of a conical divergent form: the velocity of the current will be thus encreaſed, the veſſel will be ſooner filled, and the depreſſion of the columns will be leſs liable to happen from very ſlight imperfections in the valves.

I am, SIR,

Your humble ſervant,

Dalton, Feb. 21, 1801. WILLIAM CLOSE.

CORRECTION.

There was a ſlight omiſſion made in one paragraph of the letter before this, which may eaſily be corrected. Pleaſe to look for the following words at the beginning, page 547.

If the connexion between the veſſel and ſyphon was made by two tubes *inſerted into the ſhorter branch one immediately above the other,* the end of the *lower* turned againſt the current, and the end of the *higher* turned from it, the ſtream, &c. Inſert the words with the line under them.

V.

On the Doctrine of Heat; particularly with regard to the States of denſe and elaſtic Fluidity in Bodies. In a Letter from Mr. Joseph Astley.

To Mr. NICHOLSON.

SIR,

THAT the ſolid, liquid, and aeriform modifications of bodies depend on nothing more than the different proportions of caloric with which they are directly combined, is an opinion ſo attractive, from its apparent ſimplicity, and ſo cloſely aſſociated with our moſt elementary notions reſpecting the phyſical laws of nature, that a degree of prejudice may perhaps be excited in many by the mere ſuggeſtion of a doubt upon the ſubject. Thoſe intimate with chemical theories have learned, however, that the apparent ſimplicity of a doctrine is as frequently the reſult of an imperfect as of intire comprehenſion of the ſubject which it regards, and that our ſolicitude to eſtabliſh the conformity of particular phenomena too often leads us to admit inconſiſtencies into our general principles of phyſical economy. The new lights which have been lately thrown on ſeveral of the phenomena in which evaporation is concerned, particularly on thoſe of meteorology, have been ſufficient to create

create ſuſpicion in the minds of many who have conſidered them attentively, that the doctrine which affirms this proceſs to conſiſt merely in a direct combination of caloric is not to be admitted without limitations. They have regarded theſe limitations, however, as only partial exceptions to a general law: and my preſent purpoſe is to ſhow that, even in its moſt general acceptation, the doctrine is open to very weighty objections.

I ſhall endeavour to explain myſelf in examining what grounds we have for the ſuppoſed analogy between vaporization and liquefaction; what indications we have as to the real action of caloric in vaporization; and whether the probability both of analogy and of facts ſhould not incline us rather to refer the vaporous modification of bodies to the claſs of chemical decompoſitions.

The moſt important and characteriſtic circumſtance attending the phenomenon of liquefaction is the uniformity of temperature at which it takes place. And it is here that a moſt obvious failure in the ſuppoſed analogy of vaporization preſents itſelf. It is not ſufficient for this analogy that in ſimilar circumſtances water, like ice, cannot preſerve its form beyond a certain temperature. It would require alſo that water, like ice, ſhould not be capable of changing its form below that temperature. So little do we find this the caſe, that many circumſtances connected with evaporation give us reaſon to conclude that, when not impeded by mechanical reſiſtance, there is ſcarcely any temperature at which it may not take place; and that, without any variation but as to the rapidity with which it is performed. I ſhall enter into no examination here of the attempt to reſcue the analogy in queſtion from this objection by referring what is called the ſpontaneous evaporation of liquids to the ſolvent action of air, becauſe in as much as reſpects the principle of the objection, the phenomenon is equally evident whether air be preſent or not.

But to come more directly to the point, let us attend to the circumſtance by which ſuppoſed analogy between liquefaction and evaporation is deemed peculiarly expreſſed. This is the appearance in each caſe of a quantity of combined caloric. It will be ſeen in the ſequel that I am far from being diſpoſed to deny this combination in the phenomena of evaporation. All that I have to object to is the idea of its indicating a correſpondence of principle between theſe phenomena and thoſe of liquefaction. The appearance from which the combination of caloric in the former caſes is inferred, are thoſe of its becoming latent in the production, and again ſenſible in the condenſation of vapour. It muſt be obſerved, however, in the firſt place, that the caloric concerned in theſe two phenomena is by no means the ſame. Mr. Watt found that vapour produced in vacuo at a temperature 70°, gave out, on its condenſation, a quantity of caloric equal to 1200° or 1300°. And, as it muſt be needleſs to ſhow that ſuch a quantity could not have exiſted in the vapour on its original production, it is evident that this at leaſt cannot be regarded as having any eſſential relation to its conſtitution.

Let us ſee how far the caloric which is actually preſent, and becomes latent in the formation of vapour, can be underſtood to denote a conformity between this modification and that of liquefaction. That this is not implied by the mere circumſtance itſelf, is evident

evident from the variety of caſes in which we ſee it to take place during chemical changes of the moſt different kinds, and even in ſome modifications which muſt be accounted purely mechanical. Before, therefore, the latent combination of caloric can ſerve as any index of phyſical analogy between two phenomena, there muſt be ſhown to exiſt a correſpondence in the other changes with which it is reſpectively aſſociated. So little do we find of this correſpondence here, however, that the changes moſt intimately and eſſentially connected with it are in the one caſe an expanſion, and in the other a diminution of bulk.

Nor do we find the compariſon at all better ſupported, if, attributing the latent abſorption of caloric to a previous change of capacity, we attempt to ſubſtitute this as an indication of the analogy in queſtion*. This change of capacity itſelf requires, no leſs than the abſorption of caloric, the ſuppoſition of a previous modification, and if, with the generality of authors, we refer this modification to change of form, the ſame difficulty occurs as before, viz. that of conceiving how a ſimilar change of form can be attended with augmentation of volume in one caſe, and diminution of it in the other. Beſides that to aſcribe the change of capacity in vaporization and liquefaction to change of form, inſtead of bringing us to the cauſe of the phenomena, is only leading us back to the phenomena themſelves, and thus returning to the ſame point from which we ſet out. The only way by which it has been attempted to eſcape from this circle has been by reſting the immediate occaſion of the phenomena on the change of temperature: and how very little this is to be truſted to as an index of analogy between vaporization and liquefaction, has already been ſeen.

But we may eaſily ſatisfy ourſelves of the fallacy of the ſuppoſed analogy between theſe two modifications of any body, if we attempt to infer from it the correſpondence of the changes which ſuch a body ſhould exhibit with reſpect to both in conſequence of the ſame changes in the conſtitution. Thus, if the analogy were to be depended on, the ſame change of conſtitution by which the freezing point of water becomes lowered, ſhould lower alſo its boiling point. I need not point out how groſsly ſuch inferences would miſlead us. And if the ſolvent action of the air may ſerve to ſcreen this analogy from the abſurdity of ſome of theſe implications, it has at the ſame time introduced other inconſiſtencies which are equally groſs. According to this, the influence of air and of caloric in producing evaporation being ſimilar, we ſhould expect that when the action of the former is added to that of the latter, the evaporation ſhould be more ſpeedy than when the latter exerts itſelf alone. In other words, that the ſame quantity of evaporation ſhould be produced

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* How little information we derive, however, from being told that the abſorption of caloric is a conſequence of increaſe of capacity, is eaſily ſeen by attending to the meaning of the term. An increaſe of capacity is a power of combining with a greater proportion of caloric at the ſame temperature. The whole ſecret of the explanation therefore conſiſts in this: that ſolid bodies, in becoming liquid, combine with a greater proportion of caloric at the ſame temperature, becauſe they thereby *acquire the power* of combining with a greater proportion of caloric at the ſame temperature.

produced in the preſence of air by a leſs quantity of caloric than is required in its abſence. The contradiction of this, by experiment, is explained by telling us the air, being after a ſhort time ſaturated with water, acts merely by its mechanical preſſure, and that to overcome this preſſure the vapour requires the combination of a greater proportion of caloric. Yet, after all, we find that vapour in the formation of which this exceſſive quantity of caloric has been employed, manifeſts conſiderably leſs caloric in its condenſation than vapour produced in vacuo with a heat of 70°.

In what, then, have we reaſon to believe the action of caloric in evaporation to conſiſt? From what has been already ſaid, and particularly from the experiment juſt noticed, we have, I think, reaſon to conclude that no known phenomena authorize us to refer it to any direct ſpecific combination. Were this ſuppoſed, it would ſurely be expected that we ſhould aſſign ſome determinate proportion to ſuch combination. If this cannot be done, if we can in no reſpect ſo proportion the caloric applied that it ſhall uniformly produce the ſame modification, however we may think ourſelves able to account for this difference of effect, our explanation muſt inevitably imply that there is ſome condition more eſſential to evaporation than the union of caloric. Thus, as in the explanation of the impediment which evaporation ſuſtains from the preſſure of the air, it cannot be pretended that this acts by preventing in the firſt inſtance the direct combination of caloric, but by preventing this combination from producing the effects which would otherwiſe accompany it, we are fully authorized to conclude that the direct combination of caloric is not, as the ordinary theory of this phenomenon, and as all analogy drawn from the phenomena of liquefaction would imply, the determinate ſpecific cauſe of evaporation.

It becomes intereſting, therefore, to inveſtigate that more eſſential condition of this phenomenon, which the common character of all the circumſtances which modify it indicates to be *expanſion:* and, ſince we find that it is by the degree in which this condition is induced, and not by the quantity of caloric actually preſent that the agency of the latter in evaporation is to be meaſured, and ſince we find that expanſion has a conſtant tendency, even independently of caloric, to cauſe evaporation, we muſt expect to derive, from tracing the modifications which this condition implies, our moſt comprehenſive ideas of the principles of theſe phenomena.

Viewing the expanſion of a body in its moſt ſimple light, merely as indicating a diminution in the attractive tendencies of its particles, there are two ways in which our knowledge of theſe tendencies ſuggeſts to us that it may be effected: one, by a change in the ſtructure or configuration of the particles themſelves; the other, by a change in the mutual

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* The influence of the air has indeed been an equal ſubject of perplexity with thoſe who have attempted to aſſociate it with the agency of caloric in evaporation, and with thoſe who have excluded it from this aſſociation. If we would believe the moſt celebrated advocate of the latter opinion, it is alike to the mechanical influence of the air (for he denies it to have any influence but mechanical) that we are to attribute the liquefaction of vapour at one degree of preſſure, and its power of reſiſting liquefaction when ſubjected to a greater. See De Luc's Theheocalagie, Vol. I. p. 20, &c.

mutual relation of the principles of which theſe particles are conſtituted. And this conſideration inculcates an important diſtinction between two proceſſes, which have been hitherto, if not altogether confounded, ſeparated at leaſt by a diſtinction worſe than none: I mean ſublimation and vaporization. The ſlighteſt attention will ſhow the abſurdity (ſuppoſing it the ſame in both) of calling by different names the action of caloric upon bodies as they are in a ſolid or in a liquid ſtate. Nor would ſo futile a diſtinction probably ever have occurred, had not a real ſpecific difference of action in the two caſes been obſerved to be generally denoted. In the former, nothing but a minute diviſion of the parts (which ſtill retained in other reſpects all their former characters) was obſervable: whilſt, in the latter, there was not merely a comminution of parts, but a total diverſity of appearance. If we ſuppoſe the action of caloric to be in the former proceſs, which comprehends all the ſimpleſt bodies, that of merely *changing the ſtructure and configuration of the particles*, whilſt in the latter it exerts itſelf in *changing the mutual relation of the conſtituent principles* themſelves, ſhall we not have at once an explanation of the phyſical nature of this diſtinction, and a ſatisfactory clue to ſome of the moſt important phenomena of nature?

Before we advert to the evidence of facts, let us ſee how far analogy would carry us in making ſuch a change in the conſtituent principles of compound bodies as a probable account of the action of caloric in their vaporization. As the ordinary proceſſes of diſtillation ſo frequently exhibit to us the ſeparation of the parts of chemical compounds from each other, according to their reſpective degrees of volatility, and as we can underſtand nothing by the different volatility of ſubſtances but their different affinities for caloric, it is obvious that as far as the firſt general dictate of analogy is to be truſted, we are decidedly authorized to expect that all bodies, whoſe conſtituent parts poſſeſs different affinities for caloric, ſhould be liable to the ſame decompoſition. And ſuch an inference can be objected to only on the ground of ſome palpable inconſiſtency with the evidence of facts themſelves. The conſtruction hitherto put upon this evidence has been, that bodies which, in common language, preſerve their conſtitution entire in evaporation, combine with caloric not according to the diſcrete affinities of their ſeveral parts, but according to their aggregate ſpecific affinity for it. How far this aſſertion is warranted by the direct teſtimony of fact will be examined preſently. In the mean time it may be remarked that not only the general analogy juſt noticed, but all the individual analogies alſo, which are any way concerned, unite in contradicting it. According to this aſſertion, the conſtituent affinity of the parts of water (for inſtance) for each other ſhould be ſo great as entirely to ſuperſede the difference of their reſpective affinities for caloric. But this is not at all conſiſtent with the circumſtances which occur in its ſynthetical formation; where foreign aid is always neceſſary to eſtabliſh the ſuperiority of the former; and where the caloric conſtantly given out during the union of the parts gives us ſtrong reaſon to preſume that its re-combination muſt indicate their diſſolution. Beſides, the mere circumſtance of expanſion accompanying the combination of caloric ſufficiently indicates its tendency to diſſolve in greater or leſs degree not only the coheſive but the conſtituent affinities of compound bodies*, In fact, if we conſider attentively, we ſhall find that we can form no accurate conception of a change produced in the former which ſhall not affect proportionally the latter. As the coheſive affinity of a compound body owes its exiſtence to the conſtituent affinities of its parts, the idea of a diminution in the former includes in itſelf the idea of a correſpondent diminution in the latter.

Thus, then, we find that every ſuggeſtion of analogy would lead us, *à priori*, to expect that a partial or complete diſunion of parts ſhould always attend the combination of compound bodies with caloric; and, conſequently, that their evaporation is to be regarded as a true decompoſition. In ſubmitting it to the direct teſtimony of facts, however, the phenomena of evaporation will be deemed in two reſpects irreconcilable with the theory which theſe analogies would ſuggeſt. The different parts of ſuch bodies, it will be ſaid, do not eſcape in that ſucceſſion which their different degrees of volatility would thus indicate: nor does their appearance after the proceſs accord with the idea of their decompoſition. With reſpect to the latter, however, to infer that there is no decompoſition, becauſe the ſubſtance collected after evaporation preſents the ſame appearances which it had before, is not an admiſſible deduction: ſince the vapour cannot be ſo collected without having its caloric attached; and as the theory ſuppoſes the combination of caloric competent to the diſunion, it implies equally that the ſeparation of caloric will be effective of the re-union of the component principles.

But there are alſo poſitive facts to be oppoſed to the objection. Numerous inſtances of evaporation occur in which we do ſee evident marks of decompoſition, and of a decompoſition which we can aſcribe to nothing but the combination of caloric. Dr. Prieſtley's experiments on the generation of air from water, viewed independently of the theory which he connected with them, afford ſtrong illuſtrations of it. The ready diſunion of the parts of vapour by many ſubſtances incapable of decompoſing it in the ſtate of water; as well as the indications which Count Rumford has noticed of the decompoſition of water in operations of cooking, muſt be admitted as at leaſt preſumptive proofs. And the combuſtive qualities of the vapour of the eolipile, and ſeveral phenomena of a ſimilar kind, preſent evidences of decompoſition ſufficiently ſatisfactory †. Nor are the evidences of a ſimilar

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* I did not know till after I had written the above, that Fourcroy, in ſpeaking of the capacity of hydrogen for caloric, had expreſſed himſelf in the following manner: " It even appears that this baſe, though " combined with oxygen in water, ſtill poſſeſſes the property of abſorbing a great deal of heat. And that " it is this property which renders vapour ſpecifically lighter than air." Diſcourſe on Modern Chemiſtry.

† To theſe may be added the very important inferences to be drawn from meteorological phenomena, reſpecting the decompoſition of water in what is called ſpontaneous evaporation. I muſt acknowledge myſelf indebted for much light on theſe phenomena to a very ingenious paper on the ſubject, in the Eſſays by a Society at Exeter, and I am diſpoſed to differ from the author only on thoſe points in which he ſeems to differ from himſelf. The true tenor of his arguments is certainly to prove that water exiſts in a decompoſed ſtate

ſimilar decompoſition in the evaporation of other bodies leſs conſpicuous. The vapour of ether, according to Lavoiſier's experiments (Mem. de l'Ac. des Sciences, 1780) becomes, by the mere admiſſion of common air, a permanently elaſtic fluid, and in this ſtate deflagrates on the application of flame, like a mixture of oxygen and hydrogen gaſes. The very intereſting experiments of the Dutch chemiſts on the decompoſition of alcohol and ether, in the paſſage of their vapours through tubes of glaſs or of porcelain, may be conſidered as affording evidence to the ſame general purport.

In anſwer to the other part of the objection, that if ſubſtances be conſidered as decompoſed in evaporation, their parts ſhould eſcape ſucceſſively, according to their different affinities for caloric, and not altogether, as is the caſe, it might be ſufficient to obſerve, that the eſcape of one from the remainder in an uncombined ſtate, all our obſervations of other phenomena teach us that this ſtate muſt neceſſarily be that of an elaſtic gas, and therefore the aeriform converſion of both parts muſt take place together. But we may go further, and affirm that it is to the phenomena, as thus explained, that we muſt have recourſe, in order to underſtand one of the moſt important and diſtinguiſhing circumſtances by which the proceſs of evaporation is marked. I mean the generation of cold. And if this can be ſhewn, the theory which I have been attempting to explain will have ſomething more than analogy for its ſupport.

ſtate in the atmoſphere. Yet he ſeems to have formed his idea of it as " a permanent gas, unchangeable, " like other *compounds*, except by an elective attraction ſuperior to that which unites its ingredients," page 368. And in this light we cannot wonder at his expreſſing his inability to explain " what occaſions " the formation of water in the air, to admit of its falling in rain at any time." Without profeſſing to enter at all into the *rationale* of this phenomenon, I may obſerve that the principal difficulties with which it ſeems embarraſſed in his theory will be removed, if we conſider water to enter into the atmoſphere, not as a *permanent compound gas*, but as decompoſed into the two original permanent gaſes belonging to its conſtitution. Theſe, having different ſpecific gravities in the atmoſphere, muſt be ſuppoſed to occupy different regions in the atmoſphere, which, however, will vary, and occaſionally intermingle with each other by the operation of different circumſtances. In ſuch caſes the affinity between their reſpective baſes, when aided by any occurrence diſpoſing them to part with the caloric which ſeparates them, may be eaſily ſuppoſed to occaſion a re-union.

According to this author's idea of the air proceeding from the evaporation of water being a permanent gas, to what can he aſſign its ſpecific diſtinction from water itſelf? Will he ſay in its combination with caloric? But he has ſhown that it is both producible without the aſſiſtance and irreſolvible by the ſubtraction of caloric. Beſides, we have poſitive evidence that this gas, in its original ſtate, is not permanent. The vapour immediately riſing from boiling water is this gas in its original ſtate, tranſparent and elaſtic. But it will not be ſaid that this is incondenſable by cold. It becomes permanent only by the local ſeparation of its parts. And if we admit this ſeparation, I think we not only ſee the arguments which the author has brought together to illuſtrate the modifications of the atmoſphere by evaporation in a ſtronger light, but we ſee the analogy of ſeveral other modifications with the ſame general ſource. We ſee in particular why a greater quantity of oxygen is obſerved in the atmoſphere during dry ſerene weather than during the prevalence of clouds, fogs, or rains; and, connected with this, why the air of lower regions has been always found charged with more oxygen than that of more elevated ones.

It requires little attention to diſcern that the fancied analogy between the cold which is produced in evaporation and in liquefaction, has in reality nothing for its ſupport but the generality of occurrence in each phenomenon. The cold which takes place in liquefaction is a mere conſequence of the ſubject of the liquefaction being ſurrounded by bodies of a higher temperature than its own, whoſe caloric therefore is in ſome part detached, on the general principle of its tendency to equilibrium *. In evaporation, on the contrary, the circumſtances are nearly the reverſe. The ſubject of this proceſs has in general a higher temperature than that of the ſubſtances around it. The tendency to equilibrium between theſe will not account therefore, as in the caſe of liquefaction, for the cold which takes place. We muſt look forward for the occaſion of it to the product of the operation.

We ſhall be aſſiſted in our examination of this by premiſing, that we have no inſtance in the whole phyſical hiſtory of nature of a ſubſtance producing cold in conſequence of any difference between its capacity and that of the ſurrounding bodies, unleſs it have at the ſame time a temperature actually inferior to their's. I have already ſhown that the cold produced in the liquefaction of ice is to be referred to this: and we may eaſily ſatisfy ourſelves that in no frigorific phenomena whatever have we reaſon to aſcribe it to any thing elſe. Thus in mixtures of ſalts and ſnow, it is not the change of capacity which is the proximate cauſe of the attraction of caloric from ſurrounding bodies. It is merely the remote cauſe of it by the reduction which it occaſions in the temperature. By this change of temperature only are we able to judge of the nature and extent of the change of capacity, and the accuracy of this idea is clearly demonſtrated by the uniformity of its correſpondence with the change which we find produced in the freezing point of the new compound.

From theſe conſiderations we have a right therefore to infer, that the vapour of water, ether, &c. cannot produce cold in the ſurrounding bodies without having a temperature actually inferior to their's. But we know that the evaporation of theſe ſubſtances produces cold when the temperature of the ſurrounding bodies is greatly inferior to that which is univerſally deemed eſſential to the ſubſiſtence of theſe vapours. We know that, were it poſſible for the vapours of water, or of ether, to ſubſiſt at the temperature here implied, inſtead of ſubtracting caloric from ſuch bodies, they would impart their own to them and undergo condenſation. We know alſo that, if it were poſſible for the vapour of water (for inſtance) to ſubſiſt at a temperature as greatly inferior to that of 212°, it would have a much greater affinity for the caloric of the evaporating water than the water itſelf: of courſe that, whilſt ſuch vapour was preſent, the evaporation could not proceed, which is contrary to fact. We are inevitably led to conclude, therefore, that the cold here produced, is not produced by the vapour (underſtanding the word in its ordinary acceptation) of the ſubject of the operation. Nor do I conceive how it is to be accounted for, but by aſcribing

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* The cold in this caſe is inaccurately ſaid to be the conſequence of change of capacity. This change of capacity merely determines the extent to which the proximate cauſe, the inequality of temperature, ſhall operate. This diſtinction, though as obvious, is not ſufficiently attended to.

it to that product of the decompoſition of the evaporating ſubſtance, which, having the leaſt affinity for caloric, is rendered gaſeous by the ſeparation of the reſt, before it has acquired ſufficient caloric to ſatisfy its reſumed capacity.

I might have added ſeveral facts of inferior importance to the teſtimony of thoſe already noticed, but I have thought it ſufficient to ſhow that the univerſal dictate of analogy, as to the combination of caloric with ſubſtances whoſe component parts have different degrees of volatility, is ſupported by the evidence of the moſt important phenomena: and that we have reaſon, therefore, to conclude, that in every ſuch caſe a reſumption more or leſs complete of the diſcrete affinities of the conſtituent parts for caloric muſt take place, and that in moſt inſtances, at leaſt of evaporation, this reſumption may be conſidered as entire, and the decompoſition complete. It is certainly not inconceivable that there may be bodies, the conſtituent affinities of whoſe parts may be ſo great as to overcome the tendency of their different affinities for caloric with any heat we can apply. It is not probable, however, that ſuch exiſt among the ſubſtances with whoſe conſtitution we are hitherto acquainted.

I am, SIR,
Your's reſpectfully,
JOSEPH ASTLEY.

Edinburgh, Jan. 31, 1801.

VI.

Account of an Electrometer. By CIT. C. L. CADET *.

THIS author, according to the uſeful cuſtom of the French writers, gives a ſhort prefatory account of the electrometers of *Dufay* with threads, *Henley* with the quadrant and index, *Canton* by the ſucceſſive diſcharge of a jar by ſparks, *Lane* by the length of the ſpark, *De Sauſſure* by the balls of *Canton*, and *Coulomb* by the method of torſion; to which he might have added the floating inſtrument of Nollet, the meaſure of repulſion by weight by *A. Brook*, the extent of electric atmoſpheres determined by the luminous point, by *Magiotto* and *Van Marum*, the inclination of a thread at a given diſtance by the ſame philoſophers, and the gold leaf electrometer by *Bennet*. He has purpoſed to conſtruct an inſtrument capable of application to the meaſurement of ſtrong as well as weak electricities, without requiring any attention to friction or weight. Of this I ſhall give the deſcription and account in his own words:

"Fig. I. Plate 11. In a glaſs tube A, 18 or 20 inches long, is incloſed another ſhorter tube X, ſealed at both ends. This tube contains a graduated ſcale: one of the ends of theſe two

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* Abridged from the author's paper in the Annales de Chimie XXXVII. 68.

two tubes is cemented in a handle of turned wood, C, by which it is held in the hand; the other end is cloſed by a braſs cap, D; the diſtance between the extremities of the ſmall tube and that of the large one is filled with red wax; B, B, on the cap D is ſcrewed at pleaſure, either a ring E, or a braſs hook F. The ring is uſed for applying the inſtrument to the ball of a conductor, and the hook when it is hung to a ring: on the cap D is a braſs ſtem G, terminating by a knob. This ſtem is bended, and the extremity of its knob muſt be directly beneath the line with which the graduated ſcale of the ſmall tube commences.

Round the large tube is a braſs ring H, half of which extends to the length of twelve or fifteen lines, in the form of a half tube P, applied againſt the ſides of the tube. This gutter ſerves to mark the degrees, by ſliding along the graduated ſcale by means of a button beneath, I. On the ring H is fixed one of the ſmall electrometers invented by Sauſſure, K, K, which is ſurmounted by a ſtem V, on which ſtem is fixed at pleaſure either a point L, or a ball M, of the ſame ſize as that which terminates the ſtem G, oppoſite which it is placed. The extremity of this point or ball muſt be placed immediately over the extremity of the half tube or ſcale P, and horizontally to the centre of the ball, which terminates the ſtem G.

At the top of Sauſſure's electrometer is a ſmall ring N, which ſerves to connect it with the chain Z, when required.

To explain the uſe of this inſtrument by a ſingle experiment, I charge a Leyden jar, till the ſpontaneous overflowing announces it to be ſaturated. I then place the ring E on the knob of this bottle, and cauſe the electrometer of Sauſſure, armed with its point, to ſlide towards it. I remark the degree at which the divergence of the thread ſtream commences, and at that inſtant I ſuppreſs the point, and adapt in its place the ball M. I continue to advance the electrometer of Sauſſure till the electric preſſure of the atmoſphere in the jar cauſes the threads to diverge. I again obſerve the degrees, replace the point L, and cloſe the ſhutters of my chamber; I then continue to advance the electrometer till the luminous point appears, which again affords new degrees. Laſtly, I replace the ball M, and fix the chain Z to the ſmall ring N; I cauſe it to communicate with the exterior coating of the jar, and advance the electrometer till the exploſion takes place. Then comparing the different degrees, I aſcertain the comparative difference between the reſpective methods.

As ſoon as theſe relative proportions have been once accurately aſcertained by attentive obſervations, one of thoſe methods alone will be ſufficient for meaſuring the intenſity of electricity, and in fact, if the body intended to be ſubmitted to examination be little charged with the electric fluid, the diverging of the threads by means of the point, will fix the limits of the electric atmoſphere: if it be more, the preſſure of the atmoſphere on the ball M, which is ſubſtituted for the point, will indicate this quantity. In ſhort, if the body be loaded with a conſiderable maſs of electric matter, it will be ſhown by the luminous point. If a Leyden jar, inſtead of being poſitively, is negatively electrified, the point indicates it at the ſame time that it meaſures the electric atmoſphere; for inſtead of a luminous point, a ſtar will be obſerved upon the ball of the jar, and another at the end of the point.

Let us now apply this electrometer to uſeful obſervations:

In order to connect the idea of a determinate quantity of fluid to each degree of the electrometer, it is neceſſary to compare theſe degrees with the known quantities. Suppoſe for inſtance I have a jar, the coating of which is ſix inches ſquare; I electrify it till a ſpontaneous diſcharge takes place, and remark, by means of Henley's electrometer, at what degree this diſcharge is effected. I again electrify the jar, till it is nearly ſaturated, and meaſuring with my electrometer, I obſerve, that the luminous point appears for inſtance, at two degrees; I then ſay, that when my electrometer, applied to an electrified body, marks two degrees, the body contains ſix inches ſquare of electricity. I repeat this experiment with a plate of glaſs, the coating of which is ſeven, eight, ten, or twelve ſquare inches, and I form a ſcale of proportion, which is of the greateſt utility in accurate experiments.

In endeavouring to aſcertain ſome of theſe propoſitions, I made an obſervation which has convinced me of the utility of my electrometer in diſcovering the capacity of electric apparatus. Having taken a jar from an electric battery, I electrified it, and meaſured it with a point which I paſſed along a ſtring of ſilk; on obſerving the diſtance at which the luminous point appeared, I joined this jar to another of the ſame ſize, and imagined that by doubling the quantity of matter, the meaſure I had taken would alſo be doubled; on the contrary, however, the latter meaſure was not more than about one third of the former: I then added a third bottle, and ſtill obtained nearly the ſame reſult; whence the following propoſition appears to be eſtabliſhed; namely, that the extent of the electric atmoſphere is in an inverſe ratio to the quantity of fluid accumulated. Another obſervation which I have ſeveral times made, on meaſuring the electric atmoſphere of a conductor, is, that the limits of this atmoſphere form an elliptic figure around the body, nearly ſimilar to that repreſented at Fig. 2.

This, doubtleſs, ariſes from the electrified body ſuſpended in a chamber, being nearer to the earth than the cieling; but it would be a curious experiment to meaſure it at an equal diſtance from every attracting body, in order to obſerve whether the fluid has not really a tendency to deſcend towards the earth, rather than in any other direction. It is my intention to repeat this experiment, as I conſider it of great importance to aſcertain whether electricity gravitates towards the globe.

From theſe firſt attempts, I conceive my electrometer would be well adapted for meaſuring the abſolute capacity of Leyden jars, and alſo their capacity with regard to their ſize, or to the quality of the glaſs of which they are conſtructed; for the latter, by its greater or leſs denſity, abſorbs a greater or leſs quantity of fluid.

VII.

Inquiries relative to the Experiments of Cit. Prevoſt, of Geneva [*], *on the expanſive Force of Odoriferous Emanations, and of Cit. Venturi, of Modena* [†], *upon the Motion of Camphor upon Water. By Dr. J.* CARRADORI DEL PRATO [‡].

AS I have been employed perhaps more than any other philoſopher, in obſerving the curious motions of camphor, and all the other oleaginous ſubſtances, as well fluid as ſolid, when caſt on the ſurface of water, and as I flatter myſelf that I have given a true explanation [§] of theſe phenomena, which had in vain been attempted by Romieu, Volta, Brugnatelli, Lichtenberg, and other reſpectable philoſophers, I was of opinion that it was proper for me to repeat the experiments of Prevoſt and Venturi on this ſubject. I have juſt done this, and find that thoſe of Prevoſt do not poſſeſs that novelty which was at firſt imagined; but on the contrary, the whole, and particularly thoſe of Venturi, tend ſtill farther to eſtabliſh the truth of my explanation.

I have proved [‖], that all the fixed and volatile oils, as well as the reſins and concrete volatile oils, for inſtance, camphor, move upon water in conſequence of the attraction which every oil, whether volatile or fixed, has for the ſurface of the water, by which it is attracted, and compelled to extend itſelf till the reſpective attraction of a determinate ſurface is ſaturated; and that there are oils, or oily ſubſtances, which have a greater attraction than others for the ſurface of this liquid. This has been preciſely confirmed by the new obſervations of Prevoſt and Venturi.

The facts related by Cit. Prevoſt are certainly true, as has been previouſly remarked by Cit. Brongniart, but the conſequences which he conceived to reſult from them are not equally accurate.

It is true, as Prevoſt has aſſerted, that if fragments of a concrete odoriferous ſubſtance, for inſtance ſmall pieces of camphor or particles imbued with an odoriferous liquor, be placed upon a plate, or on a piece of wetted glaſs, that is to ſay covered with the ſlighteſt film of water, the latter will be immediately ſeen to retreat from thoſe parts where the ſubſtances have fallen or ſettled, as if it were repelled by ſome force proceeding from thoſe bodies.

But it is not true that the retreat of the water from the abovementioned ſmall bodies is cauſed by the atmoſphere of odoriferous emanations, which proceed from them with impetuoſity, and repel whatever they meet with: but the fact is, as Venturi has already obſerved,

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[*] Annales de Chimie, Vol. XXI. p. 254.

[†] Ibid, Vol. XXI, p. 262. See alſo our Journal, Vol. I. p. 153, and 205.

[‡] Tranſlated from the Annales de Chimie XXXVII. 33.

[§] Ann. di Chimica e d'Hiſtoria natur. di Brugnatelli, tom. v.

[‖] See my memoir on this ſubject, in the Opuſc. Scelti di Milano. Ann. di Chimic. et Giorn. fiſico-medico di Brugnatelli, and particularly Vol. XVI. of the laſt mentioned work.

ſerved, that the oil of camphor, as well as all the other oils, have a greater attraction than water for the ſurface of the plate. In fact, Venturi obſerved, and the ſame remark was afterwards made by myſelf, that the part of the plate whence the water has retreated when touched with a ſmall piece of camphor, or any body dipped in volatile oil, remains varniſhed with a coat of very ſubtile oil, which is perceptible by the eye*; whence he rightly concluded, that the water retires to obey its affinity of aggregation, which becomes free from the attraction of the ſurface of the plate as ſoon as that ſurface is occupied by the oil; and this effect happens only becauſe this affinity of aggregation or coheſion has become greater than the attraction of the water to the plate. This is ſo true that the ſame phenomenon always occurs, whatever oil be employed, whether volatile and odoriferous, or not, ſuch as oil of olives or lintſeed. As ſoon as the wet plate is touched with the finger ſlightly dipped in one of theſe oils, the water retreats in a circle, as if it were touched with any odoriferous body.

There is doubtleſs, as Cit. Venturi has obſerved, a ſtronger attraction between the oil and the ſurface of the plate, than between the latter and the water; which is evidently proved by the obſervations: let a drop of olive, or any other fixed oil, be dropped on a ſmall plate of porcelane, glaſs, or any kind of glazed earth; and afterwards pour ſome drops of water from a certain height, which on uniting together quickly flow like a ſmall torrent over the oil; it will be ſeen, that notwithſtanding the force with which this ſmall torrent ſtrikes the drop of oil, it is not capable of diſplacing it and carrying it off, but paſſes over while the oil adheres to the ſurface of the plate, like a varniſh, which the impetuous and continued paſſage of the torrent cannot remove. It is well known that plates, or other culinary utenſils, cannot be cleaned by water when ſoiled by oil or fat, as it has not force enough to detach the oily particles from their ſurface, though it can produce this effect with earth, and diſſolve extractive ſubſtances.

If a ſmall piece of camphor be fixed upon a plate or other veſſel, and as much water be added as will ſlightly cover the camphor, the water, as Prevoſt has obſerved, can never attain to a level above the camphor, but forms a ſmall cavity or gulph, in the figure of an inverted cone, in which the ſummit of the piece of camphor is always perceptible; this evidently proves, according to him, that there proceeds from the odoriferous body an elaſtic fluid of ſufficient force to repel the water that ſurrounds it, which according to the laws of hydroſtatics, ought otherwiſe to come in contact with and cover the camphor.

Cit. Venturi has likewiſe obſerved this phenomenon; but he conceives it to be the effect of an oily fluid which proceeds from the camphor, and prevents the adheſion of the water with the piece of camphor, and with the columns of water that ſurround it.

For my own part, after the moſt accurate examination, I am of opinion, that the retreat of the water is neither attributable to the emanation of an elaſtic fluid, to an odoriferous vapour, as Prevoſt has ſuppoſed, nor to the obſtacle that prevents the adheſion of

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* Loco citato, p. 207.

water with the camphor, produced by a ſubtile oil proceeding from the camphor itſelf; but that it is owing merely to the coheſion between fat or oily bodies and water. Oils poſſeſs much adheſion or attraction of ſurface * with water; but they have no coheſion or affinity of aggregation, nor of compoſition with that liquid. The water which ſurrounds oily or fat bodies, ſuch as a piece of camphor, having no degree of coheſion with that ſubſtance, appears to retreat from it; that is, by abandoning itſelf intirely on its own force of aggregation it is drawn off on all ſides, which cauſes a vacuity round the camphor. If it were an elaſtic odour, or an oily fluid proceeding from the camphor, which produces this phenomenon, the ſame effect would not take place in bodies which have no ſmell. But the phenomenon is the ſame, when inſtead of camphor, ſmall pieces of white wax or hardened ſuet are employed, which are ſubſtances as every one knows, which have ſcarcely any ſmell, containing an oil which is not volatile, and requiring a conſiderable degree of heat to become fuſed.

Hence it appears, the experiments of Prevoſt do not prove that odoriferous bodies are ſurrounded with an atmoſphere poſſeſſing a degree of elaſticity or power capable of impelling the water which ſurrounds them; and that it has been falſely imagined that by this means the emanations from odoriferous bodies might be rendered perceptible.

I did not obſerve, what Prevoſt ſays he remarked, that fragments of camphor about the ſize of a pea, placed on a round piece of gold or ſilver foil floating upon water gave it motion. The camphor itſelf is not ſuſceptible of any ſpontaneous motion, nor can it impart it to light bodies which are on the water, unleſs it immediately touches that liquid. It is the ſame with regard to the oils, whether volatile or fixed; they cannot impart the leaſt degree of motion to the lighteſt body that floats upon water unleſs they touch it. I have made ſeveral trials, by putting drops of ethereal or volatile oils on ſmall pieces of gold or ſilver leaf that floated upon water. The ſlighteſt impreſſion would have cauſed them to move, yet I never obſerved the leaſt motion in any of them. Venturi alſo had the ſame reſult in an experiment with burning or heated camphor, placed on a piece of cork which floated upon water. When the camphor came in contact with the fluid, it imparted a rapid motion to its ſupport, which, on the contrary, became motionleſs when the water was not touched by the camphor. Hence he concluded, that the motion in queſtion does not depend on the projection of volatile particles proceeding from the camphor, but that it is the water alone which excites it by its action.

Prevoſt and Venturi remark, that if the ſurface of the water be touched with oil of olives, or any other volatile oil or odoriferous fluid, when the camphor begins to move, the motions immediately ceaſe. This only tends to confirm the explanation I have given of the motion of the camphor and oleaginous bodies on the ſurface of water. The camphor derives its motion only from the expanſion of an oil, which is attracted by the ſurface

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* See my explanation in *Ann.* di *Chim.* Italiani, tom. V.

of the water *. The reaſon therefore why a drop of oil, whether fixed or volatile, ſtops the motion of the camphor, is, that theſe oils are more ſtrongly attracted by the ſurface of the water, than by that of the camphor; hence they adhere to that ſurface, and by covering it prevent the effect of its attraction for the oil of the camphor, and conſequently prevent the expanſion of that fluid †. This property of preventing the motion of the camphor is not peculiar to the oils; all the cereal and leguminous farinæ, &c. the juices of the tithymalæ, and other lacteous plants, which, as I have obſerved in the memoir before-mentioned, have a great affinity for the ſurface of the water, are inveſted with ſimilar properties.

Cit. Venturi has nearly diſcovered the real explanation of the phenomenon. Having obſerved that a ſmall quantity of oil ſtopped the motion of camphor in a baſon of water, he concluded that this oil, which extends over the water like an imperceptible film, is the cauſe of the ſuſpenſion of motion in the camphor, becauſe, by occupying the ſurface of the water, it prevents the camphor from expanding. Whence he ſeems to be of opinion, that the motion of pieces of camphor upon water proceed from the expanſion of an oily ſubſtance from the camphor upon the water itſelf. This is the explanation which I have given of the above-mentioned, and other ſimilar phenomena, in ſeveral memoirs publiſhed previous to the year 1794.

Cit. Venturi however, appears in ſome degree to follow the opinion of Prevoſt, ſince he aſſerts that the volatility and odoriferous property of camphor are not qualities requiſite to produce the revolving motions which this ſubſtance exhibits when cut in ſmall pieces and caſt upon water; but that its volatility is nevertheleſs neceſſary for the continuance of ſuch revolutions.

The motions of camphor, as I have proved in the memoir ſo often quoted, and which is inſerted in the xxth vol. of the "*Opuſcules choiſis de Milan,*" depend on two cauſes, *viz.* the extenſion of an oily fluid over the ſurface of the water, and on the rapid operation (evaporation) of this fluid itſelf.

When camphor cut in ſmall pieces is thrown on the ſurface of the water, the inſtant they touch this fluid they begin to move in a circular direction, and run with rapidity in different directions; but ſhortly afterwards their motions decreaſe, and the particles collect in ſmall heaps, which, though they have not an abſolute, have nevertheleſs what may be called an inteſtinal motion. All this takes place in conſequence of the conſumption of the particles of camphor which form the ſmall heaps; for according to the former remarks of *Lichtenberg*, as they neceſſarily change their figure, they are compelled almoſt every inſtant, being ſubject to follow the attraction of the maſs, which varies the contacts according to their diverſity of figure. The water, by the attractive power of its ſurface cauſes the extenſion of the volatile oil of the camphor. This oil, by expoſing a great ſu-

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* Opuſc. Scelti di Milano, tom. XX.

† Vide my Memoir "*Sulle' attrazion di ſuperficie,*" Journal des Phyſ. et Ann. di Chimic. Italiani, tom. XVII.

perficies

perficies to the air, evaporates more ſpeedily, and as its evaporation is in proportion to its extent upon the water, the reaſon is evident why the camphor is ſo ſpeedily conſumed, and why by this rapid conſumption the particles are induced to change their figure.

It is true that the oil camphor, which is extended over the water, gradually evaporates or entirely volatiliſes. Of this I have been clearly convinced by the following experiment: after two pieces of camphor which had been in motion on the ſurface of water in a glaſs were conſumed, I expoſed the glaſs to the air, that the oily film of oil camphor which covered the ſurface might evaporate; four hours afterwards I put in other pieces, but did not obſerve the accuſtomed winding motions; I ſoon removed them, and at the expiration of four hours I again threw in freſh pieces; and the motions took place as uſual. Hence it appears, that after four hours the oil of the camphor had not totally evaporated, though in eight hours it had diſappeared, and the water remained perfectly clear.

Venturi, however, was well acquainted with the attraction that exiſts between oil and the ſurface of water; ſince he obſerves that a drop of oil, if it had no affinity with the ſurface of water, would remain upon it without extending; it may therefore be ſaid, that an attraction exiſts between the ſubſtance of oil and the ſurface of water.

The experiments made by Venturi with ſmall columns or ſticks of camphor, plunged half their height perpendicularly in water, and cut horizontally at the level of that fluid, in the ſpace of twenty-four hours, only proves that the greateſt ſolution of camphor is effected exactly where it touches the ſurface of the water; and conſequently they tend to confirm my concluſion, that camphor contains an oil which expands over the ſurface of water in conſequence of an attraction between it and this ſurface; and that it is by ſuch extenſion that it more ſpeedily evaporates. There is not, as he imagined, a greater activity exerted upon the camphor at the points where the air and water meet in contact with this ſubſtance, but only where the camphor touches the ſurface of the water. Air only conduces to this influence upon the phenomena, in proportion as it favours the evaporation of the oil of camphor which is extended over the ſurface of the water. I am well convinced that air here has no influence, and that the whole effect ought to be attributed to the union of the ſurface of the water.

But it appears that Venturi has at length diſcovered his error, ſince he ſays, he obſerved that the ſolution of camphor at the ſurface of water takes place the quicker the more that ſurface is extended. For in ſmall veſſels the cutting of the columns is not ſo quickly effected.

Prevoſt has alſo confirmed the attraction of the ſurface of water; as he obſerved that camphor when in a humid air, or ſubmerſed in water, does not undergo ſo much loſs or conſumption, as it ſuffers when it immediately touches the water; but he attributes this to the water, which favours the evolution of the odoriferous fluid.

The rapid motion of odoriferous bodies upon water, and particularly that of camphor, is prevented after ſome time, as Cit. Prevoſt has obſerved, in conſequence of the attraction of the ſurface of the water for the oil which expands being diminiſhed: nevertheleſs, the ſmall pieces of camphor which collect on the water after their revolving motion has ceaſed, always

always retain a ſlight degree of motion, by which they reſemble groupes of capillary eels, that move ſpontaneouſly, till they are entirely conſumed by evaporation: this motion depends on the ſlow expanſion of the volatile oil of camphor which takes place at the ſurface of the water, in proportion as that which remains is evaporated, as we have already mentioned.

At firſt I entertained doubts whether the motions of the camphor upon water were ſuſpended by the expanſion of olive oil, or any other oleaginous ſubſtance, merely becauſe that of the camphor, being more denſe, did not poſſeſs the faculty of expanding as quick as the liquid oils. To elucidate this point, I made the following experiments: I melted a ſmall quantity of camphor in a ſpoon by the flame of a candle, and poured it upon hot water, on the ſurface of which a drop of olive had previouſly expanded: in its melted ſtate it was not at all capable of extending, and expelling the olive oil; ſtill leſs of giving motion to farinaceous matter. I next threw ſome pieces of the inflamed camphor on the ſurface of cold water; and they moved extremely well. As I ſuppoſed that it was the heat of the inflammation, which by melting the oil of the camphor obliged it to continue to expand, I poured on this water ſome drops of olive oil; the motion of the camphor then immediately ceaſed, and did not recommence, though the ſubſtance continued to burn. Hence it muſt with certainty be concluded, that the oil of camphor has leſs attraction of ſurface for water than olive oil, or ſeveral other oleaginous ſubſtances, ſuch as the flour of wheat.

Camphor, well heated, and fuming, as Venturi has obſerved, being brought near very light bodies floating on the ſurface of water, ſuch, for inſtance, as pieces of gold or ſilver leaf, exerts upon them a repulſive force, exactly the ſame as when it is thrown upon cold water; and, according to him, this effect is owing to the elaſtic fluid, which ſtrikes againſt thoſe bodies: this notion alſo occurred to Prevoſt. For my own part, I think this effect is produced only by an oil which expands upon the water: the oil of camphor, reduced to vapour by heat, when it comes in contact with water, expands on the ſurface, and thus imparts a motion to the bodies it meets with, and ſeems to repel them. In fact, if we attentively obſerve the ſurface of the water around the floating body which is moved, it will be ſeen to be covered with a ſmall very thin and whitiſh film (of varniſh) which is, doubtleſs, a proof that the oil of the camphor, when volatiliſed, is extended over the water, and by cooling depoſits a fine incruſtation.

It is not, therefore, proved by any of theſe experiments that the atmoſphere, which is formed by the odour of the bodies, poſſeſſes, as Prevoſt pretends, an expanſive power capable of being rendered perceptible. Thoſe bodies which are moſt effectually odorant, that is to ſay, ſurrounded, like the others, by an atmoſphere of odoriferous emanations, but which are not mixed with a volatile oil, do not cauſe any repulſion in bodies floating in water, however near they may be brought to each other.

And if the mere emanation and projection of an elaſtic odoriferous fluid were the only cauſes of the motions of camphor and other odoriferous bodies by water, theſe motions would take place on throwing in any one of ſuch bodies; but if, for example, even the ſmalleſt piece of camphor be dropped upon water with a confined ſurface, the motions are not produced; it is therefore evident that they are cauſed by the ſurface of the water alone.

VIII.

Account of the original Invention of the circular Inſtrument for performing Calculations on the Principle of Gunter's Rule made by Clairault le Père, and communicated to the Royal Academy of Sciences in the Year 1727.* (W. N.)

IN the firſt volume of this Journal, page 372, I gave an account, with drawings, of a ſpiral inſtrument conſiſting of a combination of the ſector and Gunter's rule, equivalent to an ordinary rule of forty feet in length, which had been communicated by me to the Royal Society about thirteen years ago, as my own invention. In the paper here referred to, I did juſtice to George Adams the father, who, as I afterwards diſcovered, had contrived and made a very powerful inſtrument of the ſame kind in 1748, ſince preſented to me by his ſon. The ſame principles of candour and juſtice call upon me at preſent to aſcribe the original thought, as well as the actual mechanical conſtruction, to the elder Clairault; of whoſe engraving, publiſhed twenty-one years before the date of Adams's manuſcript, I have here given a copy. An abridged account of his paper will ſhew that he was well aware of the properties and advantages of this apparatus.

He begins by obſerving that the beſt expedients for the ſolution of right lined triangles are reducible to linear conſtruction, or logarithmic calculations; that the firſt, by means of the ſector, requires much ſkill, to produce a moderate degree of accuracy: while the latter demands a certain quantity of reſearch and computation, which render it deſirable to ſimplify the proceſs. With this intention he propoſed to unite theſe two excellent diſcoveries which he performed by placing all the logarithms diſtinctly on the ſurface of a circle of twenty-one inches in diameter, upon concentric circumferences; marking not only the natural numbers as far as ten thouſand, but alſo the ſines of the degrees and minutes. The figure in plate 1 repreſents a twelfth part of the circular inſtrument, and the external ſhaded branches denote the ſector between which the intervals are meaſured. In the year 1716, the author executed his firſt ſyſtem on a ſquare of one foot, filled with parallel lines, conſtituting altogether a rule of 1500 French feet: and it was not till the year 1720 that he thought of the ſpiral form. But as this ſet of lines, when numerous, preſented ſome difficulties, principally, as I ſuppoſe, from the difficulty of marking and counting the intervals upon the blades of the ſector, he gave the preference to concentric circles. I need not enter into the minute particulars of the conſtruction, becauſe this has been already done in our Journal, and in the Philoſophical Tranſactions. He does not overlook the advantage (which alſo occurred to me) of this ſtructure that it affords by the enlargement of the external arcs, a remedy for the ſmallneſs of the differences in certain parts of the logarithmic lines.

The learned writer concludes his paper by giving ſome numerical examples of its uſe and application.

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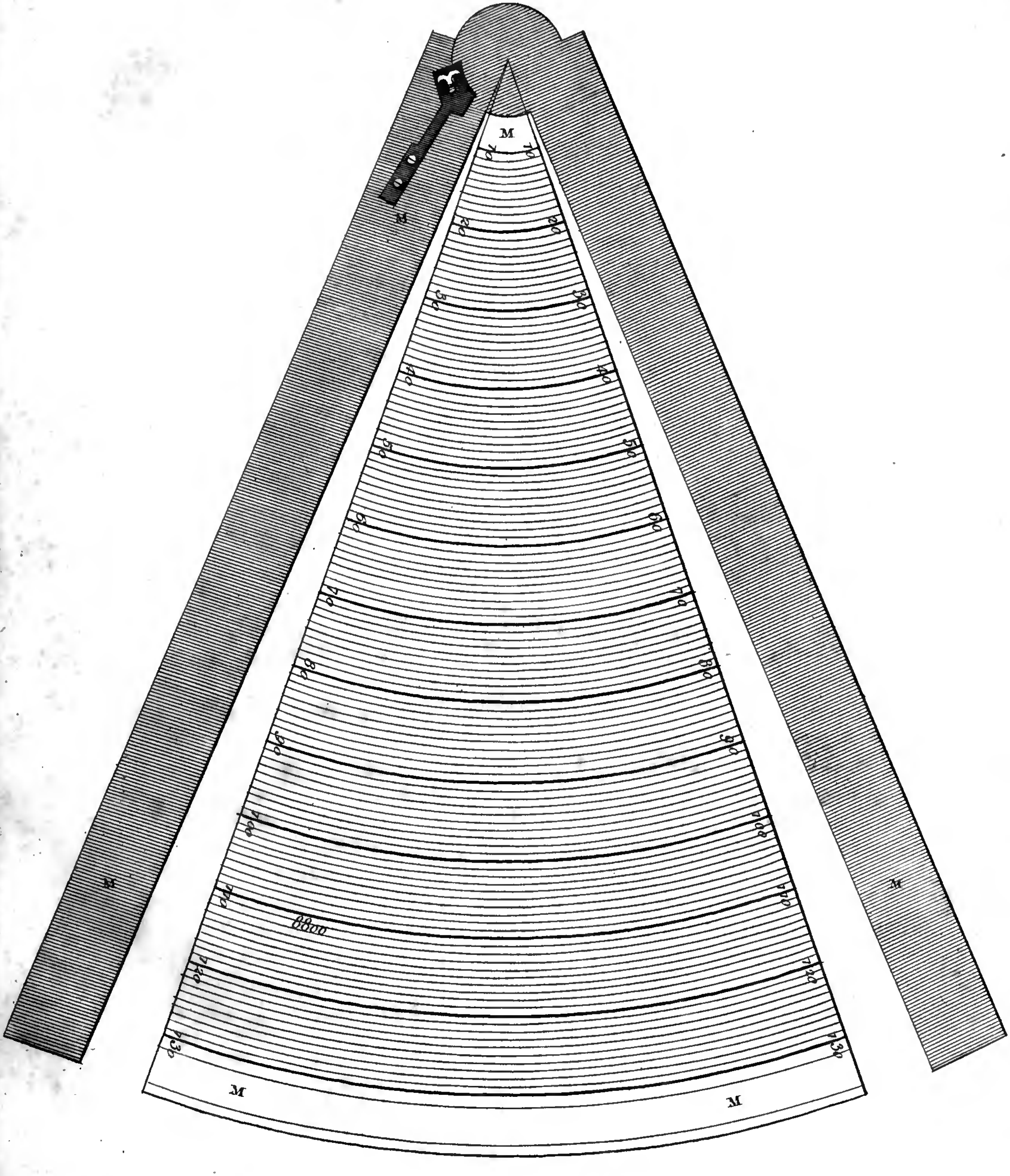
* Machines approvées par l'Acad. Roy. tome V.

M. Clairaut's Instrument for Logarithmic Computation.



Mutlow Sculp.

Electrometer of Cit. Cadet.

Fig. 1.

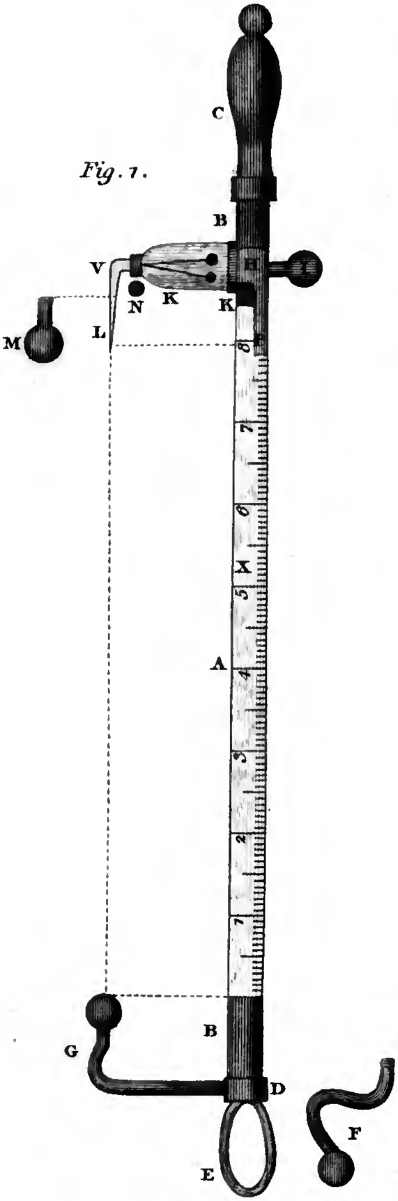
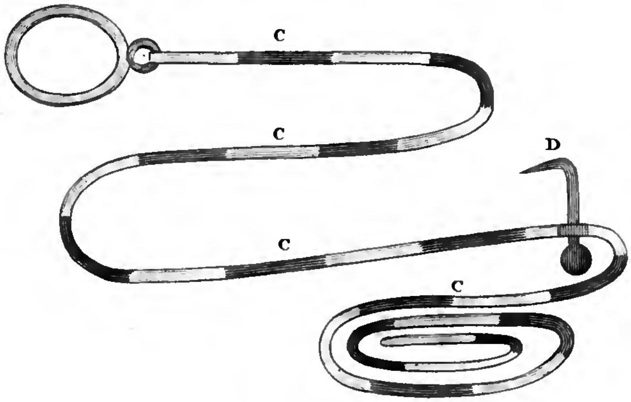


Fig. 3.



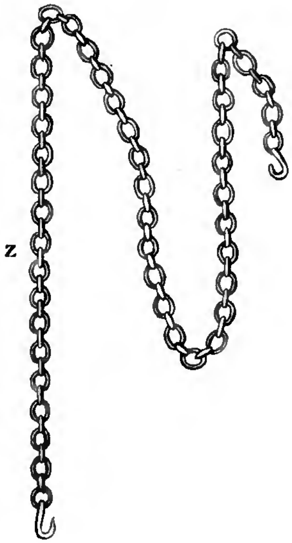
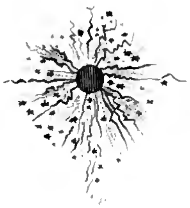


Fig. 2.



Mutlow Sculp.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MAY, 1801.

ARTICLE I.

Obſervations and Experiments on the Galvanic Power. By a Correſpondent.

To Mr. NICHOLSON.

SIR, *Edinburgh.*

THE readineſs with which you admitted a former communication of mine (notwithſtanding that its deficiencies muſt have been apparent to your acuteneſs, and that there are now ſeveral parts of it which later experiments would induce me to correct or explain), has encouraged me to lay before you ſome phenomena that I have obſerved in the intereſting ſcience of Galvaniſm.

I began my inquiries on that ſubject almoſt immediately after the firſt publication of the account of Signor Volta's Pile in your Journal, in concert with a friend, who, to the experience of years, added all the ardour of youth in the purſuit of ſcience, but whoſe life was ſhortened by an unfortunate accident in a vigorous old age: I premiſe this account of my own experiments, by reſcuing from oblivion the few obſervations that his time and health would allow him to make; and which will come with no ſmall intereſt to the world, when it is known they were thoſe of the intimate friend of a Franklin and of a Black.

His firſt attempts led him to compare Galvaniſm with the old electricity; and he was decidedly of opinion, that there was a ſufficient diſtinction betwixt them to entitle the former to the rank at leaſt of a new ſpecies; this obſervation my own experience has every day confirmed, eſpecially ſince I have been able to give ſo very conſiderable a degree of ſtrength to the pile as I have latterly done.

He attempted to uſe reſin as a lining for a white iron caſe to the pile, in which, after ſtanding for a ſhort period, its power ſtopped; and upon examining the pieces he found the reſin completely diſſolved into a ſubſtance reſembling turpentine, and forcibly drawn betwixt the ſilver and zinc; this ſhows a violence of action which has never as yet been obſerved in Galvaniſm, and may ſerve as a caution to others againſt uſing reſinous ſubſtances with the pile, at the ſame time that it opens a field of inquiry with regard to its effects on that matter.

His next and moſt important obſervation was made on a moſt ſingular phenomenon, which he perceived on the ſides of the ſilver and zinc pieces which were in immediate contact, when ſtrong paſteboard, moiſtened with a ſolution of common ſalt, was uſed for ſetting up the pile: the pieces of paſteboard were of a polygonal form, and ſmaller than the metallic pieces; their figure was exactly ſtamped in a black colour, not on the ſides of the pieces in contact with them, but on the dry metallic faces which were in contact with metal only; this very ſingular appearance, which I had frequent opportunities of examining, conveys an idea that ſomething, circumſcribed in its action by the figure of the moiſtened diſks, and capable of penetrating through the pores of the metals and of oxidating them, formed a conſtituent part of the Galvanic fluid.

He alſo obſerved, what might be readily expected, that people advanced in years felt the ſmall ſhocks of Galvaniſm leſs readily than younger perſons; but what was ſingular, they felt the ſtrong ſhocks more ſeverely; this may throw ſome light on the nervous fluid, and the change that it undergoes by age.

He found that when the experiment of decompoſing water was made in an open glaſs, that the water, although kept for a week after, unconnected with the pile, and a ſufficient quantity of ſalt had been added, to prevent all ſymptoms of putrefaction, yet continued to emit a gas, and a mucous matter was precipitated from it, of a whitiſh colour; this ſeems to have ſome connection with an extraordinary liquor, procured by the ingenious Dr. Moyſe, by evaporating the waſtings of the pile; but the account of which I by no means wiſh to anticipate, as it is to be hoped that the learned Doctor will favour the world with it himſelf.

You will no doubt regret that ſo truly philoſophic an obſerver ſhould be loſt to mankind, and had you known the individual, you would have found that the powers of his mind could only have been equalled by the virtues of his heart.

I now, with diffidence, ſubjoin an account of my own experiments.

Being acquainted by obſervation, and by the experience of others, with the Galvanic powers of black lead, I attempted to form a range of plumbago crucibles, into a Galvanic apparatus,

apparatus, which has the advantage of cheapneſs, and conſiderable power, and would no doubt anſwer very well, if a way could be diſcovered for preventing the moiſture from penetrating through the pots, though I have not been able to effect it by any mode of coating them: in caſe anybody ſhould be inclined to try this method, I have annexed a drawing of the manner in which the pots are connected, at the ſame time obſerving that the zinc pieces ought to be ſoldered to the braſs wire. If ever Galvaniſm can be applied to the arts, by procuring alkali in an economical proceſs, the above bids fairer to attain that end than any thing I have yet met with, as the pots are in a ſhort time covered with an efflorescence of ſoda when common ſalt is uſed, which might be collected tolerably pure from their outſides.

In this range the zinc became the gaſeous, and the plumbago the oxidating ſide, which ſhows that it is as far beyond zinc, as zinc is beyond ſilver, in the arrangement of Galvanic oxidating power, though there is no reaſon to believe that the plumbago undergoes any change itſelf. When connected with a bent ſyphon, filled with diſtilled water, by either ſilver, braſs, or iron wires (even though the bend was filled with quickſilver), both ſides ſoon changed the reddened litmus paper to the purple, though the plumbago ſide reſtored it to be a perfect blue; ſhowing that a much greater quantity of alkali was formed on that ſide than on the other.

The only apparently electric phenomenon, independent of the ſhock, that I could obſerve by this range, was that the powder of the plumbago aſſumed the form of little ſtars on the ſurface of the water, which ſome people might be inclined to think proceeded from a ſtate of poſitive electricity.

Having lately diſcovered a mode of encreaſing prodigiouſly the powers of the pile, I have laid aſide the cumberſome and bulky apparatus of the range, and cannot recommend it for any thing but its cheapneſs, and the poſſibility of being ſatisfied at a moderate expence, by means of the larger ſized crucibles, of the proportion that bulk or ſurface bear to alternated numbers in Galvanic power.

I alſo annex a drawing of the box, in which I encloſe my pile, and which I can recommend for convenience and portability; the improvement that I have made in the pile is on the matter by which the pieces are put together, to which I was led by theory.

I took a pound of pipe clay, an ounce of plumbago powder, an ounce of black oxide of manganeſe, and two ounces of common ſalt, and made them into a paſte with water; in the uſe of clay I took advantage of the obſervations of Dr. Moyſe.

This mixture gave 126 pairs of ſilver and zinc of the half-crown ſize, a power beyond any thing I have yet heard deſcribed, and gave the ſpark moſt vividly, in a room where the ſun was ſhining, either from a bit of common coak, or the point of a needle; but even when two pieces of metal were held in the hand, though perfectly dry, and the wires touched by them, a ſhock was felt much greater than that procured from a very conſiderable number of pieces with the aſſiſtance of moiſture, ſet up in the common way. When the pile begins to weaken a little, if one of the wires is allowed to vibrate on the piece of metal in one hand, and the other wire held by the other, the ſenſation of this dry ſhock brings a complete recollection of what is commonly called the ſleeping of a limb, viz. that prickling that is felt after the limb has been long in a conſtrained poſition, and when, I believe, medical people ſuppoſe there has been a preſſure on a nerve.

I am almoſt inclined to believe, that the natural ſenſation proceeds from the Galvanic fluid in the human ſyſtem attempting to reſtore its equilibrium by repeated ſhocks. A pile in this very ſtrong ſtate gives no ſigns of electricity by a very delicate electrometer (which I uſed to the excluſion of the modes of accumulating electricity, on which I think little dependence can be placed), and no effect could be excited, even though the pile was inſulated, and the ſhock taken through the metallic arm that ſupported the electrometer, by a perſon on an inſulating ſtool.

I alſo attempted to charge a Leyden vial in the way * mentioned by you, as uſed ſucceſsfully by Mr. Cruickſhanks, viz. connecting one end of the pile with the outſide, and the other with the inſide of the jar. It ſtrikes me that it is impoſſible a jar would be charged in that manner, as the two ſides, in that caſe, are connected by conducting ſubſtances, the very mode uſed for diſcharging a jar; if I have miſtaken your meaning, or that of Mr. Cruickſhanks, I beg you will correct me. I alſo connected both wires with the outſide, and both with the inſide, and even the ſilver wire of one pile, and the zinc wire of another, alternately with the internal and external ſides, without producing any effect.

I then attempted to charge the vial through the inſulated pile, connecting the wires by turns with the conductor of an electrical machine and the inſide of the jar. By comparing the change produced by a definite number of the revolutions of the machine, when attached and unattached to the pile, and when the ſilver and zinc wire were alternated, in the manner above deſcribed, no difference could be perceived either in the ſhock or ſize and length of the ſpark: alſo when the jar was placed between the conductor, and the inſulated pile, no difference could be perceived by taking the ſhock or the ſpark through the pile.

Theſe experiments were often repeated with the ſame reſults; and I think they ſhow clearly that the charge is neither accelerated nor retarded by the pretended *plus* and *minus* of the pile.

I have ſeveral times obſerved, and wiſh it may be attended to by others, that when women and men form a part of the Galvanic circuit, that the women feel almoſt the whole

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* Your words are---" That Mr. Cruickſhanks, of Woolwich, has charged a large jar ſo as to give a " ſhock merely by placing the pile between its coatings." This I ſuppoſe was a private communication, as I have in vain looked over Mr. C.'s own papers for the fact.---C.

The communication was made in converſation. I think the arguments and inductions at p. 243-- 245 of our IVth volume remove many of the difficulties of theory, ariſing from the low intenſity of the power; but not this of the actual ſhock. I have charged talc as well as thin ſilk unequivocally; but expect ſoon to repeat the experiments with a higher power.---N.

ſhock,

ſhock, when the men feel little or nothing; this even happens when only two perſons are in that ſituation, and with whatever ſide of the pile they may be connected, and when one woman ſtood between two men.

I believe we owe to the learned profeſſor of botany, in this univerſity, the obſervation, that plants are not conductors of the Galvanic influence; a moſt ſtriking diſtinction betwixt it and the electric fluid: I connected the pile, in its ſtrongeſt ſtate, with the ſtalk of a live and herbaceous plant, the ſtem of which was even moiſtened with ſalt water. The ſhock could not be procured from any part of the plant, however near to the wire, and one of its leaves applied to the wire, ſufficiently interrupted the Galvanic power *

The Galvanic ſpark procured from charcoal, has a very different appearance from the electric received in the ſame way; the former being of a red colour, and reſting upon the eye, whilſt the latter, notwithſtanding its ſuperior velocity, can be diſtinctly perceived, to have the blueiſh caſt of burning ſulphur.

The intention of this communication will be fully anſwered, next to preſerving a memorial of a perſon deſervedly dear to all his friends: if I ſhall be found to have added a drop to the ocean of ſcience, or to have facilitated in the ſmalleſt degree, the purſuits of others on the ſame ſubject,

I am, SIR,
with great reſpect,
A FRIEND TO THE EXPERIMENTAL RESEARCH OF TRUTH.

Figure 1ſt. A A repreſents the black lead crucibles, and as a range for Galvanic experiments. B B the connecting wires, to which pieces of zinc, as C, are attached by ſolder.

Figure 2d. is the pile box, of which D D D D D are the ſpaces, in which the piles ſtand. The ſhelf E E E, moveable eaſily for two thirds of the height of the box, by means of the rod F F, which paſſes through the top of the box, and may be faſtened by the flat wedge G; the rod is preferable to a ſcrew, being leſs hurt by moiſture.

Figure 3 is the ſhelf E, taken out to ſhew the manner in which it is formed.

Fig. 4, is one of the diviſions of the box which are expreſſed by H H, in Fig. 2, I being cut out for the ſhelf to run in.

N. B. A front piece ſhuts the box, by ſliding in a groove, on each ſide from the top downwards, which is ſloped away at the edges, and can be thruſt under the baſe of the box; which gives it an inclination backwards, that facilitates the building of the pile to any height. I have lately much improved the box, by lining it with oiled ſilk, with a view to prevent the wood from abſorbing moiſture.

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* I have undoubted information that a portion of the aloe-plant, which was placed for a night in the connecting line of a pile of 36 half crowns and zincs was killed by its action, the other parts remaining unhurt.---N.

Third

II.

Third Commmunication on the Trains of Watches. By the Rev. W. Pearson.

To Mr. NICHOLSON.

SIR,

WHEN I tranſmitted to you the two papers, contained in the third Volume of your Journal, reſpecting the philoſophical uſes and trains of watches, I had not an opportunity of ſatisfying myſelf with regard to one material conſideration in the conſtruction of thoſe delicate machines; namely, whether or not the diameters of the wheels ought to be ſucceſſively diminiſhed from the center wheel to the end of the train, in proportion as the maintaining power becomes exhauſted; *four* beats in the ſecond were then fixed upon, for the new trains recommended, for no other reaſon but becauſe this ſeemed a convenient number to be divided and ſubdivided without a remainder, and was alſo very nearly equal to what one of the watches, under examination at that time, was governed by: ſince, however, I came to reſide near London, I have inquired of ſome eminent watchmakers what number of beats in a ſecond, is deemed in practice moſt deſirable for preſerving a ſteady equable motion in the going of a watch, and the information I have received, particularly from Mr. Iles, of St. Martin's Court, is, that about *five* is found to be the beſt number of any: now as the number of beats of a watch in a ſecond has been ſhown to depend upon what may be called the third, or laſt portion of a train, (in a watch which ſhews ſeconds) with the contrate wheel, its pinion, and the balance wheel; and as five beats in a ſecond require higher numbers, in either one or both of theſe wheels, than four do, the inference is, that, allowing 15 to be the beſt practical number for a balance wheel, the contrate wheel muſt neceſſarily be larger than I have before given it, in order to produce the deſired effect of five beats in each ſecond.

Hence ariſes the neceſſity of my troubling you with a third communication on this ſubject, which, I am perſuaded, you will readily lay before the public, not only becauſe watches are in the hands of every reader, but alſo becauſe you are aware, that they can be made to meaſure fractional parts of a ſecond for as little expence, as they are generally conſtructed now that no attention is paid to the exact meaſure of a beat.

In order to make a watch beat exactly five times in each ſecond, whilſt 15 is the number of the balance wheel, the contrate wheel and its pinion muſt be to each other in the ratio of [*] 10 to 1; ſo that if 6 be the pinion, 60 will be the wheel; if 7 then 70; if 8, 80, &c. and as experience ſeems to point out the expediency of retaining 15 as the moſt

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[†] When the beats of a watch are four per ſecond, the ratio between the contrate wheel and its pinion is only as 8 to 1.

eligible number for the balance wheel, any one of the above pairs of wheels and pinions may be adopted, that the artiſt finds his other work will admit of:—what appears to me, on conſidering the ſubject in a practical point of view, as the moſt eligible train, out of the many that have been ſuggeſted, is $\frac{64}{8}\times\frac{60}{8}\times\frac{60\times15\times2}{6}\div3600=5$, which may be otherwiſe, perhaps more intelligibly, expreſſed thus:

64 Center wheel
Pin. 8—60 Third wheel
Pin. 8—60 Contrate wheel
Pin. 6—15 Balance wheel
2 Palettes.

Beſides the advantage that this train, or one of a ſimilar value, has over thoſe before ſpecified, in point of producing a ſteady equable motion, it has the further recommendation of being capable of being counted *decimally* without any trouble; for when any number of beats are reduced into ſeconds by the diviſor 5, the remainder *doubled* will, in every inſtance, be ſo many tenths of a ſecond over, and will be an even or odd number, accordingly as the termination of counting by the beats coincides with a beat, or falls in the interval between two; in the latter of which caſes there will be, beſides the beats remaining after diviſion, an half beat to be alſo doubled: thus, for example, in 132 beats of a watch, which beats five times in a ſecond. there are $26\frac{2}{5}$ or 26,4 ſeconds, but in $132\frac{1}{2}$ there are $26\,\frac{2\frac{1}{2}}{5}$ or 26,5.

When your readers conſider what expence has been incurred in improving watches by the introduction of detached eſcapements, compenſation balances, &c. whilſt notwithſtanding a *ſecond* is the ſmalleſt portion of time they are hitherto intended to meaſure, they will, doubtleſs, ſee the propriety of making choice of ſuch numbers in the conſtruction of thoſe portable chronometers, as will not only tend to improve their rate of going, but will render them, without any additional expence, capable of ſubdividing the ſecond into exact *tenths*, and conſequently of becoming admirably ſubſervient to a great variety of philoſophical reſearches, in which a minute attention to time is indiſpenſably neceſſary.

In a watch conſtructed to beat five times in a ſecond, the main-ſpring muſt be ſomewhat ſtronger than in one which beats only four times, becauſe the contrate wheel, which is a *driver*, has its diameter enlarged; but what appears to me to be the greateſt practical objection to this conſtruction, is that which applies to the deſtructibility of the laſt pinion; by a careful examination of the works of an old watch, it will appear, that the pinions wear out before the wheels, and firſt of all that pinion which is placed on the arbor or axis of the balance wheel, by reaſon of the number of its rotations: on this account, particularly where the ratio between the contrate wheel and pinion in queſtion is ſo high as 10 to 1, the *temper* of the pinion ought to be well attended to, which is uſually left too *ſoft* by the

the workmen; partly, I ſuppoſe, becauſe the pivots are afterwards more expeditiouſly finiſhed, and partly becauſe a watch is thereby liable to be the ſooner unſerviceable.

There is, however, one recommendation more of a large contrate wheel, which I ſhall juſt mention before I conclude the ſubject: as the arbors of the contrate and balance wheels of watches of a common conſtruction, placed one horizontally, and the other perpendicularly, they are neceſſarily laid at one ſide of each other, ſo as juſt to eſcape touching as they croſs, by reaſon of which poſition an obliquity is occaſioned in the direction of their action, which is the greater the ſmaller the contrate wheel is, and *vice verſâ*; and which frequently occaſions the ſtopping of the watch: an enlarged contrate wheel will conſequently greatly diminiſh this cauſe of imperfection, and will therefore conſtitute an additional improvement.

I am, SIR,
Your's reſpectfully,
W. PEARSON.

Parſon's Green, April 13, 1801.

III.

A Method of meaſuring a Ship's Way at Sea, with Obſervations on other Branches of the Art of Navigation. By JOHN COOKE, *Eſq. M. R. I. A. Communicated by the Author.*

LET a tube with a few inches of one end bended to a right angle, be affixed to a ſhip, in ſuch a manner that the longer part may enter through the ſhip's bottom below the water line, and be faſtened in a poſition parallel to the maſt, and ſo that the recurved end ſhall be immerſed in the water in a poſition parallel to the keel, with its orifice pointed towards the ſhip's head. Let the upright part of this tube be furniſhed with a ſmall ſpouting aperture within the ſhip, at the level of the external water; then ſuppoſe it to move ſteadily through ſmooth water in the direction of its length, the impulſe of the ſtream on this orifice will be as the ſquare of the ſhip's velocity (ſee note A). And ſince the column of water thrown up in the erect tube by this force muſt be in equilibrio with it, the perpendicular height of this column of water will be as the ſquare of the ſhip's velocity alſo; in which caſe the water will flow from the aperture into the ſhip, while the veſſel moves forward, and the quantity diſcharged will be in a ſubduplicate ratio of the perpendicular altitude of the column in the erect tube; that is in the ſimple ratio of the ſhip's velocity. If therefore the quantity diſcharged through this aperture in paſſing over a mile be obſerved, the actual diſtance run at any time may be found, if the water ejected be preſerved and meaſured (ſee note B).

But the pitching of a veſſel at ſea, and the motion of lee way, would cauſe the ſtream to fall obliquely on the orifice, and conſequently affect the preſſure and alter the ratio which

which exiſts between it and the ſhip's motion in direct progreſs. To prevent this, a ſhort tube is to be fitted to the extremity of the immerſed tube, in ſuch a manner as to be capable of moving freely in a vertical, in an horizontal, or in any intermediate arch (which may be effected by a joint conſiſting of a portion of a ſpherical ſhell containing a ſimilar ſhell) to be furniſhed with vanes extending behind the joint, which are to be placed in ſuch a manner, that the ſtream acting on them may point the orifice ſo as to meet it perpendicularly; but a ſhip at ſea is often deflected from her path into a curvilinear track by the figure of the waves; and ſince the ſtream is cauſed by the relative motion of this water to the ſhip's motion, which may always be conſidered as in the direction of the keel, it muſt meet the orifice of the inſtrument in the direction of the keel, (ſee note H) that is perpendicularly, and therefore the water delivered through the ſpouting aperture muſt repreſent the undulating curve through which the ſhip moves, inſtead of its abſciſs, or true diſtance, (ſee note C) which might ſometimes differ conſiderably from it; for which reaſon the following method of correcting it becomes neceſſary. Let a barometer made narrow in the middle to check the ſudden motions of the mercury, be placed againſt a bulk-head, or tranſverſe partition of the ſhip, and ſo loaded and hung that when the ſhip heels it may ſwing in an arch deſcribed on the ſurface of this partition, ſo as not to partake of this lateral inclination (which is not difficult to accompliſh, the motion of heeling being neither very ſudden nor fluctuating) but in ſuch a manner as to receive the intire effect of the pitching motion which it is intended to repreſent; then if the curve which the ſhip deſcribes be ſuppoſed to be divided into indefinitely ſmall equal right lines, any one of them will be to the portion of horizontal diſtance which it overhangs, as radius to the coſine of the angle which it forms with the horizon; but the length of the column of mercury in the inclined poſition of the barometer, is to its length in an erect poſition as radius to the coſine of the ſame angle; therefore the length of the column of mercury at any time will be to its perpendicular height, as the rectilinear portion of the path which the ſhip ſhall be then deſcribing to true diſtance; and ſince theſe rectilinear portions are equal, and paſſed through in equal times, the mean of them obtained by the narrowed tube will repreſent them all, therefore the length of the column of mercury marked on the ſcale of the barometer will always be to its perpendicular height as the ſum of theſe rectilinear ſpaces; that is, as the curved path of the ſhip repreſented by the water diſcharged by the inſtrument, is to the ſum of the horizontal ſpaces overhung by them; that is the true horizontal diſtance, from which data the inſtrumental diſtance may be eaſily corrected at any time. But it is to be obſerved, that the height of the mercury in a perpendicular poſition is variable, on account of the difference in the weight of the atmoſphere at different times; therefore when it is required, the barometer muſt be taken from its place, and held carefully by the hand for a few minutes in an upright poſition, until the mercury ceaſes to deſcend, when it will ſhew the true perpendicular height neceſſary to this experiment, and at the ſame time the barometrical ſtate of the atmoſphere (ſo uſeful at ſea) more accurately than any ſuſpended barometer.

The agitations of the ſea affect alſo that part of the inſtrument which lies within the ſhip, and it remains to be conſidered what conſequences ariſe therefrom. Heeling, or an inclination of the veſſel to one ſide, and pitching, or the librations of it fore and aſt recline the upright tube, and thereby lengthen the column of water contained; but ſince the diſcharge of water is affected by *perpendicular* height only, this is but an apparent cauſe of error; the ſame preſſure at the immerſed orifice will elevate the water to the ſame *perpendicular* height whatever the length of the column may be, and therefore the velocity of the effluent water will be the ſame with the ſame degree of preſſure whether the tube be erect or inclined; plunging, or the aſcending and deſcending motion, occaſioned by the waves which alternately elevate and depreſs a veſſel in the ſea, by altering the weight on the orifice increaſe or diminiſh the efflux of water from the tube, to remedy which a part of the upright tube below the water line is made narrow, in the manner hereinafter deſcribed, whereby the aſcent and deſcent of the contained water will be rendered gradual, ſo that its altitude may become the mean of ſeveral alternations of preſſure; and ſince the waves ſucceed each other at equal intervals, and of equal dimenſions for a conſiderable time together, this mean height will be ſufficiently ſteady and exact; (ſee note D) and though this motion and pitching may ſeem to diſturb the operation of the ſpouting aperture, by raiſing and ſinking it above or below the plane of the water line, it occaſions equal and oppoſite errors thereby, which remedy each other, and narrowing the tube is really uſeful only to procure a more equable flow from the aperture; but heeling, which is a more durable poſition, has a tendency to raiſe the ſpouting orifice above the plane of floatation; however, this elevation is ſo ſmall, that the error ariſing from it is inſenſible.

Some mechanical difficulties preſent themſelves in applying this inſtrument to a ſhip, and in adapting it to the purpoſes of navigation; firſt it may be thought unſafe to make a hole in the ſhip's bottom to receive the tube, but the experiment has been tried to ſupply fire engines with water on ſhip-board, and no inconvenience aroſe from the practice when the receiving pipe was cloſely fitted to the perforation. It might alſo be paſſed, if thought preferable, through a hole above the water line, and the neceſſary curvature given without the ſhip.

Secondly, it appears from calculation, that the water in the erect tube would ſometimes aſcend ſo high, as to require a tube inconveniently long to contain it; but if it be cut off any where above the ſpouting aperture, and if the upper orifice be cloſely ſtopped, it will anſwer the purpoſe as well as before, becauſe the preſſure which the ſtopper receives, is that which would ſupport the column of water cut off, and therefore the preſſure which this ſtopper gives, is that which would ariſe from the weight of the column cut off. Hence the preſſure of the water within the tube continuing the ſame, the rate of diſcharge will remain unaltered by ſhortening it.

Thirdly, it is difficult to aſcertain the true mean level of the ſea when the ſhip is in motion, which may often be requiſite; in this caſe, if a flat piece of metal be fixed on the

top

top of a pole, and held out of the ſhip ſo as to defend the external orifice from the percuſſion of the water, the contained water will ſink to the true mean level, and the poſition of the ſpouting aperture, if fixed in a ſliding plate, may be adjuſted thereby *

Fourthly, if dirt or weeds get into the inſtrument, it may be cleared by forcing out the contained water by means of the plunger hereafter deſcribed; or if this plunger and the ſtopper be drawn out, the water will ſpout up forcibly in ſailing at a moderate rate, and thus remove the obſtruction.

The advantages expected from this inſtrument are, that it propoſes a *perpetual* meaſure, which adapts itſelf to every alteration of the ſhip's motion, and therefore corrects that error ariſing from the log, the uſe of which ſuppoſes the ſhip's motion to be uniform during the invervals of experiment, which is highly improbable.

And ſecondly, that the inſtrument being ſunk to a conſiderable depth, will not be affected by ſuperficial currents occaſioned by wind, to which the log is expoſed.

The cauſes of error which affect this inſtrument, are the ſame which attend the methods of navigation in general uſe.

The error of yawing is that which ariſes from unſteady ſteering, and will deflect the orifice from the true poſition.

If it be admitted, that the generality of ſeamen ſteer ſo as to confine the yaws within certain limits, the true line of motion may be conſidered as a ſerpentine line, which has a fixed ratio to the right line which joins its extremities in all caſes; but if the quantity of diſcharged water which repreſents a mile, be found from a mean of ſeveral experiments made at ſea (which experiments are of courſe affected by yawing) it follows that the ſtandard meaſure thus eſtabliſhed needs no correction, the effect of yawing being proportionate, and being allowed for in aſcertaining the ſtandard quantity.

The error of *lee-way* may be corrected, if a metal rod be extended along the axis of the upright tube, and paſſing through its bottom into the water below it, it ſhould be capable of being turned round in any direction by the ſtream acting on a vane like that of a weathercock, which is to be annexed to it below the tube; on the top of the tube is to be placed a circular plate engraved with the points of the compaſs, through the center of which this rod is to paſs, and to bear an index round it, which will point out the angle of lee way on the compaſs plate. Since the ſtream below will keep the vane in the ſhip's true line of motion, if this rod be furniſhed with a circular protuberance, nearly filling the tube below the ſpouting aperture, the ſudden current of the water within it will be thereby checked and rendered gradual, as was propoſed before; and if this rod be divided into two parts juſt below the protuberance, and if the termination of the lower part be ſquare, and if the bottom of the protuberance contain a ſquare ſocket, theſe parts may be occaſionally ſeparated

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* Or otherwiſe, if the external aperture of the tube itſelf, or of any other tube, could be placed at right angles to the ſhip's motion, the water would during that time ſtand at and ſhew the external level.---N.

ſeparated to plunge and clear the tube when neceſſary (ſee note G). The variation of the needle from a polar direction is another courſe of error in navigation, but the correction by azimuths and amplitudes affords a perfect remedy for it, and when the quantity of variation is diſcovered, the dead reckoning may be corrected thereby by well known means; alſo the variation may be found in a more ſimple manner at all times of the day, by annexing any of the univerſal ſun-dials to the compaſs box, in ſuch a manner that the meridian of the dial may be parallel to its ſide, and by adjuſting it to the latitude of the place; for if ſuch a dial be turned until the ſhadow points to the hour found by a lately corrected watch, its meridian, and the ſide of the box to which it is attached, muſt be in a true north and ſouth direction, and the deviation of the north point of the card from this meridian will be the angle of variation. (See note E)

Currents are the ſources of the greateſt errors to which navigation is liable; the method of finding their ſet and drift by the courſe and rate at which a ſunken body ſeparates from a drifting boat is erroneous, being grounded on a ſuppoſition, that this body meets with ſtill water, whereas it is believed, that ſuch a maſs of water moves in a current together, that our experiments only penetrate its ſurface; however, this method ſeems to be ſufficient to determine the exiſtence of a current, and the rhumb on which it flows, becauſe every ſtratum of moving water runs with a different degree of velocity, and this will be the caſe even if the ſunken body ſhould deſcend into ſtill water, or into a contrary current (and ſince interſecting currents have never been obſerved, we may conclude, that this experiment will, in all caſes, prove the exiſtence of a current, and ſhew the line of its motion, though it remain uncertain to which extremity of that line it may flow.

If the preceding methods of determining courſe and diſtance can be relied on, the error ariſing from a current which does not flow in a parallel of latitude may be aſcertained by attributing the error of dead reckoning to the line of the currents motion, and then by adding or ſubtracting, as the caſe may be, ſuch a ſupplementary diſtance in the direction of that line, as will make the dead reckoning correſpond with the latitude by obſervation, which will give the true place of the ſhip; and if the currents be regular and periodical, as is ſuppoſed, numerous experiments made in this way at various times and places, might afford materials for the conſtruction of tables, or hydrographic charts, pointing out the drift ſet and periods of theſe currents, which would enable the navigator to make the neceſſary allowance for their effect in all caſes.

Alſo if the true direction of the wind can be aſcertained in a current, the lines of its motion being known, the ſet and drift may alſo be known, becauſe if the ſhip lie to, the apparent wind will be a compoſition of the true wind, and of the wind ariſing from the relative motion of the air produced by the running water (ſee note F), which, in moſt caſes, bears a very ſenſible proportion to the true wind (ſee Encyclopædia Britt. Vol. XVII. p. 204); therefore if the veſſel ſail along the current line, varying the rate of her motion until the wind vane lie exactly in the direction of the true wind, there will be no current

wind,

wind, and, conſequently, no current, and the veſſel being then ſtationary with regard to the earth, the rate and direction to the ſtream will be the ſet and drift of the current. However, ſince no adequate method is known to determine the true direction of the wind in a current in all caſes, this obſervation would be uſeleſs; but that there are ſome grounds to ſuſpect that within the tropics the apparent deviations from the trade winds ariſe from the deflecting force of current winds. If this be ſo, and if the general motion of the trade wind be aſſumed as the true direction of the wind, this rule will ſhew the ſet and drift of the currents in a great part of the ocean.

NOTES.

A. This law has been denied in caſes where a diminution of preſſure on the back part of the moving body ariſes from its motion, but the altitude of the water in this tube is affected only by the *abſolute*, and not the relative, impulſe of the impinging current (which is apparent from the form of it), and Mr. Vince's late experiments tend to prove the truth of the theory in ſuch caſes.

B. If the ſpouting aperture be very ſmall, the theory is true, but when the aperture is large, it is not ſo certain for this reaſon; and alſo becauſe it will be more convenient to receive a ſmall than a large quantity of water into the ſhip, the hole through which the water iſſues ſhould be very ſmall, with even thin edges. See Vince's Hydroſt. p. 54.

C. It may not be uſeleſs to obſerve, that if the ſpace repreſented by the inſtrument in paſſing over any certain number of ſuch waves be obſerved and divided by the number of theſe waves, it will give what is called the breadth of the wave; and ſince the celerity of a wave is demonſtrated to be as the ſquare root of its breadth, the rate at which the wave moves may be found by a table at any time by the help of this inſtrument.

D. Nairne's marine barometer is formed on this plan, and alſo the tide guage.

E. It may be curious, if not uſeful, to obſerve, that it is poſſible to ſteer by the ſun alone without the aid of magnetiſm.

If there be a large watch, whoſe hand performs one revolution in twenty-four hours, with ſuitable graduation on the dial plate; this dial plate, with a perpendicular ſtyle in the centre, when reclined according to the latitude, becomes a ſun dial; and when the ſhadow of the ſtyle coincides with the hand of the watch, the ſtyle muſt hang over the true meridian: therefore if this meridian be ſet to the ſhip's courſe by a motionleſs compaſs card, and if the helm be ſo managed as to keep its ſhadow conſtantly on the hand of the watch, the true poſition of the ſhip will be preſerved until the time of the watch or the elevation of the ſtyle require correction. It is poſſible to effect the ſame thing by a ſtar, by having a dial plate formed of ſome reflecting ſubſtance.

In climates where the ſky is ſeldom obſcured, ſuch an inſtrument would afford a ſteady guide to the mariner, not being liable to the errors of variation, aurora borealis, or to the deflections of libration, which diſturb the ſuſpended card very conſiderably.

F. It is a fact, well known at ſea, that the apparent wind is compoſed of the true wind, and of the wind occaſioned by the ſhip's motion; therefore it is that a ſhip ſailing on different tacks, or with a different degree of velocity in the ſame line, will have the wind in a different point, becauſe one of the component forces is altered in poſition in the former, and in intenſity in the latter caſe. What we experience in riding through a ſhower of rain is a familiar example of this fact; while the horſe's motion is uniform, the direction of the drops appears to be conſtant, but if it be encreaſed, the direction of the drops will be more in the rider's face; if it be diminiſhed, it will be more behind him; and if his road be changed to the right or left, the ſhower will apparently alter to a point more oppoſite to his motion than the point it ſeemed to be in before.

G. Should it be conſidered adviſeable to diſencumber this inſtrument by ſeparating the lee-way machinery from it, it may be annexed to the compaſs box in the following manner:

A metal rod is to be extended down the ſtern poſt, and ſo hung as to be turned eaſily on its axis by a vane at the bottom, which is to be acted on by the ſtream. To the upper part of this rod, juſt below the level of the deck, a croſs bar of a few inches long is to be annexed. The compaſs box muſt reſt on an upright foot, extending a little below the deck, and ſupported by a ſocket, in which it is to turn freely. Juſt below the deck a croſs bar is to be annexed to this foot equal to that on the ſtern poſt rod, and the extremities of the two croſs bars are to be connected by two ſlender equal bars moveable on pivots, ſo that the four bars may form rhomboides, in conſequence of which the oppoſite ſides muſt be always parallel, and the motion of the foot will be the ſame as that of the ſtern poſt rod: therefore the vertical ſection of the compaſs box, which paſſes through the centre and ſteering mark of the box, will be parallel to the vane; and ſince the ſtream will point the vane exactly to the ſhip's true line of motion, it follows, that the ſteering mark on the compaſs box will be pointed to the true line of motion alſo, and the angle contained between this mark and the north point of the card will conſequently be the true magnetic courſe clear of the error of lee-way.

IV.—*On*

IV.

On the Eſcapements of Time Pieces. By Mr. THOMAS REID.

To MR. NICHOLSON.

SIR,

Edinburgh, April 6, 1801.

BEING lately favoured (by a friend, and one of the moſt eminent philoſophers of this place) with a reading of your uſeful Journal, chiefly with a view that I might ſee the account given of Mr. Prior's eſcapement, in which I was ſomewhat intereſted, having made one of the ſame kind in the year 1793, and which watch is at preſent in the hands of Mr. Howells, No. 15, White Hart Row, Kennington Lane, Surry.

The originality of invention that ſeems to attach to Mr. Prior, I lay no claim to; as by a note it appears that he had not ſeen the eſcapement of Mr. Mudge, and I may add, nor perhaps that of Lepaute's. Although I had not ſeen Mr. Mudge's eſcapement, ſome hints reſpecting it were given me in an obſcure manner, but yet ſo as by them I could comprehend it. The eſcapement of Lepaute's I was perfectly acquainted with; and it was in attempting to combine his with Mudge's that led me to make the one now in queſtion.

In mine, the time of reſt of the teeth, and the flanch for the impulſe, is on the inſide of the pallets; and in that of Mr. Prior's they are on the outſide.

There is a very material circumſtance which Mr. Prior has not perceived. His pins are arranged in the ſame line of circle on both ſides of the wheel. This will make a conſiderable difference, eſpecially if the pins ſhould be left pretty thick, which will be the ſame as if the pallets were impelled by wheels of unequal diameters, and they will conſequently receive unequal impulſes.

That pallet which moves inwards, receives its impulſe from the outward part of the pins. The pins ought to be ſo diſpoſed as to give equal impulſes. For that purpoſe, let a very fine circle be traced on the ſide of the wheel, let the holes for the pins, that are to act on the pallet outwards, be drilled inſide of the circle, and ſo as that the edge of the holes ſhall coincide with it: thoſe pins for the inward pallet muſt be drilled without the circle, and to have the edge of their holes alſo to coincide. In this caſe, the impulſe given to the pallets will be equal; but then it may be ſaid that the reſt is not equal, this laſt is ſo trifling, that of evils it is the leſſer.

In this caſe of an eſcapement wheel *with pins* on each ſide, the impulſes and times of reſt are brought nearer to an equality than in Mudge's, yet his teeth have ſome advantages which the pins want.

The roller attached to the verge, and ſerving as a pallet, is not new. Le Roy, Berthoud, and others, had uſed ſuch, nor do I ſee much benefit to be derived from it. The watchmakers of Chaux de Fonds, Locle, and of Geneva, think ſo little of this, that they uſe ſimply

ſimply a piece of braſs for this pallet. You muſt know that *they* have long ago and often made up watches and clocks with Mudge's eſcapement to them; probably from Berthond's carrying it to Paris with him; Mr. Mudge having ſhewed him the eſcapement when he was in London, in 1766.

I ſee you wiſh to be informed, Who was the inventor of the compenſation watch at preſent in uſe? I can inform you this, it was the invention of Le Roy, as you may ſee in a book, entitled, "Voyage fait par ordre du Roi in 1768, pour éprover les montres Marines, inventées par M. Le Roy, par M. Caſſini, Fils."

The invention of the horizontal eſcapement you have attributed to Debaufre. The eſcapement of Debaufre has not the ſmalleſt affinity to the horizontal one, nor can it give any idea of it, or tend in the leaſt to lead to it.

Mr. Tompion certainly gave the idea of the horizontal eſcapement, in 1695; to be ſure it was not in his hands juſt what it now is, but in the ſtate ſuch as he left it, was Mr. Graham (who, I believe, was his apprentice) afterwards led to improve it, and ultimately to complete it, prior ſometime to 1730.

Debauſre's eſcapement had two balance wheels on the ſame axis, and was a contrate wheel watch. It was the firſt watch that perhaps was ever jewelled in the verge holes. In his attempting to improve the eſcapement, he formed a connection with a M. Facio, a native of Geneva, who, in 1700, had invented the piercing of rubies and of watch jewelling, and not being well received with his invention at Paris, Facio came over to London; and ſet to work with Debaufre at the ſaid eſcapement: its pallet was of diamond.

There are watch-makers at preſent in London who may remember a Debaufre, a watch-jeweller, and one of the earlieſt of that buſineſs, who died about 1774, or thereabouts: he was old, and perhaps might be the ſon of the former. The introduction of watch jewelling* into Britain, you ſee, aroſe from an accidental circumſtance; and it is ſtrange that to this day it is hardly known in France, unleſs of late ſome one may have left this conntry, unfortunately by want or by opinion, and carried the art there. Should theſe lucubrations be favourably received, I may have ſomething more to communicate to you afterwards.

I am SIR,

With much eſteem,

Your moſt humble ſervant,

THOMAS REID.

Watch-maker, Edinburgh.

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* Watch jewelling has contributed not a little to the celebrity of our watches.

Pray

Pray who firſt applied the duplex eſcapement, in London, to watches? It is the invention of a Dutertre early in the eighteenth century *. How long is it ſince watches with the virgule eſcapement were ſeen? and who was he who applied it firſt?

See Regle Artificielle du Temps, par M. Sully, reſpecting Debaufre's eſcapement; it is but rather treated of there obſcurely, yet I am quite well aware of its conſtruction. In the ſpelling of eſcapement I have uſed an elliſion for the *e*; however, you may adopt which way you think beſt. We uſe the word *'ſcapement* in common converſation.

I wonder much at thoſe gentlemen of l'Academie Royale des Sciences ſo groſsly miſtaking or miſrepreſenting what Sully has written; nothing ſo clear to me as the account Sully gives of Tompion's eſcapement being the foundation of the horizontal one. Tompion gave it up for a bad job, and this might be one reaſon why it was ſo late before it was completed under the hands of Graham. Thoſe gentlemen of the academy are equally miſtaken in their ideas of improvement given by P. Le Roy to Debaufre's eſcapement, which they call the horizontal. What P. Le Roy did was, he took one of the wheels, and ſubſtituted another pallet; but nothing here all the while but is moſt diſtant from the horizontal one. It is true that Sir Iſaac Newton had one of Debaufre's watches, which he ſhowed to Sully in 1704. I do not know who this Gourdain was, who is alſo mentioned. It muſt be Gaudron that they mean. He was a man of genius, and watch-maker to the Duke of Orleans, during the Regency †.

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* N. B. Enderlin takes the merit of being the inventor as well as Debaufre, and P. Le Roy claims that of Dutertre. See Lepaute, Thiout, &c.

† Gourdain was a watch-maker probably of Paris. His communications were made to the academy in 1742.

The commiſſion of the academy, in their report on M. Gourdain's improvement of Baufré's eſcapement (Recueil des Machines, VII. 137), deſcribe this laſt as follow: I tranſlate verbally.

“ To remedy this inconvenience (the acceleration in the crown wheel and pallets, when the firſt mover is augmented) “ the Sieur Baufré, a French watch-maker, ſettled in London, imagined, in 1704, the eſcapement called dead beat (*qu'on nomme à repos*). He ſuppreſſed the two pallets and the crown wheel, and fixed on the arbor of the balance a kind of cylinder, rather thick, interrupted in one of its halves, and which, inſtead of its being terminated in its ſection by the plane which would paſs through its axis, is on the contrary (terminated) by two inclined planes, one on each ſide of the axis.

“ Oppoſite theſe two inclined planes are two wheels fixed parallel to each other on the ſame axis, placed ſo that the teeth of the one are oppoſite the interval formed by the teeth of the other. By this means, as ſoon as one tooth has ſlided along one of the inclined planes and impelled the balance, the other tooth repoſes *on the ſuperior baſe of the cylinder* which carries the inclined plane; ſo that the balance can obey the whole impulſe it receives, and run through arcs as large as are neceſſary, which gives a facility of regulating the going of the watch, and taking advantage of all the accuracy which the ſpiral ſpring attached to it is capable of giving.”

The reader will perceive how I myſelf, and not the academicians, have been miſtaken in my note at page 53 of the ſecond volume of this Journal, in concluding this to be the horizontal eſcapement, to which, indeed, the deſcription would apply if we were to overlook the words here put in Italic, as I did, and infer that the places of repoſe were on the curved ſurfaces of the cylinder.

Gourdain's eſcapement is preciſely enough deſcribed in the note laſt quoted.---N.

What has given credit to Pinchbeck as the inventor of a watch thermometer, muſt have been the circumſtance of his purchaſing from Berthoud, when in London, 1766, for his preſent Majeſty, a watch with center ſeconds, the common Verge Eſcapement, and having a thermometer, or compenſation piece, which Berthoud ſays, was the firſt pocket watch made with ſuch a thing. Kendal copied this compenſation work in ſome watches that he made afterwards.

Sully wrote his Regle Artificielle in 1717, and J. Le Roy gave an edition of it in 1737. He was an Engliſh watch maker; what time he went to France I know not, but he was a great genius, much eſteemed there, and had the merit of putting the buſineſs in a better train, reared up workmen, and gave them quite new methods. Julien Le Roy thought highly of him, and was himſelf a man of very uncommon ability.

Excuſe this deſultory manner, a little trouble may make it more connected.

T. R.

V.

Inquiries relative to the Laws of Affinity; by Cit. Berthollet, *Member of the National Inſtitute of Arts and Sciences* *.

(Concluded from page 21.)

ARTICLE X.

On the Determination of the Elective Affinities.

1. TO determine the elective affinity of two ſubſtances for a third, according to the notion which we ought to conceive of it, conſiſts in aſcertaining in what proportion the third ſubſtance divides its action between the two firſt, and to what degree of ſaturation each of theſe muſt arrive when their forces are equally oppoſed. The relative affinity will be in proportion to the degree of ſaturation they would attain, on account of the quantity which ſhall have acted; ſo that if the quantities were equal, the comparative degrees of ſaturation would afford the meaſure of the reſpective affinities.

2. When I ſpeak of the ſaturation of a ſubſtance, I do not mean that abſolute ſaturation in which all reciprocal action would ceaſe, but a degree of ſaturation which is eaſily diſtinguiſhed, and is common to all combinations. It is that of neutraliſation, in which none of the properties of the conſtituent parts predominate. The term of cryſtalliſation of ſalts, does not always coincide with that of neutraliſation; for example, with reſpect to the carbonates of alkali, which ſtill continue to afford indications of alkalinity, and with the acidulous tartrite of pot-aſh, which on the contrary retains an exceſs of acid. Nevertheleſs, we may take the laſt combination at the term in which it is neuter, becauſe it ſtill poſſeſſes the

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* Tranſlated from the Annales de Chimie, XXXVII, 151.

the property of cryſtalliſing; it is this tartrite alſo which is neceſſarily obtained, when an exceſs of the baſe is preſent in the experiment; but when an exceſs of acid is required, the degree of ſaturation in which the acidulous tartrite exiſts, may be determined by the quantity of pot-aſh which is neceſſary to effect its neutraliſation.

3. One conſideration which is immediately ſeen to deſerve much attention, is, that on comparing the affinities, it is neceſſary to employ in all the experiments, the ſame proportions of all the ſubſtances which might be ſucceſſively put in oppoſition; becauſe, if the proportions vary, the reſult of the action will be no longer the ſame, nor can the affinity be repreſented by the ſame number. I ſhall proceed to illuſtrate this obſervation by an example:

Let the number 100 repreſent the pot-aſh, which is to be ſaturated by 100 parts of ſulphuric acid; I oppoſe to it 100 parts of ſoda; I ſuppoſe that after the action the pot-aſh will be found to have abſorbed 60 parts of the acid, and the ſoda 40; I ſhall then conclude, that the affinities of thoſe two baſes for the ſulphuric acid are in the proportion of 60 to 40. But 40 parts of pot-aſh remain uncombined; theſe ſtill continue to act, and contribute by their action to the diviſion of the acid; ſo that if this quantity were to be varied, the reſult would no longer be the ſame. If inſtead of 100 parts of pot-aſh and 100 parts of ſoda, I take only 80 parts of each, the uncombined remainder will afford 20 parts of pot-aſh, and another quantity of ſoda, ſo that the forces exerted by thoſe two parts, are no longer in the above-mentioned proportion; whence it appears that the two ſaturations cannot be in the proportion of 60 to 40.

4. But in order to aſcertain the degree of ſaturation acquired by each of theſe ſubſtances, it is neceſſary to effect a ſeparation; and this can only be obtained by elaſticity, by cryſtalliſation, by precipitation, or by the action of a ſolvent. But we have ſeen that theſe different means ought to be conſidered as foreign powers which alter the reſults, and determine the combinations that are formed, without our being able to meaſure their effect, ſo as to diſengage that of the elective affinity; and conſequently, that thoſe ſeparations which, in the articles I, II, III, have only been conſidered as the proportional effect of elective affinity, are really the effect of ſeveral joint forces, as is proved by the obſervations ſubſequent to thoſe articles.

When it is neceſſary, for inſtance, to aſcertain the quantity of ſulphate of pot-aſh and ſoda that might be formed, the force of coheſion not differing conſiderably in theſe two ſulphates, it is probable that it would not effect any great change in the proportion of the two ſalts which would cryſtalliſe; but it would be neceſſary to ſeparate the exceſs of alkali by alcohol, in order to obtain the whole cryſtalliſation. Now, the action of the alcohol being unequal upon pot-aſh and ſoda, would again produce a change. To theſe conſiderations we may add, that a change of proportions would not only cauſe the force of the ſoda and the pot-aſh (No. 1) to vary, but alſo that of the alcohol, without mentioning the affinity of the water, which ſerves as the ſolvent.

But

But the changes would be much more conſiderable, if barytes were to ſerve as a compariſon with either pot-aſh or ſoda; the force of coheſion poſſeſſed by the ſulphate of barytes would then operate in ſuch a manner, that it would leave the alkali only a very ſmall quantity of ſulphuric acid, which would be much nearer to a meaſure of the force of coheſion of the ſulphate of barytes, compared with that of the ſolvent, than of the affinity of the barytes compared with that of the alkali.

This is ſo true, that if, in order to compare the affinity of the acids for barytes, we were to begin by treating barytes with an exceſs of ſulphuric acid, it would be almoſt intirely precipitated, unleſs the acid were extremely concentrated; we could not diſtinguiſh the combined portion from that which is not, and aſſert, that barytes has a greater affinity for the former part than for the latter. This however is really ſtated, when it is affirmed, that the ſulphuric acid has a greater affinity for barytes than any other acid, becauſe a ſulphate of barytes is formed by precipitation. That effect is attributed to elective affinity, which depends more particularly upon the force of coheſion peculiar to the ſulphate of barytes.

It is therefore manifeſt that the elective affinity of any two ſubſtances, with regard to a third, cannot be determined by a direct experiment, even when the trial is made with two ſubſtances in a liquid ſtate, and which can be neutraliſed by ſaturation; ſince it is neceſſary to introduce foreign powers in order to aſcertain the ſaturation.

5. It has been ſeen in articles II and III, that the affinity of a ſubſtance may be compenſated for by its quantity.

From this conſideration, it would appear to be ſufficient if we were to diſcover the capacities of ſaturation which different baſes have for an acid, or different acids for a baſe, in order to aſcertain the proportion of their relative affinity: for this ſhould ſeem to be in the inverſe proportion of the quantities neceſſary for producing the ſame degree of ſaturation.

This conſequence is erroneous, when we wiſh to apply it to the elective affinities, becauſe as ſoon as two ſubſtances are preſented in order to combine with a third, new forces are eſtabliſhed, which not only determine other reſults, but even change the conſtitution of thoſe ſubſtances.

I compare the ſulphuric and carbonic acids. It is certain that if I bring a quantity of pot-aſh to a term of neutraliſation by the carbonic acid, it exerts a force as great as the quantity of ſulphuric acid which would be neceſſary to produce the ſame effect; and yet if I pour the ſulphuric acid upon the firſt mentioned combination, the whole of the carbonic acid will be diſengaged; becauſe, being no longer retained by an equal force, it again aſſumes the elaſtic ſtate; and even if it be abſorbed and retained by a ſufficient quantity of water, it will no longer be in the ſame ſtate of compreſſion; it will not have the ſame conſtitution; it will no longer be the ſame ſubſtance relative to chemical action. And laſtly, we muſt apply the obſervations made in the preceding numbers to the action of ſubſtances unequally ſaturated.

The compariſon between the capacities of ſaturation, though it may lead to important conſiderations, cannot therefore be applied to the determination of elective affinities.

ARTICLE XI.

On ſome Errors which proceed from a falſe Notion of Elective Affinity.

1. I ſhall here diſcuſs ſeveral opinions which have been adopted relative to elective affinity; I ſhall ſhew how unfounded they are, and ſhall oppoſe to them the application of the principles laid down in the preceding articles:

Baumé has obſerved, that when the ſulphate of pot-aſh is diſſolved by means of heat, in an equal weight of nitric acid, cryſtals of the nitrate of pot-aſh are obtained by cooling: he attributes this decompoſition of the ſulphate of pot-aſh to reciprocal affinities which produce oppoſite combinations, but does not determine the cauſe of this contrary effect.

2. The explanation of this remarkable fact has been contradicted by Bergman. He obſerves, that there are ſome ſalts which have a tendency for an exceſs of acid, ſuch for inſtance as the acidulous tartrite of pot-aſh. He conceives that when theſe ſalts exiſt in a ſtate of neutraliſation, it is neceſſary to conſider their baſe as divided into two parts; one of which is particularly affected by rhe whole action of the acid, in order to form the acidulous ſalt, while the other part only tends to ſatisfy that portion of acidity which remains in the acidulous ſalt. This part of the baſe is therefore only retained by a feeble acidity, and may be carried off by an acid very inferior to that which enters into the firſt combination. Thus the acetous acid may carry off that part of the pot-aſh which, in the tartrite of pot-aſh, is ſuperfluous to the combination which conſtitutes the acidulous tartrite of pot-aſh, though this acid has an affinity much weaker than that of the tartareous acid itſelf.

Now, the ſulphate af pot-aſh is among the number of the ſalts which have a tendency to form an acidulous ſalt. Nearly two thirds of its baſe enter into this combination, and it is this part alone that is ſubject to the whole affinity of the ſulphuric acid. The remaining third may be ſeparated by an acid of an affinity inferior to that of the ſulphuric acid, ſuch as the nitric acid, the muriatic acid, and the tartareous acid. When the decompoſition is carried to this point, it ceaſes, whatever may be the quantity of the acid oppoſed: and if the quantity be not too great to prevent the cryſtalliſation, or if the exceſs be expelled by heat, an acidulous ſulphate is obtained by ſolution and evaporation, which forms permanent cryſtals in the air.

3. How could the illuſtrious Bergman have deviated from the path traced out for him by obſervation? It is proved even by his own experiments, that the acid which is ſuperabundant to the acidulous ſulphate of pot-aſh exerts its affinity, that it exiſts in combination, and acts in proportion to its quantity. For he obſerves, that if ſulphuric acid be added to the acidulous ſulphate of pot-aſh, this ſalt is diſſolved, and loſes its property of cryſtalliſing; that this exceſs of acid is expelled with difficulty, even by diſtillation in a retort; and in order to obtain this effect, it is neceſſary to fuſe the ſaline combination in a crucible, or to employ ſeveral times the repeated action of very pure alcohol.

4. The limit given by Bergman to the action of the acids upon the acidulous ſulphate is therefore imaginary. This ſulphate acts like all the ſalts which are capable of reſiſting, to a certain

a certain degree, the action of an exceſs of acid or of baſe (Art. V. No. 4), as well as that of another acid, or a foreign baſe. The only difference between them in this reſpect depends on the force of coheſion, which may act more or leſs ſtrongly to produce cryſtalliſation, which is peculiar to certain proportions of the acid and its baſe, probably in conſequence of the figure aſſumed by the particles of which the combination conſiſts.

5. When an acid has the property of forming a precipitate, by combining with a baſe, it has been ſuppoſed to poſſeſs a greater affinity for that baſe than the acid with which it was before combined, without even examining to what point the new acid may have effected the decompoſition; and without conſidering that an oppoſite decompoſition would have taken place merely by a change of the proportions, and might conſequently in that caſe have juſtified an oppoſite concluſion.

In this manner as the tartareous acid has the property of forming, with pot-aſh, an acidulous ſalt of difficult ſolubility, and conſequently affords a precipitate with all thoſe ſalts that have pot-aſh for their baſe, and are not diluted with too great a quantity of water, it has been ſuppoſed to have a greater affinity for pot-aſh than the other acids. Bergman has made an exception with reſpect to the ſulphuric acid, becauſe he ſuppoſed that the tartareous acid could only act upon that pot-aſh which exceeds the combination of the acidulous ſulphate of pot-aſh; a ſuppoſition which I truſt has been refuted in the preceding numbers. He has alſo excepted the nitric and muriatic acids, becauſe he imagines that the tartareous acid acts, with reſpect to the nitrate and muriate of pot-aſh, in the ſame manner as with the ſulphate; and neverthelеſs he has not aſcertained the exiſtence of an acidulous nitrate and muriate of pot-aſh ſimilar to the acidulous ſulphate of pot-aſh.

He alſo concludes, from experiments made with the ſalts which have ſoda for their baſe, without however making known theſe experiments, that the tartareous acid ought to be placed after the oxalic acid; but, theſe exceptions apart, the tartareous acid, according to him, completely decompoſes all the other ſalts that have a fixed alkali for their baſe.

What embarraſſes Bergman is, that the tartareous acid does not produce a precipitate with the ſalts which have ſoda for their baſe. He believes that this apparent difference is a conſequence of the ſoda not having the property of forming a ſalt of ſparing ſolubility with an exceſs of acid; but then the indication which is thought to afford evidence of the decompoſition is wanting; and it becomes neceſſary to be ſatisfied with the probability, that the affinities of one fixed alkali follow the ſame order as thoſe of the other.

All this claſſification of affinities is founded upon the falſe ſuppoſition, that one acid will expel another from its combinations by its affinity alone, conſidered as a conſtant force; and this ſuppoſition neceſſarily demands ſeveral others, in order to explain as exceptions, thoſe facts which are naturally derived from a general property.

6. I have examined the decompoſition of the acidulous tartrite of pot-aſh, by the nitric acid, which, according to the received opinions, then alſo adopted by me, ought to decompoſe it, by completely depriving it of its baſe. I therefore put ſome acidulous tartrite of pot-aſh and nitric acid in digeſtion together, and obtained by cooling, ſome fine cryſtals of nitrate

of

of pot-aſh. I repeated the experiment ſeveral times, by adding nitric acid till nitrate of pot-aſh was no longer ſeparated. I then expoſed the liquid to heat capable of making the diſengaged portion of nitric acid evaporate, without altering the tartareous acid. After this the liquid had acquired an oily conſiſtence; it had no ſmell, and did not announce the exiſtence either of the nitric acid or of pot-aſh; but on expoſing it to a ſtrong heat, much nitrous gas was diſengaged, the tartareous acid was reduced to coal, and its aſhes afforded a conſiderable quantity of carbonate of pot-aſh.

7. In this operation one portion of nitrate of pot-aſh is ſeparated by the force of cryſtalliſation in that ſalt, as far as that point at which that force is overcome by the ſuperabundant acid. The acidulous tartar is rendered ſoluble by the action of the nitric acid, which, at the ſame time, deprives it, by cryſtalliſation, of a part of the baſe neceſſary to make it inſoluble.

On the other hand, the tartareous acid, poured on a ſolution of the nitrate of pot-aſh, deprives it of its pot-aſh to a certain degree, and forms an acidulous tartarite which is precipitated; but as it has not the property of forming an acidulous tartarite of ſoda, of ſparing ſolubility, it does not produce any precipitate with thoſe ſalts that have ſoda for their baſe.

In both circumſtances, all that portion which cannot be ſeparated by the force of coheſion, forms a liquid, in which the ſubſtances act in proportion to their preſent maſſes.

Nothing, therefore, can be concluded about the reſpective affinity from ſuch ſeparations as are effected by precipiration or cryſtalliſation, ſince by the mere change of proportions, oppoſite decompoſitions may frequently be obtained.

8. The precipitations which take place when, on comparing the affinities of baſes, one of them forms an inſoluble combination, has given riſe to an error of the ſame kind; and on this ground alone it has been eſtabliſhed that lime has a greater affinity than alkali for the fluoric, phoſphoric, and arſenical acids, or, in a word, with all thoſe which form with it an inſoluble compound; and that conſequently it has the property of entirely decompoſing ſalts formed by the alkali and theſe acids. This precipitation is not the reſult of elective affinity, and it is not complete; but its quantity is determined by the proportion which the action of the fluid bears to the force of coheſion in the precipitate: hence it is that the precipitate is frequently diſſolved again on increaſing the quantity of the liquid ſubſtance oppoſed to it.

9. Although Bergman has well deſcribed the changes which heat is capable of producing in chemical action, when the ſubſtances have a tendency to become volatile, and has alſo warned us againſt employing too ſtrong a heat in evaporation, the whole influence of heat in thoſe operations by which ſalts are ſeparated, in order to form a judgment of their affinities, has not yet been ſhewn.

It would not have been concluded that the ſulphuric acid has more affinity for the fixed alkalis than the nitric or muriatic acids, from the ſimple circumſtance that, by a ſtrong heat,

heat, it expels thoſe acids from their combinations; if attention had at the ſame time been paid to the fact, that even by the heat employed for cauſing the evaporation and obtaining the cryſtalliſation of ſalts, the proportions of the volatile acids may be conſiderably changed by the ſulphuric acid which remains oppoſed to them, and which may even at laſt expel them entirely, by means of the difference which exiſts between its fixity and that of thoſe acids. (Art. VII. No. 5).

10. We are indebted to Bergman for ſome uſeful obſervations relative to the errors that may originate from the ſolubility of a ſubſtance which is diſengaged, and of which the ſeparation is not perceived. He remarks that pot-aſh or ſoda do not affect the tranſparence of a ſolution of ſalt with baſes of lime, if this ſolution be diluted with fifty times its quantity of water; becauſe the lime which is ſeparated, being ſoluble, remains in the water. But he does not conſider that if the lime did then poſſeſs only its natural ſolubility, this would be a very feeble obſtacle to the precipitation; for it would require nearly ſeven hundred parts of water to effect its ſolution. What adds ſo greatly to its natural ſolubility is, that it continues to remain in combination with the acid (Art. V. No. 5), and that it cannot be ſeparated without retaining a part of this acid (Art. III. No. 9), by which its ſolubility is ſo increaſed.

11. Notwithſtanding his general obſervations, Bergman has miſtaken the effects of ſolubility in a variety of circumſtances. For inſtance, he did not believe that the nitric and muriatic acids were capable of acting upon the phoſphate of lime, though the only difference that can be aſcertained in this reſpect between theſe acids and the ſulphuric, without being acquainted with the comparative force of their affinities, is, that the two firſt-mentioned only form ſoluble combinations, while that produced by the ſulphuric acid may in a great meaſure be withdrawn by the force of cryſtalliſation.

12. It is the ſolubility of lime, as well as of barites, acquired by the action of the acid upon thoſe earths, which prevents ammonia from affording any precipitate in the ſolution of thoſe ſalts of which they are the baſe. Nevertheleſs the firſt portion of ammonia which is mixed, for inſtance, with the muriate of lime, emits ſcarcely any ſmell, which indicates that it has entered into combination; and its action may be rendered perceptible, as we ſhall ſhew.

I mixed ſome ammonia with a ſolution of muriate of lime, and evaporated the liquid in a retort. When it was reduced to a certain point, a conſiderable quantity of precipitate was formed. I continued the experiment: towards the end the quantity of precipitate was much diminiſhed: a pellicle was formed, and by cooling a great quantity of cryſtals in needles of a tolerable length were obtained. It was a triple ſalt, from which ammonia might be diſengaged by lime. This ſalt, on being again diſſolved and evaporated in the open air, no longer afforded indications of ammonia when tried by lime.

It is therefore evident, that when the water was not too abundant, the ammonia precipitated part of the lime, though it was rendered much more ſoluble by the acid, and though the action of the ammonia was conſiderably weakened by heat, which diminiſhed both

both its affinity and its quantity. In proportion as the latter was reduced, the precipitate was again diſſolved; notwithſtanding which, there were ſome remains of ammonia after a long evaporation, and it was only by means of the action of the air that it was entirely diſſipated. The ſeparation of the lime would doubtleſs become much more ſenſible, if ammoniacal gas were received in a ſlightly diluted ſolution of the muriate of lime.

When ammonia produces a precipitate with the ſalts that have alumine for their baſe, it is becauſe this earth is leſs ſoluble than lime, even when it is combined with the portion of acid which it retains when precipitated.

ARTICLE XII.

On Complex Affinities.

1. I ſhall proceed to examine, under the more general name of *complex affinity*, what has been conſidered as due to the concurrence of four affinities, and which has been commonly known by the name of *double affinity*.

To give an idea of the action of four affinities, Bergman examined the effect which takes place on mixing the ſolution of the ſulphate of pot-aſh with that of the muriate of lime. It is, ſays he, the ſame as if the proportions of ſulphuric and muriatic acid, of lime and pot-aſh, which entered into the compoſition of theſe ſalts, were put in the quantity of water employed; the two baſes act by their affinities upon the two acids: but though the affinity of the pot-aſh for the ſulphuric acid be ſtronger than what it has for the muriatic acid, yet the affinity of the latter for the pot-aſh, added to the affinity of the ſulphuric acid for the lime, gives an aggregate of forces greater than the affinity of the ſulphuric acid for the pot-aſh, and that of the muriatic acid for lime: thus an exchange of baſes is determined; ſo that inſtead of ſulphate of pot-aſh and muriate of lime, we have ſulphate of lime and muriate of pot-aſh. This explanation is always founded on the ſuppoſition that the affinities are conſtant powers, independently of the quantities and ſtate of ſaturation.

2. When two baſes act together upon an acid, the latter is divided, or rather divides its action in proportion to their maſſes; and if inſtead of one we have two acids, and no ſeparation is effected either by precipitation or cryſtalliſation, both the acids will act equally upon the two baſes in the proportion of their maſſes. If each of theſe acids were originally combined with a baſe, the amount of the mutual forces of the acids and alkalis, after mixing the ſolutions of the ſalts, will be the ſame as before. No muriate of pot-aſh nor ſulphate of lime are formed; but there will be a combination of pot-aſh, lime, ſulphuric acid, and muriatic acid, which will give the ſame degree of ſaturation as before the mixture. Hence it is, that on mixing two ſalts, which, by exchange, ought to produce combinations that would have very different proportions of principles, there is not, as has been juſtly remarked by Guyton, either the acidity or the alkalinity which would neceſſarily appear if ſuch a change were really to take place.

3. The exchange of baſes has been infered merely from the reſult of the precipitation and cryſtalliſation which have been obſerved; but this effect has not been attributed to its real cauſe.

In Art. V. it has been ſhewn that the force of coheſion determines the ſeparation which, in the elective affinities, takes place by precipitation or cryſtalliſation. It is alſo the ſame force which produces the ſame effect in the complex affinities. When I mix the ſolution of ſulphate of pot-aſh with that of the muriate of lime, the quantity of water not being great, the lime, in its contact with the ſulphuric acid (No. 1), is ſubject to the effect of the force of coheſion in a higher degree than the pot-aſh. Thus a new force is added to thoſe which previouſly exiſted; and this muſt determine the combination of the ſulphuric acid with the lime, at the ſame time as its precipitation.

4. If we take a rapid view of all the known decompoſitions which are owing to the complex affinities, and we ſhall ſee that it is always to the ſubſtances which have the property of forming a precipitate, or a ſalt that may be ſeparated by cryſtalliſation, that an exceſs of affinity has been attributed beyond thoſe oppoſed to them: ſo that we may foretel by the degree of ſolubility of the ſalts which are formed in a liquid, which are the ſubſtances of which Bergman and other learned chemiſts have pretended to repreſent the forces in their ſymbolical tables, by always attributing a ſuperiority of affinity to the two ſubſtances which are diſpoſed to form an inſoluble combination relative to the quantity of the ſolvent.

Lime, magneſia, barites, and ſtrontian, form, with the carbonic acid, inſoluble ſalts; all the ſoluble combinations of theſe earths, when mixed with carbonates of alkalis, produce an exchange, from which the formation and precipitation of carbonates with earthy baſes reſult.

Barites forms, with the ſulphuric acid, an inſoluble ſalt. Every time the ſolution of a ſulphate is mixed with that of a ſalt with baſe of barites, a ſalt having a ſimilar baſe, is formed and precipitated.

As lime makes a ſparingly ſoluble ſulphate, which is in a great meaſure precipitated, if there be not much water, it alſo changes its baſe with all the ſoluble ſulphates, till the period when precipitation ceaſes, by the ſolubility of the ſulphate of lime. The ſulphate of lime being ſtill more ſoluble than the ſulphate of barites, the more ſoluble ſalts with baſe of barites decompoſe the ſulphate of lime.

The oxide of ſilver forms an inſoluble ſalt with the muriatic acid: all the ſalts of ſilver which are ſoluble, being mixed with ſoluble muriates, the muriate of ſilver is precipitated. Mercury, which is not too much oxided, acts in the ſame manner.

As the muriate of lead is ſcarcely ſoluble, the ſalts which the oxide of lead forms with the other acids, and which poſſeſs ſolubility, produce a precipitate with the ſoluble muriates; but as it makes an inſoluble ſalt with the ſulphuric acid, the ſolution of muriate of lead affords a precipitate of ſulphate of lead, when mixed with ſoluble ſulphates.

5. When, therefore, water is evaporated, in which ſeveral ſalts have been put in a ſtate of ſolution, they will be found to ſeparate according to the order of their ſolubility, and it is from this property that we are able to aſcertain the changes of baſe which may be effected.

But the ſolubility of ſalts varies according to the temperatures. It is, therefore, the ſolubility relative to the different temperatures which ought to be conſidered. The nitrate of pot-aſh, mixed with the muriate of ſoda, will cryſtalliſe at a low temperature; but the muriate of ſoda will ſeparate even during the time of evaporation. No exchange of baſes will be effected, becauſe the nitrate of ſoda is a little more ſoluble in cold than the nitrate of pot-aſh, and on the contrary, the muriate of pot-aſh is rather more ſoluble in heat than the muriate of ſoda.

6. I here attend only to the principal reſult, ariſing from a force of coheſion ſo great, that it cauſes the effect of the forces oppoſed to it to diſappear. But when this power is not conſiderable in the combinations that may be formed, the mutual action of the ſubſtances which remain in the liquid ſtate, that of the ſolvent, and the proportions which vary by the cryſtalliſation of a combination that is ſeparated, muſt produce different effects. The experiments I have began will not fail to elucidate this ſubject.

7. Another circumſtance may change the action of the complex affinities; namely, the formation of a triple ſalt which precipitates; but by knowing the degree of ſolubility of this combination, we may ſtill foretel the decompoſition that will take place. The ſame conſideration is applicable to the affinities called *elective.*

8. A precipitate is ſometimes effected by the mixture of two ſaline ſubſtances which have the ſame acid; for inſtance, by a mixture of the muriate of magneſia and the muriate of lime. It is probable that two combinations are then formed, one with an exceſs of acid and a ſmall part of the two baſes, the other with the greateſt part of the two baſes and a ſmall portion of acid.

This effect is analogous to what we have obſerved in Art. IX. No. 3; but here it is the mutual affinity of the two baſes which determines the precipitation.

9. We have ſeen in Art. VII. that heat, by increaſing the volatility of a ſubſtance, weakens its combination. This cauſe acts no leſs in the complex than in the elective affinities. It is a force added to thoſe which act, and determines the union and the ſeparation of thoſe ſubſtances which have the greateſt tendency to form a volatile combination.

Whenever, therefore, it is deſired to know what will happen, by expoſing two ſalts to the action of heat, it is only neceſſary to examine whether one of the two baſes and one of the two acids poſſeſs a greater degree of volatility than the remaining baſe and acid, and we may be aſſured that, on applying a ſufficient degree of heat, the combination of the moſt volatile baſe and acid will be formed and ſublimed, while the more fixed baſe and acid will alſo remain combined. Among the baſes, ammonia and the oxide of mercury, and among the acids, the carbonic and muriatic, afford ſeveral illuſtrations of this truth.

10. Efflorescence ought alſo to be conſidered as a force which, in the complex affinities, may determine a combination poſſeſſing this property, and to this is owing the formation of natron in the valley of the Lake of Natron, and other places where the ſame circumſtances occur.

The obſervations which I have preſented to the Inſtitute of Egypt, which will form a ſequel to the intereſting diſcovery of the valley of the Lakes of Natron, for which we are indebted to General Andreoſſi, prove that the circumſtances neceſſary to the formation of natron are, 1. a ſand, which contains much carbonate of lime; 2. moiſture; and, 3. the preſence of the muriate of ſoda: I have alſo remarked that the ſtalks of the reed in a great degree facilitate the production of this ſubſtance. I have promiſed to explain the formation of the carbonate of ſoda by means of theſe circumſtances, which I ſhall now endeavour to perform.

Calcareous ſand, conſtantly impregnated with moiſture, may be conſidered as a ſolution of the muriate of ſoda, which acts upon the carbonate of lime. Now it follows, from what has been explained in Art. IV. that inſolubility cauſes a great diminution in the mutual action of a ſolid and a liquid ſubſtance, but that it does not deſtroy it. This action is oppoſed to the inſolubility of the carbonate of lime, which is not abſolute. A ſolution muſt therefore be formed of a ſmall quantity of carbonate of lime, and conſequently (No. 1 and 2), the conſtituent parts of this carbonate and of the muriate of ſoda, which are in ſolution, exert a reciprocal action; otherwiſe the preſence of the carbonate of lime would not be a condition neceſſary to the formation of the carbonate of ſoda*.

We muſt, therefore, conſider the humidity of the calcareous ſand, in which the carbonate of ſoda is formed, as a ſolution of muriate of ſoda and a ſmall quantity of carbonate of lime: hence the ſoda is found in the preſence of the carbonic acid, and the efflorescence, which is a property of the carbonate of ſoda, ought to be conſidered as a new force that tends to remove it from this combination. In ſhort, when, in a ſoil impregnated with muriate of ſoda, we meet with the ſtalks of reeds which favour the efflorescence, the carbonate of ſoda not only accumulates round theſe ſtalks, but ſometimes it is not formed

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* The ſolution of the carbonate of lime by the muriate of pot-aſh, and by the ſulphate of pot-aſh, which indubitably act like the muriate of ſoda, has been proved by a direct experiment, for which we are indebted to Guyton (*Mem. de Scheele*, part II. note de la page 18). "The ſolution of ſulphate of pot-aſh, "muriate of pot-aſh, &c. poured in lime water which has been rendered milky by water impregnated with "carbonic acid gas, immediately cauſed the precipitate to diſappear. There was likewiſe no earthy preci-"pitate when water, charged with carbonic acid gas, was poured in a mixture of lime water and a ſolution "of theſe neutral ſalts; the liquor always contained a portion of uncombined alkali." Guyton combats the opinion of Scheele, who did not obſerve any decompoſition with the muriate and ſulphate with baſe of pot-aſh, but only with the ſalts with baſe of ſoda.

The difference of opinion between thoſe celebrated chemiſts aroſe from the circumſtance that one aſcertained the decompoſition only by the efflorescence, which is peculiar to ſalts with baſe of ſoda (Art. VIII.) while the other obſerved it in a liquid; but in the latter inſtance it is much more limited.---B.

without ſuch aſſiſtance, when certain circumſtances, ſuch as a too argillaceous nature of the ſoil, &c. are little favourable to its production; ſo that at a ſhort depth muriate of ſoda only is diſcoverable.

I wiſhed to aſcertain the truth of this explanation in a trough placed in one of the gardens belonging to the Inſtitute. For this purpoſe ſome carbonate of lime and ſiliceous earth, both well waſhed, were mixed together, to which was added a certain proportion of muriate of ſoda: a hole was made in this mixture, for the purpoſe of pouring in water occaſionally, and keeping up the neceſſary degree of moiſture. An incruſtation of muriate of ſoda is formed at the ſurface, which already ſtrongly changes the colour of paper tinged with fernambuc, like the alkalis; but we cannot expect to obtain an effloreſcence ſo conſiderable as to be perceptible to the ſight, till after a much greater length of time.

11. The preceding obſervations ſhew that the only difference which diſtinguiſhes the complex affinities from thoſe called *elective*, is, that in the former, ſubſtances are brought into action which are nearly in an uniform degree of ſaturation; and that in the latter, there is a ſubſtance preſent which is not yet ſaturated (or ſeveral ſuch ſubſtances may be preſent); ſo that in the former, a new degree of ſaturation is eſtabliſhed only in proportion to the combinations which are capable of being ſeparated; whereas, in the latter, the action of the unſaturated ſubſtances becomes in equilibrio with that of the ſubſtances which were already ſo; whence it happens that the force of coheſion and that of elaſticity produce their effect more completely in the complex than in the elective affinities.

VI.

Experiments on the Solar and on the Terreſtrial Rays that occaſion Heat; with a comparative View of the Laws to which Light and Heat, or rather the Rays which occaſion them are ſubject, in order to determine whether they are the ſame or different. By WM. HERSCHELL, *LL.D. F.R.S.*[*]

IN the former part of this paper, of which an abridgment was given at page 360 of our laſt volume, it was ſhewn that heat derived immediately from the ſun, or from ignited terreſtrial ſubſtances, is occaſioned by rays emanating from them, and that ſuch rays are ſubject to the laws of reflection and of refraction. The ſimilarity between light and heat was the moſt remarkable feature of the experiments there related. In the preſent memoir the author proceeds to point out ſome ſtriking and ſubſtantial differences in the rays of light and heat which afford data for diſcuſſing whether there two effects be produced by one and the ſame power.

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[*] Abridged from the Philoſophical Tranſactions, 1800, page 437.

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The various refrangibility of the rays of heat from thoſe of light was ſhewn by thoſe experiments with the priſm, which undoubtedly conſtitutes a great point of reſemblance. But he points out a very material difference, conſiſting in the much greater range over which the rays of heat are diſperſed when compared with thoſe of light. He has exhibited this by delineating a ſpectrum of light of a certain aſſumed length repreſented by the line G Q (in Fig. 5, Plate III.), divided into ſeven parts, according to the dimenſions aſſigned to the colours by Newton; and to this he has drawn perpendicular lines, repreſenting the intenſity of illumination poſſeſſed by the light at each particular part. The length of the maximum perpendicular being arbitrary, he has aſſumed it to be $\frac{27}{3\cdot 3}$ of the whole line, namely, L R at the confines of the yellow and green. The lengths of the other ordinates to the curve, namely, K M H I N O P, are alſo determined in relative length and poſition from his former experiments.

To find a figure in the ſame manner to expreſs the heating power of the refracted priſmatic rays, which may be called the ſpectrum of heat, he examined the extent of the inviſible rays, and found that its line was in proportion to that of the viſible ſpectrum as 2 to 3, or rather, as ſome allowance muſt be made beyond the laſt ordinate, as $2\frac{1}{4}$ to 3. Whence the whole calorific ſpectrum proves to be as $5\frac{1}{4}$ to 3, with regard to the luminous ſpectrum. And if the maximum of heat be expreſſed by a line of the ſame length as that which denotes the maximum of light, and other ordinates be drawn to expreſs the proportions of degrees indicated by the thermometer at the various points of the whole ſpectrum, another curve B C D E F G H I K L M N O P will be obtained to expreſs the intenſities of heat. From the mere inſpection of theſe two figures, thus drawn as lying upon one another, it is ſeen how very differently the priſm diſperſes the heat making rays and thoſe which occaſion illumination. Theſe rays neither agree in their mean refrangibility nor in the ſituation of their maxima. At R, where there is moſt light, there is but little heat, and at S, where there is moſt heat, there is no light at all!

Experiment 21. *The ſines of refraction of the heat making rays are in a conſtant ratio to the ſines of incidence.* A priſm was placed half an inch beyond the laſt viſible red colour of the ſolar ſpectrum from a priſm, and another at the diſtance of an inch, while a third was placed beſide the ſpectrum as a ſtandard. The two firſt exhibited change of temperature, as in the former trials of this kind, while the laſt underwent no change. Other experiments, with the like event, were tried with priſms of different refracting angles and of different kinds of glaſs, and alſo of water. Hence, as the viſible part of the rays has long been determined to have the conſtant ratio here mentioned, and the inviſible part always bore the ſame relative poſition to theſe, it muſt follow that that part alſo is deflected according to the ſame law.

Experiment 22. *The different refrangibility of heat is corrected by contrary refraction in different mediums.* A priſm of crown glaſs, having an angle of 25 degrees, another of flint glaſs, with an angle of 24, and a third of crown glaſs, with an angle of 10 degrees, being

being put together as they are placed when experiments of achromatic refractions are to be made, were found to give a ſpectrum nearly without colour. The thermometer being placed in the middle of this ſpectrum, had its temperature raiſed two degrees; but a thermometer placed near either end of the ſpectrum was not affected. This reſult ſhews that the different refrangibility of heat, as well as that of light, admits of priſmatic correction, and it alſo confirms the conſtant ratio of the ſines of refraction to thoſe of incidence.

Experiment 23. *In burning glaſſes the focus of the rays of heat is different from the focus of the rays of light.* A burning lens was placed in the rays of the ſun, its aperture being reduced to three inches, in order to leſſen aberration. The place of the luminous focus was aſcertained by throwing hair powder into the air with a puff. A ſtick of ſealing wax was then held 1″, 6 or 4 beats of a chronometer in the contracted pencil half an inch nearer to the lens than the focus. In this time no impreſſion was made upon the wax. It was then applied half an inch farther from the lens than that focus, and in eight-tenths of a ſecond or two beats of the ſame chronometer it was conſiderably ſcorched. When the ſealing wax was alſo expoſed to the focus of light, it was equally affected in the ſame time. Whence the Doctor apprehends we may ſafely conclude, notwithſtanding the little accuracy to be expected from ſo coarſe an experiment, that the focus of heat was certainly farther from the lens than the focus of light, and probably not leſs than one quarter of an inch; the heat at half an inch beyond the focus of light being ſtill equal to that in the focus itſelf.

In the next place our author proceeds to the ſubject of the tranſmiſſion of heat through diaphanous bodies. The experiments made by the priſm, the lens, and the mirror, are ſubject to certain natural imperfections, which muſt impair the progreſs of ſcientific induction. The priſm refracts, reflects, tranſmits, and ſcatters them at the ſame time, and the laws by which it acts in every one of theſe operations require to be inveſtigated. A lens not only partakes of the errors of the priſm, but alſo is ſubject to the aberration from its ſpherical figure. And a mirror, beſides its natural incapacity of ſeparating the rays of light from thoſe of heat, ſcatters them very profuſedly; but our author adds that the ſcanty proviſion of materials to act upon theſe rays has been partly our own fault, as every diaphanous body may become a new tool in the hands of a diligent obſerver.

(*To be continued.*)

VII.—*Outlines*

VII.

Outlines of Experiments and Inquiries reſpecting Sound and Light. By THOMAS YOUNG *M. D. F. R. S.*[*]

IT has long been my intention to lay before the Royal Society a few obſervations on the ſubject of ſound; and I have endeavoured to collect as much information, and to make as many experiments, connected with this inquiry, as circumſtances enabled me to do; but, the further I have proceeded, the more widely the proſpect of what lay before me has been extended; and, as I find that the inveſtigation, in all its magnitude, will occupy the leiſure hours of ſome years, or perhaps of a life, I am determined, in the mean time, leſt any unforeſeen circumſtances ſhould prevent my continuing the purſuit, to ſubmit to the Society ſome concluſions which I have already formed from the reſults of various experiments. Their ſubjects are, I. The meaſurement of the quantity of air diſcharged through an aperture. II. The determination of the direction and velocity of a ſtream of air proceeding from an orifice. III. Ocular evidence of the nature of ſound. IV. The velocity of ſound. V. Sonorous cavities. VI. The degree of divergence of ſound. VII. The decay of ſound. VIII. The harmonic ſounds of pipes. IX. The vibrations of different elaſtic fluids. X. The analogy between light and ſound. XI. The coaleſcence of muſical ſounds. XII. The frequency of vibrations conſtituting a given note. XIII. The vibrations of chords. XIV. The vibrations of rods and plates. XV. The human voice. XVI. The temperament of muſical intervals.

I. *Of the Quantity of Air diſcharged through an Aperture.*

A piece of bladder was tied over the end of the tube of a large glaſs funnel, and punctured with a hot needle. The funnel was inverted in a veſſel of water; and a gage, with a graduated glaſs tube, was ſo placed as to meaſure the preſſure occaſioned by the different levels of the ſurfaces of the water. As the air eſcaped through the puncture, it was ſupplied by a phial of known dimenſions, at equal intervals of time; and, according to the frequency of this ſupply, the average height of the gage was ſuch as is expreſſed in the firſt Table. It appears, that the quantity of air diſcharged by a given aperture, was nearly in the ſubduplicate ratio of the preſſure; and that the ratio of the expenditures by different apertures, with the ſame preſſure, lay between the ratio of their diameters and that of their areas. The ſecond, third, and fourth Tables ſhow the reſult of ſimilar experiments, made with ſome variations in the apparatus. It may be inferred, from comparing the experiments on a tube with thoſe on a ſimple perforation, that the expenditure is increaſed, as in water, by the application of a ſhort pipe.

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[*] Philoſ. Tranſactions, 1800, p. 106.

Table

Table I.

A	B	C
.00018	.25	3.9
.00018	.58	11.7
.00018	1.	15.6
.001	.045	7.8
.001	.2	15.6
.001	.7	31.2
.004	.35	46.8

A is the area, in ſquare inches, of an aperture nearly circular. B, the preſſure in inches. C, the number of cubic inches diſcharged in one minute.

All numbers throughout this paper, where the contrary is not expreſſed, are to be underſtood of inches, linear, ſquare, or cubic.

Table II.

A	B	C
.07	1.	2000.
.07	2.	2900.

A is the area of the ſection of a tube about two inches long. B, the preſſure. C, the quantity of air diſcharged in a minute, by eſtimation.

Table III.

A	B	C	D
.0064	1.15	.2	46.8
.0064	10.	.45	46.8
.0064	13.5	.35	31.2
.0064	13.5	.7	46.8

A is the area of the ſection of a tube. B, its length. C, the preſſure. D, the diſcharge in a minute.

Table IV.

A	B	C
.003	.28	46.8

A is the area of an oval aperture, formed by flattening a glaſs tube at the end: its diameters were .025 and .152. B, the preſſure. C, the diſcharge.

II. *Of the Direction and Velocity of a Stream of Air.*

An apparatus was contrived for meaſuring, by means of a water-gage communicating with a reſervoir of air, the preſſure by which a current was forced from the reſervoir through a cylindrical tube; and the gage was ſo ſenſible, that, a regular blaſt being ſupplied from the lungs, it ſhewed the ſlight variation produced by every pulſation of the heart. The current of air iſſuing from the tube was directed downwards, upon a white plate, on which a ſcale of equal parts was engraved, and which was thinly covered with a coloured liquid; the breadth of the ſurface of the plate laid bare, was obſerved at different diſtances from the tube, and with different degrees of preſſure, care being taken that the liquid ſhould be ſo ſhallow as to yield to the ſlighteſt impreſſion of air. The reſults are collected in Tables v. and vi. and are exhibited to the eye in Plate IV. Figs. 1—12. In order to meaſure with greater certainty and preciſion, the velocity of every part of the current, a ſecond cavity, furniſhed with a gage, was provided, and pieces perforated with apertures of different ſizes were adapted to its orifice: the axis of the current was directed as accurately as poſſible to the centres of theſe apertures, and the reſult of the experiments, with various preſſures and diſtances, are inſerted in Tables vii. viii. and ix. The velocity of a ſtream being, both according to the commonly received opinion and to the experiments already related, nearly in the ſubduplicate ratio of the preſſure occaſioning it, it was inferred, that an equal preſſure would be required to ſtop its progreſs, and that the velocity of the current, where it ſtruck againſt the aperture, muſt be in the ſubduplicate ratio of the preſſure marked by the gage. The ordinates of the curves in Figs. 13—23, were therefore taken reciprocally in the ſubduplicate ratio of the preſſure marked by the ſecond gage to that indicated by the firſt, at the various diſtances repreſented by the abſciſſes. Each figure repreſents a different degree of preſſure in the firſt cavity. The curve neareſt the axis, is deduced from obſervations in which the aperture oppoſed to the tube was not greater than that of the tube itſelf; and ſhows what would be the diameter of the current, if the velocities of every one of its particles in the ſame circular ſection, including thoſe of the contiguous air, which muſt have acquired as much motion as the current has loſt, were equal among themſelves. As the central particles muſt be ſuppoſed to be leſs impeded in their motion than the ſuperficial ones, of courſe, the ſmaller the aperture oppoſed to the centre of the current, the greater the velocity ought to come out, and the ordinate of the curve the ſmaller; but, where the aperture was not greater than that of the tube, the difference of the velocities at the ſame diſtance was ſcarcely perceptible. When the aperture was larger than that of the tube, if the diſtance was very ſmall, of courſe, the average velocity came out much ſmaller than that which was inferred from a ſmaller aperture; but, where the ordinate of the internal curve became nearly equal to this aperture, there was but little difference between the velocities indicated with different apertures. Indeed, in ſome caſes, a larger aperture ſeemed to indicate a greater velocity: this might have ariſen in ſome degree from the ſmaller aperture not having been exactly in the centre of the current; but there is greater reaſon to ſuppoſe, that it was occaſioned by ſome reſiſtance derived from the air returning between the ſides of the aperture and the current entering it. Where this took place, the external curves, which are ſo conſtructed as that their ordinates are reciprocally in the ſubduplicate ratio of the preſſure obſerved in the ſecond cavity, with apertures equal in ſemidiameter to their initial ordinate, approach, for a ſhort diſtance, nearer to the axis than the internal curve: after this, they continue their courſe very near to this curve. Hence it appears, that no obſervable part of the motion diverged beyond the limits of the ſolid which would be formed by the revolution of the internal curve, which is ſeldom inclined to the axis in an angle ſo great as ten degrees. A ſimilar concluſion may be made, from obſerving the flame of a candle ſubjected to the action of a blowpipe: there is no divergency beyond the narrow limits of the current; the flame, on the contrary, is every where forced by the ambient air towards the current, to ſupply the place of that which it has carried away by its friction. The lateral communication of motion, very ingeniouſly and accurately obſerved in water by Profeſſor Venturi, is exactly ſimilar to the motion here ſhown to take place in air; and theſe experiments fully juſtify him in rejecting the tenacity of water as its cauſe: no doubt it ariſes from the relative ſituation of the particles of the fluid, in the line of the current, to that of the particles in the contiguous ſtrata, which is ſuch as naturally lead to a communication of motion nearly in a parallel direction; and this may properly be termed friction. The lateral preſſure which urges the flame of a candle towards the ſtream of air from a blowpipe, is probably exactly ſimilar to that preſſure which cauſes the inflection of a current of air near an obſtacle. Mark the dimple which a ſlender ſtream of air makes on the ſurface of water; bring a convex body into contact with the ſide of the ſtream, and the place of the dimple will immediately ſhow that the current is inflected towards the body; and, if the body be at liberty to move in every direction, it will be urged towards the current, in the ſame manner as, in Venturi's experiments, a fluid was forced up a tube inſerted into the ſide of a pipe through which water was flowing. A ſimilar interpoſition of an obſtacle in the courſe of the wind, is probably often the cauſe of ſmoky chimneys. One circumſtance was obſerved in theſe experiments, which it is extremely difficult to explain, and which yet leads to very important conſequences: it may be made diſtinctly perceptible to the eye, by forcing a current of ſmoke very gently through a fine tube. When the velocity is as ſmall as poſſible, the ſtream proceeds for many inches without any obſervable dilatation; it then immediately diverges at a conſiderable angle into a cone, Plate IV. Fig. 24; and, at the point of divergency, there is an audible and even viſible vibration. The blowpipe alſo affords a method of obſerving this phenomenon: as far as can be judged from the motion of the flame, the current ſeems to make ſomething like a revolution in the ſurface of the cone, but this motion is too rapid to be diſtinctly diſcerned. When the preſſure is increaſed, the apex of the cone approaches nearer to the orifice of the tube, Figs. 25, 26; but no degree of preſſure ſeems materially to alter its divergency. The diſtance of the apex from the orifice, is not proportional to the diameter of the current; it rather appears to be the greater, the ſmaller the current, and is much better defined in a ſmall current than in a large one. Its diſtance in one experiment is expreſſed in Table X, from obſervations on the ſurface of a liquid; in other experiments, its reſpective diſtances were ſometimes conſiderably leſs with the ſame degrees of preſſure. It may be inferred, from the numbers of Tables VII and VIII, that in ſeveral inſtances a greater height of the firſt gage produced a leſs height of the ſecond: this aroſe from the nearer approach of the apex of the cone to the orifice of the tube, the ſtream loſing a greater portion of its velocity by this divergence than it gained by the increaſe of preſſure. At firſt ſight, the form of the current bears ſome reſemblance to the *vena contracta* of a jet of water: but Venturi has obſerved, that in water an increaſe of preſſure increaſes, inſtead of diminiſhing, the diſtance of the contracted

ſection

ſection from the orifice. Is it not poſſible, that the facility with which ſome ſpiders are ſaid to project their fine threads to a great diſtance, may depend upon the ſmall degree of velocity with which they are thrown out, ſo that, like a minute current, meeting with little interruption from the neighbouring air, they eaſily continue their courſe for a conſiderable time?

Table v.

A	1.	2.	3.	3.8
B	C	C	C	C
1.	.1	.1	.1	
2.	.12	.12	.2	
3.	.17	.25	.3	
4.	.2	.4	.4	
5.	.25	.5		
6.	.30	.52		
7.	.35	.54	.5	
8.	.37	.56		
9.	.39	.58		
10.	.40	.6	.6	.5
15.		.7		
18.	.50			
20.				

The diameter of the tube .07. A is the diſtance of the liquid from the orifice. B, the preſſure. C, the diameter of the ſurface of the liquid diſplaced.

Table vi.

A	1.	2.
B	C	C
1.	.1	.1
2.	.13	
3.	.2	.2
4.	.25	.3
6.	.3	.4
7.	.35	.5
10.	.35	.6
15.	.35	.7
20.	.35	.7

Diameter of the tube, .1. A, B, and C, as in Table v.

Table vii.

A	.5	
B	.06	.15
C	D	D
.1	.083	
.2	.16	
.3	.25	.1
.4	.35	
.5	.45	
.6	.53	.2
.7	.6	
.8		.3
1.	.5	
1.2	.4	.4
1.5	.6	
2.	.67	.55
4.	1.3	1.
8.		2.
9.	.3	
14.	.5	

Diameter of the tube .06.

A is the diſtance of the oppoſite aperture, from the orifice of the tube. B, the diameter of the aperture. C, the preſſure, indicated by the firſt gage. D, the height of the ſecond gage.

Table

Table VIII.

A	.5				1.				2.				4.			
B	.06	.15	.3	.5	.06	.15	.3	.5	.06	.15	.3	.5	.06	.15	.3	.5
C	D	D	D	D	D	D	D	D	D	D	D	D	D	D	D	D
.1	.05	.05			.03				.017							
.2	.1	.1			.12	.08	.02		.034							
.5	.2	.22			.1	.00			.00							
1.	.32	.36	.1		.17	.1	.1	.05	.04							
2.	.52	.6	.2		.28	.22	.21	.08	.07							
3.	.8	.9	.3		.4	.36	.32	.12	.12		.1	.1				
4.	1.1	1.2	.4		.58	.52	.42	.16	.18		.15	.14				
5.		1.5	.5		.8	.68	.52	.2	.23		.2	.18	.04		.04	.05
6.		1.7	.6		1.	.83	.63	.25	.3		.25	.22	.05		.05	.06
7.		1.9	.7		1.2	1.	.75	.3	.35		.3	.26	.06		.06	.07
8.		2.1	.8		1.5	1.2	.88	.34	.4		.34	.3	.07		.07	.07
9.		2.3	.9		1.7	1.4	1.	.37	.45		.37	.34	.08		.08	.08
10.		2.6	1.		1.9	1.6	1.1	.4	.5		.4	.37	.09		.09	.09

Diameter of the tube .1. A, B, C, and D, as in Table VII.

Table IX.

A	1.15				3.3			4.
B	.15	.3	.5	1.	.06	.15	1.	.06
C	D	D	D	D	D	D	D	D
.5	.1	.1	.1					
1.	.2	.2	.2					
2.	.4	.35	.34	.13	.1	.1	.125	
3.	.6	.5	.5	.2	.15	.15	.18	.1

Diameter of the tube .3. A, B, C, and D, as in Table VII.

Table X.

A	B
.4	6.
.8	3.
1.2	1.5
1.8	1.
2.	.5
4.	.0

A is the preſſure. B, the diſtance of the apex of the cone from the orifice of a tube .1 in diameter.

III. *Ocular Evidence of the Nature of Sound.*

A tube about the tenth of an inch in diameter, with a lateral orifice half an inch from its end, filed rather deeper than the axis of the tube, Fig. 27, was inſerted at the apex of a conical cavity containing about twenty cubic inches of air, and luted perfectly tight: by blowing through the tube, a ſound nearly in uniſon with the tenor C was produced. By gradually increaſing the capacity of the cavity as far as ſeveral gallons, with the ſame mouth-piece, the ſound, although faint, became more and more grave, till it was no longer a muſical note. Even before this period a kind of trembling was diſtinguiſhable; and this, as the cavity was ſtill further increaſed, was changed into a ſucceſſion of diſtinct puffs, like the ſound produced by an exploſion of air from the lips; as ſlow, in ſome inſtances, as 4 or 3 in a ſecond. Theſe were undoubtedly the ſingle vibrations, which, when repeated with ſufficient frequency, impreſs on the auditory nerve the ſenſation of a continued ſound. On forcing a current of ſmoke through the tube, the vibratory motion of of the ſtream, as it paſſed out at the lateral orifice, was evident to the eye; although, from various circumſtances, the quantity and direction of its motion could not be ſubjected to exact menſuration. This ſpecies of ſonorous cavity ſeems ſuſceptible of but few harmonic ſounds. It was obſerved, that a faint blaſt produced a much greater frequency of vibrations than that which was appropriate to the cavity: a circumſtance ſimilar to this obtains alſo in large organ pipes; but, ſeveral minute obſervations of this kind, although they might aſſiſt in forming a theory of the origin of vibrations, or in confirming ſuch a theory drawn from other ſources, yet, as they are not alone ſufficient to afford any general concluſions, are omitted at preſent, for the ſake of brevity.

(To be continued.)

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Galvanic Pile conſiſting of one and the ſame Metal throughout.

MR. Humphry Davy, who is at preſent attached to the Royal Inſtitution, has diſcovered, that the difference of the metals is not eſſential to the effect produced in the pile of Volta. From his former reaſoning on theſe important phenomena, (ſee our Journal IV. 380, 394) he was led to make trial of powers by which that difference of oxydation at the ſurfaces, which appears to be eſſential to the effect, and has hitherto ariſen from the habitudes of the metals themſelves, might be produced with ſimilar conſequences by changing the ſolvents. The metals hitherto tried each to form a pile by itſelf, and as far as my recollection ſerves are ſilver, copper, zinc, and lead; and one of the arrangements of the chemical agents is thus; a plate of metal, cloth ſoaked in diluted nitrous acid, cloth ſoaked in water, cloth ſoaked in ſulphuret of pot-aſh; then another plate of the ſame metal, and the three cloths as before: in which manner the repetitions may be continued at pleaſure. If a trough be uſed with cells, and the ſeparation between the acid and the ſulphuret of pot-aſh be made by a plate of horn inſtead of the cloth imbibed with water, the two fluids may be connected by a ſlip of wetted cloth hung over the upper edge of the horn. This will complete the communication without occaſioning any mixture, becauſe water is lighter than either of the other fluids. It is remarkable that the effects of this pile are the reverſe of what happens when the difference of action depends on the metals; that is to ſay, the extrication of hydrogen in the tube of communication, is made from the oppoſite wire to that from which it would have been made with the common pile.

Among the reflections and concluſions to which this valuable experiment muſt give riſe, it is not one of the leaſt important, that it overthrows the theory of the learned inventor Volta, who conſiders the current of electricity as being produced by the difference of conducting power in the different metals. And in this obſcure region of reſearch, though we are altogether unacquainted with the power by which the electricity is accumulated, it is one ſtep towards a true theory to have aſcertained, that one of thoſe already offered is without foundation.

Philoſophical Tranſactions of the Royal Society of London, for the Year 1800. *Part* III. *Quarto, p.* 295, *with twelve Plates.* London; ſold by Elmſley. Price 17s. 6d.

THIS part contains:—19. Experiments on the ſolar, and on the terreſtrial rays that occaſion heat; with a comparative view of the laws to which light and heat, or rather the rays which occaſion them, are ſubject, in order to determine whether they are the ſame or different. Part II. By William Herſchel, L. L. D. F. R. S. 20. An Account of the Trigonometrical Survey, carried on in the years 1797, 1798, and 1799, by order of the Marquis Cornwallis, Maſter General of the Ordnance. By Captain William Mudge, of the Royal Artillery, F. R. S. Communicated by his Grace the Duke of Richmond, F. R. S.

Analytical Eſſays towards promoting the Chemical Knowledge of Mineral Subſtances. By Martin Henry Klaproth, Profeſſor of Chemiſtry, Aſſeſſor to the Royal College of Phyſicians, Member of the Royal Academy of Sciences at Berlin, and various other learned Societies. Tranſlated from the German. 1 vol. 8vo. p. 592. London. Sold by Cadell and Davies.

IT is altogether unneceſſary to appriſe the ſcientific world of the value of the analyſes of this great improver of chemical ſcience. I ſhall therefore only inform the reader in this place, that we are indebted for the preſent accurate and only tranſlation to the learned editor of Gren's Chemiſtry, Dr. Gruber, who promiſes in his preface to give an immediate Engliſh edition of the next collection which Klaproth ſhall publiſh, and of which he has given hopes to the editor.

An Epitome of Chemiſtry in three Parts. Part I. intended to facilitate to the Student the Acquiſition of Chemical Knowledge, by minute Inſtructions for the performance of Experiments. Part II. Directions for the Analyſis of Mineral Waters; of Earths and Stones; of Ores of Metals; and of Mineral Bodies in general; and Part III. Inſtructions for applying Chemical Facts and reagents to various uſeful Purpoſes. By William Henry. Duodecimo, 216 Pages. Printed for Johnſon.

THE title of this work is ſufficiently copious to exhibit its objects and plan. The practical, as well as ſcientific knowledge of the author, have enabled him to produce a manual which I doubt not will be received with avidity by the public. I will not incur the imputation of vanity by aſſuming the office of an approver, where the author's powers are ſo well known: but I think I ſhall not deviate from the line of my duty in obſerving that, as far as my obſervation extends, he has, on the preſent occaſion, exerciſed thoſe powers with the diligence and attention which are peculiarly demanded in a manual of ſcience.

Electric Shock by means of Galvaniſm.

IN anſwer to a letter on the ſubject of the note at page 44, Mr. Cruickſhank has favoured me with an account of the experiment in which the common electric jar was charged by Galvaniſm, ſo as to give a perceptible ſhock: a fact on the poſſibility of which all doubt has, with reſpect to myſelf, been removed by my firſt experiments with talc, with ſilk, and with common air; a plate of which, over the condenſer of Volta, may be ſo charged with Galvaniſm, from a weak power, as to exhibit every electric intenſity that Bennet's electrometer is capable of, merely by varying the thickneſs of the ſtratum of air.

Mr. Cruickſhank made his experiment as follows: A coated jar, whoſe inſide and outſide coatings were moiſtened, was placed upon one of the extremities of the pile, and a wire connected with the inſide coating, was then made to touch the other extremity. After this proceſs the jar was taken in the hand which was wet, and the wire brought to touch the tongue. In this caſe, when the machine was very powerful, and gave ſtrong ſparks, he was ſeveral times confident that he received a ſlight ſhock in the tongue, but this did not always ſucceed, nor could he account for its failure. In truth, the reaſoning on theſe facts by application of what we know of the laws of common electricity, is ſufficiently obvious; but till we are better acquainted with the laws of electric condenſation in the pile, we muſt expect anomalies tending to embarraſs our proceſſes of deduction. I have met with ſeveral; of which like Mr. Cruickſhank, in the preſent inſtance, I have deferred the publication till time ſhould admit of repeating them with ſuch variations as the facts themſelves have ſuggeſted.

From the ſame private communication I copy the following curious extract:—" I ſtill " uſe the new apparatus or troughs, which was deſcribed in your Journal, and have never " once had occaſion to remove the pieces. Indeed, when freſh filled with a ſolution of " common ſalt, to which a little muriatic acid is added, it is quite as powerful as when " firſt conſtructed. I prefer the muriatic acid to the nitrous, as its effects are much " more permanent, and it does not in any ſenſible degree act on the ſilver. The ſhocks " given by the troughs are exactly ſimilar to thoſe of common electricity, and have none " of that grating or tearing ſenſation ſo remarkable in thoſe from the pile. From two " troughs, containing in all 120 pair of pieces, a ſpark of conſiderable ſize may be taken, " and the report or ſnap may be heard in an adjoining room. The ſpark may likewiſe " be taken from water, or from a conductor at a diſtance, which I never could do with any " pile. If a point be preſented to the water, a curious hiſſing is perceived, and a portion " of the fluid is inſtantly converted into gas and vapour: I have ſometimes diſſipated a " whole drop in this way. I have likewiſe ſeen a very ſtrong ſpark run along a plate of " zinc, to get to water in a manner which I cannot yet explain: this is frequently accompanied with a hiſſing noiſe. In ſhort, I have obſerved a number of curious facts " with this apparatus, which had eſcaped notice, when I employed the piles only."

Galvanic Apparatus.

Fig. 1.

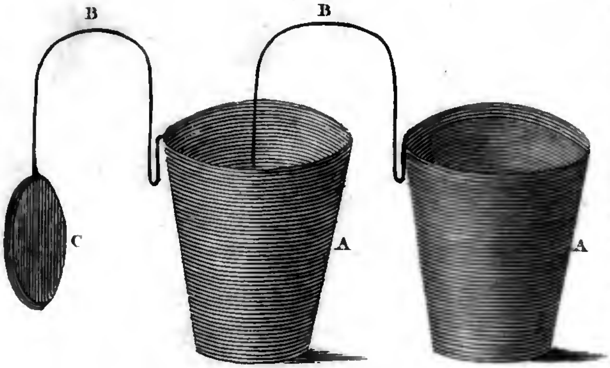


Fig. 2.

Fig. 4.

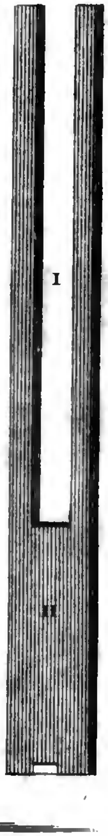
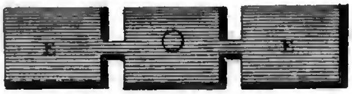
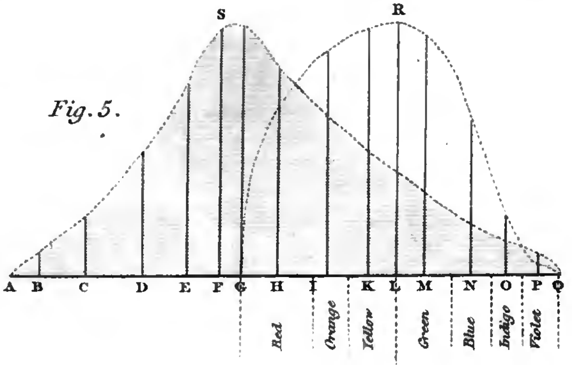


Fig. 3.



Dispersion & Intensity of solar radiant heat & Light.

Fig. 5.



Mutlow Sc. Russell Court.

Dr. Young's Inquiries respecting Sound.

Fig. 1

Fig. 2

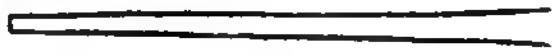


Fig. 3

Fig. 4

Fig. 5

Fig. 6

Fig. 7

Fig. 8

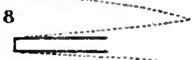


Fig. 9

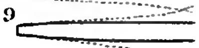


Fig. 10

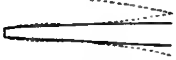


Fig. 11

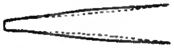


Fig. 12

Fig. 13

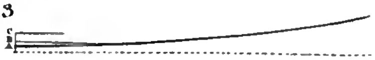


Fig. 14

Fig. 15

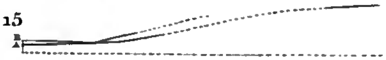


Fig. 16

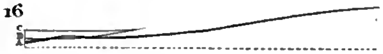


Fig. 17

Fig. 18

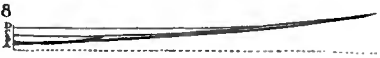


Fig. 19

Fig. 20

Fig. 21

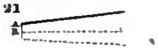


Fig. 22

Fig. 23

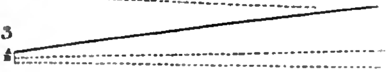


Fig. 24.

Fig. 25.

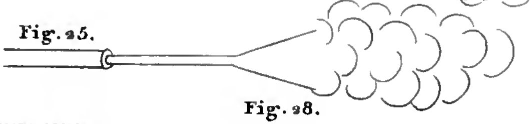
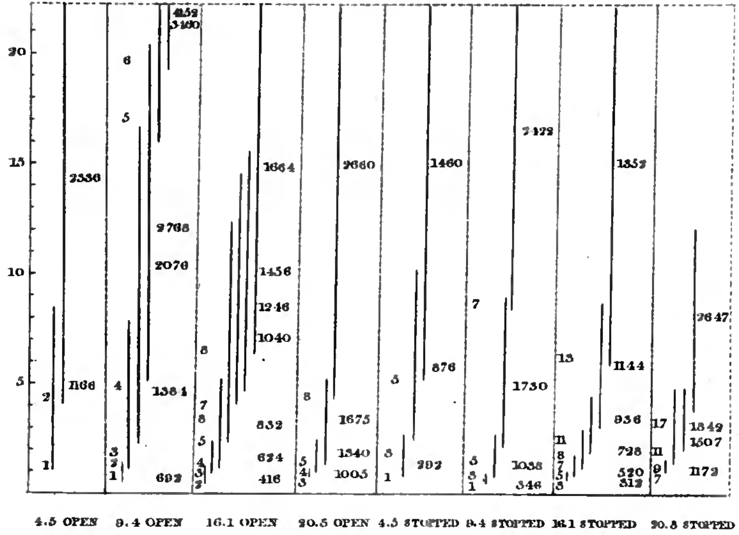


Fig. 26.

Fig. 27.

Fig. 28.



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A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JUNE, 1801.

ARTICLE I.

Outlines of Experiments and Inquiries reſpecting Sound and Light. By THOMAS YOUNG, *M.D. F.R.S.*

(*Continued from page* 78.)

IV. *Of the Velocity of Sound.*

IT has been demonſtrated, by M. de la Grange and others, that any impreſſion whatever communicated to one particle of an elaſtic fluid, will be tranſmitted through that fluid with an uniform velocity, depending on the conſtitution of the fluid, without reference to any ſuppoſed laws of the continuation of that impreſſion. Their theorem for aſcertaining this velocity is the ſame as Newton has deduced from the hypotheſis of a particular law of continuation: but it muſt be confeſſed, that the reſult differs ſomewhat too widely from experiment, to give us full confidence in the perfection of the theory. Corrected by the experiments of various obſervers, the velocity of any impreſſion tranſmitted by the common air, may, at an average, be reckoned 1130 feet in a ſecond.

V. *Of ſonorous Cavities.*

M. de la Grange has alſo demonſtrated, that all impreſſions are reflected by an obſtacle terminating an elaſtic fluid, with the ſame velocity with which they arrived at that obſtacle. When the walls of a paſſage, or of an unfurniſhed room, are ſmooth and perfectly parallel, any exploſion, or a ſtamping with the foot, communicates an impreſſion to the air, which is reflected from one wall to the other, and from the ſecond again towards the ear, nearly in the ſame direction with the primitive impulſe: this takes place as frequently in a ſecond, as double the breadth of the paſſage is contained in 1130 feet; and the ear receives a perception of a muſical ſound, thus determined in its pitch by the breadth of the paſſage. On making the experiment, the reſult will be found accurately to agree with this explanation. If the ſound is predetermined, and the frequency of vibrations ſuch as that each pulſe, when doubly reflected, may coincide with the ſubſequent pulſe proceeding directly from the ſounding body, the intenſity of the ſound will be much increaſed by the reflection; and alſo, in a leſs degree, if the reflected pulſe coincides with the next but one, the next but two, or more, of the direct pulſes. The appropriate notes of a room may readily be diſcovered by ſinging the ſcale in it; and they will be found to depend on the proportion of its length or breadth to 1130 feet. The ſound of the ſtopped diapaſon pipes of an organ is produced in a manner ſomewhat ſimilar to the note from an exploſion in a paſſage; and that of its reed pipes to the reſonance of the voice in a room: the length of the pipe in one caſe determining the ſound, in the other, increaſing its ſtrength. The frequency of the vibrations does not at all immediately depend on the diameter of the pipe. It muſt be confeſſed, that much remains to be done in explaining the preciſe manner in which the vibration of the air in an organ pipe is generated. M. Daniel Bernouilli has ſolved ſeveral difficult problems relating to the ſubject; yet ſome of his aſſumptions are not only gratuitous, but contrary to matter of fact.

VI. *Of the Divergence of Sound.*

It has been generally aſſerted, chiefly on the authority of Newton, that if any ſound be admitted through an aperture into a chamber, it will diverge from that aperture equally in all directions. The chief arguments in favour of this opinion are deduced from conſidering the phænomena of the preſſure of fluids, and the motion of waves excited in a pool of water. But the inference ſeems to be too haſtily drawn: there is a very material difference between impulſe and preſſure; and, in the caſe of waves of water, the moving force at each point is the power of gravity, which, acting primarily in a perpendicular direction, is only ſecondarily converted into a horizontal force, in the direction of the progreſs of the waves, being at each ſtep diſpoſed to ſpread equally in every direction: but the impulſe tranſmitted by an elaſtic fluid, acts primarily in the direction of its progreſs. It is well known, that if a perſon calls to another with a ſpeaking trumpet, he points it towards the place

place where his hearer ſtands: and I am aſſured by a very reſpectable Member of the Royal Society, that the report of a cannon appears many times louder to a perſon towards whom it is fired, than to one placed in a contrary direction. It muſt have occurred to every one's obſervation, that a ſound ſuch as that of a mill, or a fall of water, has appeared much louder after turning a corner, when the houſe or other obſtacle no longer intervened; and it has been already remarked by Euler, on this head, that we are not acquainted with any ſubſtance perfectly impervious to ſound. Indeed, as M. Lambert has very truly aſſerted, the whole theory of the ſpeaking trumpet, ſupported as it is by practical experience, would fall to the ground, if it were demonſtrable that ſound ſpreads equally in every direction. In windy weather it may often be obſerved, that the ſound of a diſtant bell varies almoſt inſtantaneouſly in its ſtrength, ſo as to appear at leaſt twice as remote at one time as at another; an obſervation which has alſo occurred to another gentleman, who is uncommonly accurate in examining the phænomena of nature. Now, if ſound diverged equally in all directions, the variation produced by the wind could never exceed one-tenth of the apparent diſtance: but, on the ſuppoſition of a motion nearly rectillinear, it may eaſily happen that a ſlight change in the direction of the wind, may convey the ſound, either directly or after reflection, in very different degrees of ſtrength, to the ſame ſpot. From the experiments on the motion of a current of air, already related, it would be expected that a ſound, admitted at a conſiderable diſtance from its origin through an aperture, would proceed, with an almoſt imperceptible increaſe of divergence, in the ſame direction; for, the actual velocity of the particles of air, in the ſtrongeſt ſound, is incomparably leſs than that of the ſloweſt of the currents in the experiments related, where the beginning of the conical divergence took place at the greateſt diſtance. Dr. Matthew Young has objected, not without reaſon, to M. Hube, that the exiſtence of a condenſation will cauſe a divergence in ſound: but a much greater degree of condenſation muſt have exiſted in the currents deſcribed than in any ſound. There is indeed one difference between a ſtream of air and a ſound; that, in ſound, the motions of different particles of air are not ſynchronous: but it is not demonſtrable that this circumſtance would affect the divergency of the motion, except at the inſtant of its commencement, and perhaps not even then in a material degree; for, in general, the motion is communicated with a very gradual increaſe of intenſity. The ſubject, however, deſerves a more particular inveſtigation; and, in order to obtain a more ſolid foundation for the argument, it is propoſed, as ſoon as circumſtances permit, to inſtitute a courſe of experiments for aſcertaining, as accurately as poſſible, the different ſtrength of a ſound once projected in a given direction, at different diſtances from the axis of its motion.

VII. *Of the Decay of Sound.*

Various opinions have been entertained reſpecting the decay of ſound. M. de la Grange has publiſhed a calculation, by which its force is ſhown to decay nearly in the ſimple ratio of the diſtances; and M. Daniel Bernouilli's equations for the ſounds of conical pipes lead to a ſimilar a ſimilar concluſion. The ſame inference would follow from a completion of the reaſoning of Dr. Helſham, Dr. Matthew Young, and Profeſſor Venturi. It has been very elegantly demonſtrated by Maclaurin, and may alſo be proved in a much more ſimple manner, that when motion is communicated through a ſeries of elaſtic bodies increaſing in magnitude, if the number of bodies be ſuppoſed infinitely great, and their difference infinitely ſmall, the motion of the laſt will be to that of the firſt in the ſubduplicate ratio of their reſpective magnitudes; and, ſince in the caſe of concentric ſpherical laminæ of air, the bulk increaſes in the duplicate ratio of the diſtance, the motion will in this caſe be directly, and the velocity inverſely, as the diſtance. But, however true this may be of the firſt impulſe, it will appear, by purſuing the calculation a little further, that every one of the elaſtic bodies, except the laſt, receives an impulſe in a retrogade direction, which ultimately impedes the effect of the ſucceeding impulſe, as much as a ſimilar cauſe promoted that of the preceding one: and thus, as ſound muſt be conceived to conſiſt of an infinite number of impulſes, the motion of the laſt lamina will be preciſely equal to that of the firſt; and, as far as this mode of reaſoning goes, ſound muſt decay in the duplicate ratio of the diſtance. Hence it appears, that the propoſal for adopting the logarithmic curve for the form of the ſpeaking trumpet, was founded on fallacious reaſoning. The calculation of M. de la Grange is left for future examination; and it is intended, in the mean time, to attempt to aſcertain the decay of ſound as nearly as poſſible by experiment: ſhould the reſult favour the concluſions from that calculation, it would eſtabliſh a marked difference between the propagation of ſound and of light.

VIII. *Of the harmonic Sounds of Pipes.*

In order to aſcertain the velocity with which organ pipes of different lengths require to be ſupplied with air, according to the various appropriate ſounds which they produce, a ſet of experiments was made, with the ſame mouth-piece, on pipes of the ſame bore, and of different lengths, both ſtopped and open. The general reſult was, that a ſimilar blaſt produced as nearly the ſame ſound as the length of the pipes would permit; or at leaſt that the exceptions, though very numerous, lay equally on each ſide of this concluſion. The particular reſults are expreſſed in Table XI. and in Plate V. Fig. 28. They explain how a note may be made much louder on a wind inſtrument by a ſwell, than it can poſſibly be by a ſudden impreſſion of the blaſt. It is propoſed, at a future time, to aſcertain by experiment, the actual compreſſion of the air within the pipe under different circumſtances: from ſome very ſlight trials, it ſeemed to be nearly in the ratio of the frequency of vibrations of each harmonic.

Table XI.

OPEN.					
A	B	C	D	E	F
4.5		0.7	8.8	$\overline{\overline{\overline{d}}}$*	1
	4.1	8.8			2
9.4		0.3	0.9	$\overline{\overline{f}}$	1
	0.8		8.0		2
	2.0		18.0		3
	5.0	8.0	20.0		4
	16.5	18.0			5
	19.0	20.0			6
16.1		0.4	1.0	$\overline{\overline{g}}$*	2
	0.8	1.0	2.2		3
	1.2	2.2	4.7		4
	2.2	4.7	11.5		5
	3.4		13.5		6
	4.0		15.0		7
	6.5	10.0			8
20.5		0.6	0.8	$\overline{\overline{b}}$	3
		0.8	1.9		4
	1.1	1.9	5.7		5
	4.5	5.7			8

STOPPED.					
A	B.	C	D	E	F
4.5		0.3	1.8	$\overline{\overline{d}}$	1
	1.2	1.7	10.0		3
	5.0	9.0			5
9.4		0.2	0.4	$\overline{f}$	1
		0.45	1.6		3
	1.1	1.6	8.5		5
	7.0	8.0			7
16.1		0.4	0.6	$\overline{\overline{d}}$*	3
	0.6	0.65	1.1		5
	0.9	1.1	2.4		7
	1.6	2.4	4.9		9
	2.5	4.8	9.0		11
	6.0	7.0			13
20.5		0.8	1.1	$\overline{\overline{\overline{c}}}$*	7
	1.0	1.1	3.8		9
	1.8		3.8		11
	3.2	3.8	12.		17
		12.		0	00

A, is the length of the pipe from the lateral orifice to the end. C, the preſſure at which the ſound began. B, its termination, by leſſening the preſſure; D, by increaſing it. E, the note anſwering to the firſt ſound of each pipe, according to the German method of notation. F, the number ſhowing the place of each note in the regular ſeries of harmonics. The diameter of the pipe was .35; the air duct of the mouth-piece meaſured, where ſmalleſt, .25 by .035; the lateral orifice .25 by .125. The apparatus was not calculated to apply a preſſure of above 22 inches. Where no number ſtands under C, a ſudden blaſt was required to produce the note.

IX. *Of the Vibrations of different elaſtic Fluids.*

All the methods of finding the velocity of ſound, agree in determining it to be, in fluids of a given elaſticity, reciprocally in the ſubduplicate ratio of the denſity: hence, in pure hydrogen gas it ſhould be $\sqrt{13}=3.6$ times as great as in common air; and the pitch of a pipe ſhould be a minor fourteenth higher in this fluid than in the common air. It is therefore probable that the hydrogen gas uſed in Profeſſor Chladni's late experiments, was not quite pure. It muſt be obſerved, that in an accurate experiment of this nature, the preſſure cauſing the blaſt ought to be carefully aſcertained. There can be no doubt but that, in the obſervations of the French Academicians on the velocity of ſound, which appear to have been conducted with all poſſible attention, the dampneſs and coldneſs of the night air muſt have conſiderably increaſed its denſity: hence, the velocity was found to be only 1109 feet in a ſecond; while Derham's experiments, which have an equal appearance of accuracy, make it amount to 1142. Perhaps the average may, as has been already mentioned, be ſafely eſtimated at 1130. It may here be remarked, that the well known elevation of the pitch of wind inſtruments, in the courſe of playing, ſometimes amounting to half a note, is not, as is commonly ſuppoſed, owing to any expanſion of the inſtrument, for this ſhould produce a contrary effect, but to the increaſed warmth of the air in the tube. Dr. Smith has made a ſimilar obſervation, on the pitch of an organ in ſummer and winter, which he found to differ more than twice as much as the Engliſh and French experiments on the velocity of ſound. Bianconi found the velocity of ſound, at Bologna, to differ at different times, in the ratio of 152 to 157.

X. *Of the Analogy between Light and Sound.*

Ever ſince the publication of Sir Iſaac Newton's incomparable writings, his doctrines of the emanation of particles of light from lucid ſubſtances, and of the formal pre-exiſtence of coloured rays in white light, have been almoſt univerſally admitted in this country, and but little oppoſed in others. Leonard Euler indeed, in ſeveral of his works, has advanced ſome powerful objections againſt them, but not ſufficiently powerful to juſtify the dogmatical reprobation with which he treats them; and he has left that ſyſtem of an ethereal vibration, which after Huygens and ſome others he adopted, equally liable to be attacked on many weak ſides. Without pretending to decide poſitively on the controverſy, it is conceived that ſome conſiderations may be brought forwards, which may tend to diminiſh the weight of objections to a theory ſimilar to the Huygenian. There are alſo one or two difficulties in the Newtonian ſyſtem, which have been little obſerved. The firſt is, the uniform velocity with which light is ſuppoſed to be projected from all luminous bodies, in conſequence of heat, or otherwiſe. How happens it that, whether the projecting force is the ſlighteſt tranſmiſſion of electricity, the friction of two pebbles, the loweſt degree of viſible ignition, the white heat of a wind furnace, or the intenſe heat of the ſun itſelf, theſe wonderful corpuſcles are always propelled with one uniform velocity? For, if they differed in velocity, that difference ought to produce a different refraction. But a ſtill more inſuperable difficulty ſeems to occur, in the partial reflection from every refracting ſurface. Why, of the ſame kind of rays, in every circumſtance preciſely ſimilar, ſome ſhould be always reflected, and others tranſmitted, appears in this ſyſtem to be wholly inexplicable. That a medium reſembling, in many properties, that which has been denominated ether, does really exiſt, is undeniably proved by the phænomena of electricity;

electricity; and the arguments againſt the exiſtence of ſuch an ether throughout the univerſe, have been pretty ſufficiently anſwered by Euler. The rapid tranſmiſſion of the electrical ſhock, ſhows that the electric medium is poſſeſſed of an elaſticity as great as is neceſſary to be ſuppoſed for the propagation of light. Whether the electric ether is to be conſidered as the ſame with the luminous ether, if ſuch a fluid exiſts, may perhaps at ſome future time be diſcovered by experiment; hitherto I have not been able to obſerve that the refractive power of a fluid undergoes any change by electricity. The uniformity of the motion of light in the ſame medium, which is a difficulty in the Newtonian theory, favours the admiſſion of the Huygenian; as all impreſſions are known to be tranſmitted through an elaſtic fluid with the ſame velocity. It has been already ſhown, that ſound, in all probability, has very little tendency to diverge: in a medium ſo highly elaſtic as the luminous ether muſt be ſuppoſed to be, the tendency to diverge may be conſidered as infinitely ſmall, and the grand objection to the ſyſtem of vibration will be removed. It is not abſolutely certain, that the white line viſible in all directions on the edge of a knife, in the experiments of Newton and of Mr. Jordan, was not partly occaſioned by the tendency of light to diverge. Euler's hypotheſis, of the tranſmiſſion of light by an agitation of the particles of the refracting media themſelves, is liable to ſtrong objections; according to this ſuppoſition, the refraction of the rays of light, on entering the atmoſphere from the pure ether which he deſcribes, ought to be a million times greater than it is. For explaining the phænomena of partial and total reflection, refraction, and inflection, nothing more is neceſſary than to ſuppoſe all refracting media to retain, by their attraction, a greater or leſs quantity of the luminous ether, ſo as to make its denſity greater than that which it poſſeſſes in a vacuum, without increaſing its elaſticity; and that light is a propagation of an impulſe communicated to this ether by luminous bodies: whether this impulſe is produced by a partial emanation of the ether, or by vibrations of the particles of the body, and whether theſe vibrations are, as Euler ſuppoſed, of various and irregular magnitudes, or whether they are uniform, and comparatively large, remains to be hereafter determined. Now, as the direction of an impulſe tranſmitted through a fluid, depends on that of the particles in ſynchronous motion, to which it is always perpendicular, whatever alters the direction of the pulſe, will inflect the ray of light. If a ſmaller elaſtic body ſtrike againſt a larger one, it is well known that the ſmaller is reflected more or leſs powerfully, according to the difference of their magnitudes: thus, there is always a reflection when the rays of light paſs from a rarer to a denſer ſtratum of ether; and frequently an echo when a ſound ſtrikes againſt a cloud. A greater body ſtriking a ſmaller one, propels it, without loſing all its motion: thus, the particles of a denſer ſtratum of ether, do not impart the whole of their motion to a rarer, but, in their effort to proceed, they are recalled by the attraction of the refracting ſubſtance with equal force; and thus a reflection is always ſecondarily produced, when the rays of light paſs from a denſer to a rarer ſtratum. Let AB, Plate V. Fig. 29, be a ray of light falling on the reflecting ſurface FG; *c d* the direction of the vibration, pulſe, impreſſion, or condenſation.

ſation. When *d* comes to H, the impreſſion will be, either wholly or partly, reflected with the ſame velocity as it arrived, and EH will be equal to DH; the angle EIH to DIH or CIF; and the angle of reflection to that of incidence. Let FG, Fig. 30, be a refracting ſurface. The portion of the pulſe IE, which is travelling through the refracting medium, will move with a greater or leſs velocity in the ſubduplicate ratio of the denſities, and HE will be to KI in that ratio. But HE is, to the radius IH, the ſine of the angle of refraction; and KI that of the angle of incidence. This explanation of refraction is nearly the ſame as that of Euler. The total reflection of a ray of light by a refracting ſurface, is explicable in the ſame manner as its ſimple refraction; HE, Fig. 31, being ſo much longer than KI, that the ray firſt becomes parallel to FG, and then, having to return through an equal diverſity of media, is reflected in an equal angle. When a ray of light paſſes near an inflecting body, ſurrounded, as all bodies are ſuppoſed to be, with an atmoſphere of ether denſer than the ether of the ambient air, the part of the ray neareſt the body is retarded, and of courſe the whole ray inflected towards the body, Fig. 32. The repulſion of inflected rays has been very ably controverted by Mr. Jordan, the ingenious author of a late publication on the Inflection of Light. It has already been conjectured by Euler, that the colours of light conſiſt in the different frequency of the vibrations of the luminous ether: it does not appear that he has ſupported this opinion by any argument; but it is ſtrongly confirmed, by the analogy between the colours of a thin plate and the ſounds of a ſeries of organ pipes. The phænomena of the colours of thin plates require, in the Newtonian ſyſtem, a very complicated ſuppoſition, of an ether, anticipating by its motion the velocity of the corpuſcles of light, and thus producing the fits of tranſmiſſion and reflection; and even this ſuppoſition does not much aſſiſt the explanation. It appears, from the accurate analyſis of the phænomena which Newton has given, and which has by no means been ſuperſeded by any later obſervations, that the ſame colour recurs whenever the thickneſs anſwers to the terms of an arithmetical progreſſion. Now this is preciſely ſimilar to the production of the ſame ſound, by means of an uniform blaſt, from organ-pipes which are different multiples of the ſame length. Suppoſing white light to be a continued impulſe or ſtream of luminous ether, it may be conceived to act on the plates as a blaſt of air does on the organ-pipes, and to produce vibrations regulated in frequency by the length of the lines which are terminated by the two refracting ſurfaces. It may be objected that, to complete the analogy, there ſhould be tubes, to anſwer to the organ-pipes: but the tube of an organ-pipe is only neceſſary to prevent the divergence of the impreſſion, and in light there is little or no tendency to diverge; and indeed, in the caſe of a reſonant paſſage, the air is not prevented from becoming ſonorous by the liberty of lateral motion. It would ſeem, that the determination of a portion of the track of a ray of light through any homogeneous ſtratum of ether, is ſufficient to eſtabliſh a length as a baſis for colorific vibrations. In inflections, the length of the track of a ray of light through the inflecting atmoſphere may determine its vibrations: but, in this caſe, as it is probable that there is a reflection from every part of the ſurface of the ſurrounding atmoſphere, contributing

tributing to the appearance of the white line in every direction, in the experiments already mentioned, ſo it is poſſible that there may be ſome ſecond reflection at the immediate ſurface of the body itſelf, and that, by mutual reflections between theſe two ſurfaces, ſomething like the anguiform motion ſuſpected by Newton may really take place; and then the analogy to the colours of thin plates will be ſtill ſtronger. A mixture of vibrations, of all poſſible frequencies, may eaſily deſtroy the peculiar nature of each, and concur in a general effect of white light. The greateſt difficulty in this ſyſtem is, to explain the different degree of refraction of differently coloured light, and the ſeparation of white light in refraction: yet, conſidering how imperfect the theory of elaſtic fluids ſtill remains, it cannot be expected that every circumſtance ſhould at once be clearly elucidated. It may hereafter be conſidered how far the excellent experiments of Count Rumford, which tend very greatly to weaken the evidence of the modern doctrine of heat, may be more or leſs favourable to one or the other ſyſtem of light and colours. It does not appear that any comparative experiments have been made on the inflection of light by ſubſtances poſſeſſed of different refractive powers; undoubtedly ſome very intereſting concluſions might be expected from the inquiry.

XI. *Of the Coaleſcence of Muſical Sounds.*

It is ſurpriſing that ſo great a mathematician as Dr. Smith could have entertained for a moment, an idea that the vibrations conſtituting different ſounds ſhould be able to croſs each other in all directions, without affecting the ſame individual particles of air by their joint forces: undoubtedly they croſs, without diſturbing each other's progreſs; but this can be no otherwiſe effected than by each particle partaking of both motions. If this aſſertion ſtood in need of any proof, it might be amply furniſhed by the phænomena of beats, and of the grave harmonics obſerved by Romieu and Tartini; which M. de la Grange has already conſidered in the ſame point of view. In the firſt place, to ſimplify the ſtatement, let us ſuppoſe, what probably never preciſely happens, that the particles of air, in tranſmitting the pulſes, proceed and return with uniform motions; and, in order to repreſent their poſition to the eye, let the uniform progreſs of time be repreſented by the increaſe of the abſciſs, and the diſtance of the particle from its original poſition, by the ordinate, Fig. 33—38. Then, by ſuppoſing any two or more vibrations in the ſame direction to be combined, the joint motion will be repreſented by the ſum or difference of the ordinates. When two ſounds are of equal ſtrength, and nearly of the ſame pitch, as in Fig. 36, the joint vibration is alternately very weak and very ſtrong, producing the effect denominated a beat, Plate VI. Fig. 43, B and C: which is ſlower and more marked, as the ſounds approach nearer to each other in frequency of vibrations; and, of theſe beats there may happen to be ſeveral orders, according to the periodical approximations of the numbers expreſſing the proportions of the vibrations. The ſtrength of the joint ſound is double that of the ſimple ſound only at the middle of the beat, but not throughout its duration; and it may be inferred, that the ſtrength of ſound in a concert will not be in exact proportion to the number of inſtruments compoſing it. Could any method be deviſed for aſcertaining this by experiment, it would aſſiſt in the compariſon of ſound with light. In Plate V. Fig. 33, let P and Q be the middle points of the progreſs or regreſs of a particle in two ſucceſſive compound vibrations; then, CP being $= PD$, $KR = RN$, $GQ = QH$, and $MS = SO$, twice their diſtance, $2\,RS = 2\,RN + 2\,NM + 2\,MS = KN + NM + NM + MO = KM + NO$, is equal to the ſum of the diſtances of the correſponding parts of the ſimple vibrations. For inſtance, if the two ſounds be as 80 : 81, the joint vibration will be as 80.5; the arithmetical mean between the periods of the ſingle vibrations. The greater the difference in the pitch of two ſounds, the more rapid the beats, till at laſt, like the diſtinct puffs of air in the experiments already related, they communicate the idea of a continued ſound; and this is the fundamental harmonic deſcribed by Tartini. For inſtance, in Plate V. Fig. 34—37, the vibrations of ſounds related as 1 : 2, 4 : 5, 9 : 10, and 5 : 8, are repreſented; where the beats, if the ſounds be not taken too grave, conſtitute a diſtinct ſound, which correſponds with the time elapſing between two ſucceſſive coincidences, or near approaches to coincidence: for, that ſuch a tempered interval ſtill produces a harmonic, appears from Plate V. Fig. 38. But, beſides this primary harmonic, a ſecondary note is ſometimes heard, where the intermediate compound vibrations occur at a certain interval, though interruptedly; for inſtance, in the coaleſcence of two ſounds related to each other as 7 : 8, 5 : 7, or 4 : 5, there is a recurrence of a ſimilar ſtate of the joint motion, nearly at the interval of $\frac{5}{15}$, $\frac{4}{12}$, or $\frac{3}{9}$ of the whole period: hence, in the concord of a major third, the fourth below the key note is heard as diſtinctly as the double octave, as is ſeen in ſome degree in Plate V. Fig. 35; AB being nearly two-thirds of CD. The ſame ſound is ſometimes produced by taking the minor ſixth below the key note; probably becauſe this ſixth, like every other note, is almoſt always attended by an octave, as a harmonic. If the angles of all the figures reſulting from the motion thus aſſumed be rounded off, they will approach more nearly to a repreſentation of the actual circumſtances; but, as the laws by which the motion of the particles of air is regulated, differ according to the different origin and nature of the ſound, it is impoſſible to adapt a demonſtration to them all: if, however, the particles be ſuppoſed to follow the law of the harmonic curve, derived from uniform circular motion, the compound vibration will be the harmonic inſtead of the arithmetical mean; and the ſecondary ſound of the interrupted vibrations will be more accurately formed, and more ſtrongly marked Plate VI. Figs. 41, 42: the demonſtration is deducible from the properties of the circle. It is remarkable, that the law by which the motion of the particles is governed, is capable of ſome ſingular alterations by a combination of vibrations. By adding to a given ſound other ſimilar ſounds, related to it in frequency as the ſeries of odd numbers, and in ſtrength inverſely in the ſame ratios, the right lines indicating an uniform motion may be converted very nearly into figures of ſines, and the figures of ſines into right lines, as in Plate V. Figs. 39, 40.

XII. *Of the Frequency of Vibrations conſtituting a given Note.*

The number of vibrations performed by a given ſound in a ſecond, has been variouſly aſcertained; firſt, by Sauveur, by a very ingenious inference from the beats of two ſounds; and ſince, by the ſame obſerver and ſeveral others, by calculation from the weight and tenſion of a chord. It was thought worth while, as a confirmation, to make an experiment ſuggeſted, but coarſely conducted, by Merſennus, on a chord 200 inches in length, ſtretched ſo looſely as to have its ſingle vibrations viſible; and, by holding a quill nearly in contact with the chord, they were made audible, and were found, in one experiment, to recur 8.3 times in a ſecond. By lightly preſſing the chord at one-eighth of its length from the end, and at other ſhorter aliquot diſtances, the fundamental note was found to be one-ſixth of a tone higher than the reſpective octave of a tuning-fork marked C: hence, the fork was a comma and a half above the pitch aſſumed by Sauveur, of an imaginary C, conſiſting of one vibration in a ſecond.

(To be concluded in our next.)

II.

On the Identity of the Pyro-mucous, Pyro-tartareous, and Pyro-ligneous Acids, with the Acetous Acid. By Citizens FOURCROY *and* VAUQUELIN[*].

THE re-union of ſpecies which have been erroneouſly conſidered as different, is in many inſtances of greater advantage to the ſcience of facts, than the diſcovery of new ſpecies. It is towards this degree of perfection that the efforts of chemiſts and naturaliſts are at preſent directed.

Three acids have been diſtinguiſhed by different appellations, in which the common property of being produced by the action of fire, and of acquiring ſeveral common characters from that agent, is well known; but it was never imagined that they are not only identically the ſame, but likewiſe analogous in their nature to the acid of vinegar, denominated acetous acid. Citizens Fourcroy and Vauquelin, in their endeavours to diſcover certain characters by which the pyro-mucous, pyro-tartareous, and pyro-ligneous acids might be diſtinguiſhed, were unable to diſcover any. By comparing thoſe which have hitherto been pointed out as the diſtinctive characters, it may be ſhewn, as they have done, that they are founded merely on ſlight differences of ſmell and colour; and are conſequently deluſive.

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[*] Bulletin de la Soc. Philom.---N.

Some

Some facts obſerved in the analyſes of vegetables, led our chemiſts to ſuſpect that theſe acids, which were proved to be the ſame, might not differ from the acetous acid, except by foreign additions. They made experiments with a view to confirm their ſuſpicions. By the diſtillation of ſugar, ſtarch, gum, &c. they obtained the pyro-mucous acid; by that of tartar, the pyro-tartareous acid; and by that of wood, the pyro-ligneous acid. They combined theſe acids with an alkaline baſis; from which, having ſeparated them by the ſulphuric acid, they obtained by this means, in ſome inſtances, with filtration through charcoal powder, an acid and limpid liquor, deprived of the empyreumatic oil, which rendered it impure, and prevented it from being diſtinguiſhed as the true acetous acid. This liquor then poſſeſſed the ſmell, taſte, and all the other characters; and, with the alkaline baſes, formed perfect acetites.

It was neceſſary to attempt to form the empyreumatic acids again by the acetous acid, by imparting to the latter the empyreumatic oil and empyreumatic ſmell, of which it had been deprived by the preceding experiments. The following is the proceſs employed by Citizens Fourcroy and Vauquelin:—They diſtilled a portion of acetous acid over the empyreumatic oils of mucilage, tartar, and wood; and thus formed pyro-mucous, pyro-tartareous, and pyro-ligneous acids. To form theſe acids again inſtantaneouſly, it was only requiſite to add a few drops of thoſe empyreumatic oils to ſome acetous acid.

To theſe facts, which are ſufficiently intereſting in themſelves, the diſcoverers have ſubjoined reflexions no leſs important, relative to the frequent production of the acetous acid under very different circumſtances.

The acetous acid is not therefore a neceſſary product of the vinous fermentation. In addition to the facts above related, which confirm the production of this acid without any fermentation, it may be obſerved, that many ſubſtances, of a nature very different from wine, contain acetites; ſuch are the ſap of vegetables kept only a few hours, ſoils, heated tan, the ſour water of the ſtarch matter, ſour pulſe, and ſour fruit, expoſed for ſome hours to a hot air. Milk, animal jellies, and that part of urine called *urée*, when they become ſour, likewiſe afford a great quantity of this acid.

Citizens Fourcroy and Vauquelin have diſcovered four very diſtinct circumſtances under which the acetous acid is produced, each of which is characteriſed by ſome particulars relative to the phenomena of its production.

The firſt mode of *acetification* is the decompoſing action of fire: in this caſe water is diſengaged with carbonic acid formed from the diſengaged carbon; and the acetous acid thus obtained is diſtinguiſhed by its colour and empyreumatic odour.

The ſecond method conſiſts in the application of the powerful acids, ſuch as the ſulphuric, nitric, and the oxigenated muriatic acids upon vegetable compounds, ſuch as ſugar, jelly, &c.; alcohol and the vegetable acids themſelves, except the acetous acid, are entirely decompoſed by theſe acids. Some water and carbonic acid are alſo produced, and the acetous acid thus obtained contains oxalic acid, malic acid, and much water.

The

The third and moſt general method is the fermentation of wine. In this proceſs neither water nor carbonic acid is produced; but an abſorption of oxygen takes place: this acetous acid contains tartar, alcohol, and colouring matter.

The fourth mode, which is nearly ſimilar to the third, the difference, perhaps, being only becauſe it is no longer the vegetable fluid called *wine* which produces the acetous acid, is a particular fermentation which takes place in certain animal or vegetable produƈts above-mentioned, but eſpecially in urines. This acid is always combined with ammonia.

III.

On the Struƈture and Uſes of the Membrana Tympani of the Ear. By EVERARD HOME, *Eſq. F. R. S.* *

THE ſubjeƈt of inquiry appointed by the Croonian Inſtitution, has been greatly elucidated at different times by ingenious members of this learned Society. A large field, however, ſtill remained open; and, reſpeƈting future inveſtigations, I ſhall have occaſion to offer a freſh proof of the aid to be derived from comparative anatomy, in aſcertaining the ſtruƈture of parts which, from their minuteneſs and ſituation in the human body admit with much difficulty of being explored.

The principal objeƈt of the preſent leƈture is to communicate a diſcovery of the ſtruƈture of the membrana tympani; which, in ſome reſpeƈts, affords a new and very curious inſtance of the application of muſcular aƈtion, and may conduce to account for certain phænomena in the ſenſe of hearing, in a more ſatisfaƈtory manner than has hitherto been propoſed.

The membrana tympani has always been conſidered as a common membrane, which, by means of muſcles belonging to the malleus being ſtretched or relaxed, became fitted, in its various degrees of tenſion, to convey the vaſt variety of external ſounds to the internal organ. Its ſhape, ſituation, and office, have procured it the name of drum of the ear; and the muſcles of the malleus having been deemed ſufficient for bracing and unbracing it, leſs attention was beſtowed on the ſtruƈture of the membrane itſelf: to which may be added, that in the human ear, and generally in the ear of quadrupeds, the membrane is ſo extremely ſmall and thin, and in its ſituation ſo peculiarly confined, as not to be got at for inſpeƈtion but with much difficulty.

The caſe is different in the elephant, where this membrane is ſo very large, that the parts of which it is compoſed are readily diſtinguiſhed: they are even conſpicuous to the naked eye; and muſcular fibres are ſeen paſſing along the membrane, in a radiated manner, from the bony rim which ſurrounds it, towards the handle of the malleus, to which the central part of the membrane is firmly attached.

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* Philoſ. Tranſaƈtions, 1800.

This

This diſcovery in the elephant having led to that of a ſimilar conſtruction in the human membrana tympani, it may not be improper to relate the circumſtances by which I became engaged in the inveſtigation of the organ of hearing in that animal.

Three different opportunities have occurred of diſſecting the elephant in London, by the deaths of thoſe which had been preſented to his Majeſty, and were kept at the King's ſtables at Pimlico. One of them was given to the late Dr. Hunter; one to his brother Mr. J. Hunter; and the third to Sir Aſhton Lever.

From my being connected with Mr. John Hunter's purſuits in comparative anatomy, I was employed throughout the whole of theſe diſſections, and became extremely deſirous of examining the internal parts of the ear, the ſtructure of that organ in the human body having at a very early period particularly engaged my attention;* but neither Dr. Hunter nor his brother could be prevailed upon to ſacrifice ſo large a portion of the ſkull as was neceſſary for the purpoſe.

When Mr. Corſe arrived from Bengal, laſt year, and mentioned his having brought over a number of ſkulls of elephants, in order to ſhow the progreſs of the formation of their grinding teeth, † the deſire to examine the organ of hearing in that animal recurred to me ſo ſtrongly, that I requeſted to have one of the ſkulls for that purpoſe, and Mr. Corſe very readily and obligingly complied with my requeſt.

After having examined the organ in the dried ſkull, the want of the membrana tympani, and of the ſmall bones, made the information thus received of a very unſatisfactory nature, and increaſed the deſire of ſeeing theſe parts in the recent head. In conſidering how this could be done, I recollected a mutilated elephant's head, preſerved in ſpirits, which had been ſent to Mr. Hunter, but, from the multiplicity of his engagements, had remained neglected in the caſk at the time of his death, and in the following year was dried, to ſhow the proboſcis, that it might not be altogether ſpoiled.

Upon examining this dried head, the bones had been ſo much broken, that one of the organs of hearing was altogether wanting: the other, however, was fortunately entire; and the membrana tympani and ſmall bones, having been little diſturbed in the drying of the parts, remained nearly in their natural ſituation.

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* In the year 1776, I injected the cochlea and ſemicircular canals of the human ear with a compoſition of wax and roſin. This was done by placing the temporal bone in the receiver of an air pump, the upper part of which was in the form of a funnel, rendered air-tight by a cork being fitted into its neck, and ſurrounded with bees' wax. After the air had been exhauſted, the hot injection, poured into the funnel, melted the wax, and the cork was pulled out by means of a ſtring previouſly attached to it; the injection immediately ruſhed into the receiver, and was forced, by the preſſure of the atmoſphere, into the cavities of the temporal bone.

† On this ſubject, a very ingenious paper has been ſince publiſhed by him, in the Philoſophical Tranſactions for the year 1799.

The

The membrana tympani, and every other part of the organ, were found to be much larger in proportion than in other quadrupeds, or in man; differing in this reſpect from the eye of the elephant, which is unuſually ſmall, when compared with the enormous bulk of the animal.

The membrane was found of an oval form; the ſhort diameter of the oval rather more than an inch in length; the long diameter an inch and $\frac{7}{20}$ths.

In the human ear, the membrana tympani is nearly circular; the longeſt diameter is $\frac{8}{20}$ths of an inch; the ſhorteſt $\frac{7}{20}$ths.

As the membrane in the elephant exceeds that of the human ear in thickneſs as much as in extent, which is as the ſquares of their diameters, or in the proportion of 135 to 14, it is natural to conclude that the muſcular fibres which are to ſtretch the one, muſt greatly exceed in ſtrength thoſe capable of producing the ſame degree of tenſion in the other.

From this ſtatement, the muſcular ſtructure in the human membrana tympani will neceſſarily be ſo much leſs diſtinct than in the elephant, as ſcarcely to be viſible to the naked eye, and will eaſily be overlooked by the moſt attentive obſerver, who is not directed by ſome previous information to examine it under the moſt favourable circumſtances; but, when theſe are attended to, it can be perceived without the aid of glaſſes.

If the membrana tympani of the human ear is completely expoſed on both ſides, by removing the contiguous parts, and the circular covering is carefully waſhed off from its external ſurface, then, by placing it in a clear light, the radiated direction of its fibres may be eaſily detected. If a common magnifying glaſs is uſed, they are rendered nearly as diſtinct as thoſe of the elephant appear to the naked eye; their courſe is exactly the ſame; and they differ in nothing but in being formed upon a ſmaller ſcale.

When viewed in a microſcope magnifying 23 times, the muſcular fibres are beautifully conſpicuous, and appear uniformly the ſame throughout the whole ſurface, there being no central tendons, as in the diaphragm; the muſcular fibres appear only to form the internal layer of the membrane, and are moſt diſtinctly ſeen when viewed on that ſide.

In examining this membrane in different ſubject, the parts were frequently found in a more or leſs morbid ſtate. In one inſtance, the membrane was found loaded with blood-veſſels, was leſs tranſparent than uſual, and was united by cloſe adheſion to the point of the long proceſs of the incus. In another inſtance, there was a preternatural oſſification adhering to it, at a ſmall diſtance from the end of the handle of the malleus.

As muſcles in general are ſupplied with blood-veſſels in proportion to the frequency of their action, it is an object of importance to determine the vaſcularity of the membrana tympani. Upon this ſubject, my own want of information has been amply ſupplied by Dr. Baillie, who, in a communication upon this ſubject, ſhowed me a preparation of the membrane, in which the veſſels had been moſt ſucceſsfully injected with coloured wax.

In this preparation, the moſt beautiful of the kind I ever ſaw, the veſſels in their diſtribution reſembled thoſe of the iris, and were nearly half as numerous: they anaſtomoſed with one another in a ſimilar manner, and their general direction was from the circumference to the handle of the malleus; from near this handle, a ſmall trunk ſent off branches, in a radiated manner, which anaſtomoſed with thoſe which had an oppoſite courſe.

This correſpondence, in the number and diſtribution of blood veſſels, between the membrana tympani and the iris, is a ſtrong circumſtance in confirmation of that membrane being endowed with muſcular action.

In the horſe, the membrana tympani is ſmaller than in man; its long diameter is $\frac{8}{20}$ths of an inch; the ſhort one $\frac{6}{20}$ths; and it is almoſt quite flat, while in man it is concave. which makes the difference of extent conſiderably exceed the difference in the diameters, In the horſe, the fibrous ſtructure is not viſible to the naked eye; it is even indiſtinctly ſeen when viewed through a common magnifying glaſs; but in a microſcope it is very viſible, and in every other reſpect agrees in ſtructure with the membrane in the human ear, and in that of the elephant.

In birds, the membrana tympani is larger in proportion than in the quadruped, and more circular in its ſhape.

In the gooſe, it is $\frac{6}{20}$ths of an inch in its longeſt diameter, and $\frac{5}{20}$ths in its ſhorteſt diameter. In the turkey, $\frac{7}{20}$ths by $\frac{5}{20}$ths. It is thinner in its coats in birds than in the horſe, and to the naked eye has no appearance of fibres; but, when viewed in a microſcope, there is a viſible radiated ſtructure, not very unlike the wire marks upon common writing paper.

In a former Lecture upon the Structure of Muſcles,* in which a comprehenſive view was taken of the ſubject, it was ſtated, that the organization neceſſary for muſcular contraction could exiſt in an apparent membrane, and that a faſciculated ſtructure was only neceſſary when muſcular action was to be enabled to overcome reſiſtance. The coats of the Tænia hydatigena were mentioned as an inſtance of the firſt; and the human heart as the moſt complex of the ſecond. In comparing the membranæ tympani of different animals, they afford a beautiful illuſtration of the truth of this poſition.

In birds, where from the ſmallneſs of its ſize the reſiſtance is very trifling, the membrane is very ſimilar to the coat of an hydatid, only ſtill thinner. In the elephant, fibres forming faſciculi are very diſtinct. The membrane of the horſe, and that of the human ear, form the intermediate gradations.

The knowledge of a muſcular ſtructure in the membrana tympani, enables us to explain many phænomena in hearing, which have not hitherto been accounted for in a ſatisfactory manner. It is principally by means of this muſcle that accurate perceptions of ſound are communicated to the internal organ, and that the membrana tympani is enabled to vary the ſtate of its tenſion, ſo as to receive them in the quick ſucceſſion in which they are conveyed to it.

* Philoſophical Tranſactions for the year 1795.

In the human ear, and in that of birds, the radiated fibres of the membrana tympani have their principal attachment to the extremity of the handle of the malleus, which is nearly in the centre of the membrane.

In the membrane of the elephant, which is oval, the attachment to the handle of the malleus is at ſome diſtance from the centre. In the horſe, deer, and cat, which have the membrane ſtill more oval than the elephant, the handle of the malleus is ſituated in the long axis of the membrane, with its extremity extending beyond the centre, reaching nearer to the circumference; and the fibres of the radiated muſcle are not only attached to its end, but alſo laterally to nearly the whole length of its handle.

This oval form of the membrana tympani, in thoſe quadrupeds, and the very extenſive attachment of the fibres of the radiated muſcle to the handle of the malleus, may be the reaſon why their ears are not equally fitted to hear inarticulate ſounds, as the ears of birds and of man.

Should this radiated muſcle of the membrana tympani (which is probably the ſmalleſt in the body that has a diſtinct action) be thought too inſignificant to have an office of ſo much conſequence aſſigned to it, let it be remembered, that the ſize of muſcles is no indication of their importance, but only of the reſiſtance to be overcome by their action; and that the more delicate actions are performed univerſally in the body by very ſmall muſcles, of which the iris in the eye furniſhes a very conſpicuous example.

(*To be continued.*)

IV.

Inquiries relative to the Laws of Affinity, by CIT. BERTHOLLET, *Member of the National Inſtitute of Arts and Sciences*[*].

(*Concluded from page 69.*)

ARTICLE XIII.

On the Precipitation of Metallic Solutions by other Metals.

1. WHEN the metals are precipitated by a ſubſtance which does not ſeize their oxigen, the precipitates retain part of the acid, and often part of the precipitant.

We have a ſtriking inſtance of this diviſion, which then takes place in the precipitate of the oxigenated muriate of mercury, by the fixed alkalis, ammonia and lime. On expoſing the precipitate to a ſufficient heat, a more or leſs conſiderable portion of mercury, according to the nature of the precipitate, is reduced, another is ſublimed, and forms a muriate, not becauſe the muriatic acid is found combined only with a portion of the oxide of mercury,

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[*] Tranſlation from the Annales de Chimie, XXXVII. 151.

cury, as I ſuppoſed, (*Memoirs of the Academy*, 1780) but becauſe the expanſive force of the heat, and the tendency to the combination of the muriatic acid, acting upon the oxide of mercury, cauſe, as we may expreſs ourſelves, a new ſharing to be made of it. If the precipitate by ammonia be examined, it is obſerved, that it retains a portion of ammonia: the precipitate of the muriate of iron by pot-aſh, retains part of the pot-aſh. Facts of this kind may be collected in great number.

There is therefore no doubt, but that the obſervations which have been made upon the precipitations of ſubſtances which loſe their ſolubility, cannot be applied to the metallic precipitates, which vary according to all the circumſtances which are capable of modifying the powers brought in action, at the moment of their ſucceſſive precipitation, and which will deſerve a particular examination in another memoir.

But when the metals are mutually precipitated from their ſolutions, their reciprocal affinity for oxigen has a great effect in the action that takes place; and ſometimes the precipitate is found in the metallic ſtate. If another force were not joined to the affinity of the precipitating metal for oxigen, it would naturally follow from the principles eſtabliſhed in this Memoir, that the oxigen would always be divided between the two metals which are in competition, according to the action they exert upon it. It is therefore neceſſary to examine, what may be the force which determines the precipitation in the metallic ſtate?

2. The affinity of mercury, gold, and ſilver for oxigen is very weak: the mutual affinity which ſtill remains between the parts of theſe metals when they are in fuſion, as mercury is at the temperature of the atmoſphere, is ſufficient to prevent their combination with oxigen in the ſtate of gas; but heat, by dilating the particles of the mercury, ſufficiently diminiſhes the force of their mutual affinity to enable them to combine with oxigen: a ſuperior degree of heat is ſufficient, by the difference of dilatation which it produces in the mercury and in the oxigen, to ſeparate them: ſo that the action of the heat begins by being more effective to this combination, in conſequence of the diminution of the mutual affinity of the parts, than inimical to it by the different dilatation of the mercury and the oxigen: but at length by augmenting this difference, it renders their combination impoſſible.

As the force of coheſion in mercury is ſufficient to prevent oxidation, this very force will tend to effect its diſoxigenation, with the aſſiſtance of a metal which will act directly upon the oxigen. It is a force analogous to that which produces cryſtalliſation and precipitations. (Art. V.)

3. The metallic particles have not only a mutual affinity, but likewiſe have an affinity for thoſe of other metals: hence proceed the amalgams and mixtures. It is only neceſſary to put copper in contact with mercury to effect a combination between theſe two metals. Thus when a metal oppoſes its action to a metallic ſolution, part of the metal may act upon the oxigen and upon the acid, while the other tends to combine with the metal of the oxide. Let us examine, whether we can diſcover in the precipitation of mercury, ſilver

gold

gold and copper, in the metallic ſtate, the influence of theſe two forces, that is to ſay, the mutual affinity of the particles of one and the ſame metal, and the affinity of one metal for another.

4. When a piece of copper is plunged in a ſolution of mercury by the nitric acid, or by the muriatic acid, the copper becomes inſtantly white, and the mercury is found to be reduced; but it has combined with the copper.

If inſtead of a plate of copper, a plate of very clean iron be plunged in the ſame ſolution, ſeveral hours elapſe before the liquid appears diſturbed, and a precipitate is perceived: at length a precipitate is formed; but with the muriatic ſolution in particular, this precipitate is partly in the ſtate of an oxide, and very probably retains a portion of acid.

If the affinity of a metal for oxigen were the only cauſe which produced the precipitation of another metal, the iron ought to act with much greater efficacy than the copper; for it is known to have a much ſtronger affinity for oxigen, and nevertheleſs its action is ſlow, difficult and incomplete, while that of copper is inſtantaneous. It is ſeen by the manner in which the indecompoſable acids are retained by the oxide of copper and the oxide of iron, when their combination is expoſed to the action of heat, that there can only be a very ſmall difference between the affinities of theſe metals for the acids. There is conſequently no doubt that the affinity of the copper for the mercury, with which it has actually combined, muſt have greatly contributed to its precipitation in the metallic ſtate; but the mutual affinity of the particles of mercury, muſt alone have decided, though with difficulty, in the experiment with iron, the reduction of the former metal; a portion was alſo precipitated in oxide, and probably retained a portion of acid, as the whole would have done, if the affinity of the iron for oxigen had been the ſole agent; and the portion precipitated in the metallic ſtate did not combine with the iron.

5. When a ſolution of ſilver is precipitated by copper, the precipitate which is found in the metallic ſtate is not of pure ſilver, but a combination of ſilver with a ſmall portion of copper: it could not take the copper from the plate itſelf which was plunged in the ſolution; it muſt therefore have precipitated with it out of the ſolution: the mutual affinity of theſe two metals has decided their diſoxigenation. By means of this force two combinations are effected, as happens alſo in ſeveral other circumſtances: one of the acids, with the oxide of copper, the other of ſilver with a portion of the copper. The action of the acid upon the oxide of copper, and that of ſilver upon copper, are thus put in equilibrio.

6. So likewiſe when a plate of copper is plunged into a ſolution of gold, the gold which is precipitated ſhews by its higher colour, that it has combined with copper, and the ſolution retains but a ſmall part of the copper which has been loſt by the plate.

If a plate of iron be put in this ſolution, the gold which is precipitated ſeizes, perhaps equally, a part of the iron, or at leaſt its precipitation is determined by the affinity of the iron for the gold, at the ſurface of which this laſt combines. For the gilding is a combination of the two metals at the ſurface, by which they are in contact: when the firſt ſtratum is formed, the precipitation is capable of continuing by the mere ſorce of coheſion of the gold to itſelf.

7. What I have juſt ſtated is verified in the precipitation of copper by iron. When a ſolution of copper is decompoſed by a plate of iron, and the copper attached to the plate is ſeparated, it is perceived by the brown colour of the interior ſurface, that the copper is not there pure, and that it contains ſome iron. After this firſt ſtratum the copper continued to precipitate, by contracting an adherence with itſelf, and afterwards with thoſe coats which are ſucceſſively formed; ſo that the affinity for the iron began the effect, which was continued by that of the copper for itſelf.

8. Phoſphorus precipitates ſeveral metallic ſolutions, as has been explained by Sage and Bouillon *. Though it has a ſtrong affinity with oxigen, yet what has juſt been explained relative to precipitation by the metals, ought to be applied to its action.

Pelletier has proved, that phoſphorus has the property of combining with metals, ſo that a part of that which is put in action is capable of combining with oxigen, while the other acts equally upon the metal.

Among the metallic ſolutions there are ſome which are not affected by phoſphorus; in others the metal is precipitated in oxide, which doubtleſs retains part of the acid of the ſolution, or of the phoſphoric acid which is formed; in others the metal is at length reduced. Gold, ſilver, copper, and mercury, are among thoſe which reſume the metallic ſtate.

On conſidering the obſervations which have been made on this precipitation, it appears that copper and ſilver are precipitated by combining with a ſmall portion of phoſphorus; thus, to precipitate twelve grains of ſilver, three grains of phoſphorus are conſumed. But only about three grains of phoſphoric acid, in a gelatinous ſtate, were obtained: now, one grain of phoſphorus only would be neceſſary to produce this quantity of acid; there conſequently remain more than two grains, which muſt have combined with the ſilver.

A part only of mercury thus treated reſumes the metallic ſtate; the reſt preſerves the ſtate of an oxide, and combines with the phoſphoric acid. Here the force of coheſion is weak; and no combination is made with the phoſphorus: hence the effect is but partial, as when iron is uſed. (No. IV.)

Though the affinity of gold for oxigen is very weak, and much inferior to that of copper, its precipitation is not ſo quickly effected, and a portion is precipitated in the ſtate of oxide; this doubtleſs happens becauſe it has little diſpoſition to combine with phoſphorus; and it is by the combination of copper with phoſphorus, that the precipitation of the latter metal is determined.

Some of the obſervations which I have here preſented require the accuracy of experiment for their entire explanation: but all appear to me indubitably to prove, that it is the force of coheſion which tends to recombine the particles of a one individual metal, and it is the

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* Journal de Phyſique, 1781.

mutual affinity of ſome metals which decides their precipitation in the metallic ſtate; ſo that this ſtate is more or leſs ſudden and complete, according to the energy with which theſe cauſes are capable of acting *.

ARTICLE XIV.

Of the reſulting Affinity.

1. I denominate that the *reſulting affinity*, whoſe action proceeds from ſeveral affinities in the ſame ſubſtance: for inſtance, the nitric acid is compoſed of oxigen and azote; this acid combines with pot-aſh; it acts upon pot-aſh by an affinity which reſults from that of the oxigen and that of the azote. The reciprocal mutual action of the pot-aſh is likewiſe a force which reſults from that exerted by it upon each of the ſubſtances which compoſe the nitric acid.

2. All bodies exiſting upon the earth have an affinity for each other. If we refuſe to admit this principle, it muſt be allowed, that the number of exceptions can be but very ſmall. I may therefore reaſon upon this ſuppoſition, and apply to all ſubſtances that which obſervation has made known relative to the affinities and their modifications. If this application be not forced, if it accounts for properties which cannot be directly eſtabliſhed upon experiment, the conſiderations which I preſent in this article may throw ſome light upon ſeveral phenomena, which are owing to a chemical action ſtill undetermined.

3. I have ſuppoſed in the definition of *reſulting affinity*, that the affinity of a compound ſubſtance is derived from thoſe of the ſubſtances which compoſe it. It is neceſſary to examine, what are the circumſtances which might modify the elementary affinities, and aſcertain the changes which muſt have ariſen in the affinity reſulting from them.

4. The chemical action of ſubſtances is weakened in proportion to their ſaturation. (Article II. No. 10.)

It muſt be concluded, therefore, that the reſulting affinity muſt be a leſs quantity than the elementary affinities when alone: for the latter have experienced a commencement of ſaturation; but other circumſtances may increaſe the action of the reſulting affinity, or may augment the weakneſs it derives from ſaturation.

5. If one of the ſubſtances that combine, change from a ſolid to a liquid ſtate, it acquires the advantages poſſeſſed by ſolvents; and its affinity, which was diſguiſed by the ſolid ſtate, becomes active, ſo that the reſulting affinity may, on this account, be much more conſiderable than the elementary affinities appeared to be.

Thus, when ſulphur is diſſolved by pot-aſh, the ſulphuret which proceeds from it exerts a ſtrong action upon oxigen gas, as ſoon as it has been rendered liquid by the addition of water, or has attracted ſufficient humidity from the air: becauſe it has by that means loſt

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* Fabroni has publiſhed ſome very intereſting obſervations on the mutual action of metals. (Journal de Phyſique, Brumaire, An. 8.)

its force of coheſion, as if it had been rendered liquid by the action of caloric, and becauſe the pot-aſh likewiſe exerts an action upon the oxigen, though much weaker than that of the ſulphur, ſince it cannot alone overcome the elaſticity of the gas. The action of the ſulphur is really diminiſhed by the whole of that attraction which it exerts upon the pot-aſh, and upon the water which ſerves as a ſolvent to the ſulphuret; but it gains much more by the liquidity which it acquires, than it loſes by this ſaturation. Strictly ſpeaking, all thoſe ſubſtances of which the ſolidity is ſurmounted by a ſolvent, do act by virtue of a reſulting force or affinity.

6. Circumſtances contrary to the preceding produce an oppoſite effect; and when ſubſtances by combining, become ſolid, or more diſpoſed to cryſtalliſe, this circumſtance is added to the loſs of force, ariſing from the ſaturation.

For example, pot-aſh and nitric acid have both the property of diſſolving in alcohol, and yet alcohol does not diſſolve the nitrate of pot-aſh; that is, the force of coheſion which belongs to that combination, and which, with water, produces its cryſtalliſation, has modified the elementary affinities into the reſulting affinity. What confirms this explanation is, that ſalts which are not cryſtalliſable in water, becauſe they oppoſe only a feeble coheſion, have, in general, the property of diſſolving in alcohol; but, in ſuch a manner, that they are capable of cryſtalliſing in that fluid, becauſe the weaker action of the alcohol cannot overcome, except to a certain point, the force of coheſion, of which they are not entirely deſtitute. The limited ſolubility of nitrate of pot-aſh in water, ariſes from the ſolubility of the pot-aſh, as well as of the nitric acid by water being greater than in alcohol.

7. Bodies act in proportion to their quantity, which exiſts within the ſphere of action. (Art. IV.)

Hence we deduce, that an action much ſtronger than that of the component parts may reſult from a combination, when the components, or one of the two, paſs from the elaſtic to the liquid ſtate; for they then carry into the ſphere of activity a more conſiderable quantity, the action of which may conſiderably exceed the loſs of force that is owing to the ſaturation.

Thus pot-aſh cannot overcome the reſiſtance which proceeds from the elaſticity of the oxigen gas and the azote gas; but if theſe two be combined in order to form nitric acid in the liquid ſtate, they act upon the pot-aſh in a quantity much greater than when they could have applied in the elaſtic ſtate; and the reſult of their action, though weakened by a commencement of ſaturation, is found to be much more conſiderable, than if the azote and oxigen had continued in the elaſtic ſtate.

8. The affinity of a ſubſtance which enters into combination with a compound ſubſtance, concurs with the elementary affinities of the latter to maintain its compoſition againſt the action of foreign ſubſtances, in proportion to the degree of ſaturation which it produces. Thus iron eaſily carries off oxigen from azote, or rather, it ſhares it, or takes a portion; but as ſoon as the nitric acid is combined with pot-aſh, iron can no longer ſeparate its oxigen

oxigen at an ordinary temperature; but at a higher temperature the difference of dilation ſufficiently deſtroys the reſulting affinity of the pot-aſh to cauſe the iron to combine with the oxigen.

In the oxigenated muriatic acid, the oxigen which has undergone but little ſaturation, and which conſequently is feebly retained by the muriatic acid, paſſes eaſily into other combinations; but though it is found in a much greater proportion in the oxigenated muriate of pot-aſh, it is carried off with much more difficulty by oxigenable ſubſtances.

The phoſphate of lime is not decompoſed by charcoal, even at a great degree of heat; but if it be in the ſtate of acidulous phoſphate, the portion of acid, which may be conſidered as in exceſs at the ſtate of ſaturation, is capable of being decompoſed by charcoal, becauſe it is not prevented by a ſufficiently large maſs of baſe; and it is this part alone which affords phoſphorus, when, in order to obtain this ſubſtance, we uſe phoſphate of lime reduced to an acidulous phoſphate by the ſulphuric acid.

9. The contrary takes place when, inſtead of a ſaturating ſubſtance, which ſerves as a ſupport to the reſulting affinity, one is added that tends to form a combination in which one of the conſtituent parts muſt enter. For example, when ſulphuric acid is added to a mixture of water and iron, this acid favours the decompoſition of the water, becauſe it tends to combine with the metal, and with a proportion of oxigen; a tendency concurring with that of the metal againſt the affinity which forms the combination of oxigen with hidrogen.

10. From the preceding remarks it may be concluded, that the properties of the reſulting affinity of compound ſubſtances may be reduced, 1. to the advantages of liquidity, and under this point of view it is neceſſary to apply to it the theory of ſolvents (Art. IX.); 2. to the diſpoſition to ſolidity, which produces contrary effects, which muſt be explained by the force of coheſion (Art. V.); 3. laſtly to the concentration of elaſtic ſubſtances; this circumſtance demands particular conſideration, but it may be found combined with one of the two preceding.

The obſervations preſented in No. 7 and No. 8 prove, that in thoſe compounds in which elaſtic ſubſtances are concentrated, there are eſtabliſhed by a change of conſtitution, affinities which may be conſidered as new; an additional force has accrued, to which we may apply the inverſe of what has been ſtated relative to the effects of elaſticity (Art. VI.)

The diſtinctive character of the complex affinities, of which I have treated (Art. XII.) compared with thoſe which reſult from the compoſition of the ſubſtances of which I now ſpeak, is, that in the firſt but very little change obtains in the conſtitution of the component parts; ſo that when the force of coheſion or elaſticity does not intervene, we may conſider them in the ſame manner as they have been conſidered in Article XII, No. 1. whereas a new force is eſtabliſhed in compounds in which elaſtic ſubſtances are condenſed; a power which may be conſidered analogous to that of the coheſion which obtains on the mixture of different ſubſtances that determine the combinations that are formed, or which require to be overcome by the forces oppoſed to it.

11. Caloric, by augmenting elaſticity, deſtroys the affinity reſulting from ſubſtances, of which the conſtituent parts have an unequal dilation, conformable to what has been ſtated in Art. VII.

12. Obſervation farther ſhews us, that when the reſulting affinity is not ſufficient to prevent the decompoſition, it ſometimes renders it very ſlow and long. It is to this ſlowneſs of action, to theſe progreſſive changes of conſtitution, to the different degrees of ſaturation which take place, that moſt of the phenomena obſervable in vegetation, fermentation, the animal economy, and in general among all bodies which contain condenſed elaſtic ſubſtances, muſt be aſcribed*. This ſubject will require ſtill farther explanation.

13. The reſulting affinity ought always to be conſidered as a ſingle force, while the ſubſtances from which it is derived remain in combination: but it is neceſſary to conſider the elements of which it is compoſed when a ſeparation takes place; the latter is then executed conformably to what has been explained in the diviſion of ſubſtances, in proportion to the oppoſite powers which act upon them.

14. It frequently happens that a ſubſtance acts partly by a reſulting affinity, and partly by its elementary affinities. When a metal is diſſolved by the nitric acid, one part of the acid exerts a reſulting affinity, and another acts by its elementary affinities, ſo that the oxigen of the latter part is divided between the metal and the azote, and the oxide which is formed is diſſolved in the undecompoſed acid.

15. It is evident, from what has juſt been explained relative to reſulting affinity, that a falſe idea may be adopted of the properties of a body, when we confine ourſelves, as is too often done, to the determination of its conſtituent parts, without paying attention to the other conditions of its conſtitution, if, among thoſe conſtituent parts there may be ſome which have undergone a conſiderable alteration in their ſtate. A quantity of oxigen gas does not poſſeſs the ſame chemical power when it is in the elaſtic ſtate, as when it exerts a reſulting force in its combination with azote, hydrogen, carbon, ſulphur, or a metal.

For example, the oxigen does not exert the ſame action, and has not the ſame reſulting affinity, in the ſulphuric and the ſulphureous acid: though in the ſulphuric acid a ſmaller proportion of ſulphur is combined, yet it adheres much more ſtrongly than in the ſulphureous acid, and being more condenſed, it exerts a much more powerful chemical action†. We ought not to confound the oxigen gas which is held in ſolution by water, with the oxigen which, by its combination with hidrogen, forms this liquid: the difference cauſed between them by the ſtate of condenſation, produces two ſubſtances very different in their chemical action.

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* I have often had recourſe to this change of conſtitution in the chemical explanations which I have had occaſion to give, and particularly in the lectures of the National School, where I defined the reſulting affinity by the name of *collective affinity*, and diſtinguiſhed it from *elementary affinities*.

† I have obſerved the effects of condenſation in a memoir on the ſulphureous acid (Annales de Chimie, 1789).

It

It is therefore neceſſary either to conſider all the conſtitution of a body, in order to explain its chemical properties, or to content ourſelves with eſtabliſhing thoſe properties by experiment; for we muſt pay attention to all the circumſtances of chemical action, to explain the reſults of that action, or we muſt confine ourſelves to their eſtabliſhment or confirmation.

ARTICLE XV.

Recapitulation.

1. It has often been remarked that the action of a ſubſtance is diminiſhed, in proportion as it approaches to a ſtate of ſaturation: and this diminution of power was made uſeful in the explanation of ſeveral chemical phenomena. It was affirmed that a metal could only take a definite portion of oxigen from the nitric acid, becauſe when the portion of oxigen in the acid is diminiſhed, the remainder is too ſtrongly combined with the azote. The property of carrying off only a portion of oxigen from certain metallic oxides was attributed to hidrogen; it was admitted that when a ſubſtance attracts humidity from the air, its diſſolving force comes to an equilibrium; ſo that according to the degrees of deſication of the air, it may either carry off or give out water to it. It was known that the reſiſtance found in expelling a ſubſtance from the laſt portions of a combination, either by the action of an affinity, or by that of heat, is much greater than at the commencement of the decompoſition, and ſometimes ſuch, that its entire decompoſition cannot be effected. Thus it was aſcertained that oxigen could be but partially diſengaged from the oxide of mangaueſe by the action of heat.

The combinations which are formed when forces are oppoſed, does not therefore depend upon the affinities alone, but upon the proportions of the ſubſtances which act. I have therefore only applied to all the phenomena of chemiſtry, what has been unavoidably admitted in ſeveral of them from obſervation. I have deduced the immediate conſequences.

2. Theſe conſequences are, that ſubſtances act in proportion to their affinity, and their quantity exiſting within the ſphere of activity; that the latter may compenſate the force of affinity, and that the chemical action of each power is proportionate to the ſaturations it produces. I have defined by the word *chemical maſs*, or *maſs*, the quantities determined by a like degree of ſaturation, and conſequently relative to the capacity of ſaturation: when two ſubſtances are in competition in order to combine with a third, each of them obtains a degree of ſaturation proportionate to its maſs. The ſubject of the combination alſo divides its action in proportion to the maſſes, and by varying the latter the reſults will alſo be varied.

3. I have conſidered all the forces which, by their concurrence or oppoſition to the mutual affinity of the ſubſtances brought in action according to the preceding principle, may have an influence upon chemical combinations and phenomena. They may be reduced to the following heads: the action of ſolvents, or the affinity which they exert according to

to their proportion; the force of coheſion, which is the effect of the mutual affinity of the parts of a ſubſtance or combination; the elaſticity, whether natural or produced by heat, which ought to be conſidered as an affinity of caloric; the effloreſcence, the cauſe of which may be attributed to an affinity which is not yet determined, and which acts only under very rare circumſtances; gravity likewiſe exerts its influence, particularly when it produces the compreſſion of elaſtic fluids; but it may always, without inconvenience, be confounded with the force of coheſion.

4. I have endeavoured to aſcertain whether it were poſſible to determine the relative affinity of two ſubſtances for a third; I have obſerved that for this purpoſe it would be neceſſary to diſcover in what proportions this third would be divided with a given quantity of each of the two firſt, or rather would divide its action; I have pointed out the inſurmountable obſtacles that would be met with in the means that muſt neceſſarily be employed to prove this diviſion of action, and the changes of conſtitution which would attend it.

5. As the tables of affinity have all been conſtructed upon the ſuppoſition that ſubſtances poſſeſs different degrees of affinity, which produce the decompoſitions and combinations which are formed, independently of the proportions and other conditions which contribute to the reſults, theſe tables are calculated only to give a falſe idea of the degrees of chemical action of the ſubſtances arranged in them.

6. The denomination of *elective affinity* is, in itſelf, erroneous, ſince it ſuppoſes the union of one entire ſubſtance with another, in preference to a third, while there is only a diviſion of action, ſubjected to other chemical conditions.

7. The action of two, three, or a greater number of ſubſtances, is ſubject to the ſame laws, and the reſult depends on their affinity, their proportion, or the degree of ſaturation in which they are found, and the concurrence or oppoſition of the forces they exert.

In every caſe of liquidity a mutual ſaturation takes place, and the reſult is a ſingle combination, in which all the forces are counterbalanced, while there is neither precipitation nor diſengagement of elaſtic matter; but as the action is divided when there is an oppoſition of forces and a difference of ſaturation, ſome ſubſtances are retained in the new combination leſs ſtrongly than before the mixture; they may conſequently yield to the powers of coheſion, elaſticity, or other affinities which they might otherwiſe have reſiſted.

8. The force of coheſion which has hitherto been conſidered merely as an obſtacle to ſolution, limits not only the quantities of ſubſtances which may be brought into action in a liquid, and conſequently modifies the conditions of the ſaturation which follows; but it is the power which cauſes the precipitations and cryſtalliſations that take place and determines the proportions of ſuch combinations as are made by quitting the liquid: it is this force which ſometimes even produces the ſeparation of a ſubſtance, without its forming any combination with another ſubſtance, as we have remarked in ſome metallic precipitations. I have diſtinguiſhed inſolubility from the force of coheſion, becauſe the one is relative only to the action of the ſolvent, and the other is the effect of the mutual affinity of the parts of a ſubſtance or combination, conſidered abſolutely.

Elaſticity acts by producing effects oppoſite to thoſe of coheſion, and which conſiſt either in withdrawing ſome ſubſtances from the action of others in a liquid, or in diminiſhing the proportion which exiſts within the ſphere of activity; but when all the ſubſtances are in the elaſtic ſtate, their action is ſubjected to the ſame conditions.

If tables were formed which would repreſent to diſpoſition, to inſolubility, or volatility, in the different combinations, they would ſerve to explain a great number of combinations which take their origin from the mixture of different ſubſtances, and from the influence of heat.

9. Caloric acts upon bodies like the other ſolvents, when it is not in a ſtate of *radiant caloric*, becauſe in this caſe it is not in combination.

It is neceſſary that it ſhould exceed the greateſt part of the force of coheſion, in order to render a body liquid, and other affinities may concur with it to produce this effect, in the ſame manner as itſelf concurs with the action of other ſolvents.

It is not diſtributed among bodies in proportion to their ponderable quantity, or their bulk, when it produces the degrees of temperature indicated by rhe thermometer, in the ſame manner as an acid does not take up an equal quantity of the different alkalis, to attain the ſame degree of ſaturation, and the tables of ſpecific caloric are ſimilar to thoſe of ſpecific acidity or alkalinity, which might be conſtructed: the former determine the capacity of caloric, the latter would determine the capacity of ſaturation *.

One difference, however, would exiſt between theſe tables is, that thoſe of acidity or alkalinity would repreſent the whole ſaturation to a certain aſſumed point, becauſe pure acids and alkalis might be employed: but the ſpecific calorics could only be determined from an unknown point of ſaturation to another point, becauſe the bodies ſubmitted to experiment are already combined with a quantity of caloric. The reſults which may be obtained between two degrees of the ſcale of the thermometer, have no known connection with the total quantities. To attempt to form a concluſion of one from the other, is the ſame as to pretend to determine the comparative ſolubility of the muriate of ſoda and the nitrate of pot-aſh in water, by experiments made only at or towards the point of ebullition or of congelation. In the former caſe we ſhould ſay that three parts of water would be required for the ſolution of one part of muriate of ſoda, and only half a part would be neceſſary to diſſolve one part of the nitrate of pot-aſh; in the latter that much leſs water would be required to diſſolve the muriate of ſoda than to diſſolve the nitrate of pot-aſh †.

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* Several chemiſts have buſied themſelves in determining the conſtituent parts of chemical combinations: no perſon has carried this point to ſuch an extent, and with ſo much ſucceſs, as the celebrated Kirwan; though the methods he employed are ſubject to ſome diſcuſſion.

The ſame chemiſt has eſtabliſhed that the affinities are proportionate to the quantities producing the ſaturation; but he conſidered them as forces independant of the proportions and other conditions which modify them.

† This conſideration alone, that ſpecific caloric has no known relation to the quantity of caloric combined in a body, ſhews that the experiments by which Rumford has lately pretended to prove that caloric is not a conſtituent part of bodies, cannot lead to ſuch a concluſion.

On taking the ſolid ſtate, the force of coheſion in a body obliges a part of the caloric to ſeparate; in the ſame manner as when a ſalt cryſtalliſes, it abandons part of the ſolvent, or even a part of the acid, or of the alkali, with which it might be combined.

10. It might be affirmed, that the affinities would be really repreſented by the tables of capacity; ſince they would afford the meaſure of the action of ſuch ſubſtance upon another, when a common term of ſaturation was found, ſuch as neutraliſation for the acids and the alkalis, and thermometric temperature for caloric; but nothing could poſſibly be concluded for chemical action at another term of ſaturation, and more particularly for another conſtitution, and for all the circumſtances in which the forces of elaſticity and coheſion might not be introduced.

11. After having conſidered all the affinities which may jointly produce chemical action, I examined how, in the compounds, they may reſult from their conſtituent parts, in order to acquire a conception how the varied powers which produce all the chemical phenomena, can be derived from one ſingle property of ſimple bodies.

The obſervations preſented on this ſubject have ſhewn, that what principally diſtinguiſhes compound ſubſtances, whoſe action is conſidered as ſimple is, the condenſation of the conſtituent parts, on which a new affinity depends, an affinity very different from that which the ſame parts poſſeſs in the elaſtic ſtate: the elementary affinities are modified by the ſtate of ſaturation, by the force of coheſion, or by the variations of elaſticity: the reſulting affinity may undergo, by combination, a new degree of ſaturation which tends to ſupport the compoſition, or may be weakened by other tendencies to combination with one of the conſtituent ſubſtances.

12. All the conſiderations which I have preſented reſpecting the modifications of chemical action, do not prevent us from uſing the term affinity to denote the whole chemical power of a body exerted in a given ſituation, either by its preſent conſtitution, its proportion, or even by the concurrence of other affinities; but we muſt avoid conſidering this power as a conſtant force which produces compoſitions and decompoſitions. By ſo doing we ſhould erroneouſly infer from its preſent effects in any inſtance, what it would be under other conditions which may indeed give it a very different degree of force; by ſo doing we ſhould neglect all the modifications it may experience from its initial action to that in which it obtains its equilibrium.

13. In this eſſay I have pointed out an uniform proceeding for the cultivation of chemical ſcience. I have conſidered all the forces which unite in the combinations and phenomena on which it is employed; I have endeavoured to determine the influence of each in different circumſtances. If the reader ſhould regret the loſs of all expectation of claſſing the chemical power of bodies, independently of the conditions which modify it, it will readily be granted, that the tables of affinity contained ſimple notes of facts nearly ſterile, and that they deprived us of the advantage of employing in our deductions the knowledge of the moſt fruitful properties, by means of which a great number of the reſults of chemical action may be anticipated; and the facts may be interpreted and directed without having recourſe to ſuppoſitions, or requiring any particular principles to explain anomalies.

V.

A Memoir on Mortars, Water Lime, Cements, and Pouzzolana. By CIT. GUYTON.

WE find in many publications accounts of proceſſes for the preparations of mortars and cements; enquiries into the compoſition of thoſe employed by the ancient in edifices ſtill reſpited by time; experiments to determine the condition of their ſolidity in the different circumſtances; obſervations on the materials proper to each of the ſeveral uſes; and deſcriptions of the characters. It has appeared to me that by collecting and comparing the facts and inferences, we might obtain a ſmall number of truths no longer ſubject to diſcuſſion, which cannot be rendered too familiar to mankind. This is the object of the preſent memoir, in which I ſhall alſo ſtate ſeveral experiments hitherto unpubliſhed.

Loriot Mortar.

In 1775, the late M. Loriot directed the mixture of one-fourth part of powdered lime in a good mortar, made as uſual of lime and ſand, as the eſſential conditions for obtaining buildings as ſolid as thoſe of the ancient Romans. See his Memoir, Journal de Phyſique, III. 231.

Having obſerved that the pulverization of the lime is a painful and even dangerous operation for the workmen, who, notwithſtanding every precaution, were ſubject to bleeding at the noſe. I propoſed to have the lime covered till it became ſpontaneouſly ſlaked into powder, and then to calcine it again in a ſmall furnace, and uſe it hot.

This proceſs is explained at full length in a memoir printed in the fourth volume of the Journal de Phyſique, page 481. It has been frequently uſed in the large way, and always with the moſt perfect ſucceſs; which is not to be wondered at, when we conſider that two very deſirable conditions are thus obtained, namely, that the lime is freſh and in very fine powder.

The deſcription of the oven, with plans, ſections, and profiles, is to be found in an inſtruction printed in 1775, by order of government, and in the Journal de Phyſique for October of the ſame year, Vol. VI. page 311. The dimenſions are ſufficient to afford an uninterrupted ſupply to thc labour of a number of workmen, and the proportions of the compoſition of the mortar, which were thought to be the beſt, were:

Fine ſiliceous land - - - - -	0. 3
Cement of well-baked bricks - - -	0. 3
Slaked lime - - - - - -	0. 2
Lime ſpontaneouſly ſlaked, and again calcined -	0. 2
	10.

Mortar

Mortar of Lafaye.

In 1777, Laſaye publiſhed his enquiries into the preparation which the Romans gave to their lime, and the compoſition of their mortars. The practical part is extracted, with much detail, in the Journal de Phyſique, IX. Part I. p. 437.

The eſſential article is the method of ſlaking the lime without liquefying it, in order to have a powder, and ſtill ſufficiently cauſtic to afford a very ſtrong lime, which ſhall grow hard with age.

For this purpoſe good freſh lime, made from hard ſtone, is taken and broken into pieces about the ſize of an egg; theſe are thrown into an open baſket, which is plunged into water, and kept there till the ſurface begins to boil; at which period it is taken out, and ſuffered to drain, when the lime is thrown into caſks, where it heats, and falls into powder.

This lime, ſo prepared, is taken as an ingredient in various kinds of mortar, according to the object. It keeps very well when the caſks are covered with ſtraw.

I have uſed this lime in the conſtruction of a ſmall aqueduct of more than fifty metres in length, intended to convey water to an artificial nitre bed near Dijon, about eighteen years ago. The mortar made of three parts of ſand, and three of cuttings of hard lime ſtone, became very firm and ſolid in a ſhort time, though it was under ground.

Concerning Poor Lime, or Water Lime (Chaux Maigre).

In ſome countries they have a kind of lime called poor lime, becauſe in equal quantities it does not give a mortar as fat as the others; but this lime has a property which renders it very valuable, namely, that of becoming ſolid and hard under water.

The cauſe of this property was unknown till Bergman obſerved that it is owing to a oxide of manganeſe, which the lime ſtone of Lena contains in the proportion 0.02, as do likewiſe other ſtones that form a mortar capable of hardening under water. Opuſc. II. Eſſay XIX. §. 10.

In 1783, I publiſhed, in the ſecond ſemeſtre of the new memoirs of the academy of Dijon, page 90, experiments on ſix ſpecies of this kind of lime ſtone, among which was the lime ſtone of Lena, which had been ſent me by Bergman. Two among them only aſſumed by calcination the brown colour which characteriſes manganeſe, and underwent comparative eſſays with the lime ſtone of Lena.

Independant of the poor lime ſtone indicated in that memoir, namely, thoſe of Lena, Brion, in the department of Saone et Loire, at the diſtance of five kilometres from Autun, and of Morex, near Geneva, I am at preſent acquainted with two others, viz. 1. that employed at Metz, in the works under water, of which the analyſis was made in the courſe of the polytechnic ſchool in the year 6;* and, 2. that which Smeaton, conſtructor of Eddyſtone light-houſe, has deſcribed in his account, which was obtained from Aberſhaw, in the county of Glamorgan. Bibl. Britanique, September, 1796, page 102.

I have no doubt but that ſtones of the ſame quality may be found in many other places, and in the territory of the French republic †, that is to ſay, containing manganeſe, and naturally mixed with a quantity rather conſiderable of ſilex and alumine; for it is evident that this laſt condition contributes to afford a poor lime ſtone, or mortar, nearly prepared by nature, and unfit to be mixed with any conſiderable quantity of ſand, or cement, or other dry matter.

Artificial Water Lime (*Chaux Maigre*).

I have long ago propoſed to uſe, in caſes when the natural lime ſtone is not to be had, a mixture of four parts of grey clay, and ſix of the black oxide of manganeſe, with ninety parts of good lime ſtone reduced to a powder.

This mixture, after having been well calcined and cooled, is worked to the conſiſtence of ſoft paſte, with ſixty parts of ſilex.

In the laboratory of the polytechnic ſchool is preſerved a ball of this mortar thus prepared, according to the above proportions, at the ſitting of the 29 Pluvioſe, in the year 5, which was immediately thrown into water, where it hardened, and acquired the ſpecific gravity of 2.231.

A very good water lime may be procured ſtill more cheaply by mixing, with common quick lime, a certain quantity of the white iron ore, which is found plentifully, and is in great part compoſed of carbonate of lime with manganeſe, more eſpecially thoſe which are poor in iron.

It is poſſible, as De Sauſſure obſerves in his travels in the Alps, that there may be ſtones, which though they contain no manganeſe, may from ſome other particulars of their compoſition, nevertheleſs afford lime capable of hardening under water, ſuch as the aſhes Cendree of Tournay, mortar made with the traſs of the Hollanders, &c. But this in no reſpect weakens the reſults of obſervations and experiments on the efficacy of the oxide of manganeſe; as for inſtance that of common mortar, which without this addition would immediately become diffuſed in water, but hardens very ſpeedily when a certain doſe of that oxide enters into its compoſition. Mr. Smeaton here affords us an important remark, namely, that balls made of lime and ſand well worked together, and then put into water, were deſtroyed, whatever were their proportions (Loco cit.)

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* In this analyſis it was found to contain in the hundred carbonic acid 39.0; lime 44.5; ſilex 5.25; alumine 1.25; manganeſe 3.5; oxide iron 3.2; water 2.25; loſs 1.05.

† Citizen Lomet found a quarry at Saintrails, in the department of Lot-et-garon, which has been ſucceſsfully employed in building under water.

Concerning

Concerning Beton, or Water Cement.

Every compoſition of mortar intended to hold under water, and capable of becoming almoſt inſtantly ſolid, is called in French Beton.

This water cement may be made in ſeveral manners: 1. the mortar Loriōt, if well made, holds very well under water. I have uſed it in the walls of a terrace through which there was not the ſmalleſt filtration. I have a maſs of this mortar hollowed out into a baſon, which when filled with water, and placed on a ſheet of paper, does not ſhow the ſlighteſt trace of moiſture.

The mortar Lafaye hardens leſs ſpeedily, but when once it has become ſolid, it holds water very well without being ſoftened.

3. The mortar ſpoken of by Smeaton, compoſed of equal parts of lime and traſs, or the volcanic aſhes of the Hollanders, is a true water cement, as it does not yield at all to that fluid.

4. The compoſitions of mortar with Pouzzolāna may alſo be uſed under water, and when once indurated, they ſurpaſs all others for the hardneſs and reſiſtance to the motions of the waters.

The moſt common water cement is made with the poor lime ſtone, or water lime. It is formed into priſms in hard paſte, which are let down into the water, and form a wall, or otherwiſe this material is poured between two walls, at the diſtance of 21 or 22 metres aſunder, in the manner of coffer work, for the purpoſe of forming a ſuitable defence or incloſure for waters in parts of canals above the ſurface of the ground.

Methods of ſupplying the Want of Pouzzolana.

Pouzzolana is a volcanic ſand which is brought from the environs of Naples. Though the ſuperiority of mortar formed with this material is well known and determined, yet it is often neceſſarily diſpenſed with on account of the expence of carriage, which ſo greatly enhances the price.

But as we are well acquainted at preſent with a number of extinct volcanos in France, we may find an abundance of materials to ſupply the place of the Pouzzolana. The experiment was made in 1787 by Citizen De Ceſſart, engineer, charged with the conſtruction of the cones of Cherbourg. In the month of Auguſt of that year I ſent him about 13 kilograms of the extinct volcano of Drevin in the department of Saone-et-Loire.

The baſaltes was firſt heated in a ſmall reverberatory furnace, then thrown red-hot into water, and ſtamped and ſifted, ſo that the largeſt grain did not exceed the ſize of a pea. The following are the reſults of experiments on maſonry conſtructed with this cement of baſaltes, compared with another cement from the Italian Pouzzolana, according to the letter of Citizen Ceſſart, dated Feb. 12, 1788.

" In order to aſcertain perfectly the tenacity of this cement, I cauſed moulds to be made ſix inches in length, ſix in width, and four in thickneſs, forming ſmall maſſes of 144 cubic inches.

" Compoſition of the cement, 180 cubic inches of baſaltes, 73 of lime flaked (fuſée à la romaine) 93 inches of ſpring water, and 288 ſtones of granite of the ſize of a nut, the whole forming a cube of 534 inches.

" Two moulds of a 144 inches each were filled with this compoſition. One of theſe was immerſed in the ſea for 76 days, the other remained in the air for the ſame time.

" When the tenacity of this maſonry was to be tried, the extremities of theſe maſſes were placed on the edge of a piece of iron: each of them was loaded with a kind of wooden trough, very narrow at bottom, placed acroſs the middle of the maſs, and gradually loaded with balls of lead.

" The ſame operation was formed with two ſimilar bricks of Italian Pouzzolana, having the ſame proportions, and having remained likewiſe 76 days in the ſea. It ſupported 123 pounds before it broke, whereas the piece which remained 76 days in the air, broke under a charge of 71 pounds."

All theſe proportions, as the writer obſerves, are infinitely too ſmall to eſtabliſh an exact ratio, but it may nevertheleſs be concluded, that the Pouzzolana baſaltes of Burgundy is nearly as good as that of Italy, which comes to us by Toulon.

Citizen Ceſſart in the ſame letter, demanded ſix thouſand weight of this Pouzzolana to repeat his experiments on a larger ſcale; to which it appeared, that the difficulties and expence of tranſportation were at that time an obſtacle. But this would no longer be any impediment; as the canal of the Saone and the Loire is in full navigation, and the extinguiſhed volcano of Drevin is no more than one myriameter diſtant from it.

Another material ſtill more common than baſaltes, and which may ſometimes be a ſubſtitute for pouzzolana, is the aſhes of pit coal, provided it be a heavy cakeing coal, leaving behind it rather a ſcoria than a cinder.

I likewiſe ſent this article to Cherbourg in 1787, under the name of light pouzzolana, to diſtinguiſh it from that which was made with baſaltes. It was ſubmitted to the ſame comparative experiments, and afforded the following comparative reſults, as is ſtated in the letter of Citizen Ceſſart.

" The bricks which had remained 76 days in the air, ſupported charges of 115 and 137 pounds before they broke.

" The ſame blocks immerſed in the ſea likewiſe for 76 days, broke with 48 and 30 pounds, conſequently they had loſt their conſiſtence in the water of the ſea."

We obſerve, that mortar made with this aſhes is evidently ſuperior to that formed with the pouzzolana of Italy, as its tenacity is almoſt double. It cannot indeed be employed under water like the cement of water lime, but it is probable that it would ſucceed very well in works which are conſtructed dry, and are not to receive water until they have acquired ſolidity.

With this view I pointed out this preparation for the maſonry of the inner faces of the canal of Saone-et-Loire, which were preciſely in this condition, and near which there were conſiderable maſſes of this matter afforded by the fires of a glaſs-houſe.

ADDITION.

Citizen Monge, who viſited the ruins of Ceſarea, of which the hiſtorian Joſephus has given ſo magnificent a deſcription (Cæſarea Stratonis) at preſent abſolutely without inhabitants, aſſures me, that he could diſtinguiſh in its ruins, by the taſte of the architecture and the ſolidity of the mortar, the different ages of the firſt conſtruction, and the works ſucceſſively added after the invaſion of the Arabs, and during the Cruſades.

" He obſerved on the ſide of the gate at preſent in ruins, the remains of a temple conſecrated to Auguſtus. Endeavours had been made to ſupport the walls in front by counterforts ſtill in being, which in ſome places exhibited a concave mould or impreſſion of ornaments of an exquiſite taſte formed of mortar, which had applied itſelf againſt reliefs which are now no longer in being.

The ſtone of the original ſtructure was corroded by the weather to a great depth, and the mortar projected out beyond it.

He attempted in vain to break off a piece. It was of a very fine and equal texture, apparently compoſed of fine ſand and very little lime well blended together.

I thought that this obſervation might be received with pleaſure at the end of the preſent memoir.

VI.

On the Combinations of the Metals with Sulphur, by Cit. Vauquelin[*].

We may divide the combinations of the metals with ſulphur into three orders, namely, firſt metals and ſulphur, which are combinations properly denominated metallic ſulphurets, or ſulphurated metals. Nature and art preſent many ſamples of this kind. 2. Metallic oxides and ſulphur; theſe ought to be called ſulphurated metallic oxides, or oxided metallic ſulphurets. They are alſo very common in nature. 3. Metallic oxides with ſulphur and hydrogen. Theſe triple combinations are called metallic hydro-ſulphurated oxides.

If the ſubſtance which predominates in theſe different combinations be taken as the baſe of the nomenclature, we ſhould reverſe the order of the common denominations; for in the ſimple ſulphurets the maſs of metal always predominates, and ſo likewiſe in the ſulphurated oxides; but in the hydro-ſulphurets at firſt the metals, then the ſulphur, and afterwards the hydrogen predominate.

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[*] Annales de Chymie, XXXVII. 57.

So

So that inſtead of metallic ſulphurets, oxided metallic ſulphurets and metallic hydroſulphurets, we ought to ſay ſulphurated metals, ſulphurated metallic oxides, and ſulphurohydrogenated metallic oxides.

But cuſtom has predominated in chemiſtry, either becauſe the preference has been dictated by the facility of pronunciation, or becauſe the terms themſelves are leſs harſh. This however is of no great importance, as it is ſufficient that the elements of bodies ſhould be expreſſed by their names, and the value of theſe names ſhould be preciſely underſtood.

Concerning the action of the acids on the metallic ſulphurets.

Sulphuric acid.

Whenever the ſum of the affinities of oxigen for a metallic ſubſtance, and of the metallic oxide formed by the ſulphuric acid, exceed thoſe of the metal for ſulphur, and of oxigen for the ſulphureous acid, there is conſtantly a decompoſition of the metallic ſulphurets; and the reſult is a metallic ſulphur and ſulphurous acid. All theſe conditions are not conſtantly neceſſary, but every one of them concurs in accompliſhing the new combinations which are formed, and accelerating the proceſs. There are nevertheleſs caſes, in which for want of one of theſe conditions no action takes place between the bodies.

The ſulphuric acid for example, decompoſes the natural ſulphurated oxide of iron, or martial pyrites very well, but the muriatic acid cannot decompoſe it, becauſe the ſulphur in this caſe is not attracted by any body, whereas in the other caſe it is attracted by the ſulphuric acid, which it converts into ſulphuric acid; for though I am aſſured by many facts, that the iron in martial pyrites is oxided nearly to the point at which it is eaſily ſoluble in acids, nevertheleſs theſe compounds cannot be decompoſed without the formation of a great quantity of ſulphurous acid.

This opinion is confirmed by the conditions required for the ſucceſs of the operation. In fact, the complete decompoſition of pyrites is not effected but by means of heat, and a large quantity of concentrated ſulphuric acid; for it is evident, that in this caſe the acid is required not only to ſaturate the oxide of iron, but alſo the ſulphur. I might alſo urge in proof of the ſame opinion, the greater affinity of the muriatic acid for the oxide of iron which it cannot nevertheleſs ſeparate from the ſulphur in thoſe circumſtances. It may ſeem aſtoniſhing, that the nitric acid, which has a leſs affinity with iron, ſhould nevertheleſs decompoſe the martial pyrites with ſo much eaſe; but we muſt not deceive ourſelves in this reſpect, by ſuppoſing that the acid takes the oxide of iron from the ſulphur by a ſtronger affinity, for it is on the contrary, by giving a new quantity of oxigen to that ſubſtance, that its affinity for the ſulphur is remarkably diminiſhed. The ſulphur does not unite in fact with iron oxided to the maximum, and if by any peculiar proceſſes theſe ſubſtances become combined, a portion of oxigen is then ſeparated.

Though the muriatic acid cannot decompoſe the ſulphurated oxide of iron, it decompoſes on the contrary with the greateſt facility the ſulphuret of iron, in which the metal is not oxigenated. But in this caſe three forces act at once, namely, that of the iron on the oxigen of the water, that of the ſulphur on the hidrogen of the ſame ſubſtance, and that of the metallic oxide formed by the muriatic acid. Under theſe circumſtances ſulphurated hidrogenous gas is conſtantly diſengaged.

The ſulphuric acid which does not decompoſe the ſulphurated oxide of iron, unleſs it be concentrated and boiled, decompoſes the ſulphuret of iron in the cold, and the more eaſily when diluted with a certain quantity of water. The reaſon of this is evident, and need not be explained.

Though a great quantity of water is inimical to the action of the acids upon metallic ſulphurets in general, it is however leſs ſo with regard to iron than other metals, becauſe ſulphurated hidrogen does not decompoſe the ſolutions of iron in acids.

Concerning the Sulphuret of Lead.

In the natural ſulphuret of lead or galæna the lead exiſts in the metallic ſtate, for which reaſon when this mineral ſubſtance is put in contact with muriatic acid, ſulphurated hidrogen is formed even in the cold. It appears even when ſulphur is united to the oxide of lead. That metal loſes its oxigen to unite to the ſulphur for the combination by this means obtained, is perfectly ſimilar to natural galena. It is eaſy to conceive how the reduction of the lead is obtained in this proceſs: a portion of the ſulphur combines with the oxigen, and forms ſulphuric acid, which is driven off by the heat, while another portion of the ſulphur unites to the metallic lead. The reduction of the lead, and the formation of galena take place nearly in the ſame manner, when a ſolution of this metal in an acid is mixed with a ſolution of ſulphurated hidrogen, or any hydro-ſulphuret whatever. There is however this difference, that in the laſt caſe it is hidrogen, which takes oxigen from the lead, and forms water at the ſame time that the ſulphur and the metal, which meet in a very extreme ſtate of diviſion, combine without heat in the midſt of the fluid. There is alſo another difference, that the galena formed by this proceſs contains a little more ſulphur than the natural compound, as is proved by the ſmall quantity of ſulphur which is ſeparated from that ſubſtance by a very gentle heat, and then leaves a compound in a ſtate abſolutely ſimilar to that of common galena.

From theſe facts it reſults that the muriatic acid, and probably all the acids which act upon the oxide of lead by their maſs and their concentration, do loſe their lead by the action of the ſulphurated hydrogen, as ſoon as theſe two forces, or even one of them is weakened; but whatever may be the quantity of acid united to lead, or any other metal, the decompoſition of the ſalt will take place, if any hydro-ſulphuret be employed in ſufficient quantity.

We cannot be ſurprized at the diminution of the affinities of the acids for the metallic oxides by water, nor their more powerful action on theſe ſubſtances by their maſſes. Antimony, mercury, biſmuth, and lead itſelf, afford ſtriking examples of this.

We know in fact, that by mixing a great quantity of water with their concentrated ſolutions, very conſiderable precipitates are formed, which have been erroneouſly taken for pure

pure metallic oxides, but which are really ſalts, containing a portion of acid too ſtrongly adherent to be ſeparated but by means of an alkali.

We may therefore conſider the combinations formed by theſe metals, and probably by many others likewiſe with acids as neutral ſalts, diſſolved in a ſuperabundance of acid, which is taken from them by water, when the ſalts are precipitated in a truly neutral and inſoluble ſtate.

From the manner in which the muriatic acid acts upon the metallic ſulphurets, it follows, that the more oxigen is abſorbed by the metals, in order to unite with this acid, the leſs of ſulphur will remain after the ſolution, and the contrary; but if theſe metals abſorb but little oxigen, if they contain much ſulphur, and if alſo their affinity for oxigen be not conſiderable, the whole of the metal will not be ſeparated from the ſulphur by the muriatic acid, becauſe in proportion as this combuſtible body became more abundant, it oppoſes the ſolution of the metal by virtue of its maſs; this is the reaſon why certain metallic ſulphurets, which are not attacked by the muriatic acid, are decompoſed by it, when a portion of their ſulphur has been ſeparated in diſtillation.

The mode of action of the nitric acid upon the metallic ſulphurets is very different from this, for though the ſulphur is not in this caſe attracted by any body, it is nevertheleſs completely ſeparated from the metal, and thrown down in all its purity, if the experiment be properly conducted: but if the combination of the nitric acid with the metal be not aided by hidrogen, that takes place with the muriatic acid, it gives out to the metal a portion of oxigen much more readily than water; and it is in conſequence of this affinity of the oxigen for the metal, and of the oxide which is formed for the undecompoſed acid, which is effected by the ſolution. Thus every time that metallic ſulphures are decompoſed by the nitric acid, there is a conſtant formation of nitrous gas, and never of ſulphurated hidrogen.

It appears that in general ſulphur has a greater affinity for the metals than for their oxides, and that this affinity diminiſhes in proportion to the quantity of oxigen they abſorb: they are ſome which do not even combine with ſulphur, except in the complete metallic ſtate; thus for inſtance, on triturating ſome oxide of lead with ſulphur, a portion of the latter deflagrates, and the other combines with the lead, which is brought to the metallic ſtate.

Hence it appears that zinc does not combine with ſulphur when it is completely metallic, at leaſt nature has not hitherto afforded us ſufficient examples, and art has not ſucceeded in effecting the combination of theſe two ſubſtances. What is known in mineralogy under the name of *blende*, or ſulphurated zinc, has been improperly conſidered by ſome mineralogiſts as a combinaion of metallic zinc with ſulphur. It is, on the contrary, oxide of ſulphurated zinc, which very frequently contains hidrogen; for, the *blendes* almoſt always diſſolve in the acids, and produce ſulphurated hidrogen gas. Thus a tranſparent blend which acts in this manner with the acids, ought to be conſidered as a real oxide of hidro-ſulphurated zinc, or rather ſulphureo-hidrogen. There may be oxides of zinc ſimply ſulphurated, and theſe will then be two ſpecies of minerals very different in their nature; the one diſſolves in the ſulphuric and muriatic acids without efferveſcence, and the other, on the contrary, affords a greater or leſs quantity of ſulphurated hidrogen gas.

Mercury, in a metallic ſtate, does not very intimately combine with ſulphur; ethiops mineral appears to be rather a mixture than a real combination; but when this oxided metal is combined with ſulphur, as in cinnabar, for example, it adheres ſo ſtrongly, that no ſubſtance is capable of directly decompoſing it; and it is only by altering one or the other of theſe principles that their ſeparation can be effected, or rather it is neceſſary to burn the ſulphur or diſoxide the mercury; cinnabar owes its fine red colour only to the great quantity of oxigen which the mercury contains.

VII.

On the Precipitate formed by mixing Muriate of Lime and Muriate of Magneſia. By W. Lambe, M. A.

To Mr. NICHOLSON.

SIR,

HAVING diſcovered, what I conceive to be, the true explanation of the appearance of a precipitate by mixing the muriate of lime and magneſia, I take leave to communicate it to you. All the accounts I have met with on this ſubject are erroneous; and that offered by the celebrated Berthollet is as much ſo as any (Journal, IV. 67). To refute it, I need only mention a ſingle fact. Let the carbonate of magneſia, before diſſolving it in the muriatic acid, be thoroughly deprived of its carbonic acid by heat, and then there will be no precipitation by mixing the two ſalts. To the carbonic acid, then, is this appearance to be attributed, of which the carbonate of magneſia cannot be wholly deprived by the addition of ſo much muriatic acid as will produce an apparent ſaturation.

Diſſolve ſome common magneſia in a ſolution of carbonic acid in diſtilled water. To the clear ſolution add a little muriate of lime, and a precipitate is immediately formed, entirely ſimilar, I believe, to that in queſtion. If the liquor be examined after ſome days, carbonate of lime is found diſſolved in it, inſtead of carbonate of magneſia. It ſeems, then, that theſe precipitates are formed by an interchange of principles between carbonate of magneſia and muriate of lime, and conſiſt of carbonate of lime and muriate of magneſia. The liquor, by evaporation, depoſits a cryſtalline matter, which efferveſces with acid, and probably is compoſed of the ſame ſalts in different proportions.

Laſtly, put ſome of the precipitate in the nitric or muriatic acids. It diſſolves ſlowly, with a gradual extrication of air bubbles, and adding acid enough, it may be wholly converted into deliqueſcent ſalts; but a good deal of acid is neceſſary for this purpoſe. Even diſtilled vinegar has this effect in part.

I intended to repeat theſe experiments with greater care and exactneſs, with a view to the improvement of the analyſis of mineral waters; but having been diſtracted by other occupations, I beg your acceptance of them for the uſe of your valuable Journal, if you deem them of ſufficient conſequence. Before I conclude, I will take the opportunity of correcting another error which, I believe, pervades the moſt recent chemical works. It is this:—Ammonia is ſaid to decompoſe ſulphate of magneſia only in part: but this, alſo, is owing to the ammonia not having been wholly deprived of carbonic acid. I precipitated the whole of the magneſia from ſulphate of magneſia, by uſing ammoniacal gas in exceſs. I then filtered the liquor, and boiled it with a fixed alkali; but I only obtained a little lime, diſcoloured ſeemingly with oxide of iron.

I am, SIR,

With great reſpect,

Your obedient ſervant,

Warwick, May 23, 1801. W. LAMBE.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Gratuitous Analyſis of Minerals, by the Britiſh Mineralogical Society.

I HAVE received the following addreſs by poſt, which I give without abridgement or alteration.—W. N.

The immenſe importance which, at all times, and particularly at preſent, is attached to improvements in agriculture, and the liberal encouragement to experiments in the art of cultivating the land offered by the patriotic views of the Board of Agriculture, have rendered it a deſirable object to aſſiſt the inquiries of the farmer in that part of experimental chemiſtry which determines the nature of ſoils by an analyſis of their ſeveral contents.

The Britiſh Mineralogical Society, having of late increaſed the number of their members, find themſelves enabled to extend the plan of their inſtitution, from the analyſis of *minerals*, in the uſual ſenſe of the term, to that of the various ſoils which are made the baſis of agricultural operations.

They therefore give this public notice that they will examine, free of expence, all ſpecimens of earths or ſoils, with a view of determining the nature and proportion of their different contents, with as much accuracy as ſhall ſeem requiſite.

They

They require, however, that the perſons ſending ſpecimens ſhall comply with the following conditions:

That the ſpecimen be about three or four pounds in weight, incloſed in a deal box properly packed with hay, and along with it an account be added of the pariſh and county from which it is procured, the name of the ſender and his addreſs per poſt, and an anſwer to the following queries——What is the depth of the ſoil?—What the nature of the ſub-ſoil?—is it on a hill or level ground? and, if the former, what is the aſpect?—how near to any river or ſtream?—and is the ſoil itſelf dry or ſpringy?—what is the rotation of crops which it uſually bears?—has it ever been limed or had any other earth laid on?——Any other particulars worthy of notice may alſo be added, and a ſpecimen of the ſub-ſoil ſhould all be incloſed.

Before the ſpecimen is packed up, it ſhould be gently dried, either in the ſun or in any warm place, for a few days, but muſt on no account be heated over a fire.

The box containing the ſpecimens muſt be ſent *carriage paid* to Mr. W. H. Pepys, Jun. No. 24, Poultry, London.

The ſociety intend, if conſiſtent with their other occupations, to return the reſult of the analyſis within two months of the arrival of the ſpecimen.

London, April 16, 1801.

Four Eſſays on Practical Mechanics; the firſt on Water Wheels; the ſecond on the Steam Engine; the third on Mills; and the fourth on the Simplification of Machinery. By Thomas Fenwick, Coal Viewer. 8vo. p. 84. Newcaſtle upon Tyne, printed for the Author, and ſold by Mawman, London.

THE particular advantage of this treatiſe is, that it is written with ſuch plainneſs that the inſtructions it contains may be carried into effect by thoſe whoſe acquiſitions extend no farther than the firſt rules of common and decimal arithmetic; in addition to which, as the facts are all deduced from obſervations and experiments on a large ſcale of actual work, all deſcriptions of men may receive advantage from his data reſpecting mills, ſteam engines, &c.

Dr. Young's Inquiries respecting Sound

Fig. 41.

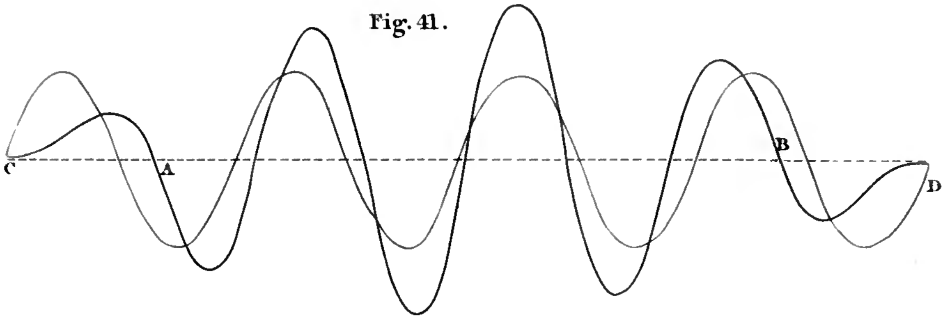


Fig. 42.

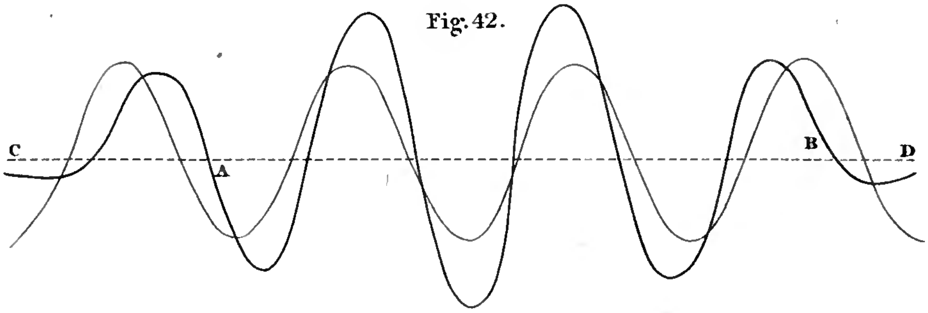


Fig. 44.

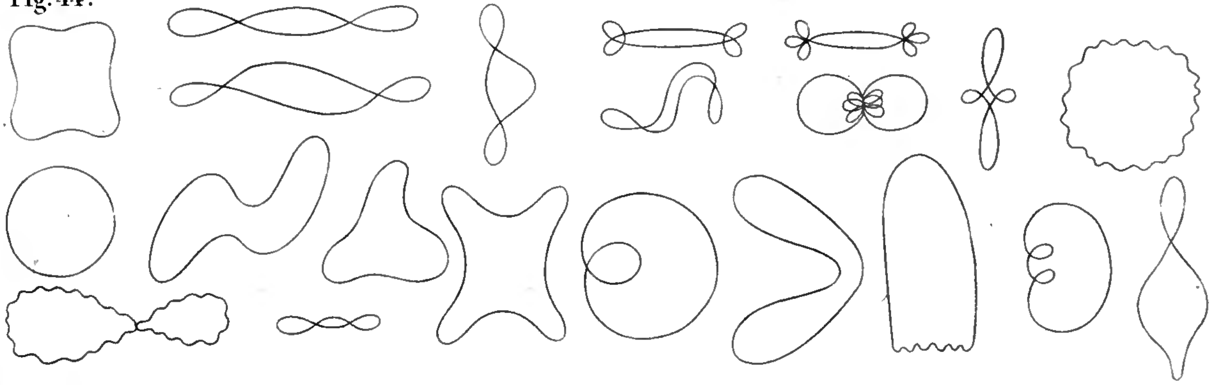


Fig. 45.

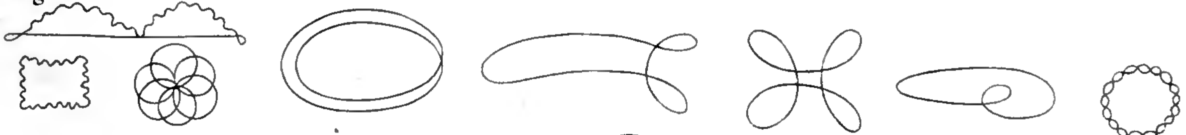


Fig. 46.

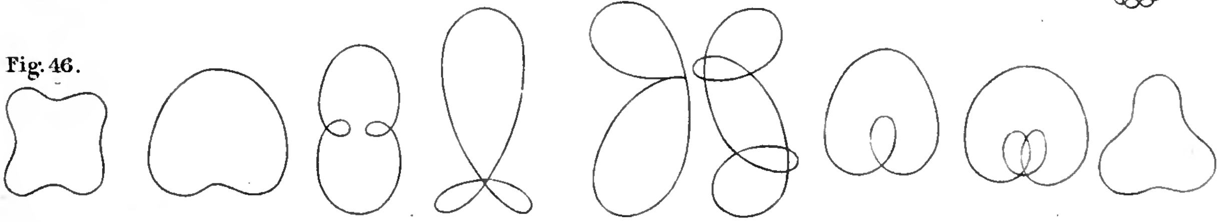
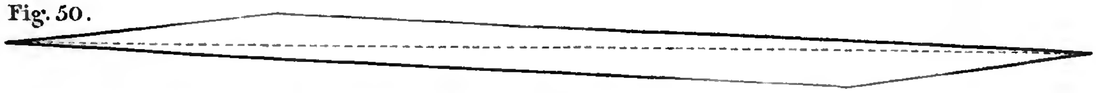


Fig. 49.

Fig. 50.



Mutlow Sc. Russell Court.

Dr. Young's Inquiries respecting Sound.

Fig. 53.

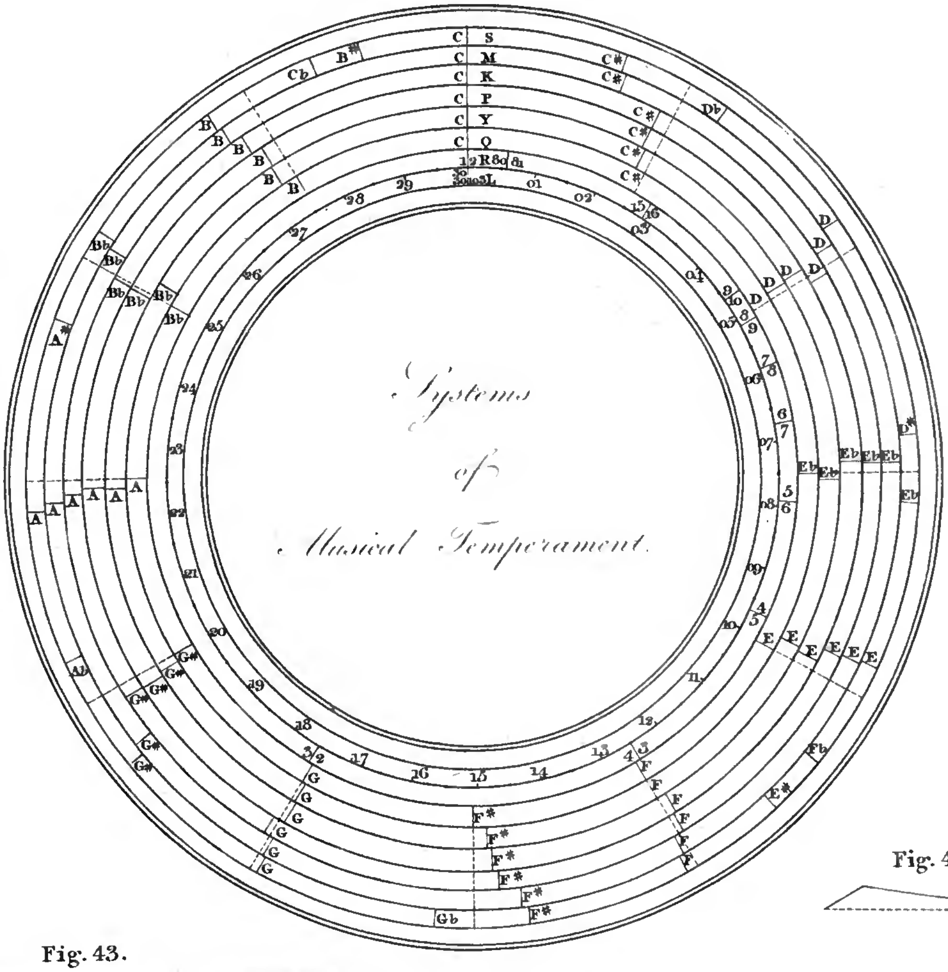


Fig. 48.

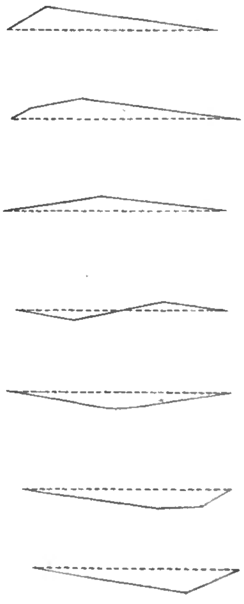


Fig. 43.

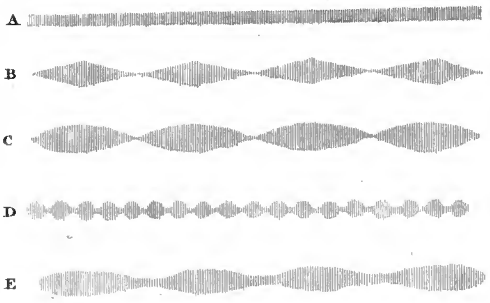


Fig. 47.

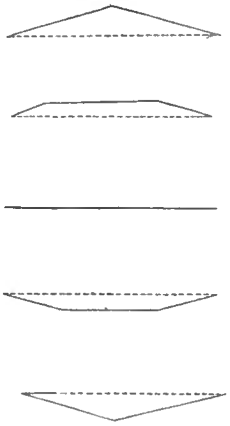


Fig. 52.

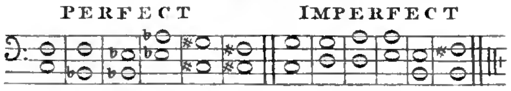



Fig. 51.

Mutlow sc. Russell Court.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JULY, 1801.

ARTICLE I.

Outlines of Experiments and Inquiries reſpecting Sound and Light. By THOMAS YOUNG, *M. D. F. R. S.*

(*Concluded from page* 91.)

XIII. *Of the Vibrations of Chords.*

BY a ſingular overſight in the demonſtration of Dr. Brook Taylor, adopted as it has been by a number of later authors, it is aſſerted, that if a chord be once inflected into any other form than that of the harmonic curve, it will, ſince thoſe parts which are without this figure are impelled towards it by an exceſs of force, and thoſe within it by a deficiency, in a very ſhort time arrive at or very near the form of this preciſe curve. It would be eaſy to prove, if this reaſoning were allowed, that the form of the curve can be no other than that of the axis, ſince the tending force is continually impelling the chord towards this line. The caſe is very ſimilar to that of the Newtonian propoſition reſpecting ſound. It may be proved, that every impulſe is communicated along a tended chord with an uniform velocity; and this velocity is the ſame which is inferred from Dr. Taylor's theorem; juſt as that of ſound, determined by other methods, coincides with the Newtonian reſult.

But, although ſeveral late mathematicians have given admirable ſolutions of all poſſible caſes of the problem, yet it has ſtill been ſuppoſed, that the diſtinctions were too minute to be actually obſerved; eſpecially, as it might have been added, ſince the inflexibility of a wire would diſpoſe it, according to the doctrine of elaſtic rods, to aſſume the form of the harmonic curve. The theorem of Euler and De la Grange, in the caſe where the chord is ſuppoſed to be at firſt at reſt, is in effect this: continue the figure each way, alternately on different ſides of the axis, and in contrary poſitions; then, from any point of the curve, take an abſciſs each way, in the ſame proportion to the length of the chord as any given portion of time bears to the time of one ſemivibration, and the half ſum of the ordinates will be the diſtance of that point of the chord from the axis, at the expiration of the time given. If the initial figure of the chord be compoſed of two right lines, as generally happens in muſical inſtruments and experiments, its ſucceſſive forms will be ſuch as are repreſented in Plate VI. Figs. 47, 48: and this reſult is fully confirmed by experiment. Take one of the loweſt ſtrings of a ſquare piano forte, round which a fine ſilvered wire is wound in a ſpiral form; contract the light of a window, ſo that, when the eye is placed in a proper poſition, the image of the light may appear ſmall, bright, and well defined, on each of the convolutions of the wire. Let the chord be now made to vibrate, and the luminous point will delineate its path, like a burning coal whirled round, and will preſent to the eye a line of light, which, by the aſſiſtance of a miſcroſcope, may be very accurately obſerved. According to the different ways by which the wire is put in motion, the form of this path is no leſs diverſified and amuſing, than the multifarious forms of the quieſcent lines of vibrating plates, diſcovered by Profeſſor Chladni, and is indeed in one reſpect even more intereſting, as it appears to be more within the reach of mathematical calculation to determine it; although hitherto, excepting ſome ſlight obſervations of Buſſe and Chladni, principally on the motion of rods, nothing has been attempted on the ſubject. For the preſent purpoſe, the motion of the chord may be ſimplified, by tying a long fine thread to any part of it, and fixing this thread in a direction perpendicular to that of the chord, without drawing it ſo tight as to increaſe the tenſion: by theſe means, the vibrations are confined nearly to one plane, which ſcarcely ever happens when the chord vibrates at liberty. If the chord be now inflected in the middle, it will be found, by compariſon with an object which marked its quieſcent poſition, to make equal excurſions on each ſide of the axis; and the figure which it apparently occupies will be terminated by two lines, the more luminous as they are nearer the ends, Plate V. Fig. 49. But, if the chord be inflected near one of its extremities, Fig. 50, it will proceed but a very ſmall diſtance on the oppoſite ſide of the axis, and will there form a very bright line, indicating its longer continuance in that place; yet it will return on the former ſide nearly to the point from whence it was let go, but will be there faintly viſible, on account of its ſhort delay. In the middle of the chord, the excurſions on each ſide the axis are always equal; and, beyond the middle, the ſame circumſtances take place as in the half where it was inflected, but on the oppoſite ſide of the axis; and this appearance continues unaltered in its proportions, as long as the chord vibrates,

vibrates at all: fully confirming the non-exiſtence of the harmonic curve, and the accuracy of the conſtruction of Euler and De la Grange. At the ſame time, as M. Bernouilli has juſtly obſerved, ſince every figure may be infinitely approximated, by conſidering its ordinates as compoſed of the ordinates of an infinite number of trochoids of different magnitudes, it may be demonſtrated, that all theſe conſtituent curves would revert to their initial ſtate, in the ſame time that a ſimilar chord bent into a trochoidal curve would perform a ſingle vibration; and this is in ſome reſpects a convenient and compendious method of conſidering the problem. But, when a chord vibrates freely, it never remains long in motion, without a very evident departure from the plane of the vibration; and, whether from the original obliquity of the impulſe, or from an interference with the reflected vibrations of the air, or from the inequability of its own weight or flexibility, or from the immediate reſiſtance of the particles of air in contact with it, it is thrown into a very evident rotatory motion, more or leſs ſimple and uniform according to circumſtances. Some ſpecimens of the figures of the orbits of chords are exhibited in Plate V. Fig. 44. At the middle of the chord, its orbit has always two equal halves, but ſeldom at any other point. The curves of Fig. 46, are deſcribed by combining together various circular motions, ſuppoſed to be performed in aliquot parts of the primitive orbit: and ſome of them approach nearly to the figures actually obſerved. When the chord is of unequal thickneſs, or when it is looſely tended and forcibly inflected, the apſides and double points of the orbits have a very evident rotatory motion. The compound rotations ſeem to demonſtrate to the eye the exiſtence of ſecondary vibrations, and to account for the acute harmonic ſounds which generally attend the fundamental ſound. There is one fact reſpecting theſe ſecondary notes, which ſeems intirely to have eſcaped obſervation. If a chord be inflected at one-half, one-third, or any other aliquot part of its length, and then ſuddenly left at liberty, the harmonic, note which would be produced by dividing the chord at that point is intirely loſt, and is not to be diſtinguiſhed during any part of the continuance of the ſound. This demonſtrates, that the ſecondary notes do not depend upon any interference of the vibrations of the air with each other, nor upon any ſympathetic agitation of auditory fibres, nor upon any effect of reflected ſound upon the chord, but merely upon its initial figure and motion. If it were ſuppoſed that the chord, when inflected into right lines, reſolved itſelf neceſſarily into a number of ſecondary vibrations, according to ſome curves which, when properly combined, would approximate to the figure given, the ſuppoſition would indeed in ſome reſpects correſpond with the phænomenon related; as the coefficients of all the curves ſuppoſed to end at the angle of inflection would vaniſh. But, whether we trace the conſtituent curves of ſuch a figure through the various ſtages of their vibrations, or whether we follow the more compendious method of Euler to the ſame purpoſe, the figures reſulting from this ſeries of vibrations are in fact ſo ſimple, that it ſeems inconceivable how the ear ſhould deduce the complicated idea of a number of heterogeneous vibrations, from a motion of the particles of air which muſt be extremely regular, and almoſt uniform; an uniformity which, when proper precautions are taken, is not contradicted by examining the motion of the chord with the aſſiſtance of a powerful magnifier. This difficulty occurred very ſtrongly to Euler; and De la Grange even ſuſpects ſome fallacy in the experiment, and that a muſical ear judges from previous aſſociation. But, beſides that theſe ſounds are diſcoverable to an ear deſtitute of ſuch aſſociations, and, when the ſound is produced by two ſtrings in imperfect uniſon, may be verified by counting the number of their beats, the experiment already related is an undeniable proof that no fallacy of this kind exiſts. It muſt be confeſſed, that nothing fully ſatisfactory has yet occurred to account for the phænomena; but it is highly probable that the ſlight increaſe of tenſion produced by flexure, which is omitted in the calculations, and the unavoidable inequality of thickneſs or flexibility of different parts of the ſame chord, may, by diſturbing the iſochroniſm of the ſubordinate vibrations, cauſe all that variety of ſounds which is ſo inexplicable without them. For, when the ſlighteſt difference is introduced in the periods, there is no difficulty in conceiving how the ſounds may be diſtinguiſhed; and indeed, in ſome caſes, a nice ear will diſcover a ſlight imperfection in the tune of harmonic notes: it is alſo often obſerved, in tuning an inſtrument, that ſome of the ſingle chords produce beating ſounds, which undoubtedly ariſe from their want of perfect uniformity. It may be perceived that any particular harmonic is loudeſt, when the chord is inflected at about one-third of the correſponding aliquot part from one of the extremities of that part. An obſervation of Dr. Wallis ſeems to have paſſed unnoticed by later writers on harmonics. If the ſtring of a violin be ſtruck in the middle, or at any other aliquot part, it will give either no ſound at all, or a very obſcure one. This is true, not of inflection, but of the motion communicated by a bow; and may be explained from the circumſtance of the ſucceſſive impulſes, reflected from the fixed points at each end, deſtroying each other: an explanation nearly analogous to ſome obſervations of Dr. Matthew Young on the motion of chords. When the bow is applied not exactly at the aliquot point, but very near it, the correſponding harmonic is extremely loud; and the fundamental note, eſpecially in the loweſt harmonics, ſcarcely audible: the chord aſſumes the appearance, at the aliquot points, of as many lucid lines as correſpond to the number of the harmonic, more nearly approaching to each other as the bow approaches more nearly to the point, Plate VI. Fig. 51. According to the various modes of applying the bow, an immenſe variety of figures of the orbits are produced, Fig. 45, more than enough to account for all the difference of tone in different performers. In obſervations of this kind, a ſeries of harmonics is frequently heard in drawing the bow acroſs the ſame part of the chord: theſe are produced by the bow; they are however not proportionate to the whole length of the bow, but depend on the capability of the portion of the bowſtring, intercepted between its end and the chord, of performing its vibrations in times which are aliquot parts of the vibration of the chord: hence it would ſeem, that the bow takes effect on the chord but at one inſtant during each fundamental vibration. In theſe experiments, the

the bow was ſtrung with the ſecond ſtring of a violin: and, in the preparatory application of reſin, the longitudinal ſound of Chladni was ſometimes heard; but it was obſerved to differ at leaſt a note in different parts of the ſtring.

XIV. *Of the Vibrations of Rods and Plates.*

Some experiments were made, with the aſſiſtance of a moſt excellent practical muſician, on the various notes produced by a glaſs tube, an iron rod, and a wooden ruler; and, in a caſe where the tube was as much at liberty as poſſible, all the harmonics correſponding to the numbers from 1 to 13, were diſtinctly obſerved; ſeveral of them at the ſame time, and others by means of different blows. This reſult ſeems to differ from the calculations of Euler and Count Riccati, confirmed as they are by the repeated experiments of Profeſſor Chladni; it is not therefore brought forward as ſufficiently controverting thoſe calculations, but as ſhowing the neceſſity of a reviſion of the experiments. Scarcely any note could ever be heard when a rod was looſely held at its extremity; nor when it was held in the middle, and ſtruck one-ſeventh of the length from one end. The very ingenious method of Profeſſor Chladni, of obſerving the vibrations of plates by ſtrewing fine ſand over them, and diſcovering the quieſcent lines by the figures into which it is thrown, has hitherro been little known in this country: his treatiſe on the phænomena is ſo complete, that no other experiments of the kind were thought neceſſary. Glaſs veſſels of various deſcriptions, whether made to ſound by percuſſion or friction, were found to be almoſt intirely free from harmonic notes; and this obſervation coincides with the experiments of Chladni.

XV. *Of the Human Voice.*

The human voice, which was the object originally propoſed to be illuſtrated by theſe reſearches, is of ſo complicated a nature, and ſo imperfectly underſtood, that it can be on this occaſion but ſuperficially conſidered. No perſon, unleſs we except M. Ferrein, has publiſhed any thing very important on the ſubject of the formation of the voice, before or ſince Dodart; his reaſoning has fully ſhown the analogy between the voice and the *voix humaine* and regal organ-pipes: but his compariſon with the whiſtle is unfortunate; nor is he more happy in his account of the falſetto. A kind of experimental analyſis of the voice may be thus exhibited. By drawing in the breath, and at the ſame time properly contracting the larynx, a ſlow vibration of the ligaments of the glottis may be produced, making a diſtinct clicking ſound: upon increaſing the tenſion, and the velocity of the breath, this clicking is loſt, and the ſound becomes continuous, but of an extremely grave pitch: it may, by a good ear, be diſtinguiſhed two octaves below the loweſt A of a common baſs voice, conſiſting in that caſe of about 26 vibrations in a ſecond. The ſame ſound may be raiſed nearly to the pitch of the common voice; but it is never ſmooth and clear, except perhaps in ſome of thoſe perſons called ventriloquiſts. When the pitch is raiſed ſtill higher, the upper orifice of the larynx, formed by the ſummits of the arytænoid cartilages

cartilages and the epiglottis, ſeems to ſucceed to the office of the ligaments of the glottis, and to produce a retrogade falſetto, which is capable of a very great degree of acuteneſs. The ſame difference probably takes place between the natural voice and the common falſetto: the rimula glottidis being too long to admit of a ſufficient degree of tenſion for very acute ſounds, the upper orifice of the larynx ſupplies its place; hence, taking a note within the compaſs of either voice, it may be held, with the ſame expanſe of air, two or three times as long in a falſetto as in a natural voice; hence, too, the difficulty of paſſing ſmoothly from the one voice to the other. It has been remarked, that the larynx is always elevated when the ſound is acute: but this elevation is only neceſſary in rapid tranſitions, as in a ſhake; and then probably becauſe, by the contraction of the capacity of the trachea, an increaſe of the preſſure of the breath can be more rapidly effected this way, than by the action of the abdominal muſcles alone. The reflection of the ſound thus produced from the various parts of the cavity of the mouth and noſtrils, mixing at various intervals with the portions of the vibrations directly proceeding from the larynx, muſt, according to the temporary form of the parts, variouſly affect the laws of the motion of the air in each vibration, or according to Euler's expreſſion, the equation of the curve conceived to correſpond with this motion, and thus produce the various characters of the vowels and ſemi-vowels. The principal ſounding board ſeems to be the bony palate: the noſe, except in naſal letters, affords but little reſonance; for the naſal paſſage may be cloſed, by applying the finger to the ſoft palate, without much altering the ſound of vowels not naſal. A good ear may diſtinctly obſerve, eſpecially in a loud baſs voice, beſides the fundamental note, at leaſt four harmonic ſounds, in the order of the natural numbers; and, the more reedy the tone of the voice, the more eaſily they are heard. Faint as they are, their origin is by no means eaſy to be explained. This obſervation is preciſely confirmed, in a late diſſertation of M. Knecht, publiſhed in the muſical newſpaper of Leipſic. Perhaps, by a cloſe attention to the harmonics entering into the conſtitutions of various ſounds, more may be done in their analyſis than could otherwiſe be expected.

XVI. *Of the Temperament of Muſical Intervals.*

It would have been extremely convenient for practical muſicians, and would have ſaved many warm controverſies among theoretical ones, if three times the ratio of 4 to 5, or four times that of 5 to 6, had been equal to the ratio of 1 to 2. As it happens to be otherwiſe, it has been much diſputed in what intervals the imperfection ſhould be placed. The Ariſtoxenians and Pythagoreans were in ſome ſenſe the beginners of the controverſy. Sauveur has given very comprehenſive tables of a great number of ſyſtems of temperament; and his own now ranks among the many that are rejected. Dr. Smith has written a large and obſcure volume, which, for every purpoſe but for the uſe of an impracticable inſtrument, leaves the whole ſubject preciſely where it found it. Kirnberger, Marpurg, and other German writers, have diſputed with great bitterneſs, almoſt every one for a particular method of tuning. It is not with any confidence of ſucceſs, that one more attempt is

made,

made, which reſts its chief claim to preference, on the ſimilarity of its theory to the actual practice of the beſt inſtrument-makers. However we eſtimate the degree of imperfection of two tempered concords of the ſame nature, it will appear, that the manner of dividing the temperament between them does not materially alter its aggregate ſum; for inſtance, the imperfection of a comma in a major-third, occaſions it to beat very nearly twice as faſt as that of half a comma. If indeed the imperfection were great it might affect an interval ſo materially as to deſtroy its character; as, in ſome methods of temperament, a minor third diminiſhed by two commas approaches more nearly to the ratio 6 : 7, than to 5 : 6; but, with this limitation, the ſum of harmony is nearly equal in all ſyſtems. Hence, if every one of the twelve major and minor thirds occurred equally often in the compoſitions which are to be performed on an inſtrument, it would be of no great conſequence, to the ſum of the imperfections, among which of the thirds they were divided: and, even in this caſe, the opinion of the beſt practical authors is, that the difference of character produced by a difference of proportions in various keys, would be of conſiderable advantage in the general effect of modulation. But, when it is conſidered, that upon an average of all the muſic ever compoſed, ſome particular keys occur at leaſt twice as often as others, there ſeems to be a very ſtrong additional reaſon for making the harmony the moſt perfect in thoſe keys which are the moſt frequently uſed; ſince the aggregate ſum of all the imperfections which occur in playing, muſt by this means be diminiſhed in the greateſt poſſible degree, and the diverſity of character at the ſame time preſerved. Indeed, in practice, this method, under different modifications, has been almoſt univerſal; for, although many have pretended to an equal temperament, yet the methods which they have employed to attain it have been evidently defective. It appears to me, that every purpoſe may be anſwered, by making C : E too ſharp by a quarter of a comma, which will not offend the niceſt ear; E : G*, and A^b : C, equal; F* : A* too ſharp by a comma; and the major thirds of all the intermediate keys more or leſs perfect, as they approach more or leſs to C in the order of modulation. The fifths are perfect enough in every ſyſtem. The reſults of this method are ſhown in Table XII. In practice, nearly the ſame effect may be very ſimply produced, by tuning from C to F, B^b, E^b, G*, C*, F* ſix perfect fourths; and C, G, D, A, E, B, F*, ſix equally imperfect fifths, Plate VI. Fig. 52. If the unavoidable imperfections of the fourths be ſuch as to incline them to ſharpneſs, the temperament will approach more nearly to equality, which is preferable to an inaccuracy on the other ſide. An eaſy method of comparing different ſyſtems of temperament is exhibited in Plate VI. Fig. 53, which may eaſily be extended to all the ſyſtems that have ever been invented.

Table

Table XII.

A		B		C	
C	50000	1 C	+ .0013487	1 A, E	— .0023603
B	53224	2 G, F	.0019006	2 D, B	.0029122
B^b	56131	3 D, B^b	.0024525	3 G, F*	.0034641
A	59676	4 A, E^b	.0034641	4 C, C*	.0044756
G*	63148	5 E, A^b	.0044756	5 F, G*	.0049353
G	66822	6 B, C*	.0049353	6 B^b, E^b	.0053950
F*	71041	7 F*	.0053950		
F	74921				
E	79752	D			
E^b	83810				
D	89304	1 E^b, G*, C*, F*			— .0000000
C*	94723	2 F, B^b, E, B			.0004597
C	100000	3 C, G, D, A			.0011562

A, ſhows the diviſion of a monochord correſponding to each note, in the ſyſtem propoſed. B, the logarithm of the temperament of each of the major thirds. C, of the minor thirds. D, of the fifths; C and D being both negative.

Thus, Sir, I have endeavoured to advance a few ſteps only, in the inveſtgation of ſome very obſcure but intereſting ſubjects. As far as I know, moſt of theſe obſervations are new; but, if they ſhould be found to have been already made by any other perſon, their repetition in a connected chain of inference may ſtill be excuſable. I am perſuaded alſo, that at leaſt ſome of the poſitions maintained are incontrovertibly conſiſtent with truth and nature; but, ſhould further experiments tend to conſute any opinions that I have ſuggeſted, I ſhall relinquiſh them with as much readineſs as I have long ſince abandoned the hypotheſis which I once took the liberty of ſubmitting to the Royal Society, on the functions of the cryſtalline lens.

I am, &c.

Emanuel College, Cambridge,
8th July, 1799.

THOMAS YOUNG.

EXPLANATION OF THE FIGURES.

(See Plates IV. V. and VI.)

Plate IV.

Figs. 1—6. The ſection of a ſteam of air from from a tube .07 inch in diameter, as aſcertained by meaſuring the breadth of the impreſſion on the ſurface of a liquid. The preſſure impelling the current, was in Fig. 1, 1 inch. Fig. 2, 2. Fig. 3, 3. Fig. 4, 4. Fig. 5, 7. Fig. 6, 10.

Figs. 7—12. A ſimilar ſection, where the tube was .1 in diameter, compared with the ſection as inferred from the experiments with two gages, which is repreſented by a dotted line. From this compariſon it appears, that where the velocity of the current was ſmall, its central parts only diſplaced the liquid; and that, where it was great, it diſplaced, on meeting with reſiſtance, a ſurface ſomewhat greater than its own ſection. The preſſure was in Fig. 7, 1. Fig. 8, 2. Fig. 9, 3. Fig. 10, 4. Fig. 11, 7. Fig. 12, 10.

Figs. 13—20. A, the half ſection of a ſtream of air from a tube .1 in diameter, as inferred from experiments with two water gages. The preſſure was in Fig. 13, .1. Fig. 14, .2. Fig. 15, .5. Fig. 16, 1. Fig. 17, 3. Fig. 18, 5. Fig. 19, 7. Fig. 20, 10. The fine lines, marked B, ſhow the reſult of the obſervations with an aperture .15 in diameter oppoſed to the ſtream; C with .3; and D with .5.

Figs. 21—23. A, the half ſection of a current from a tube .3 in diameter, with a preſſure of .5, of 1, and of 3. B ſhows the courſe of a portion next the axis of the current, equal in diameter to thoſe repreſented by the laſt figures.

Fig. 24. The appearance of a ſtream of ſmoke, forced very gently from a fine tube. Fig. 25 and 26, the ſame appearance when the preſſure is gradually increaſed.

Fig. 27. See Section III.

Fig. 28. The perpendicular lines over each diviſion of the horizontal line ſhow, by their length and diſtance from that line, the extent of preſſure capable of producing, from the reſpective pipes, the harmonic notes indicated by the figures oppoſite the beginning of each, according to the ſcale of 22 inches parallel to them. The larger numbers, oppoſite the middle of each of theſe lines, ſhow the number of vibrations of the correſponding ſound in a ſecond.

Figs. 29—33. See Section X.*

Fig. 34. The combination of two equal ſounds conſtituting the interval of an octave, ſuppoſing the progreſs and regreſs of the particles of air equable. Figs. 35, 36, 37, a ſimilar repreſentation of a major third, major tone, and minor ſixth.

Fig. 38. A fourth, tempered about two commas.

Fig. 39. A vibration of a ſimilar nature, combined with ſubordinate vibrations of the ſame kind in the ratios of 3, 5, and 7.

Fig. 40. A vibration repreſented by a curve of which the ordinates are the ſines of circular arcs increaſing uniformly, correſponding with the motion of a cycloidal pendulum, combined with ſimilar ſubordinate vibrations in the ratios of 3, 5, and 7.

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* By ſome overſight the figures 29 to 40 incluſive are not inſerted in the Engravings. They ſhall appear in our next.---N.

Plate V.

Figs. 41 and 42. Two different poſitions of a major third, compoſed of ſimilar vibrations, as repreſented by figures of ſines.

Fig. 43. A contracted repreſentation of a ſeries of vibrations. A, a ſimple uniform ſound. B, the beating of two equal ſounds nearly in uniſon, as derived from rectilinear figures. C, the beats of two equal ſounds, derived from figures of ſines. D, a muſical conſonance, making by its frequent beats a fundamental harmonic. E, the imperfect beats of two unequal ſounds.

Fig. 44. Various forms of the orbit of a muſical chord, when inflected, and when ſtruck.

Fig. 45. Forms of the orbit, when the ſound is produced by means of a bow.

Fig. 46. Epitrochoidal curves, formed by combining a ſimple rotation or vibration with other ſubordinate rotations or vibrations.

Figs. 47 and 48. (in Plate VI.) The ſucceſſive forms of a tended chord, when inflected and let go, according to the conſtruction of De la Grange and Euler.

Figs. 49. The appearance of a vibrating chord which had been inflected in the middle, the ſtrongeſt lines repreſenting the moſt luminous parts.

Fig. 50. The appearance of a vibrating chord, when inflected at any other point than the middle.

Plate VI.

Fig. 51. The appearance of a chord, when put in motion by a bow applied nearly at one third of the length from its end.

Fig. 52. The method of tuning recommended for common uſe.

Fig. 53. A comparative view of different ſyſtems of temperament. The whole circumference repreſents an octave. The inner circle L is divided into 30103 parts, correſponding with the logarithmical parts of an octave. The next circle R ſhows the magnitude of the ſimpleſt muſical and other ratios. Q is divided into twelve equal parts, repreſenting the ſemitones of the equal temperament deſcribed by Zarlino, differing but little from the ſyſtem of Ariſtoxenus, and warmly recommended by Marpurg and other late writers. Y exhibits the ſyſtem propoſed in this paper as the moſt deſirable; and P the practical method nearly approaching to it, which correſponds with the eleventh method in Marpurg's enumeration, except that, by beginning with C inſtead of B, the practical effect of the temperament is preciſely inverted. K is the ſyſtem of Kirnberger and Sulzer; which is derived from one perfect third, ten perfect and two equally imperfect fifths. M is the ſyſtem of mean tones, the *ſiſtema participato* of the old Italian writers, ſtill frequently uſed in tuning organs, approved alſo by Dr. Smith for common uſe. S ſhows the reſult of all the calculations in Dr. Smith's harmonics, the ſyſtem propoſed for his changeable harpſichord, but neither in that nor any other form capable of practical application.

II.

On the Means of foretelling the Character of the Summer Seaſon, and the Benefits to be expected from the Cultivation of Graſſes which vegetate at low Temperatures. In a Letter from Dr. THO. BEDDOES.

To MR. NICHOLSON.

SIR,

MANY years ago it occurred to me, that the general nature of our ſummers might be anticipated in the ſpring, and ſometimes in winter. I have frequently hazarded a prediction, and I begin to acquire ſome confidence in my rule.

It is founded on the effect of melting ſubſtances, particularly ſnow, upon temperature. In applying this fact to meteorology, I ſay, *if there be an unuſual fall of ſnow in the countries to the N. and N. E. our ſummer,* cæteris paribus, *will be cold and wet.* In the winter of 1798 and 1799, I obſerved in the public papers frequent mention of immenſe falls of ſnow in theſe hyperborean regions; and you know what our ſummer was. From the beſt information I could procure, I collected the contrary during the two laſt winters; and I propheſied accordingly.

I do not take the word *ſummer* in the ſtrict aſtronomical ſenſe, otherwiſe it would be too ſoon to talk of that of 1801; and ſtill the approaching ſeaſon may ſeem to put my ſyſtem to ſhame. But obſerve, Sir, that I do not pretend to be acquainted with all the different cauſes of atmoſpherical refrigeration; and I only point out one, the moſt deciſive perhaps that exiſts, but ſtill capable of counteraction, and for which there may be a ſubſtitute when it is not preſent.

Some of your readers may perhaps think of trying what I advance by a reference to meteorological tables. But you will remark, that they do not ſupply the requiſite data. This is at leaſt the caſe with thoſe to which it is in my power to have acceſs; and I wiſh any hint from me may occaſion a fact ſo eſſential to us, to be annually aſcertained as far as it can be done with accuracy. The ſnow that may fall in England is of ſmall account; though I ſuppoſe an unuſual fall here may often accompany an unuſual fall in the countries from Jutland to Tobolſk.

I ſhall now trouble you with another winter ſpeculation, that may lead to experiments in future beneficial. In mild winters there can, I ſuppoſe, be no doubt of the vegetation of the graſſes. It has often appeared to me, that it goes on unequally in different graſſes; and I believe I could name ſome which grow at a temperature not greatly exceeding 40°. However, I ſhall content myſelf with propoſing it to perſons of more leiſure, to aſcertain by actual weight and meaſure, *what is the conſtitution of the different individuals of this family with*

with regard to temperature. If certain graſſes of good quality ſhould grow at a temperature lower than that at which others will grow, it would evidently be deſireable to have winter paſtures and water-meadows of ſuch graſſes.

It would likewiſe be deſireable to extend theſe experiments to our higheſt ſummer heat, with due attention to moiſture and light.

I am, SIR,
Your's reſpectfully,
THOMAS BEDDOES.

Clifton, June 9, 1801.

III.

Analyſis of Manachanite from Botany Bay. By RICHARD CHEVENIX, *Eſq. F. R. S. and M. R. I. A. Communicated by the Author.*

ABOUT three months ago, I received from the Right Honourable Preſident of the Royal Society, a quantity of ſand, which had been found on the ſea coaſt in Providence Iſland, and thence brought over to England in the courſe of the winter. It may not be very eaſy to determine the mineralogical character of any ſubſtance in the ſtate of fine powder; but, from its general appearance, the object of our preſent inquiry was judged to be Manachanite. It was in fine grains, almoſt as ſmall as ſea ſand; it was mixed with many white, and ſome red globules of the ſame ſize with the black; and, in this alone, differed from the manachanite of Cornwall. The white globules ſeemed to be quartz; and the red, as far as could be judged, appeared to be garnet, more than ruby. The ſpecific gravity of ſo fine a ſand, I did not think a character more certain than any other; therefore did not give myſelf much trouble to take it with accuracy. From the rough experiment I made, it did not appear to be very different from that of the known manachanites. It was ſlightly magnetic alſo; but not ſatisfied with any external marks, I proceeded to try what information I could receive from chymical analyſis.

100 parts of the above ſand from Providence Iſland, were expoſed to a red heat for half an hour, and weighed upon cooling. No ſenſible augmentation or diminution of weight had taken place.

Sulphuric, nitric, muriatic, and nitro-muriatic acids, were heated for twelve hours upon a given quantity; and, upon trial with various re-agents, exhibited no marks of having operated an effectual ſolution.

1. I then took one hundred grains, and treated them with pot-aſh, in a platina crucible, in the uſual manner of treating hard ſtones. In about two minutes after the pot-aſh was in fuſion,

fuſion, the maſs became browniſh, and quickly changed to a rich emerald green. After keeping it red hot for half an hour, the crucible was withdrawn from the fire, and the maſs diluted in water. The liquor, not without ſediment, was of a very beautiful green, as is uſual in foſſils, where iron is abundant, and but little oxidated.

2. Muriatic acid was then poured in, and a total ſolution enſued. This liquor was ſet on a ſand heat to evaporate at a moderately clevated temperature. After ſome time, a white matter, much like curdled milk, began to appear, and afterward increaſed to a conſiderable volume. This ſubſtance, ſeparated by filtration, was well waſhed, and the liquor again expoſed to the ſame temperature was evaporated to dryneſs.

3. Upon pouring in a large quantity of diſtilled water to re-diſſolve the maſs, a precipitate, which was ſilica, remained behind; but the liquor paſſed very turbid through the filtre, although repeated attempts were made to render it limpid by that method.

4. A ſolution of pot-aſh was poured in exceſs into this turbid liquor, a red and conſiderable precipitate fell to the bottom. The whole was made to boil; but no diminution took place in the apparent volume of the precipitate. The liquor was filtered, and eſſayed for alumina; but the pot-aſh did not appear to have diſſolved any thing.

5. The red precipitate was again diſſolved in muriatic acid, and eſſayed by the known re-agents, for the different earths and metals. From theſe trials it appeared to be oxide of iron, mixed with a ſmall portion of the ſame ſubſtance as that which had coagulated in the firſt attempt to evaporate the intire ſolution, as above ſtated.

6. I now proceeded to examine the nature of the white ſubſtance (Exp. 2d.) which had ſeparated itſelf by ſpontaneous precipitation. I attempted to re-diſſolve a portion of it in muriatic acid, but could not ſucceed. Nitric acid was much more effectual; but ſulphuric acid, after being gently heated with it, operated a perfect ſolution. Some trials, made with the different re-agents upon the latter ſolution, afforded the following reſults:

With pot-aſh, ſoda, and ammonia, a white precipitate; with carbonates of pot-aſh, of ſoda, and of ammonia, ditto; with a muriate or a nitrate, no precipitate; with an oxalate, a white precipitate; with a tartrite, a white precipitate; with a phoſphate, a copious white precipitate; with an arſeniate, a ditto ditto; with ſulphurated hydrogen water, no precipitate; with an alkaline hydro-ſulphuret, a dirty glaſs green precipitate; with a pruſſiate, a dark blueiſh green, which ſhewed that there was ſtill ſome iron; with tincture of galls, a blood red, and very thick precipitate; with phoſphoric acid, a white precipitate; with arſenic acid, a white precipitate.

Theſe experiments, and particularly the laſt three, were ſufficiently characteriſtic, and proved that this ſubſtance could be no other than oxide of titanium. However, it was ſtill contaminated with iron; and a part of it appeared to have remained with that red precipitate, which was already mentioned. To effect a complete ſeparation was ſtill a *deſideratum*. For this purpoſe, I repeated the experiments upon different portions of the ſand. After fuſion with pot-aſh, and ſolution in muriatic acid, I attempted to precipitate the whole, and then to diſſolve in an exceſs of carbonate of ammonia, the oxide of iron, become red by the various treatments. But although red oxide of iron is eaſily ſoluble in this ſalt, it is not ſufficiently ſo to anſwer the purpoſe, when oxide of titanium is preſent. Such inſtances of the inefficacy of methods, as analytic means, although the primitive facts upon which they are founded are rigidly true, are frequent in chymiſtry; and indeed it may almoſt be ſaid, that no two ſubſtances come in contact, without producing a reciprocal change in their properties. Hence we muſt be contented to make ſlow progreſs in the art of analyſis; and perhaps there may be an inherent principle in the chymical nature of all bodies, that will long impede our way to abſolute preciſion. I was therefore obliged to renounce this method.

I had perceived that phoſphoric and arſenic acids formed a precipitate in ſulphate of titanium, while neither of theſe acids produced any alteration in ſulphate of iron. I therefore precipitated, by phoſphoric acid, an intire ſolution of the ſand in ſulphuric acid, and by that means operated as good a ſeparation of the oxides of iron, and of titanium, as is generally obtained in chymical proceſſes. But we are ignorant of the quantity of titanium contained in ſalts of this metal, or in its oxide; for as yet it has never been obtained in ſufficient abundance, reduced to the metallic ſtate; and we have not been able to determine the proportion of oxigen that it may take up in its different ſtages of oxidation. But I had found about eleven of ſilica, and forty-nine of black oxide of iron. The remainder muſt therefore be oxide of titanium, and the proportions in the quintal will ſtand thus:

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

proportions which differ but little from the Corniſh manachanite. Some allowance, however, ſhould be made for the inevitable loſs.

The degree of fire neceſſary to operate a perfect reduction of titanium, exceeds what we have been able to produce. At leaſt neither Mr. Klaproth nor Mr. Vauquelin appears to have ſucceeded in obtaining any large quantity.

I have never been ſo fortunate as to obtain this metal reduced and pure, except in ſmall ſpicula, diſſeminated through the flux I had uſed; therefore not in a ſtate fit to be ſubjected to experiment; but I have obtained it, combined with another ſubſtance, and without the aid of any alloy. I mixed with charcoal ſome phoſphate of titanium, and, having added a flux of borax, put it into a double Heſſian crucible well luted, and expoſed the whole to a violent heat in a forge, into which the wind was impelled through four holes, and by two pair of bellows heavily loaded. After giving at firſt a gentle heat, increaſing it by degrees for three quarters of an hour, and then ſuddenly raiſing it as high as I could raiſe it by the above apparatus, and maintaining at the ſame degree for half an hour, the crucible was withdrawn and broken. The upper crucible, which ſerved as cover to the whole, was lined with a ſhining metallic coat, and a metallic button was found in the bottom.

bottom. This button was phoſphuret of titanium; it was of a pale white, not devoid of luſtre. It was eaſily broken, and its fracture was grainy and white. It did not melt before the blow pipe, and ſeemed not very fuſible. Muriatic and ſulphuric acids did not attack it, nor did nitric acid very eaſily when cool; nitro-muriatic acid had much more effect upon it, and the liquor gave traces of titanium, and of phoſphoric acid.

Phoſphoric and arſenic acids were obſerved by Mr. Klaproth, when he diſcovered this metal, to precipitate oxide of titanium from the other acids, by direct affinity. However, an exceſs of muriatic acid can diſſolve phoſphate and arſeniate of titanium; but ſulphuric, or any other acid I have tried, has not that property.

In order to obtain oxide of titanium totally free from every other metal, with which it is accompanied in nature, ſuch as iron or manganeſe, either of the above acids may be uſed, and that precipitate treated with fixed alkalis, or their carbonates.

Mr. Vauquelin has given a very ingenious explanation of the reaſon, for which nitric and muriatic acids when heated, cannot retain the oxide of titanium in ſolution. With the former, it is becauſe the oxide takes a new portion of oxigen from the nitric acid, and becomes too much oxidated to remain in ſolution. With the latter the reverſe is the caſe, and oxigenated muriatic acid is diſengaged. However, in evaporating a large portion of the ſolution of this ſand, which had been effected by the means of pot-aſh, and muriatic acid, as above ſtated, I obtained at the end a ſalt well cryſtallized, which, upon ſtrict examination, exhibited all the properties of oxigenated muriate of pot-aſh. Hence it is evident that the oxigen had paſſed, probably in its naſcent ſtate, into the muriate of pot-aſh already formed; for the firſt portion of muriatic acid, and long before a total ſolution of the ſand could have taken place, muſt, by the laws of affinity, have gone to ſaturate the pot-aſh employed. I have attempted to produce a ſimilar combination by the means of manganeſe, but without ſucceſs. The circumſtances are not indeed rigidly ſimilar; for in one caſe we effect a ſolution, and in the other, a precipitation of the metal that gives out the oxigen, and that may produce ſome modification.

If the mineralogical riches of the country, from which this ſand was brought, are at all to be prejudged, from the valuable productions with which we have become acquainted in the vegetable and animal kingdoms, we may look forward to many intereſting diſcoveries; and already, a metal is found to be a native of thoſe climates, which has not long been known, and has been but rarely met in Europe.

IV.

A Series of Experiments upon Metals with an Electrical Battery, ſhewing their Property of abſorbing the Oxigen from the Atmoſphere when exploded by Electric Diſcharges. By JOHN CUTHBERTSON, *Philoſophical Inſtrument Maker, No.* 54, *Poland Street, London. Communicated by the Author.*

THE diſcovery that metals could be fuſed by electric diſcharges, gave riſe to a ſuppoſition that they might alſo be converted into oxides of the ſame means. Many attempts have been made to aſcertain this, but the fact has hitherto remained without proof. In the year 1787, Dr. Van Marum and myſelf produced floculi from different metals by ſubjecting them to ſtrong electric diſcharges, and alſo formed beautiful figures upon paper by exploding metallic wires extended at a ſmall diſtance from its ſurface. We imagined that the floculi, as well as the coloured figures upon the paper, were oxides of the metals we made uſe of; and in order to prove this, we entered upon a courſe of experiments. But having, perhaps from improper management, the misfortune to break ſeveral glaſſes in the proceſs, Dr. Van Marum declared himſelf ſo much diſcouraged by theſe accidents, as to decline proſecuting the ſubject.* When we conſider, however, the opulence of the ſociety of which he was director, it is not eaſy to conceive that ſo trifling an accident as the fracture of a few glaſſes ſhould be a ſufficient reaſon for his relinquiſhing this inveſtigation, more eſpecially as he had the command of an electrical apparatus which I made for that ſociety, not only the moſt proper for that purpoſe, but unequalled in the whole world, and from which I now fear we have little to expect.

In 1793—1794, I undertook two different courſes of experiments at Amſterdam upon the ſame ſubject; but both ended unſatisfactorily, and afforded very little more information than we before poſſeſſed. †

On my return to London, after delivering a courſe of lectures upon electricity, I once more returned to the above-mentioned ſubject. My previous experiments had taught me that it was neceſſary to have an apparatus into which atmoſpherical air or gas could be introduced, and any deſirable quantity of metallic wire, ſo that it would be exploded at pleaſure, and the diminution of the air or gas accurately meaſured. I invented the following apparatus, which poſſeſſed all the requiſites.

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* Second Continuation des Experiences faites par la Moyen de la Machine Electrique Teglerienne par Martinus van Marum à Haarlem, page 272.

† Eigenſchappen der Electricitiet 3de. deel blad. 2, 143. Amſterdam, 1794, door John Cuthbertſon.

Deſcription

Deſcription of an Apparatus in which Metallic Wires are converted into Oxides by Electric Diſcharges.

Fig. 11, Plate VII. *a b*, *c d* is a glaſs receiver, about ten inches high, and ſix inches diameter. To the wideſt end is cemented a large round braſs plate, *a b*, perfectly air tight, the other end is mounted with a braſs collar, upon which a braſs cap, *p q*, is ſcrewed, alſo perfectly air tight. *f* is an air cock, one end of which is ſcrewed into the middle of *p q*, and the other into a foot on which the inſtrument ſtands; *n o* are two air cocks ſcrewed faſt to the broad braſs plate, with holes communicating with the inſide of the receiver. Upon each of theſe cocks is ſcrewed a glaſs tube bent, as is repreſented; their communication with the inſide of the receiver is opened or ſhut by means of *n* and *o*. In the inſide of *p q* is a wheel and axis moving upon two pivots, ſerving to wind up a quantity of wire in readineſs for exploding. *m* is a braſs tube about three inches long, and three quarters of an inch in diameter, ſtopped at each end with cork, and the middle filled with hogs'-lard; this ſerves to draw the wire through ready for exploding.

The wire which is intended for uſe, muſt be bound to a packthread of the ſame length, at different places, and at about four inches aſunder; it muſt be wound upon the axis of the wheel in *p q*; the tube *m* muſt be ſcrewed off from the plate, and the end of the wire muſt be directed through the hole wherein *m* was, by means of a long braſs needle, which muſt be puſhed through the cork and hogs'-lard, in *m*, with the packthread and wire in the eye of the needle; when it is drawn through, the packthread and wire will follow, and be kept air tight by the hogs'-lard and cork; *m* muſt be ſcrewed to the plate as before, and the packthread drawn through *m* till it is ſeen nearly ſtraight in the inſide of the receiver, as at *h*. In this ſtate, pour into the lower bend of *k* about half of an inch of mercury, and in the lower bend of *i* a little water, repreſented by the dots in each. It is evident, from the conſtruction of the inſtrument, that if all the ſcrews be perfectly air tight, the cock *f* ſhut, and the cocks *n o* open, the leaſt alteration that happens to the air in the inſide of the receiver, with reſpect either to encreaſe, decreaſe, denſity, or rarefaction, will be ſhewn either by the mercury or water, in either one or both gages; if conſiderable, the mercury will ſhew it, and if too little to affect the mercury, the water gage will ſhew it: if by any proceſs the temperature of the air within the receiver ſhould be raiſed, it will cauſe the air to expand, and the mercury and water in the gages to move towards *u u*, but if it ſhould be lowered, the mercury and water will move towards *i k*; ſo likewiſe if any alteration, with reſpect to temperature, happens in the ſurrounding air, the gages will ſhew it accordingly; and as this is continually changing, it is neceſſary that the apparatus, after is prepared for the experiment, and ready for exploding of the wire, ſhould be ſet in ſome medium which is not ſo changeable. For this purpoſe water ſeems to be the moſt convenient; but as it is a conductor of electricity, the apparatus muſt not remain in when the diſcharge is given through it; ſo that before, and after the diſcharge, it muſt only be be ſet into the water, care being taken to let it remain a ſufficient length of time that the air in the inſide may become of the ſame temperature, which will be ſhewn by the gages being ſtationary. If this circumſtance be not obſerved with great attention, errors in the concluſion will be the conſequence.

EXPERIMENT I.

A ſufficient quantity of leaden wire, of $\frac{1}{90}$th part of an inch diameter, was bound to a cord at different places about four inches diſtant, as has been already mentioned, and wound upon the axis of the wheel in *p q*, the end of it, by means of the long braſs needle, was drawn out at the top of the receiver through the tube *m*, which was ſcrewed in its place, and the cord drawn tight, as repreſented at *h*. The cock *f* was then cloſed, and the whole inſtrument ſet in the water ſo deep, that the braſs plate was about one inch under the ſurface of the water. The cocks *n o* were opened to admit the external air into the receiver, as the air within became condenſed by the coldneſs of the water. When it had remained nine minutes under the water, the gages were ſcrewed on to the cocks perfectly air tight, and both appearing ſtationary, the cocks were ſhut. The inſtrument was then taken out, wiped dry, and ſet as near the electrometer, Fig. 2, as poſſible, not to influence the motion of the ball A, namely, at the diſtance of about one foot. One end of a wire, *a b*, was hooked to the cock *n*, and the other to the hook of the electrometer.* One end of another braſs wire, *s t*, was hooked to the cock *f*, and the other end made to touch the outſide of the battery, Fig. 1, which is compoſed of fifteen jars, containing about ſeventeen ſquare feet † coating, and connected with the electrometer by means of a wire L. The battery was charged till the ball A of the electrometer, which was loaded with twenty-ſix grains, deſcended and directed the diſcharge through the wire *h*, which was converted into a blueiſh white impalpable powder, at firſt appearing like a white cloud, and filling the whole receiver (the cord was left entire); the inſtrument was then releaſed from the electrometer, and ſet in the water as before. After it had remained there five minutes, the cocks under the gages were opened. No change could be perceived in the mercury gage, but a motion in the water gage was juſt perceptible. Hence I concluded that the powder thus produced had abſorbed no air.

EXPERIMENT II.

The apparatus was taken out of the water, and another length of wire drawn forward, and placed at the electrometer and battery as before; the battery was charged till the electrometer cauſed the diſcharge, which again produced the powder. The apparatus was not put into the water as before, but was left to remain till another length of wire was drawn forward, and the diſcharge directed through it. And this proceſs was repeated in the

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* For a deſcription of this electrometer, ſee this Journal, II. 528.

† For an account of this battery, ſee Journal, II. 526.

whole

whole by five diſcharges through five lengths of wire, each length was ſeven inches. In this manner, therefore, thirty-five inches of wire was converted into a blueiſh white impalpable powder. The apparatus was then placed in the water as before, and after it had remained the ſame length of time, the mercury gage was opened. No motion in the mercury was perceived, and when the water gage was alſo opened, no alteration was there ſeen. Hence I concluded that no abſorption of air had taken place, though five times the original length of wire had been reduced to powder. The air in the apparatus was then eudiometrically examined, and it appeared to be of the ſame purity as the ſurrounding atmoſphere.

EXPERIMENT III.

The apparatus was taken out of the water, and the powder which was produced in the receiver carefully taken out; the apparatus was then furniſhed with the ſame length of wire as before, all the ſcrews and cocks were well examined, and the apparatus ſet in the water as before. After it had ſtood its time, the gages were ſcrewed tight, it was taken out of the water and ſet to the battery when the laſt experiment was repeated, and it was afterwards ſet in the water, and the gages opened. No abſorption having taken place, it was left in the water till the ſame hour on the following day, when the mercury gage was opened, and indicated that an abſorption of about one cubic inch had taken place. The water was of the ſame temperature as at firſt. This experiment conſequently ſhews that the metal was firſt divided, and afterwards oxigenated by a ſlow proceſs.

EXPERIMENT IV.

The laſt experiment was repeated with double the length of wire, and the reſult was the ſame.

EXPERIMENT V.

A ſmaller receiver was now taken, being only four inches diameter, which was half the diameter of the former. The ſame kind of powder was produced, and a diminution of air happened when thirty-five inches had been reduced to powder, which cauſed the water gage to riſe one inch, and the mercury $\frac{1}{20}$ inch.

EXPERIMENT VI.

Wires of different diameters were now prepared in the laſt receiver, of $\frac{1}{100}$ and $\frac{1}{140}$ inch diameter, and of each thirty-five inches; and the reſult was the ſame with reſpect to air, but the powder was whiter.

By the reſult of the three laſt experiments, it appeared that the abſorption of air by the exploded metal apparently depended upon the diameter of the receivers wherein the proceſs was carried on, for which reaſon I determined to repeat the experiments in receivers of much leſs diameter.

EXPERIMENT VII.

Inſtead of the laſt-mentioned receiver, Fig. 4, was taken a glaſs tube, of about one inch diameter, and nine inches long,[*] and furniſhed with lead wire of $\frac{1}{85}$ inch diameter, being the ſame as that uſed in the three firſt experiments. After it had been placed in water, with the cock only above the ſurface and open; and it had remained three minutes, the cock was ſhut, the inſtrument taken out and wiped dry, one end of it was annexed to the chain *h*, of the electrometer, and the other end to the chain which was in communication with the outſide of the battery, the ſame ſtrength of charge was directed through the wire, by the electrometer, as in the laſt experiments, and the ſame kind of powder with reſpect to colour appeared; the inſtrument was again laid in the water, with the cock above the ſurface, and the water gage was ſcrewed on to the cock; when three minutes were expired, the cock was opened, and there appeared ſo extraordinary diminution of air, that the water in the gage run over the upper bend into the inſtrument: this diminution appeared ſufficient to make a mercury gage ſenſible.

The inſtrument being cleaned, and provided with thirty-five inches of wire of the ſame diameter as above, five ſeparate diſcharges were directed through the wire; ſeven inches were exploded at each diſcharge, and the ſame proceſs with reſpect to laying of it in water before and after each diſcharge was obſerved; and the

	Inches.
Firſt diſcharge cauſed ſo extraordinary diminution, that the mercury roſe in the gage - - - - - -	1
Second diſcharge - - - - - - -	$1\frac{3}{10}$
Third - - - - - - - - - -	$0\frac{5}{10}$
Fourth - - - - - - - - -	$0\frac{5}{10}$
Fifth - - - - - - - - -	$0\frac{3}{10}$
In the whole	$3\frac{6}{10}$

which is nearly equal to two cubic inches. The contents of the whole inſtrument was nine cubic inches.

The air thus diminiſhed in the inſtrument was left to ſtand till the ſame hour next day; but on opening the cock no further diminution had taken place.

The purity of this reſidue of air was tried by the teſt of flame, and it extinguiſhed a candle eight times. Azotic gas, in the ſame glaſs, extinguiſhed the ſame candle ten times, ſo that all the oxigen gas contained in that quantity of atmoſpherical air was nearly abſorbed; the powder produced was nearly ſame as that in the large receiver.

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[*] It was mounted in the ſame manner as Fig. 3, excepting that on account of its diameter only one air cock and gage was uſed.

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This laſt experiment was repeated with wires of a leſs diameter, with nearly the ſame reſult; the five diſcharges produced a diminution, which cauſed the mercury to riſe between three and four inches.

The reſult of the laſt experiments induced me to try receivers of a ſtill leſs diameter; ſo one of $\frac{4}{10}$ inch diameter, and eight inches long, was taken; but on account of its ſmallneſs of diameter, no more air could be exploded than the length of the tube, without admiſſion of freſh air.

EXPERIMENT X.

Accordingly, one diſcharge of the ſame degree of force was directed by the electrometer through one length of wire $\frac{1}{32}$ inch diameter.

		Inches.
The mercury gage roſe		$0\frac{3}{10}$
Second diſcharge through wire of	$\frac{1}{60}$	$3\frac{3}{10}$
Third	$\frac{1}{85}$	$3\frac{3}{10}$
Fourth	$\frac{1}{150}$	$3\frac{3}{10}$

The wire through which the firſt diſcharge was directed, was only converted into a numberleſs ſmall particles, retaining their metallic luſtre. The ſecond, which ſhewed the greateſt degree of abſorption, was but juſt converted into powder, mixed with particles of lead. The reſidue of this air was tried by flame, in a glaſs five inches high, and one and a quarter diameter, but at the mouth only three quarters, and the candle was extinguiſhed in it nine times.

Theſe experiments, I think, will be found ſufficient to ſhew that the ſmaller the diameter of the receiver in which lead is calcined by an electric diſcharge, the greater proportion of air will be abſorbed, and the ninth and thirteenth experiment proves that, that which is abſorbed is the oxigen gas, which is by the proceſs ſeparated from the azote of the atmoſphere.

It is well known that metals require certain degrees of temperature to give them the property of abſorbing the greateſt quantity of oxigen gas; this is the point which I had in view when I uſed wires of different diameters, becauſe I know no way how to increaſe the temperature communicated by an electric diſcharge to metallic wires, but by diminiſhing theſe diameters or lengths in a certain proportion, or increaſing the force of the diſcharge, which is the ſame thing, and the laſt experiment ſhews that it makes very little difference whether wire of $\frac{1}{60}$, or $\frac{1}{120}$, which is only half that diameter, be uſed, ſo that it does not ſeem here to follow the ſame law as common heat.

EXPERIMENT XI.

To conclude with this metal; the firſt-mentioned large receiver was taken, and 130 inches of wire, of $\frac{1}{85}$ of an inch in diameter, was prepared in the ſame, being nearly four times

times the quantity that was uſed before. This was exploded and converted into an impalpable powder by twenty ſeparate diſcharges, and when the gages were opened, there appeared to have been no air abſorbed.

EXPERIMENT XII.

The powder or calx laſt produced, was preſerved and put into a very ſmall glaſs retort, with an intent to try what would happen, or what alterations would take place by the application of heat. No air was given out by ſuch treatment, excepting a little common air, which I ſuppoſe was contained in the retort. The powder, which was firſt of a blueiſh white, changed into a dark leaden colour, and afterwards into a yellowiſh green; and when the glaſs was red hot, nearly melting the powder, was only partially melted into lead.

Laſtly, twenty-four grains of this powder, which had abſorbed no oxigen, was taken and put into a clean crucible, with a cover to it, ſet in a furnace, and kept in a white heat about one hour. When taken out, there was found in the bottom of the crucible a perfect clear glaſs of a rediſh brown colour. Thus we find a powder obtained which poſſeſſes the moſt diſtinguiſhing properties of an oxide of lead, though it appeared to have abſorbed no oxigen in its preparation.

Lead burns with a yellowiſh flame by the electric diſcharge.

Theſe experiments appear to be ſufficient to prove, that the abſorption of the oxigen gas from the atmoſpherical air, by the powder thus produced, wholly depends upon the diameter of the receiver wherein the proceſs was carried on. For we find by the tenth experiment, that when eight inches of wire were calcined in a receiver only $\frac{4}{10}$ of an inch diameter, it then abſorbed half a cubic inch of air. In this proportion the wire laſt calcined ſhould have abſorbed ſixteen times that quantity, and the reſult was that it had abſorbed none; the reaſon of which I muſt own I do not underſtand.

Theſe experiments were made in the winter ſeaſon, when the temperature of the atmoſphere was ſeldom lower than 28°, and not higher than 45°, and the reſult was as mentioned; but not being able to reconcile myſelf to ſuch paradoxical reſults, I ſhowed the experiments to ſeveral philoſophers, who, however, were equally as much at a loſs to account for the reſult as myſelf.

On the 10th of May, 1798, I was favoured with the company of ſome gentlemen of the Royal Society to ſee the experiments. I uſed only two receivers, one of an inch diameter, and the other of ſix inches diameter, as follows:

EXPERIMENT XIII.

A piece of leaden wire, of nine inches long, and $\frac{1}{80}$ of an inch in diameter, was exploded by an electric diſcharge in the receiver of one inch diameter. After it had laid in water three minutes, the mercury gage roſe $\frac{13}{20}$ of an inch, which was a degree of diminution of about half a cubic inch.

EXPERIMENT XIV.

Eight lengths of wire, of the ſame diameter, was exploded in the receiver of ſix inches diameter; and after it had been in water five minutes, the mercury gage roſe $\frac{2}{10}$ of an inch, which was a degree of diminution of about $3\frac{1}{8}$ cubic inches, and conſequently not agreeing with my former experiments.

The reſult of theſe two experiments occaſioned ſeveral repetitions, and all nearly with the ſame reſult; but on repeating them in the winter, when the temperature was low, I found the reſult as I firſt had (namely) a diminution of air in the ſmall receiver, and none in the large one. It then occurred to me that the temperature muſt have occaſioned this variation, and that putting the receiver in water for five minutes was, perhaps, not ſufficient to bring the air in the inſide of the receiver to its former temperature. I therefore placed a thermometer in the inſide of the large receiver, and by exploding one length of wire, the temperature was raiſed about two degrees; and when the receiver was ſet in water, the air did not return to its former temperature in leſs than twenty minutes; conſequently this pointed out to me one error, which I was not aware of; but it was not ſufficient to explain the whole, becauſe this only would indicate a diminution but leſs than truth. By repeating the experiment at different times, I perceived that the firſt exploſion in a low temperature cauſed the mercury to riſe near two degrees, and that every ſubſequent exploſion affected it leſs. After ſeven or eight exploſions had been given, hardly any riſe in the mercury could be perceived. And alſo when the temperature was at about 60°, the firſt exploſion did not then cauſe the mercury to riſe to above 1°, and it would ceaſe to riſe after the third or fourth exploſion. When the temperature was at about 70°, hardly any riſe in the mercury could be perceived, even at the firſt exploſion; this unfolded the whole, becauſe it appeared that when the temperature was low, the exploſions raiſed it ſo much as to cauſe an expanſion in the incloſed air, which kept up the preſſure upon the mercury in the gages, and of conſequence hindered them from indicating any loſs; and when the temperature was high, the exploſions did not cauſe ſuch expanſion, conſequently permitted the gages to indicate the true loſs, or quantity of air which had been abſorbed by the divided metal.

I thought it proper to mention the above in full, not only for the advantage of ſuch as might undertake ſuch experiments, but likewiſe to prevent wrong concluſions being drawn from the reſult of the firſt ſet of experiments with large receivers, as they have been ſhewn to many, and ſome authors have brought them forward to ſupport their own erroneous notions.

We may alſo conclude from the facts that the difference of diameters of the receivers wherein leaden wire is exploded, neither favours, nor hinders, the diminution of the atmoſpherical air therein contained. If two leaden wires of equal lengths and diameters be exploded by equal electric diſcharges, in unequal receivers, the diminution of the air which they contain will be equal in both; and if the receivers contain atmoſpherical air, that which is ſeemingly loſt by the proceſs is oxigen gas, abſorbed by the exploded metal, which becomes converted into a true metallic oxide.

If it be required to collect the produce for further inveſtigation, a receiver of the largeſt diameter then becomes neceſſary, becauſe the oxide is more perfect, and a greater quantity of powder can be produced, and the experiment oftener repeated, before the receiver becomes opaque, which is the caſe when receivers of ſmall diameters are uſed.

The foregoing experiments upon lead, prove that a receiver of one inch diameter is ſufficiently wide, and therefore preferable to wider, becauſe when the temperature of the air in the inſide is raiſed by the exploſions, it is much ſooner brought to its former temperature, and is not ſo liable to equivocal reſults. I have not, therefore, thought it proper to relate the experiments which were made in wider receivers upon the following metals: ſuch receivers being neceſſary only when a large quantity of oxide is required.

EXPERIMENT XV.

Tin.

Sixteen inches of tin wire, of $\frac{1}{120}$ part of an inch in diameter, was put into Fig. 4, and after it had been laid in water ſo long, that the air in the inſide was become of the ſame temperature as the water itſelf; it was afterwards exploded by two diſcharges, and by that means converted into an impalpable powder, nearly the ſame as the lead wire, but of a purer white. The apparatus was then again laid in water * till the air had gained its former temperature; when the cock was opened, the mercury roſe three inches, and the air, by the teſt of flame, ſeemed to be as as highly azotic as that in which lead had been exploded in the ninth experiment.

The laſt experiment, repeated with wire of $\frac{1}{100}$ of an inch diameter, and the reſult was the ſame.

Tin burns with a vivid yellow flame when exploded.

EXPERIMENT XVI.

Iron.

Thirty-two inches of iron wire, of $\frac{1}{230}$ inch diameter, was put into Fig. 4, and exploded by five diſcharges: the mercury roſe three inches.

The wire was converted into an impalpable powder, of a reddiſh brown colour; the reſidue of air ſeemed to be the ſame as that left in the ninth and fifteenth experiments, wherein lead and tin had been exploded. It extinguiſhed a candle nine times.

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* The apparatus was always laid in water a ſufficient time to cauſe the air in the inſide to become of the ſame temperature as the water, before and after the metal was exploded; but to avoid prolixity I have not mentioned it in the relation of the following experiments.

EXPERIMENT XVII.

Wires of different diameters were taken, with a view to try what degree of fuſion was required to cauſe the greateſt abſorption of air, or, in other words, to make the moſt complete oxide.

The ſame degree of ſtrength of charge was directed through wire of $\frac{1}{100}$ inch diameter; no air abſorbed; the wire was only red hot.

Wire of $\frac{1}{100}$ was exploded and converted into balls; the mercury roſe	$0\frac{5}{10}$
—— $\frac{1}{130}$ converted into very ſmall powdered balls - -	$0\frac{6}{10}$
—— $\frac{1}{150}$ into flocculi and balls - - - -	1
—— $\frac{1}{240}$ into an impalpable powder of reddiſh brown colour -	$1\frac{1}{10}$

EXPERIMENT XVIII.

Copper.

Thirty-two inches of copper wire, of one 260 part of an inch diameter, was prepared in Fig. 4, and exploded by five diſcharges; the mercury in the gage roſe $1\frac{1}{10}$ inches.

It was converted into an impalpable powder, of a purple brown colour; the reſidue of air extinguiſhed the flame of a ſmall wax taper twice.

This metal burns with a green flame when exploded.

All the foregoing experiments were made by myſelf, at my houſe in Poland Street; but the following were made at the houſe of Mr. Campbell, in South Audley Street, who honoured me with his aſſiſtance. The apparatus we uſed were of my own conſtruction, being a double plate machine; each plate twenty-ſix inches diameter, and a double battery of thirty jars of my common ſize, containing about thirty-four ſquare feet coated glaſs. The electrometer was the ſame as I have deſcribed in Nicholſon's Journal, Vol. II. page 528, excepting that inſtead of uſing pins of certain weights to put into B, the arm C B was divided into grains, with a ſlider upon it, which was ſet to the weight required, as I commonly make them at preſent; being more convenient.

EXPERIMENT XIX.

Pure Silver.

Forty-five inches of ſilver wire, of $\frac{1}{130}$ part of an inch diameter, was exploded by five diſcharges; the ſlider upon the electrometer was ſet at forty grains; it was converted into an impalpable black powder: the mercury roſe in the gage about half an inch, and the flame of the taper was extinguiſhed nine times.

Silver burns with a greeniſh flame, nearly reſembling that of copper.

EXPERIMENT XX.

Grain Gold.

Fifty-four inches of wire drawn from grain gold, of $\frac{1}{125}$ part of an inch diameter, was exploded by ſix diſcharges; the ſlider upon the electrometer was ſet at forty-two grains; it was converted into an an impalpable powder, of a dark brown purple colour: the mercury roſe in the gage about a quarter of an inch, and the flame of a taper was extinguiſhed four times.

EXPERIMENT XXI.

Nine inches of the ſame ſort of wire, but of a leſs diameter, being 130 part of an inch, was exploded in the ſame receiver by one diſcharge: the mercury roſe about $\frac{2}{10}$ of an inch, and the flame of the taper was extinguiſhed four times.

EXPERIMENT XXII.

Platina.

Twenty-ſeven inches of platina wire, of 130 part of an inch diameter, was exploded by three diſcharges. When the gage cock was opened, the mercury roſe a quarter of an inch, but on applying the flame to the reſidue, it was not extinguiſhed.

As platina is eaſily fuſed by electricity, the ſlider upon the electrometer was only ſet at twenty-two grains for the laſt experiment, and the reſidue of air did not extinguiſh the flame. It was ſuppoſed that too low a charge had been uſed. The ſlider was therefore ſet at thirty-two grains, and the experiment was repeated with the ſame length and diameter of wire, and exploded by the ſame number of diſcharges; when the cock was opened, the mercury roſe about one inch, and the flame of the taper was extinguiſhed five times: the metal was converted into a black impalpable powder, reſembling that of ſilver.

EXPERIMENT XXIII.

Zinc.

Nine inches of zinc wire, of $\frac{1}{100}$ part of an inch in diameter, was exploded by one diſcharge; when the gage cock was opened, the mercury roſe half an inch, and the reſidue of air extinguiſhed the flame of a taper twice: the metal was converted into an impalpable powder of a white colour, reſembling that of tin.

This metal being ſo difficult to draw into wire, I was obliged to conclude with the above-mentioned length.

From the reſult of the foregoing experiments, it may be ſafely concluded, that all the ductile metals can, by electric diſcharges, be ſublimed and converted into proper oxides, by abſorbing the oxigen of the atmoſphere; and though ſome of the metals reſiſt the action of common fire, and require different ſolvents to convert them into oxides, yet they all yield to the action of the electric fluid.

It is remarkable that platina, though it reſiſts the action of common fire, is more eaſily fuſed by electric diſcharges than copper, ſilver, or gold, and ſeems to be as greedy of oxigen as any of the other metals. But theſe experiments have not been ſufficiently extended to ſettle the laſt mentioned property.

It is well known that all metals which are ſublimable by common fire, abſorb oxigen in different degrees, and likewiſe in different proportions, according to the different degrees of heat employed; this ſeems likewiſe to take place when they are ſublimed by electric diſcharges, but the proper degree of diſcharge for each metal remains for inveſtigation; and as different metals differ as conductors of the electric fluid, (which has never yet been well defined) this furniſhes matter ſufficient for a future purſuit.

The lateſt experiments upon the conducting power of metals were made by Dr. Van Marum and myſelf, at Haerlem, but as I had not at that time invented the electrometer which I have at preſent, and which the nature of the experiment required, I have ſome reaſon to think that they are not perfectly accurate. I have concluded theſe experiments, without proceeding to the nonductile metals, becauſe I know no way of arranging the others, ſo that they can be acted upon with ſufficient accuracy.

The experiments which have been made upon the different metals mentioned, have been very numerous, excepting zinc, but for the ſake of brevity, and to avoid tautology, I have only mentioned thoſe which I thought to be abſolutely neceſſary.

V.

Deſcription of an Improvement in the Steam Engine, by means of which the Boiler is conſtantly ſupplied with Water already heated nearly to the point of Ebullition. Communicated by Mr. PETER KEIR, *Engineer.* *

THE ſteam engine, of which we have ſo many accounts in introductory and other works, and which for its ſcientific contrivance and extenſive utility cannot be too highly regarded, may be traced in its hiſtory through a few great and ſtriking degrees of improvement, the intention of every one of which conſiſts in producing the greateſt quantity of moving force with the leaſt poſſible conſumption of fuel. It is probable that the original engine of the Marquis of Worceſter may have been a mere veſſel into which water flowed by its weight, and was thence driven upwards by ſteam: Capt. Savary ſeems to have been the firſt who uſed the vacuum formed by ſteam; into which the water was urged by the atmoſphere: Papin in a crude way, and Newcomen and Cawley in a more perfect method, uſed a ſolid piſton: and laſtly, James Watt greatly improved the engine of the latter operators

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* In converſation. I am very ſorry that the lateneſs of the date of this ſheet at preſs, does not allow me time to ſend and inquire the name of the inventor, which I omitted to write down. The improvement is under patent, which, with the name, I ſhall mention in my next.

rators by condenſing his ſteam in a cold veſſel at a diſtance from the working cylinder. The other improvements in the mechaniſm in the furnace, in the double ſtroke, &c. &c. though highly valuable and honourable to the ſkill and contrivance of the inventors, appear to be of ſubordinate importance, and in many particulars are little more than thoſe variations of faſhion which in mechanics, as well as other leſs important departments of human induſtry, are more or leſs prevalent.

It was among the early improvements of the ſteam engine, to feed the boiler out of that hot water which was afforded by the condenſation of the ſteam along with the ſtream of injection. When the boiler receives a quantity of cold water, it becomes neceſſary either to increaſe the fire, or to admit the engine to work with leſs effect from the diminiſhed quantity or elaſticity of the ſteam produced; and when the water thus ſupplied is afforded in the manner juſt mentioned, it will be hotter the leſs the injection, and the contrary. But there is a mean which muſt be attended to in the adjuſtment of this heat. A ſparing injection will not afford a good vacuum, and therefore the engine will work with diminiſhed effect ; and a copious injection will afford water at a low temperature to the boiler. When the adjuſtment is at the beſt, the water of the ſupply is not hotter than the hand can bear, that is to ſay, about 120°. It becomes therefore an object of value, to aſcertain the means by which the condenſation of the ſteam may be rendered as perfect as the introduction of cold water can make it, and at the ſame time, that a greater part of the heat it imparts may be employed in raiſing the temperature of the water intended for the boiler.

Plate VIII. repreſents a ſection of the parts of a double engine; that is to ſay, an engine in which the piſton is alternately impelled by the elaſticity of ſteam acting above and below. A A is the great cylinder in which the piſton moves; O, P, are the ſteam boxes, or metallic receptacles, into which the ſteam is firſt introduced from the boiler by pipes at O and P, not repreſented in the figure. They contain each two valves, B, C, and *b*, *c*, which are opened or ſhut by the uſual apparatus attached to the working beam; *d*, *d* is the paſſage to D D the condenſer, terminating in H the air pump. E F G are the additional parts of which the new improvement conſiſts.

Let us then ſuppoſe the engine in actual work, the valve *b* in the lower ſteam box being open, and *c* ſhut, while B in the upper ſteam box is ſhut, and C open. From this diſpoſition ſteam will ruſh in from the boiler through *l*, beneath the piſton, while the other ſteam which had been admitted before above the piſton, will have a communication through L, C, K and *d*, to D the condenſer, where a conſtant injection J being kept up, it becomes condenſed and leaves a vacuum. Conſequently the piſton muſt riſe by the whole force of the ſteam beneath. When it has arrived near the top of the cylinder, the valves in the ſteam boxes *b* and C are ſhut, and the others B and *c* are opened; ſo that the ſteam from the boiler ruſhes through L into the ſpace above the piſton, while the other ſteam beneath paſſes through *l*, *c* and *k* to the condenſer, where it aſſumes the denſe form and leaves a vacuum, into which the piſton is urged in its deſcent. When the deſcent is completed, the

the valves reſume their firſt ſtate, and the piſton muſt again riſe, &c. &c. The pump H is kept conſtantly at work by the motion of the engine, and carries off the water of the injection heated by the ſteam, and alſo the permanently elaſtic fluid, which may be extricated from the water: and of this hot water it is that the boiler in ſteam engines is uſually repleniſhed. Inſtead however of this direct ſupply in the preſent engine, the hot water is ſuffered to flow into the ſmaller reſervoir G, G, G, which it keeps conſtantly full and overflowing, and from which a portion is raiſed by the ſmall pump E through the pipe F, M, F, F, tranſmitted through the paſſage to the condenſer, and conſequently having its exterior ſurface in contact with the hot ſteam as it ruſhes towards the condenſer, and by that means having its contents, and conſequently thoſe of the veſſel G G G, greatly heated by the circulation effected by the pump. The boiler is ſupplied by the pipe N, placed ſo far below the return M, that the preſſure of the column I M may exceed the re-action of the ſteam in the boiler, which might otherwiſe prevent its introduction.

I am not prepared to ſay to what extent this improvement may add to the power of the engine.

VI.

Inquiries relative to the Laws of Affinity. By Cit. Berthollet.*

(*Continued from page* 108.)

On the Influence of the Proportions in Complex Affinities.

1. IN my former inquiries I have aſcertained the principal reſults afforded by complex affinity, when the force of coheſion, or that of elaſticity are ſufficiently great to produce that change of baſe which had been attributed to the ſuperiority of the divellent affinities over the quieſcent affinities. But I have neglected to enter into the neceſſary details for determining the changes which may ariſe from different proportions of the ſubſtances which are brought into action, when the force of coheſion is not ſufficiently great to cauſe the effects of this difference to diſappear. I have promiſed (in Art. XII. No. 6,) to return to this ſubject, which I ſhall now proceed to do.

According to the theory which I have laid down (Art. V. No. 5), all ſubſtances exert a mutual action during the time they are in the liquid ſtate; ſo that in a ſolution, for example, of ſulphate of pot-aſh and muriate of ſoda, theſe two ſalts are not diſtinct, while there is no cauſe to determine the ſeparation from their combination; but that there exiſts in this liquid, ſulphuric acid, muriatic acid, ſoda, and pot-aſh. I ſhall, however, continue to make uſe of the ordinary language, which after this notice, ought not to produce any ambiguity.

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* Annales de Chimie, XXXVIII. 3.

2. I

2. I begin with mixtures in which a conſiderable force of cryſtallization muſt determine the combinations which are formed.

Experiment A. Equal parts of nitrate of lime and ſulphate of pot-aſh were mixed: after the ſeparation of the ſulphate of lime which was firſt formed, and of which I ſhall ſay more in the following experiments, the liquid was evaporated, and nothing was obtained by the ſucceſſive operations but nitrate of potaſh and ſulphate of lime. After the laſt evaporation, however, ſome cryſtals of ſulphate of pot-aſh were procured; there remained but a very ſmall quantity of uncryſtallizable liquid, which precipitated with the carbonate of ſoda and with the nitrate of barites; ſo that it conſiſted of a little ſulphuric acid and lime, and very probably a larger portion of nitrate of pot-aſh.

The quantity of ſulphate of lime which was depoſited in the courſe of the evaporation, was much more conſiderable than would have been obtained from ſimple ſolution of this ſalt in water; ſo that its ſolubility was augmented by the action of the other ſubſtances.

Experiment B. Two parts of ſulphate of pot-aſh, and one of nitrate of lime, gave by a firſt evaporation ſulphate of potaſh and ſulphate of lime; and by the ſucceeding evaporations, nitrate of pot-aſh with the two ſulphates, the proportions of which continued to decreaſe to the laſt cryſtallization. There remained only ſome drops of the liquid uncryſtallized; they did not precipitate with the carbonate of ſoda, but did with the nitrate of barites: hence they were probably formed of ſulphate of pot-aſh, and a ſmall proportion of nitrate of pot-aſh.

Experiment C. Two parts of nitrate of lime, and one part of ſulphate of pot-aſh, afforded during the firſt evaporation a ſmall quantity of ſulphate of lime, and by cooling, nitrate of pot-aſh; the other evaporations only produced nitrate of pot-aſh. In the latter, however, ſome cryſtals of ſulphate of lime were perceived at the ſurface of the liquid. Notwithſtanding the reſidue, which was abundant, was ſeveral times ſubmitted to evaporation and cooling, it no longer afforded cryſtals of any ſalt. This uncryſtallizable reſidue treated with alcohol, formed an abundant depoſition, which having been diſſolved in water, ſcarcely afforded any precipitate with the nitrate of barites; ſo that it contained little or no ſulphuric acid, and conſiſted of pure nitrate of pot-aſh: the portion diſſolved by alcohol was nitrate of lime, with a ſmall portion of nitrate of pot-aſh. The uncryſtallizable reſidue was therefore compoſed of nitrate of pot-aſh and nitrate of lime.

In this experiment we ſee, that the ſulphate of lime was rendered much leſs ſoluble than in the preceding experiments; but that a conſiderable quantity of nitrate of pot-aſh loſt the property of cryſtallizing by the action exerted on it by the nitrate of lime.

3. In theſe three experiments ſulphate of lime muſt have been formed, becauſe the lime and ſulphuric acid coming in contact, would ſeparate on acccount of the inſolubility which belongs to their combination.

The ſulphate of lime in the experiment A and in the experiment B, was rendered much more ſoluble than it naturally is, by the action of the ſubſtances which were in ſolution; but in the experiment C, its ſolubility was not ſenſibly increaſed, probably becauſe the nitrate of lime and the nitrate of pot-aſh which formed the uncryſtallizable liquid, mutually experienced a degree of ſaturation, which conſiderably weakened their action upon the ſulphate of lime.

4. From theſe conſiderations, I ſhall proceed to deduce firſt, the theory of the uncryſtallizable reſidues which are found in the ſolutions of ſalts which are evaporated: it will be confirmed by the ſucceeding obſervations.

Saline ſubſtances exerciſe upon each other an action which increaſes their ſolubility, an effect that has been particularly eſtabliſhed by the experiments publiſhed by my learned colleague Vauquelin (*Annales de Chimie*, tom. xiii.) This mutual action varies in the different ſalts. It has however been thought, that ſalts with an earthy baſe do not increaſe the ſolubility of the nitrate of pot-aſh, though theſe in reality increaſe it the moſt.

There is doubtleſs in this reſpect, a difference in the effect produced by the ſalts, which depends on their nature; but this difference is in general very ſmall, compared with that which proceeds from the force of cryſtallization.

Experiment D. A mixture of equal parts of nitrate of pot-aſh and ſulphate of pot-aſh, afforded by evaporation, ſucceſſively and in proportion to their ſolubility, ſulphate of pot-aſh and nitrate of pot-aſh, without leaving any uncryſtallizable liquid; but the ſame experiment being made with a mixture of nitrate of ſoda and ſulphate of ſoda, both of which have only a ſlight tendency to cryſtallize, and are nearly of equal ſolubility, only a ſmall quantity of ſulphate of ſoda is ſeparated by cryſtallization, all the reſt remaining liquid, without any cryſtallization. A mixture of muriate of ſoda and ſulphate of alumine having been ſubmitted to the ſame proof, it was clearly perceived that the two ſalts had become more ſoluble; but they were intirely ſeparated by the alternate evaporation and cooling.

It is therefore ſeen that ſubſtances which poſſeſs a conſiderable force of cryſtallization, though rendered more ſoluble, ſeparate on account of their inſolubility, and leave very little or no uncryſtallizable reſiduum.

But when ſalts are found which have only a feeble diſpoſition to cryſtallize, their mutual action counterbalances their force of cryſtallization, ſo that there then remains much liquid which cannot cryſtallize; and this happens particularly when the reſiduum contains a ſubſtance in itſelf uncryſtallizable; as in experiment C, where by the proportions employed, there was found a ſuperabundance of nitrate of lime, which by its action upon the nitrate of pot-aſh reduced a conſiderable quantity of that ſalt into an uncryſtallizable liquid.

Whence therefore does it happen, that the moſt learned chemiſts, Lavoiſier, Fourcroy, Vauquelin, Guyton, and the Commiſſaries of the Academy of Sciences, among whom myſelf was one, could have been led by the experiments made upon the proof of ſalt-petre, to believe, that the nitrate of lime exerts no action upon the nitrate of pot-aſh, nor increaſed its ſolubility? (*Ann. de Chimie*, tom. xi. xiii. xv. xxiii.) It was becauſe, in the experiments that were made, a water of nitrate of pot aſh was put to digeſt upon dried nitrate of lime. The latter neceſſarily cauſed a diviſion of the water upon which it has a

ſtrong

ſtrong action. It would therefore have precipitated a conſiderable quantity of nitrate of pot-aſh, if this effect had not been compenſated, or nearly ſo by the ſolubility which it imparted to that ſalt; but if the ſolution had been evaporated, there would have been extracted by cryſtallization much leſs nitrate of pot-aſh than was contained in the liquid, and an uncryſtallizable reſiduum would have been left ſimilar to that of experiment C. The proof therefore which might be conſidered as concluſive for the object propoſed, has induced an error reſpecting the chemical phenomenon.

Hence it follows, that when the mother waters of the ſalt-petre works are decompoſed by pot-aſh, the nitrate of pot-aſh obtained conſiſts not only of that which has juſt been formed, but alſo that which was rendered uncryſtallizable by the ſalts with an earthy baſis.

5. I made mixtures of cryſtallized ſulphate of ſoda and nitrate of lime. Here the ſulphate of ſoda, and the nitrate of ſoda which might proceed from the operation, differed little in point of ſolubility, and had a cryſtallizing power leſs conſiderable than the ſulphate and nitrate of pot-aſh.

Experiment E. Equal parts of cryſtallized ſulphate of ſoda and nitrate of lime, reduced to the ſtate of dryneſs, gave by evaporation only a ſmall quantity of nitrate of ſoda. The uncryſtallizable reſidue, which was abundant, did not precipitate with the muriate of barites, but with the oxalic acid.

Experiment F. Two parts of ſulphate of ſoda, and one part of nitrate of lime, afforded after evaporation, a greater quantity of nitrate of ſoda than the preceding experiment; the uncryſtallizable reſidue did not precipitate with the oxalic acid, but with the muriate of barites. In order to compare the two laſt experiments with the experiments A, B and C, it muſt be obſerved, that the cryſtallized ſulphate of ſoda contains more than half its weight of water of cryſtallization.

Even in experiment F, no ſulphate of ſoda is formed, as ſulphate of pot-aſh was formed in experiments A and B, becauſe the ſulphate of pot-aſh has a force of cryſtallization much greater than the ſulphate of ſoda.

The reſidue of experiment E did not contain ſuch a quantity of ſulphuric acid as could be rendered perceptible by the muriate of barites; but it was formed of nitrate of ſoda and nitrate of lime, which probably, by their mutual action, ſo much exhauſted their diſſolvent power, as not to prevent the ſeparation of the ſulphate of lime No. 3.

In experiment F the reſidue was not precipitated by the oxalic acid, but by the muriate of barites; ſo that this reſiduum was compoſed of ſulphate of ſoda, and a greater proportion of nitrate of ſoda, which mutually prevented their cryſtallization, as in experiment D.

6. *Experiment* G. Equal parts of nitrate of pot-aſh and ſulphate of ſoda, gave by ſucceſſive cryſtallizations, 1. ſulphate of pot-aſh, and ſome ſmall cryſtals of nitrate of pot-aſh; 2. a little ſulphate of pot-aſh, and a greater proportion of nitrate of pot-aſh; 3. ſmall cryſtals of nitrate of pot-aſh, and much nitrate of ſoda; there was an uncryſtallizable reſidue, notwithſtanding

notwithſtanding the care which had been taken to obtain the utmoſt cryſtallization; this reſidue was formed of nitrates and ſulphates; for it precipitated abundantly with the nitrate of barites, and after drying it flowed on ignited charcoal. This experiment ought to be compared with experiment F.

Experiment H. One part of nitrate of pot-aſh, and two of ſulphate of ſoda, gave, 1. ſulphate of pot-aſh; 2. ſulphate of pot-aſh, and ſome needles of nitrate of pot-aſh; 3. ſulphate of pot-aſh in ſmall priſms, ſuſpended from a pellicle formed by nitrate of ſoda, fine cryſtals of nitrate of pot-aſh and nitrate of ſoda; the reſidue contained nitrates and ſulphates.

In theſe two experiments, the leaſt ſoluble ſalt of thoſe which were formed, *viz.* the ſulphate of pot-aſh, was the firſt which cryſtallized. When the proportions had been thus changed, the action of the nitric acid on the pot-aſh reſumed its ſuperiority, and nitrate of pot-aſh was formed, though the liquid ſtill contained ſulphuric acid.

In the ſecond, the more abundant ſulphuric acid gave riſe to a greater quantity of ſulphate of pot-aſh; but after the firſt cryſtallization, more nitrate of pot-aſh was formed, though there would have been a ſufficient quantity of ſulphuric acid to cauſe a complete change of baſe, if this exchange could have been made as has been imagined. The reſidue was formed even in experiment G, of ſulphate of ſoda and nitrate of ſoda; and probably of a ſmall quantity of ſalt with baſe of pot-aſh.

7. *Experiment* I. Equal weights of nitrate of pot-aſh and muriate of lime were mixed together: by evaporation there were obtained, 1. nitrate of pot-aſh; 2. muriate of pot-aſh, in which was found a little nitrate of pot-aſh: the reſidue gave with the ſulphuric acid an abundant precipitate of ſulphate of lime, and vapours of muriatic and nitric acid were diſengaged.

Experiment K. The experiment having been made with two parts of muriate of lime, and one of nitrate of pot-aſh, an abundant cryſtallization was effected of muriate of pot-aſh with no appearance of nitrate; the reſidue treated as in the preceding experiment, gave analogous reſults.

Experiment L. A mixture was made the reverſe of thoſe of the preceding experiments; it was formed of equal parts of muriate of pot-aſh and nitrate of lime. It afforded, 1. nitrate of pot-aſh, mixed with a little muriate of pot-aſh; 2. muriate of pot-aſh, which was mixed with a ſmall quantity of nitrate of pot-aſh. The uncryſtallizable reſiduum was diſſolved in alcohol; there was a ſeparation of nitrate of pot-aſh, which was fuſed upon ignited charcoal, but contained a little muriate of pot-aſh, as was ſeen by the teſt of the ſolution of ſilver. The ſulphuric acid ſhewed, that the part diſſolved by the alcohol contained muriatic acid, nitric acid, and lime.

In the experiments above deſcribed, and in which ſubſtances were employed whoſe combinations could not have a conſiderable force of cryſtallization, and differed but little among each other in this reſpect, it is evident, that the formation of the ſalts which were afterwards obtained by cryſtallization, depended on the proportions of the ſubſtances which mutually acted among each other. In experiment I, which might afford nitrate and muriate of pot-aſh; as theſe two ſalts differed little in their ſolubility, which however is rather the

the leaſt in the latter, the nitrate of pot-aſh was obtained by the firſt cryſtallization; but as in experiment K the muriatic acid exiſted in a greater proportion, muriate of pot-aſh only was then obtained: one part of pot-aſh with muriatic acid, nitric acid and lime formed the reſiduum. The proportions employed in experiment L, differed little from thoſe of I, and the reſults were nearly the ſame.

Oppoſite combinations therefore are obtained according to the proportions employed, or according to the period of cryſtallization; that is to ſay, according to the proportions of the ſubſtances which remain in action, when a ſufficient force of coheſion does not exiſt in the combinations that might be formed. But according to the opinion adopted by chemiſts, the entire change of baſe which it is affirmed muſt take place, may be judged of even by a firſt cryſtallization.

If, for example, nitrate of pot-aſh be firſt obtained from the proportions which have been employed of muriate of pot-aſh and nitrate of lime, a concluſion is made, that an exchange of baſe has been effected between the muriatic and nitric acids. If other proportions had been employed which would have afforded muriate of pot-aſh, as in experiment K, an oppoſite conſequence would have been drawn. Chemiſts have even gone farther: from the concluſion reſpecting the change of baſe, for example, between the muriatic and nitric acids, a farther inference has been made, that the combinations oppoſite to thoſe which afforded ſuch a change, would not be at all affected by their mixture.

8. I have examined what change might be produced in the reſults by a ſubſtance which has the property of forming triple ſalts, ſuch as magneſia.

Experiment M. Equal parts of ſulphate of pot-aſh and muriate of magneſia, afforded, 1. ſulphate of pot-aſh; 2. ſulphate of pot-aſh, a little muriate of pot-aſh, and a triple ſalt, compoſed of ſulphuric acid, pot-aſh, and magneſia; this ſalt forms fine rhomboids, which, by expoſure to the air, do not loſe their tranſparency; its ſolubility is nearly the ſame as that of the ſulphate of pot-aſh: 3. muriate of pot-aſh and ſulphate of magneſia. The reſiduum contained ſulphuric acid, muriatic acid, pot-aſh, and magneſia.

Experiment N. Two parts of muriate of magneſia, and one of ſulphate of pot-aſh, afforded, 1. ſulphate of pot-aſh; 2. muriate of pot-aſh, and the triple ſalt of the preceding experiment; 3. muriate of pot-aſh, and ſulphate of magneſia: the reſiduum was analogous to that of the preceding experiment.

In experiment M, ſulphate of pot-aſh was obtained by two cryſtallizations; but in experiment N, where the muriatic acid was in a greater proportion, it was obtained on the firſt cryſtallization; the triple ſalt, which in ſolubility nearly approaches the ſulphate of pot-aſh, cryſtallized after the ſecond evaporation. When the proportion of ſulphuric acid was ſufficiently diminiſhed by theſe cryſtallizations, then the muriate of pot-aſh was ſeparated, nearly according to the order of ſolubility; and laſtly the magneſia, which being ſtill abundant, cryſtallizes with part of the ſulphuric acid.

We ſee that a different opinion might have been formed in theſe two experiments, according to the period of the cryſtallization to which the attention might have been directed,

and

and how far diſtant from truth the eſtabliſhed opinion is, that a complete change of baſes takes place on the mixture of muriate of magneſia and the ſulphate of pot-aſh.

In experiment M, where the ſulphuric acid was preſent in greater proportion, ſulphate of pot-aſh was obtained in both the firſt cryſtallizations; but in experiment N, where the muriatic acid had more influence by its relative quantity, ſulphate of pot-aſh was obtained only in the firſt cryſtallization; the magneſia, for the moſt part, remained in the uncryſtallizable reſidue, becauſe it has no force of cryſtallization with the muriatic acid, and only a weak power with the ſulphuric acid; other ſalts which would in other circumſtances have been cryſtallizable, are retained in the reſidue which oppoſes their cryſtallization.

The difference obſerved between the reſults of theſe two experiments, and thoſe of the experiments A, B, and C, in which a deliqueſcent calcareous ſalt was put into action with the ſulphate of pot-aſh, correſponds exactly with the difference of ſolubility which exiſts between the ſulphate of lime and the ſulphate of magneſia.

9. After having treated of ſaline ſubſtances in the preceding experiments, as if they formed ſeparate or diſtinct combinations in a liquid, I ſhall now conſider ſome effects which are owing to the mutual action which is in reality exerted by them all when they are mixed in this ſtate.

Experiment O. Aqueous ſolution of the acetite of lead was gradually poured into a ſolution of muriate of ſoda, till no more precipitate was produced. The liquid which remained above the precipitate ſtrongly reddened blue paper, which effect was not produced by the mere ſolution of the acetite of lead, nor by the muriate of ſoda; but it aſſumed a deep colour with the hydro-ſulphurets, and afforded an abundant precipitate with the muriatic and ſulphuric acids. During its evaporation a depoſition was formed of a ſcarcely ſoluble muriate of lead, and alſo a cruſt which had not a cryſtalline appearance, and which will be ſpoken of hereafter. Laſtly, ſome fine cryſtals were obtained, which were an acetite of ſoda, and an oxide of lead. On diſſolving the ſaline cruſt, a depoſition took place of muriate of lead with exceſs of oxide, analogous to that deſcribed by Vauquelin (*Annales de Chimie*, t. xxxi.) It was neceſſary to repeat the ſolution and cryſtallization ſeveral times, before the depoſition ceaſed. The ſaline cruſt was thus, or at this period, divided into two ſubſtances, muriate of ſoda, and muriate of lead.

If the muriatic acid remain engaged in the acid together with the acetous, as both acids are volatile, the exceſs which is but ſlightly engaged, and which may be expelled by the action of heat, muſt be compoſed of two acids. In fact, having diſtilled a mixture ſimilar to the preceding, after having ſeparated the depoſition which was firſt formed, the liquid which paſſed into the receiver contained acetous and muriatic acid; this circumſtance deſerves particular attention, and ſerves to explain ſeveral obſervations.

10. Citizen Prieur has obſerved that when lead is employed for purifying the ſolution of ſilver mixed with muriatic acid, a part of the laſt-mentioned acid paſſed over by diſtillation. This happens becauſe the muriate of lead is ſoluble, and even very much ſo by the action of the nitric acid. The liquid is therefore compoſed of oxide of lead, muriatic acid

and

and nitric acid. The oxide of lead divides its action upon the two acids, and both are ſubjected to the action of expanſibility produced by heat.

If the ſulphuric acid were to be retained, lead would be an efficacious medium; 1. becauſe the ſulphate of lead is much leſs ſoluble than the muriate; 2. becauſe the ſulphuric acid is much leſs volatile than the muriatic acid.

The muriate of ſilver being much leſs ſoluble than the muriate of lead, ſilver is much better than lead for retaining the muriatic acid which is found mixed with nitric acid. Velter and Bonjour have nevertheleſs obſerved that muriatic acid always came over in diſtillation, when the operation was made without the precautions pointed out by thoſe learned chemiſts. To obtain a pure nitric acid immediately, it is neceſſary to make the operation upon an acid little concentrated, in order that it may not hold in ſolution any muriate of ſilver, and to ſeparate the muriate of ſilver which precipitates before the liquid is ſubjected to the action of heat, or which is preferable, to precipitate by the ſolution of ſilver, the muriatic acid from the nitrate of pot-aſh; after which, by decompoſing this nitrate, the nitric acid is obtained perfectly free from muriatic acid.

When muriate of ſilver remains in the ſolution, a very pure nitric acid may nevertheleſs be obtained by diſtillation, by ſetting aſide the firſt portion which diſtils over, till it is ſeen by the teſt that no more muriatic acid is afforded. For as the chemiſts I have juſt quoted obſerved, the muriatic acid in this proceſs aſſumes the nature of oxigenated muriatic acid, and is diſengaged in this ſtate at the beginning of the operation.

Experiment P. Sulphate of pot-aſh having been treated with the acetite of lead, in the ſame manner as the muriate of pot-aſh, ſulphate of lead was precipitated. The liquid retained only a ſmall quantity of oxide of lead. By the progreſs of evaporation ſome cryſtals of ſulphate of pot-aſh were obtained, though before the evaporation the acetite of lead produced no more precipitate; and, laſtly, the acetite of pot-aſh, which retained a ſmall quantity of oxide of lead. The decompoſition of the ſulphate of pot-aſh was much more complete than that of the muriate of ſoda.

In the experiments I have juſt deſcribed, we ſee, then, that the decompoſitions, or exchange of baſes, alſo follow the order of the ſolubilities of the combinations which may be formed, and that the only difference obſerved in moſt of the preceding experiments ariſes from the property poſſeſſed by the oxide of lead of forming triple combinations, which ſometimes again ſeparate in combinations of different degrees of ſolubility, as happens with the muriate of ſoda and of lead Experiment O.

11. The following experiment will confirm the truth that the force of coheſion produces a different effect, according to the properties of the ſolvent.

Experiment Q. I mixed an aqueous ſolution of plombate of ſoda with the water of ſulphate of ſoda, a ſlight precipitate only was produced, though the ſulphuric and muriatic acids, if added, would have produced an abundant precipitate: when the experiment was made with the muriate of ſoda, the precipitate was much more abundant than with the ſulphate of ſoda.

On the firſt view, theſe effects may appear to be contrary to the principles I have eſtabliſhed. For the ſulphate of lead is much leſs ſoluble than the muriate of lead; ſo that it might be expected that a more abundant precipitate would be obtained in the experiment made with the ſulphate of ſoda than in that with the muriate of ſoda. This happens otherwiſe becauſe the ſulphate of lead is much more ſoluble in ſoda, as I have aſcertained, than the muriate of lead, with exceſs of oxide, ſuch as is precipitated in the preceding experiments. And the precipitation is not a direct conſequence of the force of coheſion, but ariſes from the exceſs of the force of coheſion beyond that of the ſolvent.

12. The obſervations which I have preſented in this memoir may be reduced to the following reſults. In the complex affinities, or double affinities, the force of coheſion, when conſiderable, and differing much in its intenſity among the combinations which may be formed, determines a change of baſes, in ſuch a manner that the moſt inſoluble combination is formed and ſeparated independently of the proportions, which have an influence only upon the ſtate of thoſe ſubſtances which remain in ſolution. The reſult of a mixture of different ſaline ſubſtances may therefore be foretold from the mere conſideration of ſolubility.

In this caſe the adopted theory of the quieſcent and divellent affinities does not miſlead us, as to the principal reſult, that is to ſay, the formation of the inſoluble ſalt. But as it is not deduced from facts of a ſuperior order, it will demand as many experiments as particular facts. The doctrine is not eſtabliſhed upon any foundation from which we can foretel the mutual actions of ſubſtances preſented to each other; and again it has the diſadvantage of affording no indication reſpecting the properties which the remaining fluid portion ought to exhibit when ſubjected to evaporation, or to the action of a new ſubſtance.

Though this certain relation in the reſults may be productive of doubt as to the theory in the foregoing caſe, it is not the ſame when there is but a ſmall diſtance between the ſolubility of the combinations that may be formed. The proportions of the ſubſtances, in quantity, then determine the formation of the different ſalts, either by a firſt cryſtallization, or by cryſtallizations which, by ſubtracting ſome of the parts, muſt alter the proportion, and occaſion a difference in the diſpoſition of the remaining principles to cryſtallize in the ſucceſſive formation of the ſalts. It is here that the application of the theory of the quieſcent and divellent affinities may produce many errors, by leading us to conclude from the commencement of the phenomenon that the ſubſequent effects will be all of the ſame deſcription, though in reality a ſucceſſion of oppoſite combinations may be eſtabliſhed, according to the forces which are made to act at the moment of their reſpective ſeparations.

The joint conſideration of the difference of ſolubility, and of the proportions employed, or which vary at the different periods of an operation, muſt conſequently be our ſole guide in the explanation of the ſucceſſive formation of different ſalts which have no great difference in ſolubility. It is true, nevertheleſs, that the mutual action of the ſubſtances themſelves may produce ſome difference in the reſults, which would be indicated by the preceding obſervations.

All ſubſtances which are in ſolution exert a mutual action, which increaſes their ſolubility. Hence the reaſon why it is difficult to obtain, by a firſt cryſtallization, each ſalt in a ſtate of purity; except in the caſe where it differs conſiderably from the others in its force of cryſtallization. Hence ariſe the uncryſtallizable reſidues which ſucceed the cryſtallizations wherein ſalts are found in the liquid, which poſſeſs but little force of coheſion. But here alſo the conſideration of the proportions and of the ſolubility will ſtill be uſeful to predict the exiſtence and compoſition of an uncryſtallizable reſiduum.

While the ſubſtances are in ſolution, the action which they mutually exert renders it eaſy to expel an acid from a combination, though, according to the received opinion, it ought to aſſume the place of another, and which was ſuppoſed to be weaker.

When inſolubility is conſidered, it muſt not be regarded as an abſolute property, but as a property relative to the liquid in which a precipitation is made. Thus a combination inſoluble in water may loſe this property when the water holds alkali in ſolution.

In all the experiments I have deſcribed, and in ſeveral others which I have thought it uſeleſs to deſcribe, I perceived no change of ſaturation, either after the mixture of the neutral ſalts, or after the ſeparation of the precipitates or cryſtallizations which took place, except in the experiments P, Q, made with a metallic ſubſtance. This permanent ſtate of neutralization, after the change of baſes which took place, ſeems to indicate that the acids have conſtant relations of quantity, in the neutral ſalts they form, with different alkaline or earthy baſes. So that if the ſulphuric acid for example, be found in greater proportion in the ſulphate of pot-aſh than in the ſulphate of lime; the muriatic acid with which it may make an exchange of baſe will be found in the ſame ratio of quantity in the muriate of lime and in the muriate of pot-aſh; a concluſion that would not agree with the proportions which have often been attributed by chemiſts to the component parts of the different non-metallic ſalts. Guyton has already made ſeveral very juſt and important reflexions on this ſubject, and he quotes the obſervations of Richeter, with whoſe work I am not yet acquainted. (*Annales de Chimie*, tom. xxv. p. 292).

(To be continued.)

The reader will perceive that I have ſuppoſed this treatiſe to have been concluded in a former number; into which error I was led by the want of any intimation in the Annals that more was to be expected. But every cultivator of ſcience will rejoice with me that theſe reſearches, which ſhew the wonderful ſkill and acuteneſs of their author, and muſt produce an almoſt total revolution in our obſervations and reaſoning on chemical effects, are more extenſive than we at firſt imagined.—N.

VII.

VII.

On the Uſe of Steam as a Vehicle for conveying Heat from one Place to another.

By COUNT RUMFORD.*

MORE than fifty years ago, Colonel William Cook, in a Paper preſented by him to the Royal Society, and publiſhed in their Tranſactions, made a propoſal for warming rooms by means of metallic tubes filled with ſteam, and communicating with a boiler ſituated out of the room; which propoſal was accompanied by an engraving, which ſhowed, in a manner perfectly clear and diſtinct, how this might be effected. Since that time this ſcheme has frequently been put in practice with ſucceſs, both in this country and on the Continent. † Many attempts have likewiſe been made, at different periods, to heat liquids by means of ſteam introduced into them; but moſt of theſe have failed: and, indeed, until it was known that fluids are nonconductors of heat, and, conſequently, that heat cannot be made to *deſcend* in them—(which is a recent diſcovery),—theſe attempts could hardly ſucceed; for, in order to their being ſucceſsful, it is abſolutely neceſſary that the tube which conveys the hot ſteam ſhould open into the *loweſt part* of the veſſel which contains the liquid to be heated, or on a level with its bottom;—but as long as the erroneous opinion obtained, that heat could paſs in fluids *in all directions*, there did not appear to be any reaſon for placing the opening of the ſteam tube *at the bottom of the veſſel*, while many were at hand which pointed out other places as being more convenient for it.

But to ſucceed in heating liquids by ſteam, it is neceſſary, not only that the ſteam ſhould enter the liquid at the bottom of the veſſel which contains it, but alſo that it ſhould enter it *coming from above*. The ſteam tube ſhould be in a vertical poſition, and the ſteam ſhould *deſcend* through it previous to its entering the veſſel, and mixing with the liquid which it is to heat; otherwiſe this liquid will be in danger of being forced back by this opening into the ſteam boiler; for the hot ſteam being ſuddenly condenſed on coming into contact with the cold liquid, a vacuum will neceſſarily be formed in the end of the tube; into which vacuum the liquid in the veſſel—preſſed by the whole weight of the incumbent atmoſphere—will ruſh with great force, and with a loud noiſe; but if this tube be placed in a vertical poſition, and if it be made to riſe to the height of ſix or ſeven feet, the liquid which is thus forced into its lower end will not have time to riſe to that height before it will be met by ſteam and obliged to return back into the veſſel.—There will be no difficulty in arranging the apparatus in ſuch a manner as effectually to prevent the liquid to be heated from

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* Journal of the Royal Inſtitution I. 34.

† Although one ſhould naturally imagine that the notoriety of theſe facts would have been ſufficient to prevent all attempts in our days to claim a right to this invention, yet it is ſaid that a patent for it was taken out only a few years ago.

being

being forced backwards into the ſteam boiler; and, when this is done, and ſome other neceſſary precautions to prevent accidents are taken, ſteam may be employed with great advantage for heating liquids; and for keeping them hot, in a variety of caſes, in which fire, applied immediately to the bottoms of the containing veſſels, is now uſed.

In dyeing, for inſtance, and in brewing; and in the proceſſes of many other arts and manufactures, the adoption of this method of applying heat would be attended, not only with a great ſaving of labour and of fuel; but alſo of a conſiderable ſaving of expence in the purchaſe and repairs of boilers, and of other expenſive machinery: for when ſteam is uſed inſtead of fire for heating their contents, boilers may be made extremely thin and light; and, as they may eaſily be ſupported and ſtrengthened by hoops and braces of iron, and other cheap materials, they will coſt but little, and ſeldom ſtand in need of repairs. To theſe advantages we may add others of ſtill greater importance: boilers intended to be heated in this manner may, without the ſmalleſt difficulty, be placed in any part of a room—at any diſtance from the fire;—and in ſituations in which they may be approached freely on every ſide. They may moreover eaſily be ſo ſurrounded with wood, or with other cheap ſubſtances which form warm covering, as moſt completely to confine the heat within them, and prevent its eſcape. The tubes by which the ſteam is brought from the principal boiler (which tubes may conveniently be ſuſpended juſt below the cieling of the room) may, in like manner, be covered, ſo as almoſt intirely to prevent all loſs of heat by the ſurfaces of them; and this to whatever diſtances they may be made to extend.

In ſuſpending theſe ſteam tubes, care muſt, however, be taken to lay them in a ſituation *not perfectly horizontal* under the cieling, but to incline them at a ſmall angle, making them riſe gradually from their junction with the top of a large vertical ſteam tube, connecting them with the ſteam boiler, quite to their fartheſt extremities: for, when theſe tubes are ſo placed, it is evident that all the water formed in them, in conſequence of the condenſation of the ſteam in its paſſage through them, will run backwards and fall into the boiler, inſtead of accumulating in them, and obſtructing the paſſage of the ſteam,—which it would not fail to do were there any conſiderable bends or wavings, upwards and downwards, in theſe tubes—or of running forward and deſcending with the ſteam into the veſſels containing the liquids to be heated;—which would happen if theſe tubes inclined *downwards*, inſtead of inclining upwards, as they recede from the boiler.

(*To be continued.*)

Mr. Cuthbertson's Experiments on the Oxidation of Metals by Electricity.

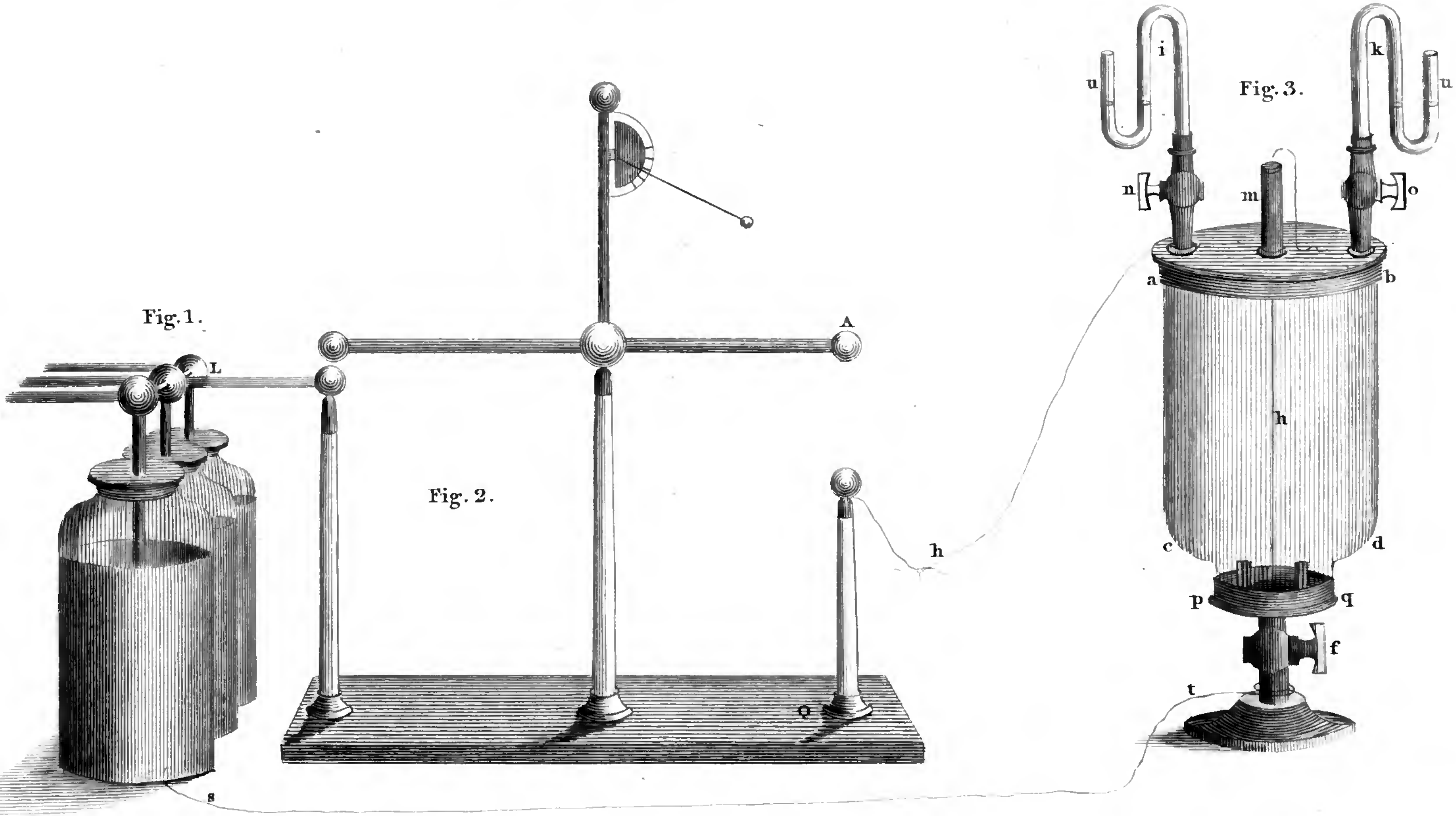


Fig. 1.

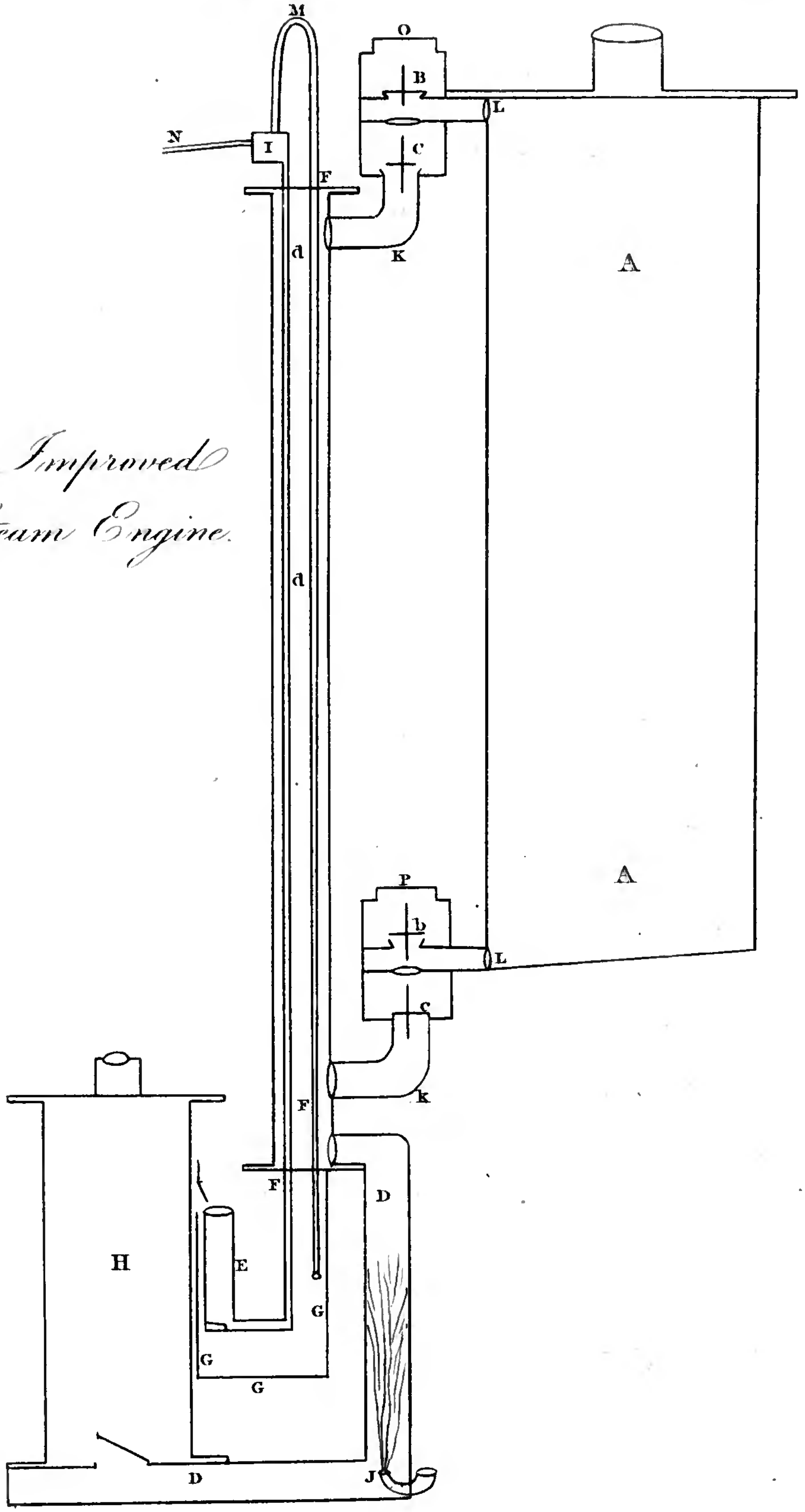
Fig. 2.

Fig. 3.

Fig. 4.

Mutlow Sc. Russell Court.

Improved Steam Engine.



Mutlow Sc. [illegible]

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

AUGUST, 1801.

ARTICLE I.

A Letter from THOMAS YOUNG, *M. D. F. R. S. Profeſſor of Natural Philoſophy in the Royal Inſtitution, reſpecting Sound and Light, and in Reply to ſome Obſervations of Profeſſor* ROBISON.

To MR. NICHOLSON.

SIR,

IN the ſupplement of the Encyclopædia Britannica, are inſerted ſeveral excellent articles by Profeſſor Robiſon of Edinburgh: one of them appears to require ſome public notice on my part, and I conſider your valuable Journal as the moſt eligible channel for ſuch a communication, eſpecially as you have lately done me the honour of reprinting the paper which gave riſe to the Profeſſor's animadverſions. But in the firſt place, I ſhall beg leave to recall the attention of your readers, by a ſummary enumeration, to the principal poſitions which I have in that paper endeavoured to eſtabliſh.

1. Sound, as tranſmitted through the atmoſphere, conſiſts in an undulatory motion of the particles of the air, Sect. III. This is generally admitted; but as the contrary has even very lately been aſſerted, it is not ſuperfluous to have deciſive evidence of the fact. Profeſſor Robiſon's experiment with the ſtop-cock furniſhes an argument nearly ſimilar.

2. A current of air, forced by a moderate preſſure through a cylindrical pipe, diverges the leſs as its velocity is leſs, Sect. II.

3. At a certain point the divergency of ſuch a current increaſes ſuddenly, and the current mixes with the ſurrounding air, Sect. II.

4. So far is ſuch a motion from ſpreading equally in all directions, that on every ſide of the current the air is urged more towards it than from it, Sect. II.

5. Sound, admitted through an aperture, does not by any means diverge equally in all directions, and is probably very weak except in directions nearly rectilinear. From poſition 2 and 4, and from experience, Sect. VI.

6. Sound probably decays in the duplicate ratio of the diſtance, Sect. VII.

7. A ſimilar blaſt of air produces nearly a ſimilar ſound, in organ pipes properly commenſurate, Sect. VIII.

8. Light is probably the undulation of an elaſtic medium, Sect. X.

A. Becauſe its velocity in the ſame medium is always equal.

B. Becauſe all refractions are attended with a partial reflection.

C. Becauſe there is no reaſon to expect that ſuch a vibration ſhould diverge equally in all directions, and becauſe it is probable that it does diverge in a ſmall degree in every direction.

D. Becauſe the diſperſion of differently coloured rays is no more incompatible with this ſyſtem than with the common opinion, which only aſſigns for it the nominal cauſe of different elective attractions.

E. Becauſe refraction and reflection in general are equally explicable on both ſuppoſitions.

F. Becauſe inflection is as well, and, it may be added, even much better explained by this theory.

G. Becauſe all the phænomena of the colours of thin plates, which are in reality totally unintelligible on the common hypotheſis, admit a very complete and ſimple explanation by this ſuppoſition. The analogy which is here ſuperficially indicated will probably ſoon be made public more in detail; and will alſo be extended to the colours of thick plates, and to the fringes produced by inflection, affording, from Newton's own elaborate experiments, a moſt convincing argument in favour of this ſyſtem.

9. The particles of air may be jointly actuated by two or more ſounds, and in this caſe, the ſeveral motions are to be added or ſubtracted, in order to find the actual joint motion, Sect. XI.

10. The grave harmonic produced by a major third is accompanied by a very audible twelfth. This circumſtance is explained, and the effect of ſubordinate notes and ſubaltern ſtops, on the quality of ſounds, is ſhown by figures, Sect. XI.

11. A noiſe returning every ſecond, if audible, would be a C. From Sauveur; with an experiment, Sect. XII.

12. A chord retains always the form of its initial vibration. From experiments, in favour of Euler's theorem, againſt the ſimple harmonic curve, Sect. XIII.

13. The

13. The vibration of a chord is ſcarcely ever performed in the ſame plane. Its revolutions, and its ſubordinate vibrations, may be rendered diſtinctly viſible under the microſcope, Sect. XIII.

14. If a chord be inflected at any point of aliquot diviſion, the harmonic ſecondary note correſponding to that diviſion will not be audible; an experiment contradictory to ſome theories of the origin and of the inſeparable nature of harmonic ſounds, Sect. XIII.

15. The human voice is analogous to the organ pipe denominated from it, which conſiſts of a tongue piece without any commenſurate tube: and the falſetto is probably formed by the upper orifice of the trachea aſſuming the functions of the glottis, Sect. XV.

16. A temperament of progreſſive imperfection is the moſt convenient for practical muſic, and is eaſily approximated by tuning ſix perfect, and ſix equally imperfect fifths, Sect. XVI.

From the detached nature of the ſubjects which I have here enumerated, and the imperfect ſtate of theſe branches of the mathematics to which they refer, it would have been in vain to attempt a very perſpicuous and detailed diſcuſſion of them. My reſearches on theſe ſubjects have been much interrupted, and probably will not be very ſhortly reſumed; but if they are of no further uſe to any perſon, I ſhall not think my labour loſt; for I flatter myſelf that the inferences, which they have led me to draw reſpecting the theory of colours, will throw new light on all the moſt intereſting parts of optics, while by a compariſon with the obvious inferences from Dr. Herſchel's important diſcoveries, they will alſo lead to ſome material illuſtrations of the phænomena of heat.

I ſhall now trouble you with ſome remarks in reply to Profeſſor Robiſon: the paſſage to which I allude is this:

"We are ſurpriſed to ſee this work of Dr. Smith greatly undervalued, by a moſt ingenious gentleman in the Philoſophical Tranſactions for 1800, and called a large and obſcure volume, which leaves the matter juſt as it was, and its reſults uſeleſs and impracticable. We are ſorry to ſee this; becauſe we have great expectations from the future labours of this gentleman in the field of harmonics, and his late work is rich in refined and valuable matter. We preſume humbly to recommend to him attention to his own admonitions to a very young and ingenious gentleman, who, he thinks, proceeded too far in animadverting on the writings of Newton, Barrow, and other eminent mathematicians." Encyclop. Brit. Suppl. Art. Temperament, p. 652.

According therefore to the author of this article, I have in the firſt place taken the liberty of giving ſevere advice, to a young mathematician who had never aſked it; ſecondly, this advice is equally applicable to my own preſumption; and thirdly, Dr. Smith's treatiſe on harmonics is a work intitled to the higheſt praiſe.

I did in fact endeavour to ſhew, that the gentleman in queſtion had overlooked the labours of ſome former authors relative to his ſubject, but I accompanied my remarks with nothing like admonition. After obſerving that at preſent a geometrician may very eaſily fancy he has made diſcoveries, when the ſame facts were known and forgotten long before he exiſted, I proceeded in theſe words:

" An inſtance of this has lately occurred to a young gentleman in Edinburgh, a man who certainly promiſes in the courſe of time to add conſiderably to our knowledge of the laws of nature. The tractory—was firſt deſcribed by Huygens: Bomie and Perks have ſhewn, that it is the involute of the catenaria.—The curve which the author calls a cycloid is the companion of a trochoid, and is only a diſtortion of the figure by which Newton had very ſimply and elegantly ſolved the ſame problem.—On the whole it appears, that this ingenious gentleman has been ſomewhat unfortunate in the choice of thoſe problems, which he has ſelected as ſpecimens of the elegance of the modern mode of demonſtration." Eſſay on cycloidal curves, Britiſh magazine for April 1800. Is this an admonition? Or is there here any remark which can at all apply to my inſenſibility to Dr. Smith's merit in harmonics?

I have read Dr. Smith's work with attention, and I imagine, from the polite manner in which Profeſſor Robiſon is pleaſed to ſpeak of my eſſay, he will not heſitate to allow that I have underſtood it. I took it up with great expectations; theſe expectations having been completely diſappointed, I thought it right to ſtate my cool and unprejudiced opinion of its merits, in order to prevent a ſimilar diſappointment in others. It is impoſſible therefore that an "attention" to any "admonitions" of a general nature, wherever they may be found, can influence ſuch an opinion; and ſo far only as I am ſuppoſed to be an incompetent judge on the ſubject of harmonics, can it be aſſerted that it was either blameable or ſuperfluous for me to expreſs that opinion. As a mathematician and an optician I value Dr. Smith highly, but I muſt ſtill beg leave to affirm, that his whole book of harmonics contains far, far leſs information, than either of the articles Temperament and Trumpet in the ſupplement of the Encyclopædia.

I do not mean to be underſtood that this work is ſo contemptible, as not to contain the leaſt particle of important matter; but it appears to me that its errors counterbalance its merits. The only improvement on which Profeſſor Robiſon himſelf ſeems to ſet a high value, is the application of the phænomena of beats to tuning an inſtrument: on the other hand, I conceive that the miſtatement relative to the non-interference of different ſounds, is an inaccuracy which far outweighs the merit of Dr. Smith's ſhare of that improvement. I have aſſerted, that " Dr. Smith has written a large and obſcure volume, which for every " purpoſe but for the uſe of an impracticable inſtrument, leaves the whole ſubject of tem- " perament preciſely where it found it," and that " the ſyſtem propoſed for his changeable " harpſichord is neither in that nor in any other form capable of practical application." Profeſſor Robiſon on the contrary ſays, " we do not ſee how it can be diſputed, that Dr. " Smith's theory of the beating of imperfect conſonances, is one of the moſt important " diſcoveries both for the practice and the ſcience of muſic, that have been offered to the " public. We are inclined to conſider it as the moſt important that has been made ſince " the days of Galileo. We are obliged to call it his diſcovery. Merſennus indeed had " taken particular notice of this undulation of imperfect conſonances, and had offered con- " jectures as to their cauſes; conjectures not unworthy of his great ingenuity. Mr. Sauveur " alſo takes a ſtill more particular notice of this phænomenon, and makes a moſt ingenious

" uſe

"uſe of it for the ſolution of a very important muſical problem." P. 652 and 651. Why then are we obliged to call it Dr. Smith's diſcovery, or indeed any diſcovery at all? Sauveur had already given directions for tuning an organ pipe by means of the rapidity of its beating with others. Mem. de l'Ac. 1701. p. 475. Ed. Amſt. Dr. Smith ingeniouſly enough extended the method: but it appears to me that the extenſion was perfectly obvious, and wholly undeſerving of the name either of a diſcovery or of a theory. If Profeſſor Robiſon thinks otherwiſe, there is nothing further to be ſaid; but in all probability Dr. Smith conſidered this improvement as a very inconſiderable part of the merit of his treatiſe. No doubt an organ may be more accurately tuned by counting the beats than by any other method, although it may be queſtioned, whether the advantage of counting the abſolute frequency of the beats will ever practically compenſate the tediouſneſs of the proceſs.

It remains to be conſidered, whether Dr. Smith's changeable harpſichord is or is not an impracticable inſtrument; for, whatever Signor Doria might exclaim, Dr. Smith himſelf does not recommend his ſcale for common uſe. It is the opinion of many unprejudiced practical perſons, that all occaſional introductions of different ſemi-tones is perfectly impracticable; and ſome who have heard the effect of Dr. Smith's inſtrument, have declared that to them it was by no means agreeable. And indeed if we pay ſufficient attention to the paſſages and modulations of the greateſt compoſers, we ſhall be convinced, that granting all poſſible dexterity in the performer, it would be abſolutely impracticable to adapt them to an inſtrument, ſo different from that for which they were compoſed, as Dr. Smith's is from the common harpſichord. It may eaſily be conceived, that an organ very correctly tuned, as Mr. Watts's probably was, for a particular key, might appear "ſopra modo belliſſimo" in that key; but the ſequel of the ſtory ſhews literally what Dr. Smith has allowed, that his temperament is inapplicable to our inſtruments, ſince it was utterly impoſſible to ſing with it in the key of E e s, a key of exceedingly frequent occurrence. I have been informed on the beſt authority, that Dr. Smith reſtricted the organiſt of Trinity College to ſuch keys and modulations, as were beſt ſuited to the ſyſtem by which the organ was tuned; and that organ, as well as the inſtruments which were made for Dr. Smith, has long been tuned according to the more common method.

I ſpoke of Dr. Smith's ſyſtem with flattened major thirds as of no value, not with regard to its intrinſic merits, but becauſe it was not intended for any inſtrument in common uſe; ſince in theſe inſtruments the difficulty is not ſo much how to divide the imperfection among the thirds and fifths of the ſame ſcale, as to proportion properly the imperfections of the thirds of different keys. Yet I do not mean it to be underſtood, that I can agree to the ſolidity of thoſe foundations on which Dr. Smith has built his ſyſtem for a ſingle ſcale: although to Stanley and to Doria it might be pleaſing, becauſe its imperfections are far too ſmall to offend the ear. Profeſſor Robiſon juſtly obſerves, that different perſons differ exceedingly in their eſtimation of the effect of the ſame temperament on different concords, and that much of this ariſes from their different diſpoſitions: it appears therefore that Dr.
Smith

Smith was too precipitate in laying down his principle for the compariſon of the effects of temperament.

With reſpect to the ſyſtem which I have propoſed, Profeſſor Robiſon thinks that the temperaments of ſeveral of the thirds which occur frequently are much too great. If we wiſh to form a judgment of any ſyſtem of temperament, it muſt be by compariſon with ſome other. It does not appear with what ſyſtem Profeſſor Robiſon would wiſh the compariſon to be made, but he rather ſeems to incline to the equal temperament, although he gives directions for tuning by another. At any rate, no temperament of an interval can be ſaid to be much too great, unleſs it be greater than that of the ſame interval in the ſyſtem of equal temperament; for if any interval be made more perfect than this, ſome other ſimilar interval muſt be as much leſs perfect. In my ſyſtem, the only thirds perceptibly greater than thoſe of the equal temperament, are the major thirds on E, Aes, B, Cis, and Fis, and the minor on C, Cis, F, Gis, Bes, and Ees. Of theſe none can be ſaid to occur frequently, except the major third on E, and the minor on C. The ſixths require no ſeparate conſideration. Now ſince the minor chord is intended to be leſs completely harmonious than the major, its character will be by no means materially impaired by this imperfection, which it would be ſomewhat difficult to remove. The third on E is not ſharp enough to be very offenſive, but in compliance with the uſual practice of making this third ſomewhat more perfect than the interval of Aes and C, I have, in the method recommended for common uſe, made it a better third than that of the equal temperament. The directions given for tuning in § 68, and in § 80, of the article, are liable to far greater objections. For inſtance, the temperament of the IIIds on Aes and Fis, in the latter, is about .00880, or more than a comma and a half; which Profeſſor Robiſon will readily allow to be "much too great" for any thirds; ſince he has aſſerted, with Dr. Smith and others, that the error of a comma would be intolerable. Mr. Maxwell has, however, very decidedly proved, in his Eſſay on Tune, that the greateſt harmoniſts, Corelli, Tartini, and Giardini, have admitted very frequently the error of a comma in their moſt refined compoſitions. And I have the authority of ſeveral celebrated performers on ſtringed and wind inſtruments for aſſerting, that they take of choice the characteriſtic ſemi-tone, leading into the key note, conſiderably ſharper than the ſame note is tuned on any keyed inſtruments, making an imperfection of nearly two commas in the relation as third of the dominant, which is the fundamental note of the chord: while in the mean time our theoriſts have been labouring, by the moſt complicated contrivances, to introduce notes into keyed inſtruments, which ſhall have exactly a contrary effect, by making the aſcending ſemi-tone as wide a ſtep as poſſible. On aſking very lately the opinion of a practical muſician of great eminence, and one, who in every reſpect does honour to his profeſſion, he decidedly agreed in the ſuperiority of ſuch a diminiſhed ſemi-tone, and obſerved that the key of E derived a very elegant character from the uſual method of tuning Dis as Ees, a minor third to C: hence, the IIIds Ees and G being very little tempered, the IIId on the dominant B muſt be be about a comma and a half too ſharp. The fact is, that in this caſe the harmony is ſomewhat impaired in order to improve the melody. The interval of 15 to 16 is far too ſmall to be diſtinctly conceived as commenſurate, it poſſeſſes therefore no melody in virtue of the perfection of its ratio, and a certain elegance of expreſſion is added by approaching to the natural and colloquial aſcent of a voice by imperceptible degrees. It muſt, however, be confeſſed, that ſome excellent muſicians prefer a purer harmony, and in this, as in all other matters of taſte, conſiderable latitude muſt be allowed for the habits and predilections of individuals.

I am, SIR,
With great reſpect,
Your obedient humble ſervant,
THOMAS YOUNG.

No. 48, *Welbeck-Street*,, *July* 13, 1801.

ERRATA IN THE PAPER ON SOUND AND LIGHT.

In Table XII, *for* E *b* 838·0 *read* E *b* 84197; *for* .0011562 *read* .0010116. In Fig. 53 the E *b* (Q) is too near D, and the E *b* (Y) ſhould be above inſtead of below it.

II.

On certain Points of Nomenclature. By a Correſpondent.

To Mr. NICHOLSON.

SIR,

London, *July* 15, 1801.

THE gas at this time, whoſe effects on breathing it are an object of great curioſity among chemiſts and amateurs, denominated by its diſcoverer Dr. Prieſtley, *dephlogiſticated nitrous gas*, and ſubſequently by the Dutch philoſophers who inveſtigated many of its properties, called *oxide of azote gas*, or *gaſeous oxide of azote*, is now univerſally ſpoken of by the name *gaſeous oxide*. The very ingenious and promiſing philoſopher, and expert experimenter, whom the managers of the Royal Inſtitution have engaged, has detected the miſtake of Deiman, Trooſtwyk &c. who found out that it was capable of ſupporting *inflammation*, but erroneouſly concluded, that it could not poſſeſs *reſpiration*. Mr. Davy thereupon very juſtly enjoys the honor of the diſcovery of this, and other new properties of this oxide; but my object now is only to obſerve, that the term *gaſeous oxide* carries no uſeful and diſcriminating import, but what is worſe than no import at all, it is very liable to produce miſtake and confuſion; as was the caſe with the terms *fixed air*, *aerial acid*, &c. formerly

formerly uſed. It appears from the analyſis of Deiman and Davy, that the oxide under obſervation conſiſts of above 0,37 of oxigen, and 0,63 of azote or nitrogen. Secondly, That *nitrous gas*, or nitrous oxide conſiſts of about 0,68 of oxigen, and 0,32 of azote. Accordingly Deiman diſtinguiſhes theſe two oxides by the names, *oxide of azote*, or of *nitrogen*, and *nitrous oxide*, which terms are little elſe but an inverſion mutually of their order, and beſides have no ſpecific import. Is it worth while to propoſe the appellations, *ſub-oxide of nitrogen*, or *azote*, and *oxide of nitrogen*, or *azote*, in place of the names in uſe? This method is analogous to the mode of the new nomenclature in its lateſt improvements, as in the inſtances ſub-muriate, and muriate of mercury, ſub-carbonate, and carbonate of alkali, ſub-phoſphate, and phoſphate of lime, &c.

I am,

YOUR FRIEND.

III.

On the Uſe of Steam as a Vehicle for conveying Heat from one Place to another.

By COUNT RUMFORD.

(*Continued from page* 160.)

IN order that clear and diſtinct ideas may be formed of the various parts of this apparatus,—even without figures,—I ſhall diſtinguiſh each part by a ſpecific name: the veſſel in which water is boiled in order to generate ſteam—and which, in its conſtruction, may be made to reſemble the boiler of a ſteam engine—I ſhall call the *ſteam boiler*:—The vertical tube, which, riſing up from the top of the boiler, conveys the ſteam into the tubes (nearly horizontal), which are ſuſpended from the cieling of the room, I ſhall call the *prime conductor*: to the horizontal tubes I ſhall give the name of *horizontal conductors*, or ſimply, *conductors of ſteam*: and to the (ſmaller) tubes, which, deſcending perpendicularly from theſe *horizontal conductors*, convey the ſteam to the liquids which are to be heated, I ſhall, excluſively, appropriate the appellation of *ſteam tubes*.

The veſſels in which the liquids are put that are to be heated I ſhall call the *containing veſſels*.—Theſe veſſels may be made of any form; and, in many caſes, they may, without any inconvenience, be conſtructed of wood, or of other cheap materials, inſtead of being made of coſtly metals, by which means a very heavy expence may be avoided.

Each *ſteam tube* muſt deſcend perpendicularly from the *horizontal conductor* with which it is connected, to the level of the bottom of the *containing veſſel* to which it belongs; and, moreover, muſt be furniſhed with a good braſs cock, perfectly ſteam-tight; which may beſt be placed at the height of about ſix feet above the level of the floor of the room.

This

This *ſteam tube* may either deſcend *within the veſſel* to which it belongs, or *on the outſide of it*, as ſhall be found moſt convenient. If it comes down on the outſide of the veſſel, it muſt enter it at its bottom, by a ſhort horizontal bend; and its junction with the bottom of the veſſel muſt be well ſecured, to prevent leakage. If it comes down into the veſſel, on the inſide of it, it muſt deſcend to the bottom of it, or at leaſt to within a very few inches of the bottom of it ; otherwiſe the liquid in the veſſel will not be uniformly and equally heated.

When the ſteam tube is brought down on the inſide of the containing veſſel, it may either come down perpendicularly, and without touching the ſides of it, or it may come down on one ſide of the veſſel, and in contact with it.

When ſeveral ſteam tubes, belonging to different containing veſſels, are connected with one and the ſame horizontal ſteam conductor, the upper end of each of theſe tubes, inſtead of being ſimply attached by ſoldering to the under ſide of the conductor, muſt enter, at leaſt one inch, within the cavity of it; otherwiſe the water reſulting from a condenſation of a part of the ſteam in the conductor, by the cold air which ſurrounds it, inſtead of finding its way back into the ſteam boiler, will deſcend through the ſteam tubes and mix with the liquids in the veſſels below; but when the open ends of theſe tubes project upwards within the ſteam conductor, though it be but to a ſmall height above the level of its under ſide, it is evident that this accident cannot happen.

It is not neceſſary to obſerve here, that, in order that the ends of the ſteam tubes may project within the *horizontal conductor*, the diameters of the former muſt be conſiderably leſs than the diameter of the latter.

To prevent the loſs of heat ariſing from the cooling of the different tubes through which the ſteam muſt paſs in coming from the boiler, all thoſe tubes ſhould be well defended from the cold air of the atmoſphere, by means of warm covering; but this may eaſily be done, and at a very trifling expence. The horizontal conductors may be encloſed within ſquare wooden tubes, and ſurrounded on every ſide by charcoal duſt,—fine ſawduſt,—or even by wool; and the ſteam tubes, and prime conductor, may be ſurrounded firſt by three or four coatings of ſtrong paper, firmly attached to them by paſte or glue, and covered with a coating of varniſh, and then by a covering of thick coarſe cloth. It will likewiſe be adviſable to cover the horizontal conductors with ſeveral coatings of paper, for if the paper be put on to them while it is wet with the paſte or glue; and if care be taken to put it on in long ſlips or bands, wound regularly round the tube in a ſpiral line, from one end of it to the other, this covering will be uſeful, not only by confining more effectually the heat, but alſo by adding very much to the ſtrength of the tube, and rendering it unneceſſary to employ thick and ſtrong ſheets of metal in the conſtruction of it.

However extraordinary and incredible it may appear, I can aſſert it as a fact, which I have proved by repeated experiments, that if a hollow tube, conſtructed of ſheet copper $\frac{1}{20}$ of an inch in thickneſs, be covered by a coating of only twice as thick, or $\frac{1}{10}$ of an inch in thickneſs, formed of layers of ſtrong paper, firmly attached to it by good glue, the ſtrength of the tube will be *more than doubled* by this covering. I found by experiments, the moſt unexceptionable and deciſive, of which I intend at ſome future period to give to the public a full and detailed account, that the ſtrength of paper is ſuch, when ſeveral ſheets of it are firmly attached together with glue, that a ſolid cylinder of this ſubſtance, the tranſverſe ſection of which ſhould amount to only one ſuperficial inch, would ſuſtain a weight of 30,000 lbs. avoirdupois, or above 13 tons, ſuſpended to it, without being pulled aſunder or broken. The ſtrength of hemp is ſtill much greater, when it is pulled equally, in the direction of the length of its fibres. I found, from the reſults of my experiments with this ſubſtance, that a cylinder of the ſize above mentioned, compoſed of the ſtraight fibres of hemp, glued together, would ſuſtain 92000 lbs. without being pulled aſunder.

A cylinder, of equal dimenſions, compoſed of the ſtrongeſt iron I could ever meet with, would not ſuſtain more than 66000 lbs. weight; and the iron muſt be very good not to be pulled aſunder with a weight equal to 55000 lbs. avoirdupois.

I ſhall not, in this place, enlarge on the many advantages that may be derived from a knowledge of theſe curious facts. I have mentioned them now in order that they may be known to the public; and that ingenious men, who have leiſure for theſe reſearches, may be induced to turn their attention to a ſubject, not only very intereſting, on many accounts, but which promiſes to lead to moſt important improvements in mechanics.

I cannot return from this digreſſion without juſt mentioning one or two reſults of my experimental inveſtigations relative to the force of coheſion, or ſtrength of bodies, which, certainly, are well calculated to excite the curioſity of men of ſcience.

The ſtrength of bodies of different ſizes, *ſimilar in form*, and compoſed of the *ſame ſubſtance*, or the forces by which they reſiſt being pulled aſunder by weights ſuſpended to them, and acting in the direction of their lengths,—*are not in the ſimple ratio of the areas of their tranſverſe ſections*, or of their *fractures*; but in a higher ratio; and this ratio is different in different ſubſtances.

The *form* of a body has a conſiderable influence on its ſtrength, *even when it is pulled in the direction of its length.*

All bodies, even the moſt brittle, appear to be *torn aſunder*, or their particles ſeparated, or fibres broken, *one after the other*; and hence it is evident, that that *form* muſt be moſt favourable to the ſtrength of any given body, pulled in the direction of its length, which enables the greateſt number of its particles, or longitudinal fibres, to be ſeparated to the greateſt poſſible diſtance—ſhort of that at which the force of coheſion is overcome,—before any of them have been forced beyond that limit.

It is more than probable that the apparent ſtrength of different ſubſtances depends much more on the number of their particles that come into action before any of them are forced beyond the limit of the attraction of coheſion, than on any ſpecific difference in the intenſity of that force in thoſe ſubſtances.

But to return to the ſubject more immediately under conſideration.—As it is eſſential that the ſteam employed in heating liquids, in the manner before deſcribed, ſhould enter the

the containing veſſel at, or very near, its bottom, it is evident that this ſteam muſt be ſufficiently ſtrong, or elaſtic, to overcome, not only the preſſure of the atmoſphere, but alſo the additional preſſure of the ſuperincumbent liquid in the veſſel; the ſteam boiler muſt, therefore, be made ſtrong enough to confine the ſteam, when its elaſticity is ſo much increaſed by means of additional heat, as to enable it to overcome that reſiſtance. This increaſe of the elaſtic force of the ſteam need not, however, in any caſe, exceed a preſſure of five or ſix pounds upon a ſquare inch of the boiler, or *one third part*, or *one half*, of an atmoſphere.

It is not neceſſary for me to obſerve here, that in this, and alſo in all other caſes, where ſteam is uſed as a vehicle for conveying heat from one place to another, it is indiſpenſably neceſſary to provide *ſafety valves* of two kinds;—the one for letting a part of the ſteam eſcape, when, on the fire being ſuddenly increaſed, the ſteam becomes ſo ſtrong as to expoſe the boiler to the danger of being burſt by it;—the other for admitting air into the boiler, when, in conſequence of the diminution of the heat, the ſteam in the boiler is condenſed, and a vacuum is formed in it; and when, without this valve, there would be danger, either of having the ſides of the boiler cruſhed, and forced inwards by the preſſure of the atmoſphere from without; or of having the liquid in the containing veſſels forced upwards into the horizontal ſteam conductors, and from thence into the ſteam boiler. This laſt mentioned accident, however, cannot happen, unleſs the cocks in ſome of the ſteam tubes happen to be open.—The two valves effectually prevent all accidents.

The reader will, no doubt, be more diſpoſed to pay attention to what has here been advanced, on this intereſting ſubject, when he is informed that the propoſed ſcheme has already been executed on a very large ſcale, and with complete ſucceſs; and that the above details are little more than exact deſcriptions of what actually exiſts.

A great mercantile and manufacturing houſe at Leeds, that of Meſſrs. Gott, and Co. had the courage, notwithſtanding the mortifying prediction of all their neighbours, and the ridicule with which the ſcheme was attempted to be treated, to erect a *dyeing houſe*, on a very large ſcale indeed, on the principles here deſcribed and recommended.

On my viſit to Leeds the laſt ſummer, I waited on Mr. Gott, who was then mayor of the town, and who received me with great politeneſs, and ſhewed me the cloth halls, and other curioſities of the place; but nothing he ſhewed me intereſted me half ſo much as his own truly noble manufactory of ſuperfine woollen cloths. I had ſeen few manufactories ſo extenſive, and none ſo complete in all its parts. It was burnt to the ground the year before I ſaw it, and had juſt been rebuilt, on a larger ſcale; and with great improvements in almoſt every one of its details. The reader may eaſily conceive that I felt no ſmall degree of ſatisfaction on going into the dyeing houſe, to find it fitted up on principles which I had had ſome ſhare in bringing into repute, and which Mr. Gott told me he had adopted in conſequence of the information he had acquired in the peruſal of my *ſeventh* eſſay. He aſſured me that the experiment had anſwered, even far beyond his moſt ſanguine expectations; and, as a ſtrong proof of the utility of the plan, he told me, that his next door neighbour, who is a dyer by profeſſion, and who, at firſt, was ſtrongly prejudiced againſt theſe innovations, has lately adopted them, and is now convinced that they are real improvements. Mr. Gott aſſured me that he had no doubt but that they would be adopted by every dyer in Great Britain in the courſe of a very few years.

The dyeing houſe of Meſſrs. Gott and Co. which is ſituated on the ground floor of the principal building of the manufactory, is very ſpacious, and contains a great number of coppers of different ſizes; and as theſe veſſels, ſome of which are very large, are diſtributed about promiſcuouſly, and apparently without any order in their arrangement, in two ſpacious rooms, each copper appearing to be inſulated, and to have no connection whatever with the others, all of them together form a very ſingular appearance. The rooms are paved with flat ſtones, and the brims of all the coppers, great and ſmall, are placed at the ſame height, about three feet, above the pavement: ſome of theſe coppers contain upwards of 1800 gallons; and they are all heated by ſteam from one ſteam boiler, which is ſituated in a corner of one of the rooms.

The horizontal tubes which ſerve to conduct the ſteam from the boiler to the coppers are ſuſpended juſt below the cielings of the rooms: they are made, ſome of lead, and ſome of caſt iron; and are from four to five inches in diameter; but when I ſaw them, they were naked, or without any covering to confine the heat. On my obſerving to Mr. Gott that coverings for them would be uſeful, he told me that it was intended that they ſhould be covered, and that coverings would be provided for them.

The vertical *ſteam tubes*, by which the ſteam paſſes down from the horizontal *ſteam conductors* into the coppers, are all conſtructed of lead, and are from $\frac{3}{4}$ of an inch to $2\frac{1}{2}$ inches in diameter; being made larger or ſmaller according to the ſizes of the coppers to which they belong. Theſe ſteam tubes all paſs down on the *outſides* of their coppers; and enter them horizontally at the level of their bottoms. Each copper is furniſhed with a braſs cock, for letting off its contents; and it is filled with water from a ciſtern at a diſtance, which is brought to it by a leaden pipe. The coppers are all ſurrounded by thin circular brick walls, which ſerve not only to ſupport the coppers, but alſo to confine the heat.

The rapidity with which theſe coppers may be heated, by means of ſteam, is truly aſtoniſhing. Mr. Gott aſſured me that one of the largeſt of them, containing upwards of 1800 gallons, when filled with cold water from the ciſtern, requires no more than *half an hour* to heat it till it actually boils!—By the greateſt fire that could be made under ſuch a copper, with coals, it would hardly be poſſible to make it boil in leſs than hour.

It is eaſy to perceive that the *ſaving of time* which will reſult from the adoption of this new mode of applying heat will be very great;—and it is likewiſe evident that it may be increaſed, almoſt without limitation, merely by augmenting the diameter of the ſteam tube: Care muſt, however, be taken that the boiler be ſufficiently large to furniſh the quantities of ſteam required. The *ſaving of fuel* will alſo be very conſiderable: Mr. Gott informed me that, from the beſt calculation he had been able to make, it would amount to near two thirds of the quantity formerly expended, when each copper was heated by a ſeparate fire.

But

But theſe ſavings are far from being the only advantages that will be derived from the introduction of theſe improvements in the management of heat: there is one, of great importance indeed, not yet mentioned, which alone would be ſufficient to recommend the very general adoption of them. As the heat communicated by ſteam can never exceed the mean temperature of boiling water by more than a very few degrees, the ſubſtances expoſed to it can never be injured by it. In many arts and manufactures this circumſtance will be productive of great advantages, but in none will its utility be more apparent than in cookery; and eſpecially in public kitchens, where great quantities of food are prepared in large boilers; for, when the heat is conveyed in this manner, all the labour now employed in ſtirring about the contents of thoſe boilers, to prevent the victuals from being ſpoiled by burning to the bottoms of them, will be unneceſſary; and the loſs of heat occaſioned by this ſtirring, prevented; and, inſtead of expenſive coppers, or metallic boilers, which are difficult to be kept clean, and often ſtand in need of repairs, common wooden tubs may, with great advantage, be uſed as culinary veſſels; and their contents may be heated by *portable fire-places,* by means of ſteam boilers attached to them.

As theſe portable fire-places and their ſteam boilers may, without the ſmalleſt inconvenience, be made of ſuch weight, form, and dimenſions, as to be eaſily tranſported from one place to another by two men, and be carried through a door-way of the common width, with this machinery, and the ſteam tubes belonging to it, and a few wooden tubs, a complete public kitchen, for ſupplying the poor and others, with ſoups; and alſo with puddings, vegetables, meat, and all other kinds of food prepared by *boiling*, might be eſtabliſhed in half an hour, in any room, in which there is a chimney (by which the ſmoke from the portable fire-place can be carried off); and, when the room ſhould be no longer wanted as a kitchen, it might, in a few minutes, be cleared of all this culinary apparatus, and made ready to be uſed for any other purpoſe.

This method of conveying heat is peculiarly well adapted for heating baths: it is likewiſe highly probable that it would be found uſeful in the bleaching buſineſs, and in waſhing linen. It would alſo be very uſeful in all caſes where it is required to keep any liquid at about the boiling point for a long time without making it boil; for the quantity of heat admitted may be very nicely regulated by means of the braſs cock belonging to the ſteam tube. Mr. Gott ſhewed me a boiler in which ſhreds of ſkins were digeſting in order to make glue, which was heated in this manner; and in which the heat was ſo regulated, that, although the liquid never actually boiled, it always appeared to be upon the very point of beginning to boil.

This temperature had been found to be beſt calculated for making good glue. Had any other *lower* temperature been found to anſwer better, it might have been kept up with the ſame eaſe, and with equal preciſion, by regulating properly the quantity of ſteam admitted.

I need not ſay how much this country is obliged to Mr. Gott, and his worthy colleagues. To the ſpirited exertions of ſuch men, who abound in no other country, we owe one of the proudeſt diſtinctions of our national character; that of being an enlightened and an enterpriſing people.

IV. *On*

IV.

On the Identity of Galvaniſm and Electricity. By a Correſpondent.

Edinburgh, June 20, 1801.

To Mr. NICHOLSON.

SIR,

I HOPE you will not conſider me as one ſtruggling againſt the conviction of truth, when I inform you, that I have repeated Mr. Cruickſhank's experiment, of charging a vial with Galvaniſm ſuccefsfully; but am inclined to draw a very different concluſion from his, in conſequence of the phenomenon that preſents itſelf, both during the performance of that experiment, and when it is enlarged and extended on the ſame principle.

The mode that I firſt uſed, was that of filling a common vial with ſalt water, which was plunged in another veſſel alſo filled with ſalt water; though no great nicety was uſed, yet ſome attention was paid to the ſurfaces of the two liquors being nearly upon a level, and by that means it very well repreſented an electrical vial, coated to the ſame height within and without; then the inſide was connected with one end of the pile, and the outſide with the other. After different intervals of time, ſome longer, and ſome ſhorter, the longeſt of which extended to ſeveral hours, the vial was lifted out in one hand intirely from the veſſel in which it ſtood, and the tongue applied to the wire connected with the inſide; the conſtant reſult was, that whether it ſtood for a longer, or ſhorter period, a ſlight ſhock, or rather taſte, was felt on the tongue. Expecting to increaſe this trifling ſenſation, by multiplying the number of the vials, I made a range of a dozen communicating all the inſides, and all the outſides together by wire; after trying this mode by every means my imagination could ſuggeſt, I could perceive no increaſe of power whatever, but only the ſlight ſenſation, perceptible by means of one vial only.

The concluſion I am inclined to draw from the above facts is, that Galvaniſm cannot be accumulated by the ſame proceſs with electricity, but that merely a ſmall proportion of it can be communicated to fluids, and that conſequently a marked diſtinction ſubſiſts between them, the Galvanic charge probably not being a fiftieth part of the power reſiding in the pile; whereas the electrical charge is oftener many hundred times beyond the power of the machine that imparts it; and that the former can be increaſed only, by increaſing the original power.

I have alſo repeated my former experiments of charging an electrical vial through the inſulated pile; and to determine the matter with the greateſt accuracy, I made uſe of Lane's electrometer; when its knobs were ſet at a given diſtance, by means of the graduated wire, the diſcharge conſtantly took place at the ſame number of revolutions of the machine, which-ever wire was connected with the jar, provided the machine was in equally

equally good order, and the weather equally favourable; but whenever an increaſe or diminution was obſerved upon trying one wire, a ſimilar change was perceived, upon changing it for the other.

I have alſo found, that plants cannot be altogether ſaid to be non-conductors of Galvaniſm; by connecting one end of the pile with the ſtem of a plant, and taking the other wire in the hand. When the tongue was applied to any of the leaves, even at a conſiderable diſtance from the ſtem, a ſlight taſte was perceived; but though I allowed the ſame plant to remain for above twelve hours with the two ends of rhe pile connected with different parts of it, it ſuſtained no injury whatever. It is to be obſerved, that the pile was ſtrong enough to give a very ſevere ſhock through ſeveral human bodies.

I think the reſult of the above muſt lead us to conclude, that though a ſmall quantity of electricity may attend the phenomena of Galvaniſm, (and there is ſcarcely an operation in nature wherein a ſmall quantity is not produced, and as you yourſelf have obſerved, that the very motions of the human body, and the drapery that covers it, will produce ſome:) yet that the principal phenomena of Galvaniſm, viz. the ſhock, the ſpark, and the ſnap, are not electrical, and we muſt here uſe the ſame mode of reaſoning, that we would in chemiſtry, that when one of the leading properties of any body was abſent, that it muſt have loſt it, either by combination with another, or by decompoſition; but as you are inclined to attribute the want of the effect of attracting light bodies to the want of velocity in Galvaniſm, I beg to be informed, whether Mr. Cruickſhank's very powerful machine affects the electrometer equally with an electrical vial of the ſame ſtrength, for I am at a loſs what to call velocity, if the elliſion of light from the air, producing a ſound to be heard in another room, and ruſhing to a conductor at a diſtance, be not inſtances of velocity equal to any obſerved in electricity.

With the greateſt reſpect, and thanks both to you and Mr. Cruickſhanks, for the readineſs with which you attended to my laſt, and the full explanation you took the trouble to honour me with,

I am, SIR,

A FRIEND TO THE EXPERIMENTAL RESEARCH OF TRUTH.

V.

An Account of a new Eudiometer. By Mr. DAVY. *From the Journal of the Royal Inſtitution.*

THE dependance of the health and exiſtence of animals upon a peculiar ſtate of the atmoſphere, and the relations of this ſtate to proceſſes connected with the moſt eſſential wants of life, have given intereſt and importance to inquiries concerning the compoſition and properties of atmoſpheric air.

This

This elaſtic fluid has been long known to conſiſt chiefly of oxigen and nitrogen, mingled together, or in a ſtate of looſe combination, and holding in ſolution water.

A variety of proceſſes have been inſtituted with the view of determining the relative proportions of the two gaſes, but moſt of them have involved ſources of inaccuracy; and lately all, except two (the ſlow combuſtion of phoſphorus, and the action of liquid ſulphurets), have been generally abandoned.

Both phoſphorus and ſolution of ſulphuret of pot-aſh abſorb the whole of the oxigen of atmoſpheric air at common temperatures, and they do not materially alter the volume, or the properties of the reſidual nitrogen; but their operation is extremely ſlow; and in many caſes it is difficult to aſcertain the period at which the experiment is completed.

I have lately employed as an eudiometrical ſubſtance the ſolution of green muriate, or ſulphate, of iron, impregnated with nitrous gas; and I have found that it is in ſome reſpects ſuperior to many of the bodies heretofore uſed, as it rapidly condenſes oxigen without acting upon nitrogen; and requires for its application only a very ſimple and a very portable apparatus.

This fluid is made by tranſmitting nitrous gas through green muriate, or ſulphate, of iron, diſſolved to ſaturation in water *. As the gas is abſorbed, the ſolution becomes of a deep olive brown, and when the impregnation is completed it appears opaque and almoſt black. The proceſs is apparently owing to a ſimple elective attraction; in no caſe is the gas decompoſed; and under the exhauſted receiver it aſſumes its elaſtic form, leaving the fluid with which it was combined unaltered in its properties.

The inſtruments neceſſary for aſcertaining the compoſition of the atmoſphere, by means of impregnated ſolutions, conſiſt ſimply of a ſmall graduated tube, having its capacity divided into one hundred parts, and greateſt at the open end; and of a veſſel for containing the fluid.

The tube, after being filled with the air to be examined, is introduced into the ſolution; and, that the action may be more rapid, gently moved from a perpendicular towards a horizontal poſition. Under theſe circumſtances the air is rapidly diminiſhed; and, in conſequence of the dark colour of the fluid, it is eaſy to diſcover the quantity of abſorption. In a few minutes the experiment is completed, and the whole of the oxigen condenſed by the nitrous gas in the ſolution in the form of nitrous acid.

In all eudiometrical proceſſes with impregnated ſolutions, the period at which the diminution is at a ſtand muſt be accurately obſerved; for, ſhortly after this period, the volume of the reſidual gas begins to be a little increaſed, and, after ſome hours, it will often fill a ſpace greater by ſeveral of the hundred parts on the ſcale of the tube, than that which it occupied at the maximum of abſorption.

This circumſtance depends upon the ſlow decompoſition of the nitrous acid (formed during the experiment), by the green oxide of iron, and the conſequent production of a

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* Dr. Prieſtley firſt obſerved this proceſs: for a particular account of it, ſee Reſearches, Chemical and Philoſophical, page 152. Johnſon.

ſmall quantity of aeriform fluid (chiefly nitrous gas)*; which, having no affinity for the red muriate, or ſulphate, of iron produced, is gradually evolved, and mingled with the reſidual nitrogen.

The impregnated ſolution with green muriate is more rapid in its operation than the ſolution with green ſulphate. In caſes when theſe ſalts cannot be obtained in a ſtate of abſolute purity, the common or mixed ſulphate of iron may be employed. One cubic inch of moderately ſtrong impregnated ſolution is capable of abſorbing five or ſix cubic inches of oxigen, in common proceſſes; but the ſame quantity muſt never be employed for more than one experiment.

A number of comparative experiments, made on the conſtitution of the atmoſphere at the Hotwells, Briſtol, in July, Auguſt, and September, 1800, with phoſphorus, ſulphurets of alkalies, and impregnated ſolution, demonſtrated the accuracy of the proceſſes in which the laſt ſubſtance was properly employed. The diminutions given by the ſulphurets were indeed always greater by a minute quantity than thoſe produced by phoſphorus and impregnated ſolutions: but the reaſon of this will be obvious to thoſe who have ſtudied the ſubject of Eudiometry. In no inſtance was it found that 100 parts in volume of air contained more than 21 of oxigen: and the variations connected with different winds, and different ſtates of temperature, moiſture, &c. were too ſmall, and too often related to accidental circumſtances, to be accurately noticed.

In analyſing the atmoſphere in different places, by means of impregnated ſolutions, I have never been able to aſcertain any notable difference in the proportions of its conſtituent parts. Air, collected on the ſea at the mouth of the Severn, on October the 3d, 1800, which muſt have paſſed over much of the Atlantic, as the wind was blowing ſtrong from the weſt, was found to contain 21 per cent. of oxigen in volume; and this was nearly the proportion in air ſent from the coaſt of Guinea, to Dr. Beddoes, by two ſurgeons of Liverpool.

If we compare theſe reſults, with the reſults gained more than twenty years ago, by Mr. Cavendiſh, from experiments on the compoſition of atmoſpherical air, made at London and Kenſington; conſidering, at the ſame time, the reſearches of Berthollet in Egypt and at Paris, and thoſe of Marti in Spain, we ſhall find ſtrong reaſons for concluding, that the atmoſphere, in all places expoſed to the influence of the winds, contains very nearly the ſame proportions of oxigen and nitrogen, a circumſtance of great importance; for, by teaching us that the different degrees of ſalubrity of air do not depend upon differences in the quantities of its principal conſtituent parts, it ought to induce us to inſtitute reſearches concerning the different ſubſtances capable of being diſſolved or ſuſpended in air, which are noxious to the human conſtitution: particularly as an accurate knowledge of their nature and properties would probably enable us, in a great meaſure, to guard againſt, or deſtroy, their baneful effects.

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* The decompoſition of nitrous acid, by ſolutions containing oxide of iron, at its minimum of oxidation, is a very complex proceſs. The green oxide, during its converſion into red oxide, not only decompoſes the acid, but likewiſe acts upon the water of the ſolution; and ammoniac is ſometimes formed, and ſmall portions of nitrous oxide and nitrogen evolved with the nitrous gas.

VI. *On*

V.

On the Diſcolouration of Silver by Bird's Eggs, &c. In a Letter from ANTHONY CARLISLE, *Esq.*

To MR. NICHOLSON.

SIR,

I HAVE been lately occupied in tracing the natural hiſtory of bird's eggs, for a paper which has been in part laid before the Linnæan Society. Among other needful inquiries, I had occaſion to make ſome experiments on the properties of the Albumen, Vitellus, and Gas contained in the *folliculus aëris* of eggs, which belong more properly to chemical hiſtory. As ſome of the reſults ſeem new, I beg leave to offer them for inſertion in your Journal.

The well known fact of ſilver being diſcoloured by eggs, induced me to try which of the ſubſtances compoſing a fowl's egg is the cauſe of this appearance. I put the albumen, or white part of a new laid hen's egg, into a clean poliſhed ſilver ſpoon, and the intire vitellus, or yolk, freed from its membrane, and all adhering albumen into another ſimilar ſpoon. They were each expoſed to a heat above that of boiling water, coagulated and reduced nearly to dryneſs, then left to cool, and twelve hours afterwards the ſilver remained untarniſhed in both.

A ſilver ſpeculum placed in water, was adapted to receive the air contained in the folliculus aeris, which remained expoſed to this gas for two days, without any diſcolouration.

I mixed the albumen and vitellus together in a ſpoon, and expoſed them to heat without diſcolouring the ſilver. The intire albumen of a new laid egg was put into ten ounces (by meaſure) of boiling diſtilled water in a glazed pipkin, and kept till it was coagulated firmly: a clean ſilver ſpoon had been immerſed with the albumen, to which it adhered in ſeveral places; after ſetting the veſſel in a cool place for an hour, the ſilver immerſed had acquired a deep blue and brown colour, with various priſmatic ſhades.

An intire vitellus, freed from all other ſubſtances, was put into boiling water, together with a ſilver ſpoon as in the laſt deſcribed experiment, but the ſilver did not receive any tarniſh.

Ten ounces by meaſure of the water in which the albumen of one egg had been boiled was filtered, and put into a glaſs veſſel with a ſilver ſpoon; after remaining there for two hours it had received a deep copper colour.

The ſame quantity of water in which the vitellus of one egg had been boiled, was put into a glaſs with a ſilver ſpoon, and remained ſix hours without producing any change of colour. The water in the two laſt experiments was at 60° of Fahrenheit.

The matter which produced the diſcolouration of the ſilver proved to be ſulphurated hidrogen, and it is worthy of remark, that this compound is not formed by the albumen of eggs without the addition of water; the water contained in the albumen itſelf ſeeming to be inſufficient to give the hidrogen required, or its combination with this ſubſtance not admitting of decompoſition*.

The ſmell of ſulphurated hidrogen appears much ſtronger in the water where albumen has been boiled, if it have had ſilver immerſed in it: perhaps the ſilver gives the prediſpoſing affinities. That ſo large a quantity of ſulphur ſhould be contained in the rudimental materials of a bird, is a curious fact: but as ſulphur is a ſubſtance ſtill imperfectly known, it is difficult to ſay whence it is derived, and for what purpoſes it is employed.

The ſerum of the human blood contains ſulphur, which in ſome experiments made for me by Mr. Fred. Accum yielded ſulphurated hidrogen abundantly.

The air contained in the folliculus aeris, appears from the few experiments which I have made to be analogous to atmoſpheric air.

I am, dear SIR,
Your much obliged, &c. &c.

Soho Square, July 4, 1801. A. CARLISLE.

VI.

Inquiries relative to the Laws of Affinity. By CIT. BERTHOLLET.†

(Continued from page 158.*)*

Of the Metallic Solutions and Precipitates.

1. IN the two preceding Memoirs, I have principally conſidered thoſe ſubſtances which are ſimple, and of which the compoſition is not variable; but it is known that the metallic oxides have different properties, according to the ſtate of the oxidation; which muſt neceſſarily affect their chemical action.

The object of this memoir is to examine, what may be the influence of the degree of oxidation of the metals in their chemical action, and in general to compare this action with that of other ſubſtances, independantly of the properties with which I have been occupied (*Inquiries*, Art. XIII.)

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* Perhaps the diſcolouring of ſilver ſpoons in eating boiled eggs, may ariſe from the water of the ſaliva being repeatedly brought into contact with the heated albumen, or from the water in which the eggs had been boiled having permeated the ſhell.

† Annales de Chimie, tom. XXXVIII. p. 113.

The ſolutions and precipitates of mercury have appeared to me to be particularly worthy of attention, becauſe chemiſts have obſerved them with more care, and it is more eaſy to determine the conditions.

2. Fourcroy has deſcribed in the Memoirs of the Academy of Sciences, 1790, a ſulphate of mercury, which he proves to be analogous to calomel, that is to ſay, formed by mercury little oxigenated. He has ſhewn that the ſulphate of mercury, particularly when ſuch a degree of heat only was employed as not to produce complete deſiccation of the ſulphuric acid and mercury, was divided by the action of the water into two ſulphates, one of which may be called oxigenated ſulphate, and the other mild ſulphate. A ſimple method of obtaining this laſt combination, is to dilute by a nearly equal volume of water, the ſulphuric acid which is treated with mercury, and to cauſe the mixture to boil; little ſulphureous acid is formed, and inſtead of oxigenated ſulphate of mercury, that ſulphate is obtained of which the properties have been ſo well inveſtigated by Fourcroy.

In the determination which Fourcroy has given of the conſtituent parts of the mild ſulphate of mercury, he fixes at 0,05 the proportion of oxigen which is combined with the mercury; but for this determination, he ſuppoſes that on decompoſing this ſalt by pot-aſh, it is the pure oxide of mercury which is precipitated. But the metallic precipitates retain a portion of acid which he has neglected; I therefore believe, that according to his experiment, the proportion of oxigen ought to be rather greater than that which he eſtabliſhes. I obſerve, that in many determinations made by chemiſts in modern times, this conſideration has been neglected, which renders them in ſome meaſure uncertain.

The mild ſulphate of mercury forms a permanent combination, which is not decompoſed by water, as the oxigenated ſulphate of mercury is, upon which I ſhall proceed to make ſome obſervations.

If the proceſs indicated by Fourcroy be employed, that is to ſay, if the concentrated ſulphuric acid be made to boil with the mercury, without letting it arrive at a ſtate of deſiccation, a white maſs is obtained, compoſed of ſweet ſulphate and oxigenated ſulphate. By the careful method of waſhing which he points out, the exceſs of acid that maintains the ſolution may be ſeparated, from the oxigenated ſulphate, and a portion of the mild ſulphate.

When the operation is carried farther, or when a ſufficient degree of heat is applied to the mild ſulphate, a greater quantity of ſulphureous acid is diſengaged; the mercury becomes too much oxided to form the mild ſulphate, and the combination is found intirely in the ſtate of oxigenated ſulphate, which varies in the quantity of ſulphuric acid it retains. Let us examine it in the ſtate it poſſeſſes when the operation has been carried to dryneſs, in which conſequently it may be conſidered as not poſſeſſing an exceſs of acid.

In this ſtate water produces a ſeparation; the maſs which was before white, turns yellow; the liquid becomes very acid, and holds a part of the ſulphate in ſolution; there is formed what has been called, ſince the time of Rouelle, a ſalt with exceſs of acid, and a ſalt with the leaſt portion of acid; but the proportions of theſe two combinations vary, 1. according

according to the quantity of acid that the firſt combination had retained; 2. according to the quantity of water employed; 3. according to the temperature; for the heat concurs with the action of the water.

If inſtead of ſimple water, an alkaline ſolution be employed, which exerts a more powerful action upon the acid than pure water, two different combinations are formed, one of which is found to be almoſt intirely the oxide, and the other in which the acid is for the moſt part engaged: the precipitate then afforded differs principally from that which water alone would have produced, by the ſmaller proportion of acid which it retains, and which depends on the degree of concentration of the alkaline liquor which acts upon it.

When the oxigenated ſulphate of mercury has preſerved a more conſiderable exceſs of acid, the action of the acid may be ſo weakened by the water, that no ſeparation will be made.

3. I have hitherto ſuppoſed, that there were but two ſulphates of mercury, one with the ſmalleſt poſſible proportion of oxigen, and the other with the greateſt proportion; but it is manifeſt that there are but theſe two extremes that are fixed, ſo that they may contain within their limits all the other degrees of oxidation: the properties of theſe intermediate combinations differ ſo much as to leave no room for the determination of their particular properties, unleſs both the degree of oxidation, and the proportion of the acid be known.

What I here obſerve as to the intermediate degrees of oxidation, muſt alſo be applied to the other metallic ſalts, ſuch as the ſulphate of iron, in which there are likewiſe only two fixed terms, that of the weakeſt and that of the ſtrongeſt oxidation.

4. Bergman had before diſcovered, that the ſolution of mercury by the nitric acid made in the cold, has different properties from that which is prepared by means of heat; and in his very excellent treatiſe on the analyſis of waters, he remarks, that the former does not ſo readily afford a precipitate with the ſolutions which contain ſulphuric acid, and that the precipitate it forms is white, whereas that of the ſolution made with heat is yellow; that the firſt then forms mild ſulphate of mercury, which is white, and more ſoluble in water than the oxigenated ſulphate, while the latter forms the oxigenated ſulphate.

When the nitrate of mercury is prepared by means of heat, ſome nitrous gas is at firſt diſengaged, but at a certain period, when the diſengagement is complete, the mercury is perceived to be diſſolved, with the production of ſcarcely any nitrous gas. I am indebted for this obſervation to Citizen Gay, a young chemiſt of the Polytechnic ſchool, who adds much ſagacity to conſiderable zeal. We ſee therefore, that by means of heat an oxigenated nitrate is formed, which if the operation be not ſtopped, afterwards combines with mercury, as in another operation the oxigenated muriate of mercury combines with a freſh quantity of this metal*. But in the fluid nitrate of mercury, there are no determinate proportions

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* By an obſervation of Fourcroy it is ſhewn, that the oxigenated ſulphate of mercury ſubmitted to ebullition with water and mercury, acts alſo upon the metal: it muſt therefore paſs to the ſtate of mild ſulphate. The corroſive mercurial muriate, when diſſolved in water, does not act upon mercury; but by trituration with mercury without water, it begins to combine with it, and to impart its oxigen: the combination becomes uniform by ſublimation.

between the mercury moſt oxigenated, and that leaſt ſo: it appears that all the intermediate proportions may exiſt, as I have obſerved with reſpect to the ſulphates.

When the nitrate of mercury is precipitated by the muriate of ſoda, mercurial muriates are obtained, which differ according to the oxidation of the mercury. With the nitrate little oxided a white precipitate is obtained, which retains a part of the nitric acid, and cannot be diſſolved in the muriatic acid, nor be taken up by the concentrated nitric acid, without giving out much nitrous gas. From the ſolution prepared by heat, a precipitate is obtained rather of a yellow colour, which is not ſoluble in the muriatic acid, but readily diſſolves in the nitric acid, giving out little nitrous gas. The ſupernatant fluid affords by evaporation, a little corroſive mercurial muriate.

If the experiment be made with a nitrate, prepared in ſuch a manner that the mercury ſhall be in the moſt oxided ſtate, and has not re-diſſolved any metallic mercury, and if it be diluted with a conſiderably large quantity of water, no precipitate is formed, but all the mercury is found in the ſtate of corroſive mercurial muriate: nevertheleſs, it is not obtained in this ſtate but in a limited quantity, and ſometimes even none is had, according to the proportion of muriate of ſoda employed, becauſe the corroſive mercurial muriate has the property of forming with the nitrate of ſoda a quadruple ſalt.

This ſalt is capable of forming rhomboidal cryſtals, grooved on their face, of a conſiderable ſize: it fuſes upon ardent charcoal; by expoſing it to a ſufficient heat in a retort, all the mercury is ſeparated in the form of corroſive mercurial muriate. The reſidue is a nitrate of ſoda which retains a little muriatic acid, ſo that the ſeparation which takes place is decided by the reſpective volatility of the ſubſtances, and by a difference of affinity between the nitric acid and the muriatic acids relative to the oxide of mercury.

After the cryſtalliſation of this ſalt, another is obtained in ſmall needles, which appear to be a complex ſalt, in which the oxide of mercury is found in a greater proportion.

Nothing certain can be eſtabliſhed as to the reſults of the mixture of the nitrate of mercury highly oxided, and the muriate of ſoda, becauſe they vary according to the proportions of the ſubſtances which act.

From the preceding obſervations we deduce, that the nitric ſolution of mercury may hold this metal in ſolution from the loweſt degree of oxidation to the higheſt, or to that which is required for the conſtitution of corroſive mercurial muriate; that it may poſſeſs it in all the intermediate degrees, but that its properties will be different according to the degree of oxidation.

5. Fourcroy lays it down as a principle, that any metallic oxide whatever gives to acids a colour ſimilar to that which it has itſelf; whence he concludes, that when a mercurial precipitate which proceeds from a white ſalt acquires another colour, a change muſt have been made in the oxidation. This opinion does not appear to me to be well founded.

I took ſome muriatic acid, and diſſolved in it ſome red oxide of mercury; the ſolution was eaſily effected, without any diſengagement either of oxigen gas, or oxigenated muriatic acid: it ſpontaneouſly afforded fine cryſtals of corroſive mercurial muriate. I may here remark,

remark, that this proceſs appears to me to be the moſt ſimple, and the leaſt expenſive for the preparation of the corroſive mercurial muriate.

The combination which I had formed contained the red oxide of mercury, with all its oxigen; yet it was colourleſs; with ammonia it would have given a white precipitate, and with lime and the alkalis a precipitate more or leſs orange coloured.

The red oxide of mercury readily diſſolves in the nitric acid, without any diſengagement of oxigen; this ſolution cryſtallizes and forms a white ſalt; but if there be not a ſufficient exceſs of acid, it gives with water alone a white precipitate; with a greater quantity of water recently diſtilled, a yellow precipitate; with lime and the fixed alkalis a precipitate of a much deeper yellow colour.

A coloured oxide may therefore form white ſalts, and afterwards aſſume other colours, without undergoing any change in its oxidation.

6. Several chemiſts have obſerved, that the muriatic acid has a greater diſpoſition to combine with very oxided metals, than the nitric and ſulphuric acids. Fourcroy applied this conſideration with advantage to explain ſeveral phænomena. On this ſubject he thus expreſſes himſelf: (*Mem. de l'Acad.* 1790, page 381.) "Every acid requires quantities of oxigen in the metals, in order to combine with them; the muriatic acid in general does not combine with thoſe metals, unleſs they be much loaded with this principle, or highly oxided: the mercury appears to be more oxided in the corroſive muriate than in the nitrate." I ſhall take the liberty to make ſome obſervations on the principles laid down by my learned colleague.

It does not appear to be exact to ſay, that each acid requires different quantities of oxigen in the metals to combine with them; the nitric, the ſulphuric, and the muriatic acids form combinations with mercury, from the leaſt to the moſt oxided terms; and it is the ſeries which reſults in the muriates, which makes the principal difference in all pharmaceutical preparations, from the corrroſive ſublimate to the mercurial panacea: but there is this difference, that the combination of the ſulphuric acid and the nitric acid with highly oxided mercury is much weaker, and more eaſily decompoſed, even by the action of water, than that of the muriatic acid, which on the contrary preſents a very permanent conſtitution.

If our attention be directed to thoſe metals which have the property of aſſuming large proportions of oxigen, ſuch as iron, tin, antimony, &c. we ſhall obſerve the ſame properties with regard to the ſulphuric, nitric, and muriatic acids, ſo that the action of the two firſt, which diminiſhes in proportion as the oxidation advances, is ſometimes ſo much weakened, that they abandon intirely, or are incapable of diſſolving ſome very oxided metals; on the contrary, the muriatic acid diſſolves them and holds them in ſolution, ſo that it cannot be perceived whether its action be weakened, or whether on the contrary, it be not increaſed by a greater oxidation.

A plauſible explanation may be given of this comparative property of the ſulphuric, nitric, and muriatic acids. Sulphur and azote, the baſes of the two former are ſaturated with

oxigen,

oxigen, ſo that their reſulting affinity for the ſubſtances that are alſo greatly oxigenated is very weak; but the muriatic acid which appears to have in its conſtitution only a very ſmall proportion of oxigen, ought to have a much greater diſpoſition to combine with oxigenated ſubſtances.

7. In the preceding obſervations I have ſuppoſed, that the different alkalis ſhare the acid of a metallic ſolution with the oxide which is precipitated. I do not here ſpeak of precipitates by ammonia, which in ſome circumſtances is decompoſed, and by that means changes the conſtitution of the precipitate, as has been particularly ſhewn by Fourcroy; but this property is ſubject to modifications which demand particular attention.

The experiments of Bayen have aſcertained beyond a doubt, that the precipitates of mercurial nitrates and muriates retain a greater or leſs portion of acid, ſo that on expoſing the precipitates of the mercurial nitrate to a ſufficient heat, ſome nitrous acid is diſengaged; and on making the ſame experiment on the precipitates of mercurial muriate, a greater or leſs quantity of inſoluble mercurial muriate is ſublimed. I ſhall here add ſome obſervations to thoſe I have already made. (*Inquiries*, Art. XIII. No. 1.)

When the oxigenated muriate of mercury is decompoſed by the preciſe quantity of carbonate of ſoda, which is neceſſary to effect its precipitation, the precipitate contains muriatic acid, carbonic acid, and oxide of mercury in exceſs; ſo that on expoſing the precipitate to the action of heat, carbonic acid gas and oxigen gas are diſengaged; nearly half the mercury is reduced to metal, and the reſt of the precipitate is ſublimed, retaining all the muriatic acid which remained in combination with the oxide, and forming the mercurial muriate obſerved by Bayen. The liquid ſupernatant over the precipitate affords by evaporation, at firſt carbonate of ſoda, and afterwards a triple ſalt in which ſoda predominated.

The carbonate of pot-aſh preſents different phænomena. A very ſmall portion only is neceſſary to produce a total precipitation in the ſolution of corroſive mercurial muriate, and the precipitate obtained does not amount to half the weight of what would be given by the ſame quantity of oxigenated muriate of mercury, precipitated by the carbonate of ſoda. This precipitate, expoſed to the action of heat, gives out carbonic acid, and ſublimes almoſt totally in the ſtate of mercurial muriate, with the leaſt portion of acid; a very ſmall portion only reſumes the metallic ſtate. The liquid that covers the precipitate makes no efferveſcence with the acids; ſo that here the whole of the carbonic acid is combined in the precipitate with the oxide, and a portion of muriatic acid nearly double that contained in the precipitate by carbonate of ſoda. The liquid when evaporated affords a triple ſalt much more ſoluble in water than the oxigenated muriate of mercury: this ſalt cryſtallizes in ſilky needles.

The carbonate of ammonia cauſes an efferveſcence on decompoſing the corroſive mercurial muriate, and no acid is found either in the precipitate or in the ſupernatant liquid. The weight of the precipitate conſtitutes nearly five ſixths of the muriate decompoſed; a diſengagement of ammonia takes place on the addition of lime. When urged by heat, the ammonia is decompoſed, and azote gas only is received. All the mercury is ſublimed without reduction. But we perceive that in this ſublimate the mercury cannot be as much oxided as in the ſublimation of the precipitates by the fixed alkalis or lime, becauſe it has loſt a part of its oxigen by the decompoſition of the ammonia. The liquid which covered the cipitate contained a combination of oxide of mercury, of muriatic acid, and ammonia.

The precipitation by ammonia preſented the ſame phenomena, except the efferveſcence.

8. By directing our attention to the other metallic ſolutions and precipitations, the character of thoſe of mercury are eaſily diſcovered, as well as the modifications which depend on the particular affections of each oxide; I therefore think we are juſtified in ſtating the following principles:

1. The acids act upon the metallic oxides the ſame as upon the other ſubſtances, in proportion to their maſs, ſince when a metal has become but little ſoluble or inſoluble, it may be diſſolved by an exceſs of acid, or may form, by means of ſuch exceſs, a more durable combination.

2. When a metallic combination is decompoſed, the alkali or alkaline earth made uſe of, produces a diviſion of the acid, in proportion to the energy of its action. If the metallic combination is weak, water is ſufficient to decompoſe it; ſalts with either the maximum or minimum of acid are then formed. In this caſe the metallic oxides follow the ſame laws as other ſubſtances; but it ſometimes happens that it is not the alkaline baſe which is added that aſſumes its ſhare of the acid, but it is the metallic oxide on the contrary that divides the precipitate with the acid; as when we form fulminating gold, or the orate of ammonia. Sometimes alſo the precipitant, the acid, and the metallic oxide from two complex combinations; one of which is inſoluble and the other remains liquid, as we have ſeen in the decompoſition of the corroſive mercurial muriate by ammonia, and in the experiments that I have deſcribed (firſt ſeries, Nos. X. and XI.)

In general, but particularly with reſpect to the metallic ſolutions, we muſt not ſeparate in our reaſonings, the ſubſtance employed by the name of precipitant from the liquid in which the precipitation is effected; but attention ought to be paid equally to all the ſubſtances preſented, and which may form new combinations.

3. The coloured oxides may produce colourleſs combinations; but on giving out a part of the acid with which they were combined, their colour will again appear in proportion to the quantity of acid they have yielded; ſo that this colour is an indication of the conſtitution thus eſtabliſhed, provided the ſtate of oxidation has not been changed by any particular circumſtance.

4. The metallic oxides cannot be compared together, unleſs they are taken in a determinate ſtate of oxidation. All the combinations they are capable of forming vary, not only from this cauſe, but alſo from the proportion of acid they retain, when this proportion is not determined by a cryſtallization, With reſpect to oxidation there are only two extremes; that of the ſmalleſt, and that of the greateſt oxigenation which can be conſidered as conſtant.

Hence it follows, that the nomenclature can only indicate in a vague manner, and with great latitude, any metallic combinations in which the oxidation and the proportion of the acid are not determined.

5. The acids do not follow the ſame progreſſion in their affinity relative to the degrees of oxidation. There are ſome in which the affinity diminiſhes with the oxidation; ſuch are the nitric and ſulphuric acids; in others again it appears to increaſe, ſuch as the muriatic acid. Hence we may perceive, independant of all the conſiderations I have mentioned, how groundleſs the pretenſion was of claſſing the affinities of metals for the different acids, by conſidering them as conſtant forces.

6. The reſults of the complex affinities of metallic ſolutions, mixed with other ſalts, may alſo vary from the proportion of thoſe ſalts, conformably to what has been obſerved (*on the influence of proportion in the complex affinities*); ſo that then the proportion of oxigen in the metallic oxide, that of the oxide in the ſolution, and that of the ſaline combination brought into action with it, all contribute to the new combinations which are eſtabliſhed.

7. We might therefore obſerve in the chemical action of the metallic ſolutions, the laws which we have eſtabliſhed in that of the other combinations, if oxidation did not cauſe a change in the affinity of the metal, either for the acids, or for the other ſubſtances, and multiply, as it were, in the metals the property of forming combinations; whence the reſults are often ſo complicated, that though by accurate obſervation we may develope the facts and the circumſtances which determine them, yet they cannot be foreſeen by theory, from the mere conſideration of the known properties, as may be done with reſpect to other ſubſtances, the action of which depends on a ſmaller number of conditions.

8. The ſtate in which oxigen is found combined with the metals, has likewiſe an influence upon the properties of the metallic oxides, and of the precipitates compared with each other. Bayen obſerved, that ſeveral mercurial precipitates, as well as the red oxide, detonate ſtrongly, though in an unequal manner when expoſed to heat, after having been mixed with ſulphur, and that ſome do not poſſeſs this property; but he has not given the reaſon of the phænomenon and the exceptions. It appears to me indubitable, that this property of the oxide of mercury, and of the precipitates in which it predominates, is a conſequence of the oxigen poſſeſſing more caloric, than it preſerves in the combination that it forms with the ſulphur, or in the ſulphuric acid. The ſame thing therefore happens as with the nitrate and oxigenated muriate of pot-aſh; except that the effect is leſs conſiderable; but in the precipitates of the corroſive mercurial muriate, part only of the mercury can be conſidered as not combined with the muriatic acid, namely, that part which may be reduced into metal by the action of the heat which produces detonation. Such of the precipitates therefore as retain a ſufficient quantity of the muriatic acid, to admit only of a ſmall reduction of mercury by the action of heat, cannot produce detonation; and ſuch in fact are the precipitates by ammonia, by the carbonate of ammonia, and by the carbonate of pot-aſh.

9. The property diſcovered by Bayen is therefore analogous to the fulminating property of the orate and the argentate of ammonia; but it is not perceptible in the other metallic oxides and precipitates. Hence the oxigen in the latter muſt neceſſarily contain a leſs portion of caloric.

10. The properties which depend upon oxidation vary therefore in each metal, on account of the proportions of oxigen; and thoſe which depend on the ſtate of concentration poſſeſſed by oxigen, conſtitute the leading cauſe of thoſe phænomena which ariſe from the changes of combination in elaſtic ſubſtances. (*Inquiries*, Art. XIV. Nos. 12 and 15.)

(To be continued.)

VIII.

Experiments and Obſervations on various Kinds of Candles, made of Wax, Tallow, Spermaceti, and of ſuch as are manufactured of a Mixture of theſe Subſtances; as well with Regard to Cheapneſs as to the Quantity of Light they afford. By Sig. Fr. Hermstaedt, *Profeſſor and Member of the Royal College of Phyſicians at Berlin**.

THE illumination of apartments is an article of the firſt neceſſity for every claſs of people, and the means by which it is produced are ſo likely to advance in their price, that this illumination continues to be an object of conſiderable importance, for every family in houſekeeping, whether numerous or moderate. But as it is impoſſible to obviate this neceſſity for lighting our apartments, it becomes the duty of every one to be attentive to the proſpects of œconomy in this reſpect: ſince the ſaving, though ſmall with regard to a ſingle candle, may nevertheleſs in time amount to a ſum by no means contemptible in houſekeeping upon a larger ſcale; more eſpecially, when the elegant enjoyments of life demand a brighter light, than is neceſſary for the mere purpoſes of domeſtic utility.

The experiments which I lately had an opportunity of making during the official examination of various ſorts of tallow candles, with regard to œconomy in their conſumption, as well as to the quantity of light afforded by their combuſtion, afforded reſults, which appeared deſerving of attention, and were ſufficient to induce me to make a new ſet of experiments, with candles made of mixed ingredients, in order to compare them with the former. Theſe, likewiſe, furniſhed reſults by no means unintereſting, and afford concluſions of equal utility. Theſe reaſons have induced me to publiſh my obſervations for further examination; and even ſhould my remarks and obſervations appear to ſome readers of very ſubordinate importance, yet I might requeſt them to conſider, that even facts of little moment have frequently afforded a foundation to more diſcoveries of greater magnitude.

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* Tranſlated from the German in *Scherer's Allgemeinen Journal der Chemie*, Vol. IV. page 40.

Firſt,

Firſt experiment with a wax candle of the ſize of ſix to a pound.—A white wax candle weighing exactly 2 ounces 11 drachms ($5\frac{1}{8}$ *Loth.*) *avoirdupois weight**, was lighted and kept burning for one hour in a calm place; after which combuſtion it exhibited a loſs of weight of 115 grains. According to this proportion the combuſtion of the whole candle would have required 11.217 hours, or which is the ſame, 11 hours and 13 minutes. The burning of this candle proceeded quietly, with a tranquil, not undulating flame, and without notable ſmoke and ſoot.

Second experiment with a mould tallow candle of ſix to the pound.—The candle weighed 2 ounces, 10 drachms, ($5\frac{1}{4}$ *Loth.*) and loſt in weight 185 grains after an hour's burning; the whole of it would therefore have been intirely conſumed in 6 hours and 45 minutes. The flame was ſlightly agitated, and emitted much vapour.

Third experiment with a mould tallow candle of eight to the pound.—The weight of this candle was 1 ounce, 15 drachms, and 4 grains ($3\frac{7}{8}$ *Loth.*) When it had burned one hour, it had loſt 140 grains; whence it might have burned 6 hours and 40 minutes. The other phænomena during its combuſtion were like thoſe of the preceding.

Fourth experiment with a mould candle of nine to a pound.—Its weight was 1 ounce 12 drachms, ($3\frac{1}{2}$ *Loth.*) and the loſs of weight after one hour's burning was 140 grains; conſequently this candle would have laſted ſeven hours before its intire conſumption. The flame reſembled that of the laſt.

The reſults afforded by theſe experiments appear to me deſerving of attention in various points of view. It was on this account that I reſolved to make various kinds of candles, compoſed of pure ſpermaceti, of ſpermaceti and tallow, and of tallow and wax, in various proportions of the ingredients. And laſtly, I made trial of a mixture of wax, tallow, and ſpermaceti. The reſults which I obtained in theſe experiments were the following:

Fifth experiment with a mould candle, made of two parts of tallow and one of wax.—This candle weighed $2\frac{1}{2}$ ounces, 24 grains (5 *Loth.*) It was lighted, and after an hour's combuſtion loſt 166 grains; it might conſequently have burned for the ſpace of 7 hours and 22 minutes. The flame of this candle was a very good one; it emitted ſcarcely any vapour, and in this reſpect it very much reſembled the candle made of pure wax.

Sixth experiment with a candle manufactured of one part wax and three parts of tallow.—The weight of this candle was 2 ounces and a half, and 34 grains. From the loſs of 193 grains, which it ſuſtained by burning one hour, it appears that it would have required 6 hours and 23 minutes to be wholly conſumed. The flame of this candle was likewiſe ſteady, and afforded but a ſmall quantity of ſoot.

Seventh experiment with a mould candle made of pure ſpermaceti.—I made a candle of this deſcription, of the weight of 2 ounces and 3 drachms, ($4\frac{3}{8}$ *Loth.*) When lighted and kept

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* The *Berlin pound* of 32 Loths, or 16 ounces, is equal to 3616.3 Engliſh grains; ſo that one ounce Berlin is equal to 226, and the drachm to 14.$\frac{1}{8}$ Engliſh grains, diſregarding the ſmall decimal fraction of 3-10ths of a grain in the pound. The ſubdiviſion of the Engliſh avoirdupois weight into 16 ounces, each into 16 drachms has been employed here without farther reduction.---Tranſl.

burning for one hour, it loſt 250 grains of weight, ſo that the whole time of its combuſtion would have amounted to 4 hours, 12 minutes. Whence it follows, that the candles made of ſpermaceti are far leſs œconomical with regard to expence, than wax or tallow. Beſides which, the flame produced by this candle was very unſteady, fleeting, and it alſo gave out a quantity of ſmoke and ſoot.

Eighth experiment with a candle compoſed of equal parts of ſpermaceti and tallow.—This candle weighed 2 ounces, 6 drachms, and 4 grains, (4¾ *Loth.*) I ſuffered it to burn one hour, and found it to have loſt 213 grains; whence the whole time of its combuſtion would have been 5 hours 22 minutes. Its flame differed very little from that obſerved in the foregoing experiment.

Ninth experiment with a candle made of one part ſpermaceti and two parts of tallow.—The weight of a candle of this mixture was 2 ounces, 6 drachms, and 5 grains, (4¾ *Loth.*) of which it loſt 200 grains by one hour's burning. Its total combuſtion, therefore, would have been accompliſhed in 5 hours, and 43½ minutes. This candle guttered or overflowed very much, and afforded a bad, ſooty flame.

Tenth experiment with a candle prepared of one part of ſpermaceti mixed with three parts of tallow.—This candle weighed exactly 2 ounces, 6 drachms. and 45 grains, (4¾ *Loth.*) By burning for one hour, it ſuſtained a loſs of weight of 235 grains; hence the whole time of its combuſtion would have amounted to 5 hours and 3 minutes. The flame of this candle alſo was not notably different from that of the laſt.

Eleventh experiment with a candle compoſed of equal parts of wax and ſpermaceti, together with three parts of tallow.—The weight of this candle amounted to 1 ounce, 6 drachms, and 56 grains, (3¾ *Loth.*) During the combuſtion for one hour it loſt 246 grains, and would conſequently have laſted 3 hours, 54 minutes, before it was intirely conſumed. In other reſpects the flame of this candle was very bad; and it depoſited a quantity of ſmoke and ſoot.

Computation of the Expence attending the Conſumption of theſe different Kinds of Candles compared with each other.

In order to obtain a very accurate proportion of the ſaving of expence that might be made by burning theſe various ſpecies of candles, it would be neceſſary, not only that the candles themſelves ſhould conſiſt of equal maſſes, but likewiſe of equal diameters or circumferences: for ſince, as I ſhall prove in the ſequel, it reſults from my experiments, that the ſaving in the burning of a candle is proportional to the ſmallneſs of its diameter, and ſince the candles employed in experiment 3 and 4 had a ſmaller diameter than the others, the following calculation with regard to the œconomy in burning theſe two ſpecies, is only an approximation; whereas it is perfectly accurate with regard to all the others, becauſe they were uſed of exactly the ſame diameter. It will be eaſily perceived, that I have calculated the ſaving to be made in the burning of a candle from the quantity of fuel or combuſtible matter, which is conſumed in a given time. Beſides which, I have aſſumed for the given time the ſpace of an hour; and in order to provide myſelf with a determined point of compariſon, I have taken that quantity of fuel which is conſumed in one hour by burning a wax candle, for the given unity as a baſe, and have thus compared the reſpective loſſes in the quantities of combuſtible matter, which the other ſorts of candles ſuſtain, while burning during an equal ſpace of time. The reſults of this calculation are, for the ſake of brevity, placed together in the following table:

If, in a given time, are conſumed of a pure wax candle	1000 Pounds:
There will be conſumed during an equal time of,	
a ... tallow candle, 6 to a pound	1,608
b ... ditto 8 ditto	1,217
c ... ditto moulded, 9 ditto	1,043
d ... candle made according to the 5th experiment	1,443
e ... ditto 6th ditto	1,678
f ... ditto 7th ditto	2,174
g ... ditto 8th ditto	1,852
h ... ditto 9th ditto	1,739
i ... ditto 10th ditto	2,008
k ... ditto 11th ditto	2,138

From theſe reſults it is very manifeſt, that of all candles thoſe made of pure and white wax burn the longeſt, and alſo that tallow candles are the more ſparingly conſumed, the thinner they are, or the ſmaller their diameter is, and that, on the contrary, ſpermaceti candles undergo the greateſt waſte of fuel; beſides, that they emit more vapours than candles of tallow, although thoſe vapours have not a very diſagreeable ſmell. He therefore, who deſires to illuminate his room with œconomy, ſhould procure very thin, mould tallow candles, ſo that about 12 or 16 of them will go to a pound, and he will find them ſtill more advantageous, than is ſhewn by the above reſult; ſince, in it, mould candles of only 9 to the pound have been reckoned. But even in that caſe we ſuppoſe, that in a certain rate of houſe-keeping 100 pounds of tallow candles of the ſize of 6 to a pound are required, the ſame illumination may be effected, according to the experiment here alluded to, with only $64\frac{3}{4}$ lb. if dipped candles of 9 to a pound be employed. Now the pound of mould candles coſts 5 *groſchen* *; conſequently 100 pound will coſt 20 dollars, 20 *groſchen,* whereas $64\frac{3}{4}$ lb. of dipped candles, reckoning the pound at $4\frac{1}{2}$ *groſchen,* will coſt no more than to 12 dollars 3 *groſchen* $4\frac{1}{2}$*d.* ſo that in ſuch a houſekeeping there is a yearly ſaving of 8 dollars, 16 *groſchen* $7\frac{1}{2}$*d.* which in another upon a larger ſcale will come to a conſiderable amount. It need not be mentioned, being obvious of itſelf, that in thinner candles the thickneſs of the wick muſt always bear due proportion to the maſs of tallow.

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* The Berlin *groſche* is equal to 1 4-15ths of a penny, or nearly one penny and a farthing Engliſh. The groſchen is ſubdivided into 12 pennies, and one Berlin dollar is worth 2 ſhillings 6 2-5ths. pence at par.

We may now from the preceding reſults, founded on experience, draw the following concluſions:

a) That in general tallow candles of a ſmaller diameter are conſumed more ſparingly than thicker ones.

b) That the reaſon of this phænomenon muſt be ſought for in the maſs of tallow, which being too great in thick candles, is heated at once, and thus volatilized without burning like the fat in roaſting meat; for which reaſon,

c) Since a ſufficient quantity of oxigen gas from the atmoſphere cannot then act upon it, but only on the exterior part of the flame, a great part of the combuſtible matter muſt be loſt in theſe vapours, at the increaſing intenſity of heat, without decompoſing the air in order to extricate light by burning.—But this on the other hand,

d) Cannot be the caſe with thinner candles, becauſe much leſs of fuel is loſt, and alſo becauſe in that caſe leſs of tallow is neceſſary to be conſumed, in order to produce the ſame quantity of light as is afforded by a thicker candle: for in this caſe more of oxigen gas is decompoſed, and more free light ſeparated, which alſo

e) Affords this great advantage, that ſuch a candle produces leſs vapour and ſoot, becauſe the fuel being then more effectually conſumed, will be volatilized, and go off as ſoot in an undecompoſed ſtate in a much leſs proportion.

But candles would afford the brighteſt and pureſt light, which have a broad, ribbon-like wick inſtead of a round one, or to which the form of hollow cylinders were given, ſo that the air might act upon the flame inwardly as well as outwardly. For by an arrangement of this kind all the combuſtible matter will be conſumed, and no ſmoke nor ſoot, but merely water and carbonic acid gas can be formed, as muſt be evident to every one acquainted with the principles of chemiſtry. But to ſuch as are not chemiſts, Argand's lamp may ſerve as a proof. At the ſame time it is equally evident from the preceding theory or explanation, that every flame, which is not obſcured by ſoot, muſt afford a ſtronger light than another in oppoſite circumſtances; and it is equally obvious, that ſuch a flame, from the total abſence of ſmoke and ſoot, cannot emit any unpleaſant odour. I intend to inſtitute further experiments on this ſubject, and to communicate them at a future time.

Laſtly, I have alſo endeavoured to aſcertain the intenſity of light emitted by the various ſorts of candles here mentioned. For this purpoſe the lighted candle was placed upon a table in a room totally darkened. I then, holding a book in the hand, withdrew backwards, until the characters became inviſible to my eyes. The ſeveral diſtances from the luminous cone were accurately meaſured, and afforded the following reſults:

The wax candle (Experiment I.) emitted light, of which light the illuminating diſtance was 10 feet, 1 inch, 6 lines Rhinland meaſure *.

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* A Rhinland foot is equal to about 1 foot and 3-10ths of an inch Engliſh meaſure.---A line is one-twelfth of an inch.---Tranſl.

The

	Feet	*Inch.*	*Lines.*
The tallow candle of Experiment II. gave	11	2	6
The ditto ditto III.	11	5	0
The drawn candle of Experiment IV.	12	2	3
The candle of ditto V.	12	9	0
The ditto ditto VI.	11	3	0
The ditto ditto VII.	9	9	0
The ditto ditto VIII.	12	6	0
The ditto ditto IX.	12	4	6
The ditto ditto X.	12	6	0
The ditto ditto XI.	12	9	0

If theſe diſtances be compared with each other, and if the intenſity of illumination ſhewn by the wax candle be taken as the ſtandard unity, the following intenſities of light, as produced by the ſeveral candles, will be the reſult by calculation.

If the illumination, effected by the burning of the wax candle according to Experiment I, be 1,000

then that of Experiment	II. will be	1,107
ditto	III.	1,127
ditto	IV.	1,203
ditto	V.	1,260
ditto	VI.	1,104
ditto	VII.	0,963
ditto	VIII.	1,234
ditto	IX.	1,234
ditto	X.	1,234
ditto	XI.	1,206

From this it follows, that, in general, tallow candles burn with a brighter flame than thoſe made of wax; notwithſtanding that the reſults of theſe experiments do not ſeem to ſucceed each other in a perfectly regular order. On this account I do not yet venture to infer from theſe reſults any decided opinion concerning the ſtrength of light; but ſhall poſtpone the fuller inveſtigation of this point to a future time and more leiſure *.

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* It is hoped the author will then likewiſe take notice of the thickneſs of the wicks, as well as of the temperatures, ſize, and other circumſtances of the rooms where his experiments were made, which on one ſide will make his inquiry more complete, while on the other it will afford a means to form by calculation a reaſonable eſtimate of the probable quantity of ſuch kind of fuel waſted by combuſtion in a given time, place, and temperature. There is alſo reaſon to conclude, that a greater degree of accuracy would have been obtained if the relative illuminations had been determined from the ſhadows of the ſame book when equally dark upon an oppoſite wall, as the intenſities of light are then univerſally as the diſtances of the candles from the wall.---Tranſlator.

IX.

Short Notice concerning the Properties and external Characters of ſome new Foſſils from Sweden and Norway; together with ſome Chemical Remarks upon the ſame. By Mr. D'ANDRADA. *In a Letter to Mr. Beyer, Maſter of the Mines at Schneeburg.*

SIR,

WHEN you had the goodneſs to inſpect ſome ſpecimens of the new foſſils, which I have found in my laſt travels in Sweden and Norway, and brought along with me, you deſired to become acquainted at leaſt with their number and names. I comply with this ſuggeſtion; but wiſh, at the ſame time, I were alſo able to communicate to you a deſcription in my own manner, as well as the reſults of the analyſes which I have already made of ſome of them, together with that of others, which at preſent are the object of my occupation, and of thoſe of which Profeſſor Abilgaard has undertaken the analyſis at Copenhagen. However, having deſtined thoſe deſcriptions and accounts partly for the Academy of Sciences at Stockholm, partly for Copenhagen, partly for the Berlin Society of Friends inquiring into Nature, and partly for the Philomathic Society at Paris, I muſt for the preſent deny myſelf the pleaſure of deſcribing them fully.

You will receive, together with this letter, only a few of theſe ſpecies of foſſils, together with a ſhort notice concerning their properties and characters.

I. ACANTHICONE.

The colour is leek and olive green, and ſometimes, though ſeldom, that of the greenfinch. Specific gravity from 3.4075 to 3.3562; but that of the more compact ſort 3.3000. It is not ſcratched by quartz, and it gives fire with ſteel. The texture of the mixed ſpecimens, or in lumps or cryſtallized, is foliated, but in the others fine ſplintery, and more compact. The laminæ are thin, very coherent, and of a three-fold croſſing.* The entire ſplinters are quadrangular parallelopipedons with oblique terminations. On the edges it is tranſparent. The internal luſtre is of the vitreous kind. The cryſtals are, (1.) quadrilateral, hexahedral and decahedral columns, terminated in dihedral, tetrahedral, and hexahedral pyramids; and ſometimes alſo without pyramidal terminations. (2.) Tables or plates, quadrilateral, oblong, rhomboidal, ſharpened off on the long narrow lateral facets.

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* *Durchgang* in the German. Many foſſils are capable of being ſplit in various directions; ſo that the laminæ croſs or tranverſe each other under various angles, and hence each particle of ſuch a foſſil muſt belong to one or more laminæ. If theſe cut each other in one direction only, as in mica, talc, &c, it is called *ſingle croſſing*; if in two directions, as in the feld-ſpar, hornblende, hyacinth, it it called *twofold croſſing*; if in three, as in ſpathoſe iron ore, ponderous ſpar, galena, &c. it is then called *threefold croſſing*, and ſo on. See *Emmerling* Lehrbuch der Mineralogie, 1793, Vol. III. 479.---Tranſl.

If two of theſe plates be rubbed upon each other, they are a little phoſphoreſcent, and emit a ſmell reſembling that of quartz by friction. Acanthicone is in ſome degree pyro-electrical. Before the blow-pipe upon charcoal it readily fuſes, with effervеſcence, into a blackiſh ſcoria replete with air-bubbles. In the cold it is inſoluble in the ſulphuric and nitric acids. This foſſil occurs in the Swediſh iron-mines, near Perſberg, Lengbanſhytta, and Norberg; but in Norway very beautiful ſpecimens of it are met with in the Kulſtad-mines at Helgoland, and near Arendal in the iron-mines Tornbiornſbo, Ulrica, Notbro, &c. Till now it was miſtaken, or confounded with ſchorl, or with green garnets. Some cryſtals are ſo large as to weigh five pounds.

II. SPODUMENE.

The colour is commonly a greeniſh-white of various ſhades. The luſtre that of the mother-of-pearl. In the ſubſtance itſelf it is little tranſparent, but much ſo on the edges. Its ſpecific gravity is 3.218. It ſcratches glaſs, but is itſelf ſcratched by quartz, and affords a white powder. It feels colder than quartz, yet dry and ſmooth. Spodumene is not at all electric, nor phoſphoreſcent; nor does it afford a quartzy ſmell by friction. Its texture is lamellar, with a double croſſing. Its perfect fragments are rhomboidal acute-angled priſms (120° and 55°). The fragments of the croſs-fracture are for the moſt part longiſh plates, that have their edges but little ſharpened. When ſubjected to the blow-pipe upon charcoal, it becomes, at the firſt gentle impreſſion of heat, opaque, dull, and yellowiſh; then developes itſelf in the direction of its laminæ, at the ſame time that it ſwells a little, falling afterwards into an inſipid powder, which by a ſtronger heat affords a very tranſparent greeniſh-white glaſs. Nitric acid does not diſſolve it, nor produce any effervеſcence. It is found, together with ſeveral other foſſils, in the remarkable formation of iron at Uton, in Skarrgarde, three miles from Dalero.

III. SAHLITE.

Its principal colour is a pale aſparagus green of various ſhades. Its external luſtre vitreous, inclining to wax, but internally it has little ſplendor. It is tranſparent, and ſometimes ſemi-pellucid, if the cryſtals are pure, and have ſuffered no decay. Specific gravity 3.2368. Sahlite barely marks glaſs, and does not ſtrike fire with the ſteel, and is eaſily broken. Its texture preſents ſtraight and ſmooth lamellæ, thrice croſſing each other in a ſomewhat acute-angled direction. The fragments of the croſs-fracture are roundiſh. Their form of aggregation is of the coarſe-grained kind, and ſometimes, if the ſtone be in lumps or maſſes, they are hamated, or indented into each other. This ſtone occurs in cryſtals of rectangular, quadrilateral columns, the lateral edges of which are ſlightly truncated, and convex. The broad terminating edges of theſe columns are ſlightly truncated. The ſtreak by raſure is white. This ſtone is ſomewhat ſoft and idio-electric. When rubbed upon a piece of the ſame kind, it emits no odour, nor ſhews any phoſphoreſcence.

eſcence. Before the blow-pipe it is infuſible. It is found in Sweden, in the Sala ſilver-mine in Weſtermannland. I have likewiſe diſcovered it in Buoen, three quarters of a mile diſtant from Anen, in Norway, where it occurs in maſſes, ſtraight and thick foliated.

IV. ICHTYOPHTALME.

The chief colour yellowiſh-white. Luſtre like mother-of-pearl, approaching to greaſy. This foſſil is tranſparent. It ſpecific gravity is 2.491. It ſcratches glaſs and eaſily admits of being filed; affording a white powder of a rough feel. Its own ſubſtance feels ſmooth, but not very dry, and is as cold as quartz. It is very difficult to pulverize it. Texture lamellar, of a more than triple croſſing. When cryſtallized the laminæ are large; but they are ſmall when the foſſil is maſſive, or exhibits a ſplintery fracture. The form of the cryſtals cannot be determined, as they are very much concreted, and ſtrongly cohere with each other. The fragments of the tranſverſe fracture are irregularly angular, in ſome inſtances orbicular with ſharp edges. When firſt acted on by heat upon the charcoal before the blow-pipe, it undergoes no change, not even with reſpect to its colour; but in the more violent ſecond heat the pointed edges run into a white enamel. With ſulphate of lime (gypſum), the ichtyophtalme is infuſible; on the contrary, it runs, when mixed with fluat of lime (fluor-ſpar), into a ſemi-pellucid milk-white glaſs, the fuſion being accompanied by a little effervеſcence. This foſſil occurs at Uton, in Sweden, and conſiſts of ſilex and a little alumine, or argillaceous earth.

V. COCCOLITE.

As to colour, coccolite is mountain, graſs, and olive-green. Its luſtre is vitreous and reſplendent. It is opaque, and its ſpecific gravity is 3.316. It ſcratches glaſs, but excites only a few ſparks with the ſteel. Its ſtreak is grey, or greyiſh-white; its texture broad foliated, in which a ſingle croſſing of the laminæ is obſervable. It is an aggregate of large coarſe and fine grained, granular polyhedrons. Theſe grains appear in ſome inſtances to be ſhort quadrilateral columns, ſharpened at both ends, and having pyramidal terminations joined to, or ſuperimpoſed upon, the lateral ſurfaces. This ſtone is infuſible by itſelf. With carbonate of pot-aſh it fuſes with effervеſcence, and ſwells to a frothy ſcoriaceous glaſs, of a dirty olive-green colour; and with borax it produces a pale-yellowiſh ſemi-pellucid glaſs. It is met with in the iron-mines Helleſta and Aſſebro, in Sudermannland, as well as in Nerike, in Sweden, and likewiſe in a beautiful form in the Arendal iron diſtricts of Norway.

VI. APHRIZITE.

Colour black, ſomewhat greyiſh. External luſtre vitreous, inclining to the gloſs of fat; internally a little reſplendent. Aphrizite is opaque. Its ſpecific gravity is 3.1481. It gives fire with ſteel, and cannot be filed; is very brittle, and readily broken. Its texture is compact. The croſs fracture is ſmooth, a little even, inclining to the flat conchoidal;

and the fragments of it amorphous, yet angular and of ſharp edges. The cryſtals are hexahedral, ſhort and thick columns, which ſometimes, from the different truncatures of the edges, have the appearance of dodecagonal columns. Both ſpecies are terminated in tetrahedral pyramids. This ſtone is feebly idio-electric, but not at all pyro-electric. It intumeſces before the blow-pipe on the very firſt action of heat, frothing at the ſame time, and yielding a greyiſh or yellowiſh white glaſs. With borax it effervesces, foaming ſtrongly, and produces a greeniſh white pellucid glaſs. It occurs at Langoe, a ſmall iſland not far from Krageroe, in Norway.

VII. ALLOCHROITE.

Allochroite poſſeſſes a yellow-grey, and, in ſome inſtances, a dark ſtraw-yellow colour. It has little luſtre of the vitreous kind, which in the recent fracture paſſes into that of wax. It is opaque. Its ſpecific gravity is 3.5754. It is juſt ſcratched by quartz, gives fire with ſteel, and is not eaſily broken by the blow of the hammer. Its texture is compact, and it is met with in large, thick ſlaty plates, with a decayed yellowiſh-white ſurface. Fracture uneven, of the ſmall and perfect conchoidal kind. Fragments angular and indeterminate, not much ſharpened at the edges. It is infuſible alone as well as with borat of ſoda (borax). When treated with microcoſmic ſalt (phoſphate of ſoda and ammoniac), it exhibits an enamel-like, more or leſs perfectly fuſed ſurface, which, on gradual cooling, ſhews at firſt a reddiſh-yellow, then a green of differently deep tints, and at laſt a dirty yellowiſh-white colour. This change of colours ſeems to indicate ſome metallic ingredients. The native place of this ſtone is the mine Wirum, in the vicinity of Drammen, in Norway.

VIII. INDICOLITE.

The colour of this ſtone is a dark indigo-blue, a little lighter in the fracture, ſo as to incline to the azure or ſky-blue. Its external luſtre is vitreous in a high degree, approaching to the metallic ſplendor. It is untranſparent, and not very heavy. Its ſpecific gravity cannot be accurately aſcertained, on account of the ſmall cryſtals bedded in it. Quartz is a little ſcratched by it. It is eaſily broken. The ſtreak is blueiſh-grey. It feels cold and dry like feld-ſpar. Its texture appears to be compact; but the longitudinal fracture is finely ſtriated, and the croſs fracture ſomewhat uneven, paſſing into the ſmall conchoidal. Its cryſtals are rhomboidal columns, much ſtriated lengthways. The fundamental form of cryſtallization ſeems to be quadrilateral; but for the moſt part theſe cryſtals are polyhedral, needle-ſhaped and ſtellular. It does not fuſe before the blow-pipe. It is found near Uton, in Sweden.

Note. This foſſil reſembles in its colour the lazulite of Profeſſor Klaproth, which I know only by deſcription: but as to its other phyſical and chemical characters it differs from it.

(To be concluded in our next.)

X.—*Experiments*

X.

Experiments on the Conducting Power of Fluids with Regard to Heat.—W. N.

AMONG the papers preſented to the philoſophical world ſince the commencement of the preſent Journal, none occupy a higher rank than the experiments and deductions of Count Rumford on heat; whether we attend to their extenſive and important conſequences, or the powers of philoſophical inveſtigation they exhibit. One of the moſt ſingular facts he has noticed is the ſlowneſs with which heat is conducted through fluids; a fact long ago pointed out and ſtrikingly impreſſed by him, and latterly rendered more remarkable by his inference, that bodies in the ſtate of fluidity have abſolutely no power of conducting heat from particle to particle, but convey it from one ſolid to another merely by their motions or currents. Whether this inference taken in the extreme of ſtrictneſs could be juſtified by his own, or indeed by any experiment, was doubted by myſelf as well as others; but without treating the ſubject in that preciſe manner, it becomes a queſtion of ſome intereſt and curioſity, to decide whether the direct conducting power of fluids be too minute to be aſcertained by careful experiments more or leſs varied from thoſe of the Count. Doctor Thomſon, of Edinburgh, is, I believe, the firſt who undertook and conducted a proceſs of this kind, an account of which is given in his able paper in our laſt volume, p. 529. I need not here advert to the particulars of that memoir, farther than to remark, that in the diſcuſſion of this ſubject in converſation by ſome philoſophical friends who meet weekly at my houſe, our conſideration was more particularly directed to the argument which is drawn at p. 534, in favour of the condenſing power of fluids from the Count's own experiments. It is there obſerved. that when ice was ſlowly melted by boiling hot water ſtanding over it, with the interpoſition of a thin ſtratum of ice-cold water, there were undoubtedly currents produced through this laſt ſtratum, becauſe water becomes denſer either by receiving or loſing heat till it arrives at 40°; but it is contended, that the heat which paſſed to the cold water in this proceſs muſt have been conducted from particle to particle, and not conveyed by currents of the ſuperior water; and farther, that the regular diminution of the numbers expreſſing the temperatures of the ſtrata of water from the ſurface downwards, is inconſiſtent with the ſuppoſition of ſuch currents, which it ſhould ſeem ought to produce a mixture of the fluid.

It appeared to us, that the principal queſtion in this caſe was, whether or not the whole conveyance of the heat were effected by the mediation of the veſſel or ſolid; which it ought to be if the ſolid be the only proper and perfect conductor preſent? That is to ſay, if heat be applied to the ſurface of a fluid, and the effect of that heat be to produce rarefaction, it muſt inevitably follow, in this ſimple ſtatement of the caſe, that no heat would paſs below the range of particles at the very ſurface where the heat was applied, unleſs the fluid itſelf were properly and ſtrictly a conductor.

But

But as the caſe is not ſo ſimple, but muſt neceſſarily include the veſſel, or ſolid coating of the fluid maſs, the conſequences will be different. If the fluid be capable of receiving and giving heat only from and to ſolids, we muſt contemplate the heated parts as rarefied, (that is to ſay increaſed in bulk) and conſequently diffuſing themſelves laterally over the reſt of the ſurface; as coming into contact with and heating the veſſel; as urged to deſcend at the circumference by the ſucceſſive arrival of other more heated particles, and conſequently converging again beneath, and aſcending near the center: we muſt conſider the ſolid ſide as conducting the heat ſtill farther down; and there cauſing an aſcending current to join its water to the converging ſtreams above. So that the water may be conſidered as receiving heat from a ſolid in contact with its ſurface near the center; giving it again to a ſolid at its circumference; and this laſt ſolid in its turn heating another ſtratum of water beneath. As the veſſel ſhould become heated to a greater diſtance downwards, the effect would be more perceptible to a greater depth in the fluid; and from the want of communication between the particles among each other, the aſcents and convergences might have different velocities, according to the temperatures at the ſeveral depths, and preſerve different temperatures in the ſeveral ſtrata regularly increaſing according to the advancement of the operation.

We thought it probable, as the paſſage of heat downwards is very ſlow, that theſe currents, if they exiſt, might alſo be ſo ſlow that different temperatures might be found at one and the ſame time in the ſame ſtratum; and we ſuppoſed the experiment would be more concluſive, if the heating ſolid were made to cover the whole ſurface of the fluid, and to reſt upon the veſſel itſelf. For in this arrangement, as the upper edge of the veſſel would be as hot as the ſurface of the fluid, we concluded that there could be no divergence, but that a ſimple aſcending current would be produced near the ſides, with a deſcending one near the axis of the veſſel. We choſe a veſſel of wood, as the worſt conductor we could make,* for obvious reaſons.

Plate X repreſents the apparatus, A B is a ſtage for philoſophical experiments contrived by Profeſſor S'Graveſande, C is a cylindrical veſſel of mahogany of the following dimenſions; outſide diameter 4.75 inches, outſide height 2.75 inches, inſide diameter 3.5 inches, inſide depth 2.0 inches. In the upper edge, which was conſequently 0.625 inches thick, there was a cylindrical excavation to the depth of 0.1 inches, and of the diameter of 4.5 inches. In the bottom of this veſſel were fixed with ſealing wax, two thermometers L and M, the one in the axis, and the other about 0.2 inches from the inſide ſurface, and the bulb of each ſtood 0.75 inches higher than the inner ſurface, or bottom of the veſſel; conſequently the clear depth of the veſſel from the upper edge to the thermometer, was 1.25

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* A cylinder of glaſs 1 inch in diameter, and 0.25 inches thick, was placed on the horizontal cover of a veſſel containing water kept boiling by a lamp, and a like cylinder of light dry mahogany was placed beſide it at the ſame inſtant. A very minute cone of tallow ſtood on the center of each piece. That upon the glaſs fell down by fuſion in one minute; that upon the wood in two. This method is much more accurate than that of Franklin with long rods deſcribed in Dr. Ingenhouſz's Eſſays.

inches. A plate of ſteel I, four inches in diameter, and 0.3 inches thick, was uſed as the cover for the veſſel; the remaining half inch of the excavation being left to receive the fluid, which would riſe by expanſion when heated. D is a tin veſſel, of which the dimenſions may be ſufficiently known from the figure. It has a looſe cover or valve at E, and is cloſed every where elſe. At H a branch proceeds, which ends in a cylinder F; and at G is a ſcreen, to prevent the heat of the lamp K from affecting C; in addition to which, during the experiment, there was placed a paſteboard ſcreen upon the ſtage near the letter K. It will now be eaſily ſeen that when the veſſel C was correctly filled (except the excavation at top) with any fluid, and the ſteel cover laid in contact with the ſurface, and the veſſel D partly filled with boiling water, and then placed in its ſituation here ſhewn, and, laſtly, the ſpirit lamp K placed beneath the outer end of the branch H,—the temperature of 212° would be conſtantly applied to the ſteel, and the ſurface of the fluid accordingly heated. Whence if any heat proceeded downwards, it would be ſhewn by the thermometers; and if ſlow currents of different temperatures prevailed ſo as to produce the variation before ſtated, theſe thermometers would differ. It was thought that ſuch a difference would prove the exiſtence of currents, and the probability that the wooden veſſel was the chief agent of the conduct; but it was not ſo clearly admitted that their agreement would prove the contrary poſition.

Brevity prevents my relating our arguments and views, and the nature of unpremeditated converſation, as well as other motives, forbid me the honour of mentioning the names of the philoſophers to whom I refer in this diſcourſe. I ſhall likewiſe, for the ſake of conciſeneſs, omit ſeveral experiments which were made with other apparatus of leſs ſimplicity, or leading to the ſame facts. One, however, with water I may curſorily notice. It was made in a glaſs veſſel, with a cork bottom, nearly of the dimenſions of the wooden veſſel here deſcribed. During the time the boiling veſſel E was kept in contact with the ſurface of the water, minute bubbles aſcended ſlowly from the ſeveral parts of the water, which appeared to indicate that there was no perceptible current. A quantity of elaſtic fluid in bubbles occupied the ſurface of the liquid, and prevented its contact with the veſſel. The thermometers were raiſed conſiderably, but ſlowly, as was the caſe in many other experiments with water; but as theſe require to be repeated, I omit the reſults.

Several trials were made with mercury and the apparatus in the Plate. The following was made with much care:

Pure diſtilled mercury was put into the veſſel, and left undiſturbed, with the ſteel cover upon it, for upwards of an hour. The veſſel D was then partly filled with boiling water, and carefully placed on the ſteel cover, and the lamp K lighted. This was at thirty-two minutes paſt eight. The concluſion of the experiment was effected by lifting off the veſſel D by the hand covered with a glove.

Time.

Time.	Inner Ther.	Outer Ther.	Time.	Inner Ther.	Outer Ther.	Time.	Inner Ther.	Outer Ther.
$8^h\ 32^m$	75½°	75½°	37^m	88°	88°	41½	105	105
33	77	77	37½	89	89	42½	106	107
33½	78	78	38	91	91	43	108	109½
34	79	79¼	38¾	94	94	43¾	111	112
34½	80	80½	39	95	95	44½	113	114
35	81	82	39¾	97	97	45	115	116
35½	83	83½	40	98	98	45¾	117	118
35¾	84	84¾	40½	100	100	46	118	118¾
36½	86	86½	41	103	103	46½	120	120¼

Another wooden veſſel, exactly of the ſame dimenſions, and fitting up, was then filled with olive oil, and left with its ſteel cover on for a ſhort time, and then the boiler and the lamp were placed. It is to be obſerved, that the lamp was very ſufficient to keep the water rapidly boiling over the water in the glaſs veſſel; that it boiled but moderately over the mercury; and with much noiſe and tremulous action over the oil. This, and the difference of time employed in heating the different fluids, muſt ſhew either a difference of facility in circulation, or in receiving and giving, or in abſolute conducting power in equal bulks. The mercury, it is ſeen, required ſeven minutes to raiſe the middle thermometer through twenty-one degrees, from 77° to 98°; but the oil required thirty-four minutes, or was nearly five times as long in conducting or conveying the ſame heat.

Table of the Experiment with Olive Oil.

Time.	Inner Ther.	Outer Ther.	Time.	Inner Ther.	Outer Ther.	Time.	Inner Ther.	Outer Ther.
$9^h\ 7^m$	72°	72°	$33\frac{1}{2}^m$	78°	79°+	$41\frac{3}{4}^m$	82¾°	84°
16	73—	73+	34¾	79	80	44	84	85½
22½	74	74⅛	37	80	81	56½	90	92—
27	75½	76	38	80¾	82	61	92	94
31	77	78	40½	82	83⅓	75	98	100

While this experiment was making, another boiler was kept in action upon a glaſs cylindrical veſſel filled with olive oil. An obſerver carefully noticed the ſmall opaque particles ſuſpended in the fluid, but he could pereceive no current during the continuance of thirty minutes.

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On Light & Sound. (see pages 72, 81, 121)

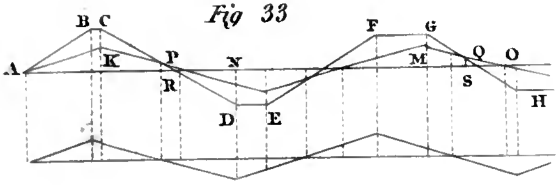


Fig 33

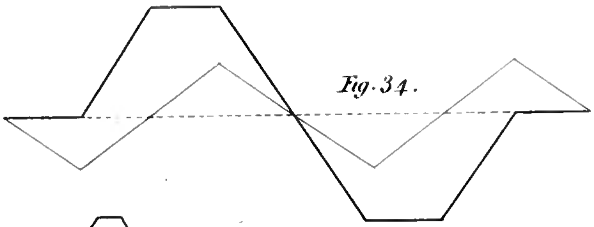


Fig. 34.

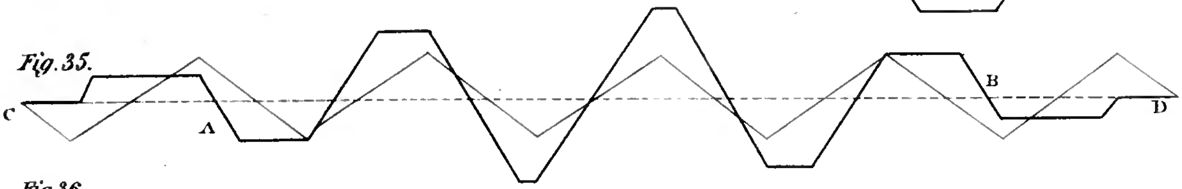


Fig. 35.

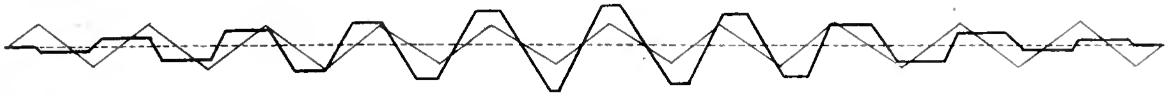


Fig. 36.

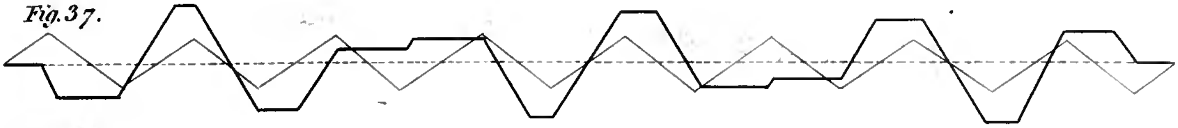


Fig. 37.

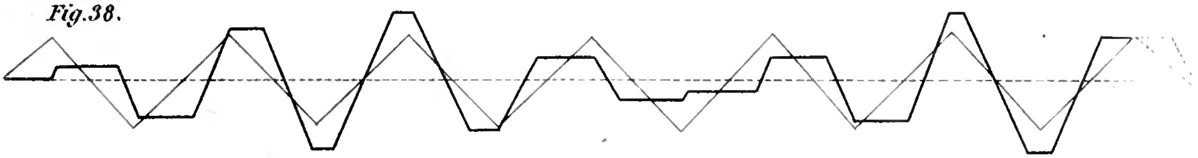


Fig. 38.

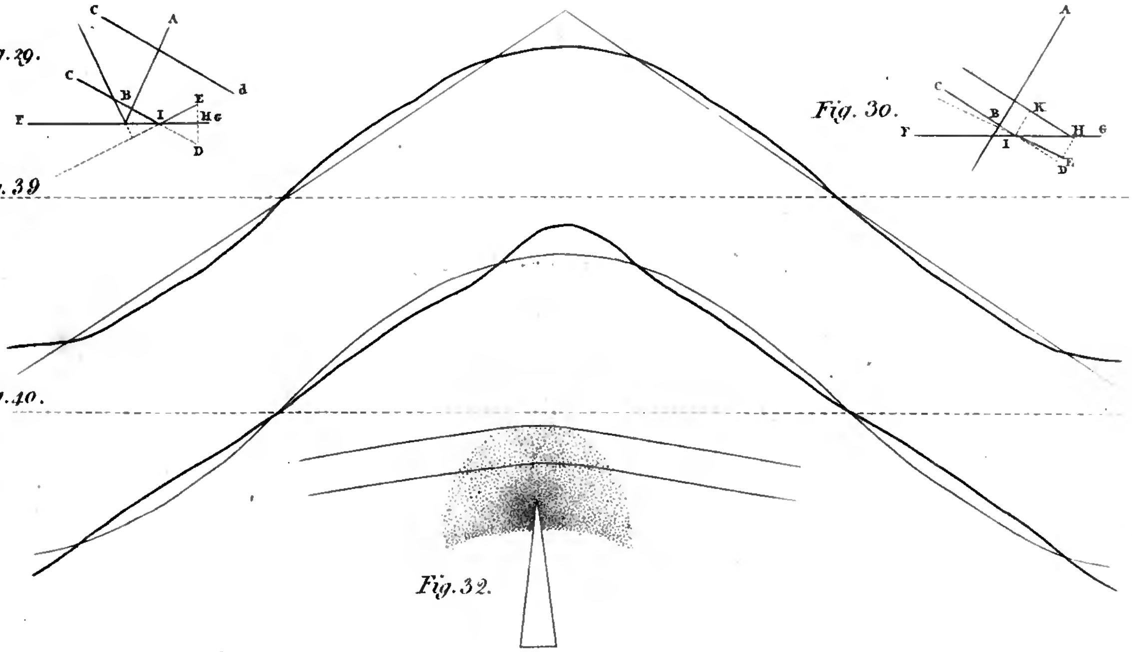


Fig. 29.

Fig. 30.

Fig. 39
Fig. 40.
Fig. 32.

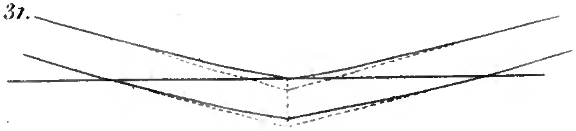
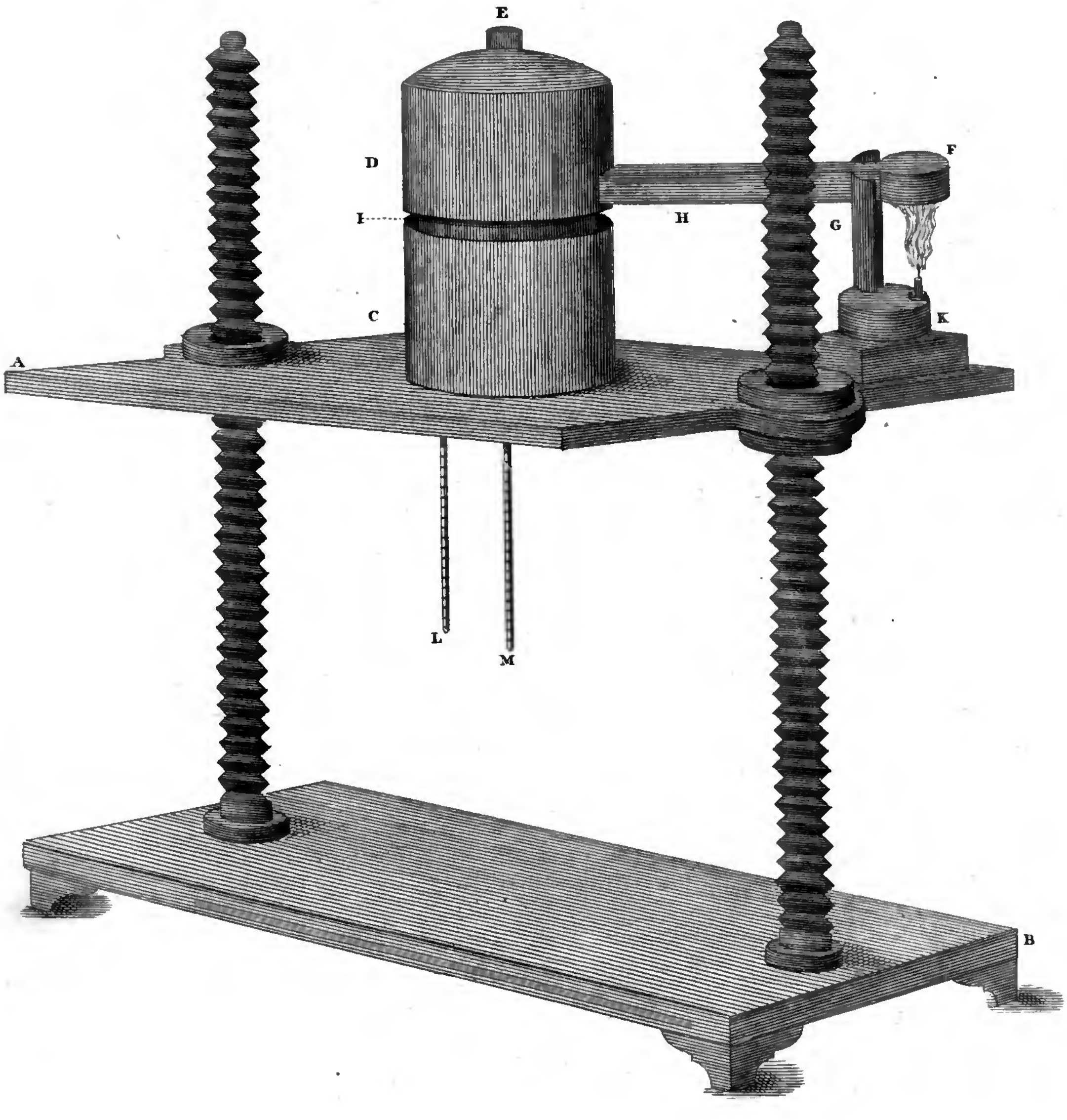


Fig. 31.

Mutlow Sculp.

Transmiſsion of heat thrō fluids downwards.



Mutlow Sculp.

A

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ARTICLE I.

Some Additional Obſervations on Hydrocarbonates, and the Gaſeous Oxide of Carbon. By WM. CRUICKSHANK, *Woolwich.*

IN a former paper on this ſubject the principal object I had in view was to point out, as briefly as poſſible, the difference between the common hydrocarbonates and the gaſeous oxide of carbon; and to ſhew that the latter, hitherto unknown, forms an intermediate ſubſtance between the pure hydrocarbonates and carbonic acid, but not being poſſeſſed of acid properties, was therefore to be conſidered as a true oxide. I likewiſe endeavoured to prove that the preſence of water was not eſſential to the production of this gas, but that it conſiſted ſimply of oxigen and carbon in the proportion of nearly two to one. From theſe facts I inferred that there could no longer be any difficulty in accounting, on the principles of the modern chemiſtry, for the inflammable gas generated in diſtilling a mixture of charcoal with any of the metallic oxides, or in expoſing to a ſtrong red heat a mixture of the carbonates of lime or barites, and iron filings, &c. For in the firſt inſtance the gas was formed by the union of the oxigen of the calx with the carbon, in conſequence of which the metal was revived; and in the ſecond caſe the carbonic acid itſelf was decompoſed, being deprived of part of its oxigen by the metal, which thereby became to a certain degree oxidated.

In the courſe of experiments from which theſe concluſions were drawn, a number of curious facts with regard to the different hydrocarbonates, as well as the gaſeous oxide, occurred; but theſe not being ſo immediately connected with the points to be aſcertained, were, for the ſake of brevity, omitted; I ſhall now, therefore, enumerate ſome of the more remarkable, as well as adduce ſeveral additional proofs of the nature and compoſition of the gaſeous oxide of carbon.

The effects of the oxigenated muriatic acid upon theſe, and indeed upon all inflammable gaſes, are worthy of attention. If the pure oxigenated muriatic acid, in the form of gas, be mixed in certain proportions with any of theſe inflammable gaſes, and introduced into a bottle filled with and inverted over water, although no immediate action may be at firſt perceptible, yet in twenty-four hours a complete decompoſition and change of principles will be found to have taken place, the products varying according to the nature of the inflammable gas employed, as will appear from the following experiments.

I introduced into a phial (with a glaſs ſtopper) filled with and inverted over water, one meaſure of pure hidrogen, and afterwards two meaſures of very pure oxigenated muriatic acid gas;* theſe nearly filled the bottle; the ſtopper was then introduced very tight under water. Before the ſtopper was introduced, a whitiſh cloud appeared in the mixture, but very little or no diminution could be perceived. The neck of the bottle being immerſed under water, was ſuffered to remain for twenty-four hours. At the end of this period the water had riſen a little in the bottle, owing to the imperfection of the ſtopper; but when this was withdrawn, the whole gas inſtantly diſappeared, except about one-tenth of a meaſure, which was found to be azote, and muſt have originally been contained in the two meaſures of oxigenated muriatic acid gas. In this caſe the products were manifeſtly common muriatic acid and water; for the water in the phial contained common muriatic acid, but did not in the leaſt ſmell of the oxigenated. This experiment was repeated with the ſame reſults, except that the reſiduary gas, on the admiſſion of water, was rather leſs.

This fact furniſhes us with an eaſy method of aſcertaining the purity of hidrogen gas in moſt caſes, and even when it may be mixed with other inflammable gaſes, as will preſently appear.

I next tried the effects of this gas upon the pure hydrocarbonates. For this purpoſe I introduced into a bottle, filled with and inverted over water, one meaſure of pure hydrocarbonate (from camphor) well waſhed, mixed with two meaſures of the oxigenated muriatic acid gas. On the firſt contact of the oxigenated gas, a white cloud, and ſome diminution, was perceived; but this ſoon ceaſed; the ſtopper was then introduced, and the bottle ſuffered to remain inverted in water for twenty-four hours. At the end of this period the ſtopper was withdrawn under water, when the fluid inſtantly roſe, and the

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* This gas was always obtained by adding the common muriatic acid to the oxigenated muriate of potaſh. It ſhould never be kept long before it is employed, as it very ſoon loſes part of its oxigen, and becomes decompoſed.

gas occupied only .43 parts of a meaſure, or a diminution of 2.57 meaſures had taken place. This reſiduary gas being agitated with lime water, .09 parts more were abſorbed, evidently carbonic acid gas. The waſhed reſiduum was inflammable, and burned with a blue lambent flame, exactly ſimilar to the gaſeous oxides, but very different from that of the original hydrocarbonate.

This experiment was repeated with nearly the ſame reſults.

In this inſtance, however, the gas had been recently obtained from camphor, which I have always found made a conſiderable difference in the proportions of the products.* On the addition of the oxigenated muriatic acid gas to the hydrocarbonate, a diminution of no leſs than three-fourths of a meaſure immediately took place, and after ſtanding for twenty-four hours, the water, on withdrawing the ſtopper, roſe ſo high, that the remaining gas occupied the ſpace of not more than .54 parts of a meaſure; hence the whole diminution muſt have been 2.46 meaſures. This reſiduary gas contained one-fifth of its bulk of carbonic acid, the remainder being inflammable. Two ſmall meaſures of this inflammable gas, fired with two meaſures of oxigen in a jar, over mercury, were reduced to 2.5 meaſures; of this 1.7 were carbonic acid, and the remaining .8 or .9 oxigen gas; for it required very nearly two meaſures of nitrous gas to ſaturate them.

Hence we perceive that two meaſures of this gas, combined with about one of oxigen, produced no leſs than 1.7 meaſures of carbonic acid; a circumſtance which proves it to have been the gaſeous oxide of carbon.

Being determined to vary the experiments as much as poſſible, to prevent any miſtake or overſight; I introduced into a bottle, filled with and inverted over mercury, two meaſures of hydrocarbonate from æther, and immediately after four meaſures of the oxigenated muriatic acid gas. Theſe completely filled the bottle, which was ſtopped to prevent the acid gas from acting upon the mercury; its neck was then plunged under mercury, and kept down by a weight placed upon it.

After ſtanding for twenty-four hours, the ſtopper was withdrawn under mercury, which inſtantly roſe and filled two-thirds of the bottle; there was a little moiſture depoſited on its ſides, which probably abſorbed the whole of the common muriatic acid gas produced. Lime water in exceſs being now added, one-third was found to be carbonic acid, and the remainder inflammable.

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* It might be ſuppoſed that this difference ariſes from the abſorption of carbonic acid gas; but this is by no means the caſe, for the proportion of this gas in the hydrocarbonates from æther, or camphor, is exceedingly ſmall, never exceeding one-tenth, and frequently much leſs. Beſides, I found, from direct experiment, that the unwaſhed hydrocarbonate required more oxigen to ſaturate it, than the waſhed, the reverſe of which ſhould have been the caſe, had the difference depended upon carbonic acid. This variation in the reſults may be thus ſtated:---One meaſure of unwaſhed hydrocarbonate requires 1.9 of oxigen to ſaturate it, the products being water, and 1.4 meaſures of carbonic acid. But one meaſure of the waſhed gas requires only 1.65 meaſures of oxigen, and produces about 1.15 meaſures of carbonic acid with water.

It appears to me, therefore, that ſomething like a vapour diffuſed through the gas, which may be ſeparated by waſhing, or depoſited on ſtanding, muſt be the cauſe of this change in their properties.

Two ſmall meaſures* of this waſhed gas, fired over mercury with one of oxigen, were reduced to 1.6 meaſures; the whole of which was carbonic acid, except one-fourth of a meaſure which contained oxigen, with a little azote.

This experiment, therefore, as well as the former, after making a deduction for the azotic gas, clearly ſhews that the reſiduary inflammable gas muſt have been the gaſeous oxide of carbon.

The reſults obtained in all theſe experiments with the pure hydrocarbonates and oxigenated muriatic acid gas, are very eaſily underſtood; for it is evident that the oxigenated acid muſt give out its exceſs of oxigen to the carbon and hidrogen of the inflammable gas from a ſuperior affinity; in conſequence of which we have no leſs than four new compounds, viz. common muriatic acid, water, carbonic acid, and the gaſeous oxide of carbon. Thus, then, we ſee that the gaſeous oxide may be obtained without the aid of metals, their calces, or even the carbonic acid; and this ſimple mode of formation appears to me to demonſtrate its nature and conſtituent principles beyond all poſſibility of doubt. This will be ſtill further confirmed by the following experiment:

One meaſure of hydrocarbonate from camphor, mixed with 3.8 meaſures of oxigenated muriatic acid gas, were introduced into a bottle, which they completely filled; this was cloſely ſtopped, and inverted over mercury. After ſtanding for twenty-four hours, the ſtopper was withdrawn under the mercury, which in conſequence immediately aſcended and occupied two-thirds of the bottle. The gas acted upon the ſurface of the metal, and retained a little of its natural greeniſh yellow colour; it was not, therefore, entirely decompoſed. A little diſtilled water being introduced, a further abſorption took place, and the reſiduary gas, amounting only to .8 of a meaſure, was entirely abſorbed by lime water, except a bubble, too ſmall to examine, or even to eſtimate. Now, by comparing this with the former experiments, we perceive that when one part of the inflammable gas is mixed with only two of the oxigenated muriatic acid gas, we have both carbonic acid and gaſeous oxide formed, becauſe the exceſs of oxigen in the oxigenated acid is not ſufficient to convert the whole into carbonic acid; but when one part of the inflammable is mixed with nearly four of the oxigenated, then the whole is converted into carbonic acid and water, the exceſs of oxigen being ſufficient for this purpoſe. The proportion of oxigen in the gaſeous oxide may likewiſe be eſtimated from theſe experiments.

The effects of the oxigenated muriatic acid on the other hydrocarbonates, were very nearly ſimilar.

When firſt mixed, a ſmall diminution in moſt inſtances took place, but after ſtanding for twenty-four hours, the oxigenated acid was completely decompoſed, in conſequence of which the hydrocarbonate was converted into water and carbonic acid gas, and theſe remained only a very ſmall proportion of inflammable gas, which had all the properties of the gaſeous oxide.

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* Two kinds of meaſures were employed in theſe experiments, the contents of the large one being ſix times that of the ſmall.

Being anxious to ſee the effect of this gas on the gaſeous oxide itſelf, I mixed two meaſures of this oxigenated gas with two of the gaſeous oxides well waſhed, and introduced them as uſual into a bottle inverted over water. After ſtanding for twenty-four hours, the ſtopper was withdrawn, and the water inſtantly roſe ſo high, that the remaining gas occupied the ſpace of only 1.8 meaſures, or a diminution of 2.2 had taken place. This, when agitated with lime water, was further reduced to $\frac{4}{10}$ of a meaſure, and the reſiduary gas was ſtill ſlightly inflammable.

This experiment was repeated with only this variation, that the bottle was completely filled, and had its neck plunged under mercury: the proportions of the gaſes were two meaſures gaſeous oxide, to $2\frac{2}{3}$ meaſures oxidated muriatic acid gas. After remaining for twenty-four hours, the ſtopper was withdrawn under water, when the fluid inſtantly roſe and occupied two-thirds of the bottle: This reſiduum being agitated with lime water, the whole was abſorbed, except one-ſixth of a meaſure, which was found to be azote. Hence we perceive that any quantity of gaſeous oxide may be converted into carbonic acid by mixing it with the oxigenated muriatic acid gas, juſt as the oxide of azote or nitrous gas are converted into nitrous acid by the ſame means.

There is another gaſeous ſubſtance, or rather vapour, which is powerfully acted upon by this ſuper-oxigenated gas, viz. the vapour of æther; if we fill a bottle of the capacity of three or four pints with the pure oxigenated muriatic acid gas, taking care to expel the water as completely as poſſible, and then throw into it about a drachm or half a drachm of good æther, covering its mouth immediately with a piece of light wood or paper; in a few ſeconds white vapour will be perceived moving circularly in the bottle; this will be ſoon followed by an exploſion, accompanied with flame; at the ſame time a very conſiderable quantity of carbon will be depoſited, and the bottle will be found to contain carbonic acid gas.*

If we employ alcohol inſtead of æther, a ſimilar effect will be ſometimes produced, but more time is neceſſary, and along with the charcoal and carbonic acid there is likewiſe a little æther formed.

We ſhall next conſider the effects produced on theſe inflammable gaſes, by mixing them with the oxigenated muriatic gas, and ſetting fire to the mixture by the electric ſpark. In many

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* Dr. Prieſtley was the firſt who diſcovered that æther, agitated with any ſpecies of gas, greatly increaſed its volume, and in moſt caſes doubled it. He likewiſe found that although this vapour was not condenſible by the ordinary degrees of cold, yet water readily abſorbed it. Having agitated ſome oxigen gas with a little æther, I found that its bulk was exactly doubled. In this ſtate the gas does not explode; but if one part of this mixture be added to three parts of oxigen, an ignited body, or the electric ſpark, then produces a dreadful exploſion, the products of which I have found to be water, with $2\frac{1}{3}$ parts of carbonic acid gas. Hence it would appear that one part of this vapour requires about ſeven of oxigen to ſaturate it, or more correctly 6.8 the products being water, with 4.6 parts carbonic acid: According to this experiment the proportion of carbon to hidrogen in the vapour of æther, or æther itſelf, ſhould be as 5 to 1 nearly; and in alcohol, from ſimilar trials, it appeared to be as 8 or 9 to 1.

many of theſe trials the reſults differed more from the former, where mixture alone was employed, than could *a priori* have been ſuppoſed, as will appear from what follows.

Three meaſures of pure hidrogen were introduced into a ſtrong glaſs jar over mercury, and four meaſures of the ox. muriatic acid gas added, the electric ſpark was immediately paſſed through the mixture, to prevent, as far as poſſible, the abſorption of the gas by the mercury; after the exploſion, which was feeble, the gas occupied the ſpace of .75 of a meaſure: water being admitted the mercury ſtill roſe higher, ſo that there remained only .2 of a meaſure. Finding that in this experiment the oxigenated gas was in exceſs, the proportions were varied as follows:

Four meaſures of pure hydrogenous gas were fired with four of the ox. muriatic gas; the reſiduum amounted only to half a meaſure, but this contained hidrogen. Hence by comparing theſe experiments together, it would appear that three meaſures of hydrogenous require three and a half of oxigenated muriatic acid gas to ſaturate them; the products being water and common muriatic acid. From the ſame experiments we can likewiſe deduce the exceſs of oxigen in the oxigenated muriatic acid, or the proportion of oxigen neceſſary to convert the common muriatic acid into the oxigenated; for according to the above proportions, two parts of hidrogen gas require 2.3 of the oxigenated to ſaturate them; now two parts of hidrogen require exactly one of oxigen (eſtimating by volume) to produce the ſame effect. Hence every 2.3 parts of the ox. muriatic acid contain one part of oxigen, or about half its bulk more than the ſame muriatic radical when in the form of common muriatic acid gas; this fact may be found uſeful in many chemical inveſtigations where the ſuper-oxigenated acid is employed. In this way too we have a ready way of determining the purity of the hidrogen, for if theſe gaſes perfectly pure, be fired together over mercury in the above proportions, the whole will diſappear; but if the hidrogen ſhould be contaminated with azote, or ſome inflammable gas, there will then be a reſiduum conſiſting either of azote, or carbonic acid gas, or both.

The next experiments were made with the pure hydrocarbonates. Here I found, when two parts of pure hydrocarbonate, recently procured from camphor, were fired over mercury by the electric ſpark, with four of the ox. muriatic acid, that a great diminution, accompanied with a conſiderable depoſition of charcoal took place; the reſiduary gas generally amounted to 1.2 or 1.5 meaſures, and of this lime water abſorbed about 1. or 1.2 meaſures, the remainder being inflammable. In this inſtance the hydrocarbonate did not yield above half the proportion of carbonic acid which it ought to have done, but this is ſufficiently accounted for, from the copious depoſition of charcoal.

When hydrocarbonate from æther was employed, the reſults were very nearly the ſame, and charcoal was more generally depoſited. Finding that in theſe proportions the quantity of oxigen contained in the ox. muriatic gas was inſufficient to ſaturate the carbon and hidrogen of the hydrocarbonates, I next fired one meaſure of the latter with four of the former; after the exploſion, the gas ſtood at two meaſures; a very little diſtilled water

water being added, the mercury roſe to 1.6, and lime water abſorbed the whole of the remainder, except .2 of a meaſure which was inflammable.

In another experiment, no leſs than five meaſures of the oxigenated gas were mixed with one of the hydrocarbonate, but ſtill at the end of the proceſs there remained a very ſmall proportion of inflammable gas, and the whole of the oxigenated was not decompoſed. In theſe inſtances, where we uſe but a ſmall proportion of the hydrocarbonate, the exploſion is very feeble, and there is ſeldom or never any depoſition of charcoal, in conſequence of which there is a more copious production of carbonic acid gas, as may be readily ſeen by comparing the experiments together. Indeed in the laſt one, the ſame proportion of carbonic acid was obtained, as if the gas had been exploded or ſaturated with oxigen; for one meaſure of pure hydrocarbonate requires very nearly two meaſures of oxigen to ſaturate it the products being water, and 1.3 or 1.4 meaſures carbonic acid gas. Now in the experiment with the oxigenated gas, one meaſure of the hydrocarbonate fired with five of this gas, produced juſt 1.3 meaſures carbonic acid, with water, &c. indeed ſince five meaſures of the ox. muriatic acid gas contain an exceſs of oxigen equivalent to two meaſures, or a very little more, this reſult ought to have been expected.

There was nothing very remarkable obſerved in exploding the impure hydrocarbonates with this gas; all of them yielded carbonic acid gas and water, but in none of them did I perceive any ſenſible depoſition of charcoal, except once from the gas obtained from the decompoſition of alcohol. Indeed the pure hydrocarbonates themſelves, in a great meaſure, loſe this property after having been waſhed, or kept over water for ſome time; that from æther retains it the longeſt. Several attempts were made at different times to explode mixtures of the ox. muriatic acid gas, and the gaſeous oxide of carbon, but without ſucceſs. On paſſing the ſpark a little whitiſh vapour was perceived round the metallic points, but this was not accompanied by any detonation, and even after a number of ſtrong ſparks had been taken in this way, it was found on the admiſſion of lime water, which immediately took up the oxigenated gas, that the gaſeous oxide had undergone no ſenſible diminution or change. This circumſtance was rather unexpected; it furniſhes us, however, with at leaſt one criterion for diſtinguiſhing this from other inflammable gaſes.

On comparing the above experiments with the reſults obtained from firing the hydrocarbonates with different proportions of oxigen, ſome obſervations not unworthy of attention may be made. We know that when any of the pure hydrocarbonates are fired with a ſmall proportion of oxigen, a little carbonic acid is produced, and the whole volume of gas inſtead of being diminiſhed is conſiderably increaſed; this increaſed gas contains no diſengaged oxigen, but is nevertheleſs very different from the original hydrocarbonate, requiring only bulk for bulk, about one fourth of the quantity of oxigen to ſaturate it *; no charcoal is ever diſengaged. But when we explode the ſame hydrocarbonate with about twice its bulk of the ox. muriatic acid gas, we have a great diminution, accompanied

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* It does not appear to be gaſeous oxide, which I believe cannot be produced this way.

with the ſeparation of much charcoal, and only a ſmall proportion of carbonic acid generated; the remaining gas, which is inflammable, and in ſmall quantity, appears to be gaſeous oxide. Thus then we find, that by combining the ſame inflammable gas with equal quantities of oxigen under different circumſtances or combinations, very different effects may be produced.

On a former occaſion I remarked, that from the great facility with which the carbonic acid was decompoſed, when a mixture of clear iron filings and chalk were heated together in a retort, it would appear that this acid at a high temperature might be decompoſed, at leaſt partially, by any ſubſtance which had a conſiderable affinity for oxigen; and that this was the only method of obtaining the gaſeous oxide in a ſtate of purity. As this is a circumſtance of conſiderable importance, the following experiments are given in further confirmation of it:

About an ounce of chalk, which had been previouſly expoſed for about ten minutes to a low red heat, was mixed with an ounce of very bright zinc filings, theſe were introduced into a coated glaſs retort, and expoſed to a heat gradually increaſed. A little carbonic acid came over at firſt, but this was ſoon mixed with a large proportion of the gaſeous oxide, and by the time the contents of the retort were thoroughly red, nothing but pure inflammable gas was diſengaged, and in prodigious quantity. It would have been troubleſome, and in fact unneceſſary, to have collected the whole, but portions of it were examined at different periods of the proceſs, and found to continue perfectly pure, and without any ſenſible mixture of carbonic acid gas. Indeed, before I made this experiment, I hardly conceived it poſſible, that ſo complete a decompoſition could be obtained in ſo ſimple a way. Before the gas had intirely ceaſed to come over, the retort was removed, and its contents examined. In the neck of the retort there was found a quantity of the oxide of zinc in the form of flowers, lining its ſurface to a conſiderable extent: a little below this, and cloſe to the belly of the retort, there adhered a little metallic zinc which had ſublimed, but was very much tarniſhed; what remained in the belly conſiſted of the zinc deſtitute of all metallic luſtre, and mixed with an imperfect lime. The pieces of zinc, or rather oxide, crumbled between the fingers into a greyiſh powder, and ſeemed to have intirely loſt their tenacity. It is unneceſſary to add, that the gas obtained was found when fired with oxigen, &c. to have all the properties of the pureſt gaſeous oxide of carbon. The above experiment ſhews in the moſt ſatisfactory manner, how the oxigen abſtracted from the carbonic acid is diſpoſed of, for in the proceſſes with the iron filings this was not ſo manifeſt as might have been expected.

A mixture of very dry chalk and clean filings of tin were next expoſed to heat in a coated glaſs retort; the firſt products collected conſiſted of carbonic acid gas, and the gaſeous oxide in the proportion of 5 to 6. The ſecond or middle portion contained two thirds of its bulk of gaſeous oxide, and what came over afterwards conſiſted of one part carbonic acid, and three parts gaſeous oxide. The quantity in all amounted to ſeveral gallons. After the proceſs the tin was found partly oxidated, and partly diffuſed through the chalk in ſmall metallic globules. The decompoſition of the acid in this experiment was very imperfect, when compared with that in the former, owing no doubt to the different affinities of the metals for oxigen.

Although there could be no doubt, that in all theſe proceſſes with the carbonate of lime and the metals, the carbonic acid muſt have been decompoſed in its naſcent ſtate, yet I conceived that it might give ſtill an additional ſatisfaction to ſhew, that the ſame ſubſtance may be readily decompoſed even when in the ſtate of gas. This was accompliſhed in two ways.

Into a ſtraight iron retort with a cylindrical belly, a quantity of dry and very pure ſand was introduced, and above this a ſtratum of chalk which had been very carefully dried; more ſand was then introduced, and rammed down, until it filled up no leſs than three inches of the cavity above the chalk, and upon this was placed a quantity of very clean iron filings, the whole occupying about two thirds of the belly. The retort was then fixed in a furnace, and made red hot as ſoon as poſſible; the firſt gas which came over was chiefly carbonic acid; the ſecond portion amounting to a gallon, conſiſted of carbonic acid gas one part, and gaſeous oxide three parts; the third portion being upwards of two gallons by meaſure, conſiſted of carbonic acid two parts, and gaſeous oxide ſeven parts; and the laſt portions examined, contained a ſomewhat greater proportion of carbonic acid gas. The gaſeous oxide was found to be of the pureſt kind. In this experiment the carbonic acid was not brought into contact with the iron in its naſcent ſtate, but after it had aſſumed its gaſeous form, and paſſed through a quantity of ſand. The iron filings too being unmixed with the chalk, ſhewed evident marks of oxidation, a conſiderable portion of them being converted into the ſtate of grey oxide.

The other method by which I decompoſed this acid in its gaſeous form, was by making it paſs through a red hot iron tube filled with iron wires. In this experiment, a bladder, mounted with a ſtop cock, and containing a quantity of carbonic acid gas, was attached to one end of the tube, ſo as to be air tight at the joining; the tube was then paſſed through a portable furnace, and an empty bladder mounted in the ſame manner fixed to its other extremity; heat was then applied to the middle of the tube until it became red, taking care at the ſame time to keep its extremities cold, by means of wet cloths wrapped round them. When the tube was ſufficiently hot, the gas was made to paſs ſlowly through it from the full bladder to the empty one, and then back again from the latter to the former. This was repeated ten times, when a little of the gas left in one of the bladders was examined, which by means of the ſtop cocks could be managed at pleaſure. This gas was found to conſiſt of one part inflammable gas, and two parts carbonic acid; the bladder was then re-applied, and the gas forced backwards and forwards ten times as before; on re-examination it was now found to conſiſt of inflammable gas, and carbonic acid gas in the proportion of 11 to 5; but by continuing the proceſs a little longer, no leſs than three fourths of the original carbonic acid gas was converted into an inflammable gas, and which on examination was found to be the true gaſeous oxide of carbon; the wires after the tube had cooled were examined, and found covered in ſeveral places with a beautiful ſhining cruſt of grey oxide, which readily crumbled and fell off on bending the wire. The carbonic acid employed in this experiment was procured from carbonate of ſoda by means of the dilute ſulphuric acid, and amounted to 64 ounce meaſures; during the proceſs very little diminution or increaſe of bulk could be perceived, which is readily accounted for, from the very trifling diminution the gaſeous oxide undergoes when converted into carbonic acid gas.

From the various facts which have been now enumerated, added to thoſe already known, there can no longer remain any doubt, that the carbonic acid, under certain elevations of temperature, may be readily decompoſed by any fixed ſubſtance, having a conſiderable affinity for oxigen; and that with regard to the metals, thoſe ſucceed the beſt which have the greateſt attraction for acids or oxigen, as is remarkably exemplified in zinc.

Upon the whole then I think we may conclude, that there are but two diſtinct combinations of carbon and oxigen capable of aſſuming and maintaining the form of gas; for although the gaſeous oxide may be obtained in a great variety of ways by direct compoſition, as well as the decompoſition of the carbonic acid, yet when pure it is exactly the ſame ſubſtance, having the ſame ſpecific gravity, requiring the ſame quantity of oxigen to ſaturate it, and producing uniformly the ſame proportion of carbonic acid. I remarked indeed formerly, that the gas obtained from charcoal (however well dried) mixed with the metallic oxides, always yielded a little water, when burned in a receiver containing common air or oxigen gas, and this impurity was aſcribed to hidrogen contained in the charcoal itſelf. Now that this may really be the caſe, appears to be in ſome degree confirmed by the following experiments.

A quantity of charcoal obtained by diſtillation in iron cylinders, was introduced into a retort without being moiſtened. A great quantity of gas was in all produced, what came over about the middle of the proceſs conſiſted of carbonic acid and hydrocarbonate in the proportion of 1 to 18.* Two meaſures of this waſhed hydrocarbonate required 1.6 meaſures of oxigen to ſaturate them, the products being one meaſure carbonic acid gas with water. Another portion of the ſame charcoal, which had for ſome time been expoſed to a red heat in a covered crucible, was introduced into a coated glaſs retort, and a ſtrong heat being applied, a conſiderable quantity of gas came over. This gas, except at the very commencement, ſcarcely contained a veſtige of carbonic acid; two meaſures of it required 1.25 of oxigen gas to ſaturate them, the products being water with only .55 parts of a meaſure of carbonic acid. Now by comparing theſe experiments together, it would appear that the gas from the charcoal which had been ſo completely deprived of its moiſture, contained bulk for bulk more hidrogen, and leſs carbon than the gas from that which had not been dried; this being the caſe, it can hardly be ſuppoſed that the hidrogen proceeded from the decompoſition of water, but muſt have ſomehow entered into the compoſition of the charcoal itſelf. The total abſence of carbonic acid may be conſidered as a further

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* The gas obtained in this way is extremely variable, and towards the beginning of the proceſs I have ſometimes met with traces of the gaſeous oxide.

proof of this. Another argument in favour of this ſuppoſition may likewiſe be drawn from the uniform loſs of weight which has been obſerved in burning charcoal in pure air or oxigen gas; that is to ſay, the weight of the carbonic acid produced has never been found equal to that of the oxigen and charcoal conſumed; this deficiency I make no doubt, has ariſen from the production of water which was afterwards held in ſolution by the carbonic acid gas, and could not therefore appear in its natural ſtate. That moſt gaſes contain a large proportion of water at the common temperature of the atmoſphere can hardly be doubted, although it may be very difficult to eſtimate the quantity, for the drieſt gaſes expoſed in cloſe bottles to freezing mixtures, will be found to depoſit a conſiderable quantity of moiſture, which will again diſappear on raiſing the temperature. It is not improbable, therefore, that water may be frequently produced without our being able to detect it. It is no doubt in a great meaſure from this impurity in the charcoal, that all the gaſes obtained from mixtures containing it, are ſo uncertain and variable in their properties.

Woolwich, Aug. 19, 1801.

II.

Short Notice concerning the Properties and external Characters of ſome new Foſſils from Sweden and Norway; together with ſome Chemical Remarks upon the ſame. By Mr. D'ANDRADA. *In a Letter to Mr. Beyer, Maſter of the Mines at Schneeburg.*

(Concluded from page 196.)

IX. WERNERITE.

ITS colour is a medium between the piſtaccia-green and Iſabella-yellow, of different ſhades of yellow. Inwardly its luſtre is of the fattiſh kind, approaching to that of the mother-of-pearl, ſometimes in a high degree reſplendent, and ſometimes a little chatoyant, but without any diſtinct play of colour. In ſmall fragments it is very tranſparent; and its ſpecific gravity is 3.6063. It cuts glaſs, but gives little fire with ſteel, and can be ſcratched by the common feld-ſpar. Its texture is ſomewhat curvilinearly foliated, and when the lamellæ are conſiderably curvilinear, the fracture appears chatoyant. The laminæ ſeem to croſs each other twice in an oblique direction. The croſs fracture is uneven and fine ſplintery; and its fragments are ſplintery, with ſharp edges and pointed. Wernerite occurs in maſſes and cryſtallized. The cryſtals are low, hexahedral columns, with tetrahedral terminations, having their terminating faces impoſed on the lateral edges. In thoſe ſpecimens, which are found in lumps or maſſes, the form of aggregation is large and coarſe granular, and very much concreted. This ſtone readily froths upon charcoal before the blow-pipe, and its edges exhibit an opaque, white, imperfect enamel. It occurs in the iron-mines Northo and Ulrica, in the Arendal territory in Norway, and alſo in Campolongo, in the Lewindale, in Swiſſerland. This foſſil bears great reſemblance to the adamantine ſpar in its colour and luſtre.

X. PETALITE.

The colour moſt frequently occurring is reddiſh, and in ſome inſtances greyiſh-white. Its internal luſtre is ordinary and glittering, now and then with a little ſplendor, and in that caſe of a faint appearance of mother-of-pearl. The edges are a little tranſparent. Specific gravity rather above 2.620. It cuts glaſs and is itſelf ſcratched by feld-ſpar. It hardly ſtrikes fire with ſteel. It occurs in lumps of a fine and alſo of a rather coarſe aggregation. Its texture is foliated or ſcaly. The lamellæ are very minute, throughout ſtrongly concreted with each other. Their croſſing is but ſingle. The fragments of the croſs fracture are angular, amorphous, and not very ſharp in the edges. It is broken with great facility, and eaſily reduced by grinding into a ſubtle, white, rough, and dry powder. When one piece is rubbed againſt another, it emits a faint ſmell reſembling quartz. When treated alone with the blow-pipe it is infuſible, without change of colour or luſtre. With borax it produces a white, tranſparent, vitreous globule, and with microcoſmic ſalt a yellowiſh-white pearly glaſs, full of fine air-bubbles. With nitric acid it does not effervеſce, whether in the ſtate of grains or of powder, but a portion is gradually diſſolved by that acid. Petalite is found near Utoen, Sala, and Fingruſan, near Nyakoperberg, in Sweden.

XI. CHRYOLITE.

Colour ſnow-white. Luſtre faint, like a weak ſplendor of mother-of-pearl. Very tranſparent. Specific gravity 2.9698. It ſcratches calcareous ſpar, but is itſelf ſcratched by fluor-ſpar. It may be readily broken in pieces, and is rather ſoft. It yields a very ſubtle, white powder, of a ſoft feel, which, if moiſtened with water, becomes tranſparent. This foſſil feels dry, and is cold, like feld-ſpar. Its texture thick and broad foliated, with an irregular roughneſs, like water that has been ſuddenly congealed to ice. Its laminæ are ſtraight, and their croſſing threefold. The parts ſeparated by ſplitting, when entire, are of a cubical form. The aggregation of its integrant parts is ſuch, that the juncture of two is always covered by a third, ſuper-impoſed like bricks, which originates from the circumſtance, that two croſſings are ſtraight and of the broad foliated kind, while the third is partly laminated, partly broken, partly uneven. Before the blow-pipe chryolite fuſes even before ignition, like ice melting without effervеſcence; and it yields a ſnowy-white opaque pearly maſs, which by a ſtronger heat becomes rough, full of bubbles, and depreſſed in the middle, being alſo cauſtic in that ſtate when put on the tongue, and poſſeſſed of a taſte ſimilar to that of borax. When heated with borax, this foſſil is fuſed to a pellucid glaſs, which, however, upon cooling is opaque and white. When fuſed with pot-aſh in a ſilver crucible, it turns to a white porcelanic maſs; which, when diſſolved in diſtilled water, and precipitated by means of nitric acid, yields a white, tranſparent paſty maſs, which after deſiccation may be fuſed again in the ſame manner as the crude foſſil. In the nitric and muriatic acid it is inſoluble. With very concentrated ſulphuric acid it ſtrongly efferveſces, emitting then whiteiſh, gaſeous vapours, which attack glaſs. This peculiar foſſil conſiſts of alumine, fluoric acid, and a little pot-aſh. It occurs in Greenland, but its native particular ſpot is not yet known; though it appears to form ſtrata.

XII. SCAPOLITE.

The colour of this ſtone is yellowiſh and greyiſh-white, and alſo ſmoky-grey. Its external luſtre vitreous, from the gloſs of wax to a brighter poliſh, but its internal luſtre has little brillliancy. This ſtone is more or leſs tranſparent on the edges, but it is entirely opaque when decayed. Specific gravity from 3.680 to 3.780. It admits of being ſcratched with a knife, but itſelf ſcratches glaſs. Its ſtreak is white. The cryſtals are nearly rectangular quadrilateral columns (having angles from 85° to 95°), with very ſlightly truncated lateral edges, without pointed terminations. Theſe cryſtals are very minute, and in that caſe almoſt needle-ſhaped; ſeldom large, with tranſverſe ſhootings, and longitudinally ſtriated. The ſmall and very ſmall ones are uſually concreted croſs ways with each other in regular cluſters *(druſen)* and priſmatic; but the larger are totally bedded in the matrix. Their texture is lamellar, but in the larger cryſtals it approaches to the ſtriated. When ſplit a viſible diagonal croſſing is perceived lengthways, and a ſecond one leſs perceptible which is longitudinal and oblique-angular. The croſs fracture of ſcapolite is uneven, and the fragments of this fracture are priſmatic (bar-like). It may be eaſily broken, and is not electric. Before the blow-pipe it readily fuſes, with frothing, into a white reſplendent enamel. It is found in the iron-mines near Arendal, in Norway.

III.

Experiments and Obſervations on the Properties of Wind Inſtruments, conſiſting of a ſingle Pipe or Channel; with Improvements in their Conſtruction. By Mr. WILLIAM CLOSE.

To Mr. NICHOLSON.

SIR,

ON every wind inſtrument of muſic, which has the intervals of its ſcale regulated by holes for the fingers of the muſician, it is impracticable to tranſpoſe the key of an air half a tone, without altogether changing the mode of fingering. If no alteration can be made in the length of the inſtrument, to perform a quick ſucceſſion of chromatic intervals is not eaſy; but when ſome of the chromatic notes are aſſumed for the key of an air, the fingering is attended with ſo many difficulties, that the melody can only be well executed by a performer of tranſcendent abilities.

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The chromatic ſemitones are generally introduced into the natural ſcale of each of theſe inſtruments by the aſſiſtance of ſeveral fingers. If they could be all conducted by one finger, or if all the notes of the ſcale could be raiſed half a tone by opening only one aperture, a ſucceſſion of ſemitonic intervals could be attended with little difficulty. It would be eaſy to introduce any number of ſharp ſounds from another key, and as eaſy to play in the modes of C ſharp, D ſharp, F ſharp, and G ſharp with a greater third, as of C, D, F, and G; of G ſharp, D ſharp, and A ſharp, with a minor third, as in the eaſy keys G, D, and A; becauſe the ſcale in each key, with all its ſounds made ſharp, would be fingered in the manner as previous to its tranſpoſition.

The pitch of an inſtrument, indeed, may be raiſed by contracting its length, but this method can be of little uſe, except for making it accompany other inſtruments. It cannot be uſed in modulation to leſſon the difficulties of fingering, as the melody muſt be ſuſpended before it can be adopted.

Among ſome experiments which I have made with a view to improve the ſtructure of certain wind inſtruments, I have endeavoured to realiſe a project for a very eaſy method of introducing the chromatic ſemitones into the natural ſcale, and of ſharping any number of the diatonic notes at pleaſure: in the courſe of the preſent letter I ſhall communicate the method of doing this, and deſcribe the principal inſtruments which have been conſtructed in the courſe of theſe experimental reſearches.

In deſcribing the inſtruments I ſhall follow the order in which I have arranged the drawings.

Early in the year 1799, I had a large flute conſtructed of pewter, in the ſhape repreſented by Fig. 1, Plate XI. which is ſtill in my poſſeſſion. Its inſide is a winding cavity or ſerpentine tube, into which are made ſeven holes at very convenient diſtances for the fingers commonly uſed in performing the ſcale of C major on the German flute. When all theſe apertures are cloſed, the loweſt ſound is in uniſon with the note repreſented by the line which is marked F by the baſs cliff, or it is one-fifth lower than the loweſt ſound of a German flute, which has three keys for the little finger of the right hand.

Fig. 2 repreſents an horizontal ſection of the tube or cavity, at any height between its higher and lower ſides, on a ſcale of one-fifth of the real ſize. The inſide of the tube is three quarters of an inch broad, and one inch deep.

That part of the inſtrument which contains the aperture for producing the ſound, is laid over three turns of the tube, and is repreſented in Fig. 3, which is a vertical ſection of this part of the inſtrument, to ſhew the depth of the ſides, and the conſtruction of the tube near the ſound hole. A ſudden dilatation in this part is avoided, by the inſertion of a piece of metal, which produces the gradual turn, repreſented by the dotted line: but none of the corners in any other part are there cut off.

Theſe pieces or plates of metal in this conſtruction of inſtruments of this kind, are bended and laid together in the manner delineated in Fig. 4, which nearly repreſents a croſs vertical ſection of the inſtrument between the ſound hole and the apertures for the fingers.

The length of the tube, and the places for the holes, were made to correſpond with the proportions of an inſtrument that was ſtraight, by meaſuring a line that was repreſented as drawn in the middle of the tube on a plan ſimilar to that delineated in Fig. 2, but of the real ſize. The line took a ſemicircular turn at the end of every partition, and deſcribed the half of that curve at five other places. The empty corners were all diſregarded in the calculation.

The holes for the fingers are marked 1, 2, 3, 4, 5, 6, 7, in Fig. 2: the hole 8 is covered with a valve or key, which is depreſſed by the little finger when the inſtrument is required to ſound G ſharp; at all other times, when the aſſiſtance of the little finger is requiſite to ſteady the inſtrument, it is put down upon the ſeventh hole inſtead of being laid upon the key, as is commonly practiſed in playing the German flute.

That all ſounds of the ſame name ſhould be perfect octaves, is one of the firſt requiſites in the ſcale of every inſtrument. It was expected that the angles or dilated parts in the anfractuous ſtructure of this flute would render the octaves too flat, but when the ſeven loweſt ſounds were tuned by adjuſting the ſizes of the holes there was very little imperfection in each correſponding ſound of the ſecond octave. D and E, anſwering to A and B in the higher octave of the German flute, are thoſe that are a little defective: they are too flat when the ſeventh hole is open, but when it is cloſed they are made true. Cloſing this aperture does not affect D and E in the lower octave.

An inſtrument of this kind ſhould be made light, as it is generally ſupported in an horizontal poſition when applied to the lips, and the chief of its weight reſts upon the thumb of the left hand. The form may be uſed for large inſtruments when it is requiſite to contract the length, and bring the holes for the fingers into a convenient compaſs.

A flute of the ſame pitch with the German flute was conſtructed of the ſhape of Fig. 1, but the ſecond octave of its ſcale was very defective, even when every precaution had been taken to fill up all the corners.

A ſmall ſtraight flute, which is about four-tenths of an inch in diameter, thirteen inches in length from where it is filled with the cork, with ſeven holes for the fingers, and the neareſt of theſe to the ſound hole about five inches diſtant from its centre, will perform a ſcale of three octaves and a full tone, and will ſound B and either of its octaves when only the firſt hole is cloſed. The compaſs of the ſcale of a common fife with ſix holes is two octaves and a ſixth. The extenſion of the ſcale by a fife of theſe dimenſions beyond that of the common one is owing to the additional piece of tube beyond the ſeventh hole. As performers on the fife generally uſe the acuteſt ſounds of the inſtrument, it will be of advantage to make this addition, even when the ſeventh hole cannot be cloſed by the little finger: it will extend the ſcale, and make ſome of the acuteſt ſounds eaſier to be taken in ſucceſſion, by rendering the mode of fingering more ſimple.

I had once a very ſmall metallic flute or fife which would play any two notes in the firſt and ſecond octave which were octaves to each other, both together, and when it was rightly blown they were perfectly in tune. The ſound hole was elliptical, and rather wide, and its edges were ſharp. The holes for the fingers were wide, and the ſides of the pipe were thin.

I ſhall now proceed to explain the method of tranſpoſing the key of an air on the German flute without any material alteration in the mode of fingering.

If one end of a round pipe, which is one-fifth of an inch in diameter, and two inches and an half long, be inſerted into the ſide of a German flute, about one inch and three quarters nearer the ſound hole than the aperture, which is uſually cloſed by the foremoſt finger of the left hand, all the ſounds in the loweſt octave from C to B will be raiſed half a tone while both its ends are open, and all, or any of them, may be reſtored to the original pitch by only cloſing its exterior orifice.

If the end of another pipe, which is rather wider than the former, and one inch and an half long, be inſerted into the flute between, and equally diſtant from, the fourth and fifth holes from the ſound hole, theſe ſounds of the ſecond octave C, D, E, will be raiſed nearly half a tone. When C is produced, only the ſecond hole from the ſound hole ſhould be open.

If the end of a third pipe, which is one quarter of an inch in diameter, and one inch long, be inſerted into the flute at the ſame diſtance from the ſound hole as the neareſt aperture but one, all the notes in the ſecond octave between C and C will be raiſed half a tone when all the three pipes are open.

To reduce this to practice: Let the firſt pipe be ſo laid by the ſide of the flute that the end which opens into the external air may be cloſed by the thumb of the left hand; and let the other two pipes be ſo diſpoſed that their ends may be cloſed by two valves or keys, which may be both opened at once by either the thumb or the little finger of the left hand.

Fig. 5 repreſents a German flute of the common ſize, which is chiefly conſtructed of tin and copper, and has the three additional pipes. A ſection of it is delineated in Fig. 6. The places of the holes for the fingers are there numbered 1, 2, 3, 4, 5, 6, 7. There is a turn in the tube between the ſixth and ſeventh holes, in order that the ſeventh may be cloſed by the little finger of the right hand inſtead of a key. The three pipes are arranged in the order that has been recommended. That for ſharping the notes of the lower octave is cloſed by the thumb, and the other two by keys, which are delineated in Fig. 5.

The flute is rather ſtraighter at the turn than in any other part, when it was not contracted between the ſixth and ſeventh holes; ſome of the ſounds in the ſecond octave, particularly F ſharp and G, were put out of tune when the ſeventh hole was cloſed.

When the above-mentioned pipes are added to flutes made of wood, they may either be let into the material, or formed by making grooves in the ſide of the inſtrument after it is lined with a metallic tube: theſe grooves may then be covered with pieces of metal, to which may be fixed the keys.

By the aſſiſtance of theſe pipes it is very eaſy to perform a chromatic ſcale, or at pleaſure to tranſpoſe the key of an air half a tone. The truth of the intervals in the key thus aſſumed depends very much upon the width of the pipes. Thoſe of the flute, delineated in Fig. 5 and 6, would be pretty true; but one or two octave notes of its original ſcale are too ſharp, and this imperfection is tranſmitted to the tranſpoſed ſcale.

To determine whether the original ſounds of the ſecond octave could be raiſed by cloſing the interior ends of theſe pipes, or (which would anſwer the ſame purpoſe in diminiſhing the internal capacity of the inſtrument) by filling them with water, I made the following experiment:

The three pipes were inſerted into that ſide of the flute oppoſite the holes for the fingers, and nearly at right angles with the tube. The lower end of each was then juſt immerſed in water contained in a ſmall veſſel, and while the flute was held in a poſition nearly horizontal, ſounding each of the notes C, D, E, F, G, A, B, of the ſecond octave, and C, D, E, of the third, the veſſels were raiſed ſo high, that the water would very nearly fill to the higher end of each pipe. No variation in any of theſe ſounds C, D, E, F, G, was perceptible: but the pitch of the five higher, A, B, C, D, E, roſe a little with the aſcent of the water. The variation, however, was ſo ſlight as to be no objection to the uſe of the pipes.

When an air is tranſpoſed out of D into D ſharp by opening theſe ducts, the key note of the original mode D muſt be uſed for the ſharp ſeventh of that which is adopted, inſtead of C ſharp, which, when fingered in the uſual manner, is too flat; for the ſame reaſon it is alſo preferable in the ſcale of D ſharp, to uſe the common C inſtead of B ſharp for the major ſixth major; and when an air is tranſpoſed out of middle C into C ſharp, to uſe the common C ſharp for the key note.

Theſe, and ſome other defects, induce me to think that the following method of tranſpoſition by one pipe will be moſt eligible, as the fingering of the ſcale in any common key need not be altered.

Inſert one end of a round pipe, three-tenths of an inch in diameter, and one inch long, into the inſide of a German flute, ſo much nearer the holes for the fingers than the ſound hole, that a line which encircles the flute, and paſſes through the middle of this laſt hole, may be ſeven-tenths of an inch from the centre of the interior orifice of the pipe, and all the ſounds of the flute will be raiſed about half a tone when the ends of the pipe are open. Turn this pipe by the ſide of the flute, and let its exterior orifice be cloſed by a valve or key, which may be opened by the thumb of the left hand.

In the model with which I made the experiment, the pipe was inſerted into the contrary ſide of the flute to that into which it ſhould be inſerted, when the inſtrument is intended for practice: it is delineated in this poſition by the two dotted lines in Fig. 6. I have not had an Inſtrument conſtructed upon this plan, but I think it much preferable to the method propoſed before. The flute may have all the common keys, and the fingering of no ſcale will

will require any change after it is tranſpoſed a ſemitone higher. The B flat key, however, cannot be uſed in ſcales that are tranſpoſed; but there will not be much occaſion for it at all, as the long key here recommended will ſupply its place, and be as eaſy to manage.

The pipe delineated by the two dotted lines in Fig. 6, was filled with water while each note of the flute was ſounding, but the variation produced in one or two notes of the ſecond octave by the aſcent of the water was ſcarcely perceptible. Entertaining ſome doubt, however, that B and its octave were not perfectly in tune with each other, I compared them both with two correſponding ſounds of an organ which were perfect octaves. The higher ſound of the flute was a true octave to the ſound of the lower toned organ pipe, but the lower was rather too ſharp to be exactly in tune with the ſound of the higher pipe, unleſs the flute was carefully blown. After the tranſpoſing tube was removed, the relation between theſe two laſt ſounds was little amended.

The octaves of A and B, rendered ſharp by opening the end of the pipe, were as true as the octave ſounds of German flutes are generally found to be when compared with other ſounds which are perfectly in tune. Without making any further remarks on this method of tranſpoſition, I ſhall proceed to other objects.

Wind inſtruments of muſic, which have their notes regulated by ſeveral apertures, will generally produce two or more ſounds, differing conſiderably in acuteneſs from one another, when all their holes are cloſed, or when any number of them are open, if the ſtrength of the current of air employed in ſounding the inſtrument be gradually increaſed. All the ſounds in any ſeries thus produced above the loweſt of the ſeries, are called harmonics: thus when C is the loweſt ſound of a German flute, the harmonics are *C*, *G*, *c*, *e*, *g*.

In the ſcales of the flute, the oboe, clarionet, and other inſtruments, a ſucceſſion of the loweſt ſounds in diatonic order are produced by opening ſeveral holes, until the higheſt is only a tone or half a tone lower than the loweſt harmonic of each inſtrument with all the holes cloſed. This harmonical ſound is then taken into the ſcale, and another ſeries is produced by opening the holes a ſecond time, while the current of air is accelerated.

On the flute and the oboe theſe latter ſounds, the aſpiring offspring of thoſe below, ſhould be each an octave to its progenitor, but on the clarionet each ſhould be a twelfth. The number of apertures that will be required to complete the lower ſeries, depends upon the number of diatonic notes which compoſe the interval between the loweſt ſound of the inſtrument and its loweſt harmonic: each note included between theſe muſt have an aperture to regulate its pitch. When the interval is an octave, ſix holes will ſuffice for this purpoſe; but when it is a twelfth or compound fifth ten muſt be employed.

The requiſite pitch of each note is obtained by adjuſting the ſizes of the holes, and making them dilate conſiderably as they approach the inſide of the inſtrument. We have before noticed the effect of opening a pipe which was all throughout of the ſame width, connected with the inſide of a flute, and from what has been ſaid, it is eaſy to underſtand why

why this ſhape in the holes is ſo univerſally adopted. As every aperture muſt regulate two ſounds, it may juſtly be conſidered a deſirable requiſite, that the width of every wind inſtrument ſhould be ſo proportioned, that the interval between each of thoſe ſounds produced with the leaſt effort, and theſe propagated by increaſing the force of the current, ſhall equal either an octave or a twelfth. Several adventitious circumſtances, however, often deſtroy the harmonical relation which might otherwiſe reſult from the dimenſions of the tube.

The harmonical part of the ſcale of the German-flute is nearly true, bnt the ſame part of the ſcale of thoſe inſtruments, whoſe ſounds are propagated by the vibrations of a piece of reed, are generally very imperfect. I ſhall only make a few obſervations reſpecting the clarionet, as I have made no experiments with any other inſtrument of this kind.

The pitch of the higher notes in the ſcale of the clarionet depends very much upon the ſtrength of the reed, when the inſtrument is only blown, and the reed not preſſed by the lips of the performer. A reed that is ſtrong enough to perform the higher ſounds in tune, unaſſiſted by preſſure, will ſoon fatigue the performer, and ſcarcely play the ſounds of the lower ſcale: on the contrary, a reed that is ſlender enough to play the loweſt ſucceſſion of ſounds full, will not play the ſcale of twelfths true, unleſs it be preſſed ſtrongly by the lips, and if any of the holes are widened ſo much as to produce the requiſite pitch in the harmonical ſound, then its progenitor will be made too high, compared with another ſound which ſhould be its octave: thus it happens, that adjuſting the ſizes of the holes to the ſounds in one part of the ſcale, deſtroys their conformity in another part. It is however abſolutely neceſſary, that the octaves ſhould be true, and that the reed ſhould be ſo ſlender as not to fatigue the performer; and therefore the higher ſounds are generally made in tune with the lower, by preſſing the reed—a method which is not agreeable, and requires much practice to regulate.

If every aperture of an inſtrument only regulated the pitch of one ſound inſtead of two, the temperament of the ſcale might be eaſily adjuſted, but from the limited number of holes that can be conveniently managed by the fingers, a ſeries of diatonic notes could ſcarcely be carried through two octaves. An inſtrument upon this principle, I have frequently thought however, would ſerve very well for performing the baſe, or ſome part between the baſe and the treble, accordingly I have couſtructed one in the ſhape of Fig. 7, with a reed like a clarionet.

Fig. 8. is a ſection, which contains the proportionate length and breadth of the inſtrument upon a ſcale of one fifth of the real ſize. The inſide of the tube A is five lines in breadth, and ſeven in depth. To this tube is fixed the piece which carries the reed, and two other pieces or branches B and C, which have the ſame dimenſions as A. There is alſo a ſmall pipe repreſented under the figures 1, 2, 3, 4, which opens into this tube. The inſtrument has nine apertures on its higher ſide marked 1, 1, 2, 3, 4, 5, 6, 7, 8, which are cloſed by the fingers; and two on its under ſide marked 9, 10. The ninth hole is cloſed by a valve or key, and the tenth by the thumb of the left hand. The end of the ſmall pipe

at

at 5 opens on the under ſide, and is cloſed by the thumb of the right hand. The holes marked 11, 12, are always open, they are on oppoſite ſides of the tube, and let out the air when all the other apertures are cloſed.

The loweſt ſound of the inſtrument is C, repreſented by the ſecond ſpace under the line marked F by the baſe cliff. The ſounds D, E, F, G, A, B, *C*, *D*, *E*, *F*, are produced by opening the holes 10, 9, 8, 7, 6, 5, 4, 3, 2, 1, 1, and theſe are all the notes that can be conveniently obtained in this manner, unleſs there be a key for the foremoſt finger of the left hand to add *G*, which ſound however, is as eaſily produced by cloſing all the holes except thoſe marked 1, 1; after this, by opening the hole 8, while 1, 1, are open, the next note *A* will be added to the ſcale. Other notes ſtill higher may be thus obtained, but they are not ſo eaſy to command. The ſmall pipe under 1, 2, 3, 4, is made of a thin piece of braſs; it is flat, and forms part of one ſide of the tube A; if it was round, it would be two lines in diameter. When any of the chromatic ſemitones between C and *G* are introduced, the end of the pipe is opened, and alſo if it is requiſite to change the key of C for that of C ſharp.

The holes marked 1, 1, are each in a different part of the tube, but as they are only ſeparated by the thin diviſion between the tubes A and C, they are as eaſily cloſed by the foremoſt finger of the left hand as one hole. When the aperture 1 in the tube A is only open, the ſound *G* is too flat, but when the hole 1 in the branch C is alſo opened, it is a true twelfth compared with the loweſt ſound of the inſtrument, although the ſecond ſound, which is produced by ſtrengthening the current of air when all the holes are cloſed, is only a compound fourth.

The branch B brings the hole 7 conveniently under the little finger of the right hand. The width of a part of this branch is contracted by the inſertion of a thin plate in the inſide, as is faintly repreſented in Fig. 7.

The hole 8 is cloſed by the little finger of the left hand: it is made wide, that the ſound F may be true when the holes 9, 10, are either cloſed or open. The length of the reed, and the ſlanted ſurface of that part over which it is placed, may be determined by the lines drawn under Fig. 8. the higher line repreſents the flat ſide of the reed, the lower ſhews the inclined ſurface of the inſtrument.

This inſtrument is portable and eaſy to play; the intenſity of its tone may be varied without any variation in its pitch; it requires a piece of reed which is very ſlender, and may be ſhaped in a few minutes. The ſcales of C and F major, and A and E minor will perform the beſt, but theſe may be tranſpoſed half a tone by opening the ſmall pipe under 1, 2, 3, 4. The muſic may be written with the C cliff placed upon the loweſt line but one of the ſtave.

The compound fourth produced by this inſtrument in place of the twelfth or compound fifth, ſuggeſted an alteration in the ſcale of the clarionet, namely, by a ſlender reed to ſubſtitute a ſcale of elevenths, or even tenths in place of the ſucceſſion of twelfths.

Fig.

Fig. 9 repreſents a metallic clarionet, which was conſtructed for this purpoſe. It will perform the notes of a diatonic ſcale to the extent of two octaves and a fourth. The common holes for the fingers are marked 1, 2, 3, 4, 5, 6, 7: between 3 and 4 there is an additional hole for the little finger of the left hand, and about half an inch nearer the reed than the firſt, and on the oppoſite ſide of the tube there is an aperture for the thumb. The proportions of this figure are equal in length to one fourth of thoſe of the inſtrument. The tube is five eighths of an inch in diameter beyond the figure 7; at 1 it is about half an inch. The loweſt ſound of the inſtrument with all the holes cloſed, is G repreſented by the ſpace immediately above the line, which cauſes the baſe cliff. When the inſtrument is blown eaſily, and the holes 7, 6, 5, 4, *llfh*, 3, 2, 1, *th*, are opened one after another, the following ſucceſſion of notes will be produced, A, B, C, D, E, F, G, *a* B: when the hole 5 is opened for C, 6 ſhould be cloſed. For B flat, cloſe the hole 1 while the thumb hole is open. For C the compound fourth of the loweſt ſound G, cloſe all the holes except the firſt and the thumb hole, or either of them. While blowing a little ſtronger cloſe theſe, and open the holes in the ſame order as for A, B, C, D, E, F, G, A, B, and the following ſucceſſion of ſounds will follow, D, E, F, G, A, B flat, B, C. If the higheſt C be too flat, open the thumb hole and cloſe the ſecond. Another ſound ſtill higher may be added, by preſſing a key with the foremoſt finger of the left hand. The compaſs of the ſcale will then be equal to two octaves and a fifth. A ſmall pipe might be introduced into the ſide of the inſtrument oppoſite the ſeventh hole, and its exterior end cloſed by the thumb of the right hand; when it is opened the loweſt ſound G, and C its compound fourth, will be raiſed half a tone. It would alſo be advantageous to have a joint near the wider end of this clarionet to vary the pitch of middle C, in the modes of C and F; when this note is raiſed a little, its octave below may be made true by opening the ſixth hole. An inſtrument of this kind may be made of wood. It ſeems well adapted for performing the parts generally aſſigned to the *firſt* clarionet. All the ſounds of its ſcale are nearly of an equal ſtrength. The fingering of its ſcale is eaſy, as none of the common notes are introduced by opening apertures which are cloſed by inanimate valves or keys. The reed is ſlender, and requires little preſſure from the lips in performing the higher ſounds of the ſcale. Keys are not neceſſary in the diatonic ſcales of C and F, but for introducing F ſharp in the lower ſeries of notes, and E flat in the higher, two keys will be of advantage. A baſs inſtrument as long again as this clarionet, with the ſame kind of a reed, and with the holes for the fingers diſpoſed in the ſame order, may be made in a ſerpentine form like the part 1, 2, 3, 4, 5, 6, in Fig. 2, but if it be intended to perform an extenſive ſcale, all dilated parts ſhould be avoided, or the ſounds in the higher ſeries will be too flat when produced by a ſlender reed.

I ſhall finiſh this long letter with an explanation of the tenth figure. When the external air is excluded from the ſide aperture of an organ pipe, and the ſound is carried with the current which produces it, through another pipe of the ſame dimenſions as that which determines the pitch, the ſound is conſiderably weakened, and its pitch is raiſed. Fig. 10, repreſents the ſection of the inſtrument with which I made the experiment. It conſiſts of two pipes A and B. A ſquare pipe was conſtructed of metal, with an aperture in one ſide like thoſe pipes of an organ in the flute ſtop. When its end was open its loweſt ſound was in uniſon with G above the fifth line of the treble ſtave.

A thin piece of metal was now turned over three ſides of the piece of wood upon which the pipe A had been turned, and its edges were ſoldered to two corners of that pipe. Two tubes being thus conſtructed, the aperture for producing the ſound was in the partition between them. When that end of the tube B which is neareſt the ſide aperture was cloſed, and the inſtrument was blown, its tone was conſiderably changed, and a full note higher.

An inſtrument of this kind may be uſed when any of the gaſes are employed in the production of ſound.

The harmonical ſounds of this pipe are A, octave to its loweſt ſound; E, twelfth; A, double octave or fifteenth; and C ſharp, ſeventeenth major. I conſider the change in its original tone and pitch to be owing to the diminution of preſſure upon the ſide aperture. Flame produces nearly ſimilar effects. Bring the flame of a candle very near the ſide aperture of a ſmall organ pipe, and its ſound will riſe conſiderably.

I think a muſical inſtrument might be formed upon a plan nearly ſimilar to that of Fig. 10. After the proper length of the pipes A and B is determined, ſix holes may be made into each, ſo near the partition, that all the twelve may be cloſed by ſix fingers.

Our ſmall wind inſtruments have many imperfections, but are the objects of ſo little direct importance to ſociety, that we do not expect much celerity in their improvement.

I am, SIR,

Your humble Servant,

WILLIAM CLOSE.

Dalton, July 24, 1801.

IV.

On the Production of artificial Cold by Means of Muriate of Lime. By Mr. RICHARD WALKER. *(Philoſ. Tranſ.* 1801.*)*

THE ſubject of the means of producing artificial cold, or the conſtitution of frigorific mixtures, I had conſidered as exhauſted, in the Papers I have already had the honour to lay before this Society: ſo far as relates to that part of the ſubject which conſiſts in generating artificial cold *without the uſe of ice*, it ſtill remains ſo with me, having nothing new to offer.

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A conſiderable acquiſition however having been made, ſince my laſt Paper "On the beſt Methods of producing artificial Cold,"* by the diſcovery that a neutral ſalt, but little known, or attended to, by chemiſts before, produced extraordinary effects of this kind with ice, it could not fail of attracting my attention.

Since the time I firſt became acquainted with this circumſtance, I have, as opportunity offered, been engaged in making a variety of experiments with this ſalt, which I flatter myſelf, if the Society have not already received a communication on the ſubject, may not prove unacceptable.

Before I relate my own experiments, it may be proper to premiſe a ſhort account of thoſe of Mr. Lowitz, the author of the diſcovery.

The reſult of Mr. Lowitz's experiments are, in *his* Memoir,† given according to the ſcale of Reaumur; but, in *this*, are throughout reduced to that of Fahrenheit.

Mr. Lowitz, profeſſor of chemiſtry in Peterſburgh, having found, by an experiment made in the winter of 1792, that cauſtic vegetable alkali, in a ſolid ſtate, produced a degree of cold far exceeding any other ſubſtance before mixed with ſnow, *viz.* 83 degrees, determined to proſecute the ſubject; and, upon reflection, conſidering that the deliqueſcent ſalts were likely to be fitteſt for his purpoſe, fixed chiefly upon the claſs of muriatic ſalts, or thoſe which have their baſe neutralized by the muriatic acid. The reſult of his experiments was the diſcovery, that cryſtallized muriate of lime ſunk the thermometer 82 degrees; and that the other neutral ſalts of this claſs, though much inferior to that ſalt, exhibited nevertheleſs remarkable powers of the ſame kind.‡

Profeſſor Lowitz, in the Memoir alluded to, obſerves that he has repeated my experiments with chemical ſalts and *ſnow*, but could not produce a degree of cold below + 2°. Here is evidently ſome miſtake; for it is ſufficiently known, that the novelty of my experiments depends on the production of cold, *without the uſe of ice* in any form.§

Pr. Lowitz having found by experiment that, at the temperature of + 27°, *four* parts of muriate of lime to *three* of ſnow produced a temperature of — 55°, and that an increaſe of the ſalt, even in the proportion of two to one, did not diminiſh the effect, determined the beſt and *ſureſt* proportions to be, three parts of the muriate of lime to two of ſnow.

But, ſince we ſhall find hereafter the temperature of + 32° to be a more convenient term of compariſon, we may fairly ſtate the fact thus; that muriate of lime three parts, and ſnow two parts, mixed at the temperature of + 32°, will give — 50°.

The ſnow, to produce the greateſt effect, he ſays, ſhould be freſh-fallen, dry, and light or uncompreſſed; and the ſalt perfectly dry, and reduced to very fine powder.

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* Phil. Tranſ. for 1795, p. 270.

† See a tranſlation from Crell's Chemical Annals for 1796, by Monſ. Van Mons, Vol. XXII. p. 297, of the *Annales de Chimie.*

‡ Profeſſor Lowitz no ſooner diſcovered the great efficacy of the muriate of lime for this purpoſe, than he gladly rejected the cauſtic vegetable alkali, on account of its burning quality; the difference being *one* degree only.

§ See the table of frigorific mixtures. Phil. Tranſ. for 1795, p. 279.

Pr. Lowitz's method is, to add at once the ſalt to the ſnow; the latter being previouſly put into a convenient veſſel. As the ſalt produces the greateſt effect whilſt it retains the greateſt quantity of water of cryſtallization, he prepares it during a freezing atmoſphere, pounds it, ſifts it, and keeps it in cloſe bottles, in a cold place. With a mixture of this kind, Pr. Lowitz froze, in one experiment, 35 pounds of quickſilver.

Pr. Lowitz obſerves, that with the above precautions and management, it is impoſſible to fail in the deſign of freezing quickſilver with it.

Pr. Lowitz found likewiſe, that the muriate of lime, prepared as above, produced 38 degrees of cold by ſolution in water; that is, by adding 3 parts of this ſalt, in powder, to 2 parts of water, each at the temperature of + 36°$\frac{1}{2}$, the thermometer ſunk to — 1°$\frac{1}{2}$.

Pr. Lowitz adds, that the muriate of lime which has been uſed for making frigorific mixtures, may be procured again repeatedly, as fit as at firſt for the ſame purpoſe, by evaporation and cryſtallization. *

Having given an account of Pr. Lowitz's experiments, I ſhall now briefly relate the reſult of a ſeries of experiments made by myſelf, which occurred to me in conſequence of Pr. Lowitz's diſcovery.

My firſt object was, to repeat the foregoing experiment under ſimilar circumſtances; but the thermometer (the temperature of the air and materials being + 32°) ſunk, in this inſtance, no leſs than — 48°.

The liquefaction, in the above inſtance, was remarkably ſudden; and the full effect was produced, as it were, in an inſtant.

Secondly, with a view either of producing a very extraordinary degree of cold, or aſcertaining the point at which this ſalt ceaſes to produce further cold, I mixed the ſame materials, previouſly cooled by art to 40° below 0, when the thermometer ſunk to 63° below 0. †

Thirdly, ſome of the ſame ſalt, in a cryſtallized ſtate, was ſet out to deliqueſce in the open air: this liquor mixed with ſnow, each at the temperature of + 32°, gave a cold of — 20°.

Hence it appears, that 52 degrees of heat were abſorbed, or rendered latent, by the liquefaction of the ſnow, and 28 by that of the ſalt, in the firſt experiment; that is, in the whole, 80 degrees.

The muriate of lime uſed in the above experiments was prepared thus: muriatic acid 1 part, and diſtilled water 3 parts, were thoroughly mixed; this liquor was then perfectly ſaturated with whiting, and, when clear, poured off. The mixture was afterwards evaporated, till it cryſtallized in air at + 32°, (which happened when the liquor had been evaporated to the conſiſtence of a thin ſyrup,) and then reduced to fine powder.

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* The muriate of lime made uſe of by Profeſſor Lowitz, in theſe experiments, was obtained from the reſidue after the diſtillation of cauſtic ammonia.

† In this inſtance, as I afterwards found, the proportion of ſnow, owing in part to a conſiderable portion of the muriate of lime remaining frozen at the bottom of the veſſel, and not mixing, was much too great.

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From the preceding account it is apparent, that Pr. Lowitz has diſcovered a ſingle frigorific mixture, by which quickſilver may be frozen whenever the temperature of the materials at mixing is no colder than + 32°; whereas, the *nitrous acid* with ſnow, which has hitherto been conſidered as the moſt powerful of frigorific mixtures, requires a temperature of + 7°, to produce the ſame effect.

At the ſame time, however, it ſhould be obſerved, that an experiment with this ſalt, prepared as above, can be made only *during a freezing atmoſphere*; the ſalt itſelf thus prepared, becoming, as may be inferred from the above, and as I have myſelf experienced, unfit for uſe by a warmer temperature.

Reflecting upon my former experiments, I determined to try the effect of this ſalt, reduced to ſuch a ſtrength, by evaporation, as to endure being kept, in a ſolid ſtate, throughout the year. For this purpoſe, I found it neceſſary that the liquor, prepared as before, ſhould be evaporated to the conſiſtence of a thickiſh ſyrup, before it is ſet by to cryſtallize; when the produce will be a ſemi-tranſparent, uniform, cryſtalline maſs, which affords, on pulverizing, a white pearl-coloured powder, which ſhould be preſerved for uſe in a bottle cloſed with a ground ſtopper.

Some of the powder above mentioned, (which had been previouſly ſubjected to a temperature of + 70°, without melting,) I mixed with ſnow, each at the temperature of + 32°; when the thermometer, to my perfect ſatisfaction, ſunk to — 40°; and, at another time, when the air was + 20°, I froze quickſilver perfectly ſolid, by a mixture of the ſame materials.

It appeared reaſonable to expect that the power of this ſalt for producing cold, might be aſſiſted by the combination with, or addition of, other ſalts, as has been found to be the caſe in other inſtances, to a conſiderable degree; and, conceiving from my former experiments, that the muriate of ſoda and nitrate of ammonia promiſed the greateſt ſucceſs in this way, theſe alone producing with ſnow a cold of — 25°, I was naturally led to combine theſe with the muriate of lime; but I obtained no advantage by this, or by any other means, worth noticing. *.

In the courſe of laſt winter, and the ſummer preceding, I repeated my former experiments with more accuracy; and likewiſe made ſome additional ones, on the power of muriate of lime for producing cold with ice, the reſult of which I ſhall here briefly ſtate.

In order to reduce the experiments made with the muriate of lime to a greater certainty, I carefully obtained the reſpective ſpecific gravities to which this ſalt ſhould be reduced by

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* M. Van Mons tried the effect of the cauſtic ſoda (which alone produces a cold of — 15° with ſnow) combined with the muriate of lime, and found the effect increaſed very conſiderably. By this means, he ſays, "In my new experiments on the effects of artificial cold, I have carried it to — 53°, old diviſion"; (viz. 87° of Fahrenheit.) He does not mention the temperature at which the materials were mixed; to produce this effect.---M. Van Mons effects a chemical union of theſe two ſalts thus: he ſlakes quicklime with a ſolution of muriate of ſoda; this mixture, when become cold, he filters, and afterwards evaporates, until when cold it will become ſolid. *Annales de Chimie*, tom. XXIX.

evaporation, before it be ſet by to cool, in order to become ſolid, in either inſtance: thus, when the muriate of lime is to be of that ſtrength which is to be prepared and uſed at the temperature of + 32°, the ſpecific gravity of the liquor ſhould be, at the temperature of + 80°, 1,450; and when of that ſtrength to be *kept* and uſed at the ordinary temperature of the air at any time, the ſpecific gravity of the liquor ſhould be 1,490, at 80° of heat.[*]

The liquor, when ſufficiently evaporated, ſhould be ſet by to cryſtallize; and the cryſtallized maſs, as ſoon as cold, ſhould be reduced to very fine powder, in a glaſs or ſtone mortar. The muriate of lime, in its ſolid ſtate, being a hard brittle ſubſtance, it is neceſſary commonly to immerſe the veſſel containing it in water ſufficiently warm to looſen the maſs, in order to remove it out of the veſſel, to pound it.

When the muriate of lime is intended to be preſerved for future uſe, the powder ſhould be put directly into a bottle, and cloſely ſtopped from the air; for this ſalt is extremely deliqueſcent, and hence, a dry ſtate of the atmoſphere ſhould be choſen for preparing it.

Theſe experiments I ſhall divide into two ſeries; the firſt of which conſiſts of thoſe made with the muriate of lime prepared ſo as to be uſed in *winter* only, that is, of the ſtrength of 1,450.

The ſecond ſeries conſiſts of thoſe made with the ſame ſalt prepared ſo as to be kept for uſe at any time, the ſtrength of which is 1,490.

SERIES I.

Exp. 1ſt.	Muriate of lime -	3,	Snow	2,	- - at + 32°	- - gave — 50° [†]
2d.	——— ——— -	2,	——	1,	- - - - 0	- - - - — 66
3d.	——— ——— -	3,	——	1,	- - - - 40	- - - - — 73
4th.	Diluted vitriolic acid	10,[‡]	——	8,	- - - - 68	- - - - — 91 [§]

SERIES

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[*] Muriate of lime evaporated to the ſtrength of 1,400, gives (if cooled ſlowly in a cold air, *viz.* + 20°) perfect cryſtals; this is the fitteſt ſtate of the ſalt for producing cold.

[†] This experiment being repeated, uſing ice-powder, (inſtead of ſnow,) gave—51°;——that is, ice ground to very fine powder with the inſtrument deſcribed in Phil. Tranſ. for 1795, page 288. (Philoſ. J. I. 498.)

[‡] Concentrated vitriolic acid 8 parts, water 4 parts, and rectified ſpirit of wine 1 part, mixed and cooled previouſly to the temperature of the air.

[§] This experiment was made on the 10th of March laſt, and conducted by a ſeries of three mixtures, thus. The materials for the *ſecond* mixture, conſiſting of muriate of lime and ſnow, ſeparated from each other by an intervening ſtratum of fine ſand, were cooled in a large veſſel, having a partition in the middle forming it into two compartments, to near 40° below 0, by means of a *prior* mixture of the ſame materials. The materials for the *ſecond* mixture, cooled as above mentioned, were then mixed in each compartment of this double veſſel, and let through an aperture at the bottom of each, (cloſed till then by a temporary partition,) into ſuch an apparatus as that repreſented in the drawing, which contained the materials for the *third* or laſt mixture, conſiſting of the diluted vitriolic acid and ſnow, which had already been ſeparately cooled in their reſpective veſſels, to near 40° below 0.

The

SERIES II.

Exp. 1ſt.	Muriate of lime	- - 5,	Ice-powder	- - 4,	at + 32°,	gave — 41 *
2d.	——— ———	- - 4,	———	- - 3,	- - 20	- - — 48
3d.	——— ———	- - 4,	———	- - 3,	- - 10	- - — 54
4th.	——— ———	- - 3,	———	- - 2	- — 15	- - — 68

In the firſt experiment, the materials were mixed at the temperature of the air.

In the ſecond, they were previouſly cooled, by a mixture of muriate of ammonia, nitrate of pot-aſh and water; temperature of the air 52°; the ſalts which formed the preparatory mixture being recovered for uſe again, by evaporation.

In the third, they were cooled previouſly, by a mixture of muriate of ammonia, nitrate of pot-aſh, ſulphate of ſoda, and water; temperature of the air 50°. And,

In the fourth, by a mixture of phoſphate of ſoda, nitrate of ammonia, and diluted nitrous acid; temperature of the air 49°. †

Having concluded my experiments with the muriate of lime and *ice*, I proceeded to try the effects of this ſalt, prepared ſo as to retain its ſolid ſtate during ſummer, *viz.* of the ſtrength 1,490, by ſolution in *water*; and found that a mixture of this kind, produced twenty-nine degrees of cold; for, by adding 5 parts of the muriate of lime, in fine powder, to 4 parts of water, each at the temperature of + 50°, I obtained a cold of + 21 : this effect was not improved by the addition of other ſalts.

Theſe experiments were made in ſuch an apparatus as I am going to deſcribe, which may at firſt appear new, but, upon examination, will be found to be only a different modification of the veſſels repreſented in Fig. 3 and 4, Tab. XXIII of Phil. Tranſ. for 1795; (Philoſ. J. I. Plate XX.) and appears to me to have all the advantages that can be obtained in an experiment of this nature.

The thermometer repreſented in the drawing was then placed in the tube of the upper veſſel; and, when the ſnow was cooled to the utmoſt, *viz.* to near 70 below 0, the cooled ſnow was forced through into the cup containing the acid, the veſſels were ſeparated, and the ſnow and liquor thoroughly mixed by means of the thermometer contained in its glaſs tube; the thermometer was then withdrawn from the tube, and ſtirred about in this laſt or third mixture, which, in ten ſeconds of time, indicated a cold of 91°¼ below 0; twenty ſeconds more elapſed before the thermometer began to riſe. The mouth of the cup in the veſſel B was, in this inſtance, cloſed with waxed paper, in order that I might invert the veſſel occaſionally, to renew the mixture in it; and the cup itſelf was coated within-ſide with wax, in order to defend it from the action of the acid.

* The ſame experiment made at + 32°, with ſnow, (inſtead of ground ice,) gave — 40°.

† The compoſition and application of theſe frigorific mixtures are given in Phil. Tranſ. for 1795.

(To be concluded in our next.)

V.

Directions for extracting Gum from the Lichen, or Tree Moſs, &c. By the EARL OF DUNDONALD.*

IT does not appear from ſuch trials as Lord Dundonald has hitherto made, that there is any very great difference in the produce of gum from the lichen collected from different trees or ſhrubs: all of them anſwer equally well for yielding a gum fit for calico printing. The lichen is moſt abundant on the trees which grow in a poor, ſtiff clay ſoil, particularly if ſituated at ſome conſiderable height above ſea-level. It ſhould be pulled in dry weather, otherwiſe it is apt to break in the pulling; beſides, in this caſe, requiring to be dried, before it can with ſafety be laid up in the ſtorehouſe, where, if put in dry, it may be kept for years. Should a ſufficient quantity of it not be found in this country, it may be had in almoſt unlimited abundance in Sweden, Norway, and in the northern parts of America, where it grows to the length of from a foot to eighteen inches, depreſſing the branches of the tree by its weight. There is, however, every reaſon to believe that a ſufficient quantity is to be had in this country. According to information from Dr. Brown, lecturer on botany, it takes three or four years in coming to maturity or its full ſize, ſo that a crop from the ſame tree may be had every fourth year.

The lichen does not conſiſt entirely of a gummy matter. There is the outer ſkin or cuticle, below that a green reſinous matter: the remainder of the plant conſiſts of partly gum, partly a matter ſomewhat analogous to animal ſubſtances, and a ſmall proportion of fibrous matter, which cannot be diſſolved by boiling, or the action of alkaline ſalts.

The firſt proceſs in preparing gum from the lichen is to free it of the outer ſkin of the plant and the reſinous matter. This is done by ſcalding the lichen two or three times with boiling water, allowing it to remain ſo long in the water, as by abſorbing it to ſwell; in doing this the ſkin cracks and comes off along with the greateſt part of the reſinous matter. Or it may be freed from them by gently boiling the lichen for about fifteen or twenty minutes, then waſhing it in cold water, laying it afterwards upon a ſtone or brick floor, where it ſhould lie for ten or twelve hours, or perhaps more. The reaſon for this is, that the expoſure for that time to air greatly facilitates the ſubſequent extraction of the gum.

The ſcalded lichen is then to be put into a copper boiler, with a due proportion of water, ſay three Scotch pints, or two wine gallons, to every pound of lichen, and boiled

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* Early in the preſent year this Nobleman convened the calico printers of Scotland, and communicated to them this important diſcovery, at a meeting at Glaſgow, where ſeveral reſolutions were made for the purpoſe of ſecuring its advantages to the country, with a proper reward to the inventor. The preſent paper was printed at Glaſgow, in ſix pages duodecimo, 1801. The ſtatement which follows was alſo drawn up by his Lordſhip, by whom I was favoured with both.

during four or five hours, adding about half an ounce or three quarters of an ounce of ſoda or pearl aſhes for every pound of lichen; or, inſtead of theſe ſalts, about half an Engliſh pint of volatile alkali. The boiling ſhould be continued until the liquor acquires a conſiderable degree of gummy conſiſtence. It is then to be taken out of the boiler, allowed to drain or drip through a wire or hair cloth, or ſearſe. The reſiduum to be put into a hair cloth bag or bags, and to be ſqueezed in a preſs ſimilar to that which is uſed by the melters or rinders of tallow.

The firſt boiling does not extract the whole of the gum. The lichen ſhould be boiled a ſecond and even a third time, repeating the proceſs as above deſcribed, diminiſhing at each proceſs the quantity of water and the quantity of alkali, which a little experience will ſoon point out. When three boilings are employed, the gummy extract of the laſt boiling ſhould be kept for the firſt boiling of a freſh batch of lichen. The extract proceeding from the firſt and ſecond boilings ſhould be mixed together, and evaporated to the conſiſtence neceſſary for block or preſs printing. The evaporating veſſels ſhould be of tin or thin lead placed over a range of ſtoves and moderately heated by fire or the ſteam of water. It has been neglected to ſtate, that before evaporating the gummy extract to the conſiſtence neceſſary, it ſhould be kept ten or twelve hours, ſo as to allow the ſediment or dregs to ſubſide. The clear liquor may either be drawn off by a ſyphon, or the dregs may be drawn off by a cock at the bottom of the wooden veſſel; the bottom of which ſhould be made ſloping, higher at the back than the fore part, in order that the dregs may run more completely off. The proportion of gummy matter remaining in the dregs may be got off by mixing them with a due proportion of boiling water, allowing the liquor to clear, and proceeding as above directed, employing this weak ſolution for boiling the next batch of lichen.

The reſiduum of the lichen, after the third boiling, conſiſts of a matter ſomewhat analogous to that of animal matter, together with a proportion of the fibrous matter of the plant. From the animal matter a kind of ſoap may be made. The proceſs is as follows:

Let a ſmall proportion of pounded roſin be diſſolved by boiling in a ſolution of alkaline ſalts. When the roſin is diſſolved, put in a certain quantity of the reſiduum of the lichen, and continue the boiling until this laſt article is pretty well diſſolved. Then add about a pound or a pound and a half of white ſoap ſhaved down or cut into thin ſlices to every ten pound weight of the reſiduum of the lichen. Continue the boiling until the white ſoap is fully diſſolved. Then paſs the ſoapy ſolution through a hair ſieve or hair bag. Let the ſoap pan then be cleaned out and the ſoapy ſolution returned to the pan, to be boiled to the conſiſtence neceſſary. A kind of ſoap very ſimilar to this may be made from ſea weed, as well as from ſeveral other articles. Of this and of other matters, highly important to calico printers, a more full and detailed account will be given in a future publication. When gum from the lichen is to be employed for making ink, manufacturing and ſtaining paper,

and

and for ſtiffening ſilks, crapes and gauzes, it ſhould be extracted from the lichen without employing any alkaline ſalts, continuing the boiling or digeſtion longer, and with a moderate degree of heat, in which caſe the gummy extract will be nearly colourleſs.

When volatile alkali is uſed, the boiler ſhould be of iron, as volatile alkali acts on copper.

Statement of the Benefits and Advantages to enſue to Individuals and to the United Kingdom of Great Britain and Ireland, from Lord Dundonald's Diſcovery of extracting Gum from the Lichen, or Tree Moſs, as a Subſtitute for Gum Senegal, and other Foreign Gums.

Gum Senegal, or other foreign gums, have hitherto been indiſpenſably requiſite for fine goods, and thoſe of light ſhades of colour.

No proper ſubſtitute had been found to anſwer for this part of calico printing, until Lord Dundonald diſcovered the preparing gum from the lichen.

Gum Senegal, and foreign gums, are at times ſcarce, and difficult to be procured.

The ſettlement of Senagambia belongs to the French. They have a monopoly of gum Senegal. The price ſince the war has riſen from 150l. to 400l. per ton.

Gum from the lichen may, all charges included, be prepared at one-fourteenth part of the preſent price of gum Senegal, and at one-ſixth of the peace-time price.

The United Kingdoms may now be ſaid to be independent of foreign ſtates for the gum requiſite for their manufactures, ſuch as calico printing, preparation of ink, ſtaining and manufacture of paper, dreſſing and ſtiffening ſilks, &c. &c. And, in conſequence of Lord Dundonald's diſcovery, large ſums of money formerly ſent abroad for the purchaſe of gum, will, in future, be ſaved.

Starch and wheaten flour have hitherto been uſed as cheap ſubſtitutes for gum, in printing coarſe goods, or to fix on other printed goods, the mordants, and ſome of the dyeing materials, for deep ſhades of colour. But there is now reaſon to expect, as gum from the lichen will coſt leſs than thoſe articles, particularly *ſtarch*, it will be uſed in their ſtead, and ſo prove the means of ſaving, or œconomizing a conſiderable proportion of wheat and wheaten flour, at preſent conſumed in calico printing.

The collecting the lichen from the foreſt and fruit trees and hedges, will give employment to a number of poor people, principally women and children.

A cheap ſupply of gum from the lichen, may perhaps, in ſeveral manufactures, ſuperſede the uſe of the higher priced article of glue or ſize, which often has a very diſagreeable ſmell.

Gum from the lichen, may perhaps, be uſed as a dreſſing in weaving, particularly fine goods, either by itſelf, or when made into a kind of ſoap, according to directions given in a ſeparate publication.

Laſtly, It is probable that all the benefits to enſue from the diſcovery of procuring gum from the lichen may not yet have occurred, it being well known, that there is no art or diſcovery that is not the parent or ſiſter of ſome other.

VI.

On the Nature of Galvaniſm, Electricity, Heat, the Compoſition of Water, and the Theory of Chemiſtry. By a Correſpondent.

To MR. NICHOLSON.

SIR,

THE more I conſider the objections which I ſtarted reſpecting the explanation of the phenomena on the application of the influence from the pile of Volta to water by means of wires, the more am I convinced that the ſyſtem of chemiſtry, according to M. Lavoiſier, is not applicable to the appearances which are continually preſenting themſelves. However that ſyſtem may be ſanctioned and ſupported by the conſent of eminent philoſophers, yet I truſt that in every diſcuſſion of ſubjects of natural philoſophy, the laudable deſire of acquiring truth will outweigh any preſumption which may appear in an oppoſition of ſuch favoured doctrines.

The communication from Dr. Babington, which appeared in your late Journal (IV. 511), was deciſive. It clearly proves, what I had ventured to ſuggeſt, that water is not compoſed of the baſes of oxigen and hidrogen air. If the two gaſes ſeparated from water by the pile were collected, and the hidrogen air burnt in the oxigen air, would not in that inſtance the two kinds of electricity be converted into ordinary culinary fire? It appears that on burning thoſe airs together, the water, when collected, has loſt none of its weight; if ſo, does not the fire produced originate from the poſitive and negative electricities? If water is not decompoſed by the pile, whence comes the hidrogen? Hidrogen not being a component part of water, its name ſurely ought to be changed. If the original weight and quantity of water be again collected after the combuſtion of thoſe airs, which have been ſuppoſed to be its conſtituent principles, by what proceſs of reaſoning can we aſſent to the baſes of thoſe airs being peculiar elements or radicals. If the matter of heat were a ſimple body, and if the gaſeous ſtate of theſe radicals depended on the greater capacity they have for caloric, whence comes the difference obſerved between them when they are becoming fixed; and why does one kind of electricity invariably produce the ſame air? The following is the manner in which the phenomena of the ſolid, liquid, and gaſeous ſtates of bodies are deſcribed according to the theory of M. Lavoiſier.

All bodies, from their combination with caloric, may exiſt in three different ſtates, which reſult from:

1. The

1. The power of attraction by which bodies cohere.
2. The quantity of caloric with which the ſubſtance is combined.
3. The greater or leſs preſſure of the atmoſphere.

The means by which a ſolid body may be converted into a liquid are therefore

1. A combination with a new and ſufficient quantity of caloric.
2. An union with another body already liquid.

A liquid is changed into the ſtate of gas

1. By being combined with a quantity of caloric ſufficient to equipoiſe the preſſure of the atmoſphere.
2. If the preſſure of the atmoſphere be either ſufficiently diminiſhed or totally removed.

According to this ſyſtem, therefore, there exiſts no difference between gaſeous bodies but from the difference of their reſpective baſes; the caloric is common to all. Why, therefore, does oxigen air, when united during combuſtion with charcoal, give out fire with flame, whilſt other gaſes, even when inſtantly changed into a ſolid, give out only heat? Or why is one portion of water from the pile wholly converted into oxigen gas? The minus ſide certainly cannot form a gas by abſtracting fire, even though it ſhould be in the ſtate of electricity.

It appears, therefore, that the theory of M. Lavoiſier is defective, in as far as it proves neither the decompoſition of water, nor the nature of permanently elaſtic fluids. I could bring forward many experiments in a new form to prove the fallacy of the ſyſtem of M. Lavoiſier, but we have already ſufficient facts before us to demonſtrate upon what a ſlight foundation it reſts.

The explanation of moſt of the phenomena of chemiſtry is involved in this diſcuſſion. The nature of the atmoſphere and of vapours, combuſtion and all the appearances of light, heat and electricity, chemical ſolution, decompoſition, &c. in ſhort, the whole ſyſtem of M. Lavoiſier totters, ſince we know that water is not compoſed of the baſes of oxigen and hidrogen air.

As I mean this only as a notice, I ſhall not inſiſt further at preſent on the ſubject. As I mean to purſue it ſyſtematically and by experiment, I hope I ſhall not intrude too much upon you by occaſionally ſubmitting to you the reſults of my inveſtigations.

I have the honour to be

Your obedient humble ſervant,

AN EXPERIMENTALIST.

March 7, 1801.

P. S. I find that heat is eaſily decompoſed, and that the Galvanic influence may be detached from a very great variety of ſubſtances. *

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* I am ſorry to be under the neceſſity of apologizing to this Correſpondent for the late appearance of his letter, which was occaſioned by its having been placed by miſtake among ſome other papers of a different nature.- -N.

VII.

Notice reſpecting the new Method of Bleaching, and its Application to the Waſhing of Linen Garments. By CITIZEN CHAPTAL.*

TWENTY months ago I communicated to the Inſtitute a proceſs, no leſs ſimple than economical, by means of which cotton goods could be bleached in a very ſhort time.

This proceſs was ſpeedily rendered public by the journals, and the Engliſh availed themſelves of it to ſuch advantage, that, a ſhort time afterwards, it was adopted and brought to perfection in the capital manufactory of linen which exiſts in Ireland.†

The ſucceſs of this proceſs, in the hands of a rival nation, has awakened the exertions, and excited the intereſt, of ſome French manufacturers. Citizen Bawens, proprietor of a manufactory of cotton-yarn and piece-goods, at La Barriere des bons Hommes, applied to me for information reſpecting this proceſs. I gave him the dimenſions of the apparatus, and the conſtruction was intruſted to Citizen Bourlier, a diſtinguiſhed chemiſt, and proprietor of the ſal ammoniac work at Gros-Caillou. The firſt experiment, which was made in the works of Bawens, was performed upon 2000 metres of cotton in the piece. The reſult was ſo advantageous in every reſpect, that Citizens Bawens and Bourlier aſſociated together to form ſimilar eſtabliſhments in various parts of the Republic. Citizen Bourlier is at this inſtant in Belgia, where he has eſtabliſhed works of this kind to ſuch an extent, as to be able to bleach a large proportion of the goods which are manufactured in that country.

I muſt obſerve that the proceſs which I have deſcribed has been ſingularly improved by Citizen Bourlier. He has contrived eaſy methods of turning the goods in the apparatus, and preſenting all their parts to the vapour. He has found that linen requires in fact only a weak ley; but that it is neceſſary to alternate the action of the ley with that of the atmoſpheric air, in order to bleach completely. He has ſucceeded in giving a degree of whiteneſs in two or three days to the coarſeſt goods which the uſual proceſs of the bleachery could not communicate but in a much longer time, and with more expence.

I have the honour to preſent to the claſs ſamples of dimities and cloths bleached by Citizen Bourlier. It will be ſeen that the colour is equal to any thing which has hitherto appeared in the market.

I ſhall not enlarge more fully on the advantages which commerce muſt derive from this method, but ſhall confine myſelf to obſerve that the firſt experiments, which are always the moſt expenſive, did not carry the price of bleaching to more than half the ordinary price.

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* Annales de Chimie, XXXVIII. 291.

† The details of this method may be ſeen in a work of Citizen D'Orelly, entitled, "Eſſai ſur le Blanchiment;" ſold by Deterville, Rue du Battoir, No. 6, at Paris.

I have no doubt but that linen garments may be bleached to advantage bythe ſame proceſs; but as it was neceſſary to bring theſe notions to the teſt of experiment, I invited Citizen Bawens to allow me to make the experiment on a large ſcale with his apparatus. Accordingly, on the 27th Pluvoiſe, in the year IX. I had 200 pair of ſheets from the hoſpital of the *Hotel Dieu* at Paris, choſen among thoſe that were moſt ſoiled, and taken to the manufactory of Citizen Bawens.

Three experiments were made upon theſe ſheets.

Experiment 1. One hundred and thirty ſheets were impregnated with a cauſtic alkaline ley, containing one hundredth part of ſoda. They were kept for ſix hours in the engine of ſteam; after which they were impregnated again, in order to be placed again ſix hours in the ſame machine.

The ſame proceſs was repeated a third time; after which they were carefully rinſed, and no ſpot of wine, greaſe, blood, or animal ejection, was to be ſeen. One quarter of a pound of ſoap was uſed in rinſing theſe cloths.

All the aſſiſtants were convinced that the ordinary proceſſes would not have given either ſo perfect a white, nor ſo agreeable a lixivial ſmell.

The texture was in no reſpect altered.

Experiment 2. The alkaline ley contained only ſix parts of ſoda, but five pounds of ſoap were alſo added. The cloths were treated in the ſame manner, and the reſults appeared more advantageous. They were more eaſily waſhed out.

Experiment 3. To the bath of the ſecond experiment a ſufficient quantity of new ley was added. One hundred and forty ſheets were treated like the preceding, and the reſult was the ſame.

It muſt be obſerved that the water of the Seine, in which theſe ſheets were rinſed, was at that time very yellow.

This experiment appeared to me to offer ſeveral reſults worthy the attention of the Inſtitute.

In the firſt place the proceſs is economical. Two hundred pair of ſheets, which were bleached by three ſucceſſive operations, demanded an expenditure, according to the account furniſhed by Citizen Bawens, which is in the proportion of 7 to 10, compared with that commonly made in the hoſpitals. This expence may be reduced to leſs than one-third, if a ſuitable place and apparatus were appropriated to this uſe.

2. Two days at moſt are required to complete the operation. This œconomy of time is incalculably beneficial.

3. The linen is neither changed nor torn, as it paſſes through the hands only once, and it is of no uſe to beat it.

4. The extreme heat to which the linen is expoſed in the apparatus, cauſes its texture to be penetrated by the alkaline fluid to ſuch a degree, that the ſubſtances with which it is impregnated cannot be maſked from its action; ſo that the putrid exhalations, and other ſubſtances attached to its texture, are neceſſarily deſtroyed or changed in their nature.

This

This effect muſt be more particularly ſeen with regard to its value, by phyſicians, who are aware with what facility the ſeeds of various diſorders are perpetuated in hoſpitals, and how inſufficient the greater number of proceſſes uſed in waſhing linen have proved to deſtroy them.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Notice relative to the Gadolinite. By CIT. HAUY.

THIS ſubſtance was diſcovered at Ytterby, in Sweden. In the year 1794, M. Gadolin announced the exiſtence of a new earth; and his aſſertion has ſince been confirmed by M. Ekeberg's analyſis of that ſubſtance, to which he has given the name of Gadolinite, in honour of the diſcoverer, and he has denominated the new earth Yttria, a name derived from the country where the mineral that contains it was diſcovered.

From the deſcription publiſhed in the Journal de Phyſique for Fructidor, 8th year, it appears that the Gadolinite has a tolerably perfect black colour; it breaks in an imperfect conchoid form; it is brilliant, and its poliſh is vitreous; its ſpecific gravity is alſo conſiderable.

To theſe external characters may be added others of a phyſical and chemical nature, already in part known, by which the Gadolinite is clearly diſtinguiſhed from ſome other minerals to which it reſembles in its aſpect, particularly the vitreous lava denominated lapis obſidianus. Its ſpecific gravity, which Cit. Hauy has found to be 4,0497, is greater than that of the above-mentioned lava, nearly in the proportion of 5 to 3; but it is leſs than that of black ſulphurated uranium, called pech-blende, in the proportion of 2 to 3. The Gadolinite likewiſe, when reduced to powder and put into nitric acid, diluted with water, is diſcoloured on heating the acid, and converted into a kind of thick jelly, of a yellowiſh grey colour. According to the obſervations of Cit. Lelievre, the Gadolinite, when expoſed to the flame of the blow-pipe, decrepitates, and emits particles apparently inflamed: but if care has been taken to ignite it by the flame of a candle, it does not decrepitate: it acquires a red tinge, intermixed with white; cracks, and does not melt, unleſs the fragment be very ſmall. The Gadolinite acts very ſenſibly upon the magnetic needle, but Cit. Hauy has not diſcovered that it has poles.

M. Ekeberg obtained from the Gadolinite 47.5 parts of Yttria, 25 of ſilex, 18 of iron, and 4,5 of alumine: the loſs was 5. Cit. Vauquelin, on repeating this analyſis, found 35 parts of Yttria, 25,5 of ſilex, 25 of iron, 2 of oxide of mangeneſe, 2 of lime, with a loſs of 10,5, the cauſe of which he inveſtigated, and attributes it principally to the water contained in the Gadolinite, and to a ſmall quantity of carbonic acid. He obſerved that the new earth has ſome analogy to glucine. Like that ſubſtance it forms ſaccharine ſolutions with

with acids, but their taſte is rather more acrid, and more nearly reſembles that of ſolutions of lead. The Yttria likewiſe differs from glucine, becauſe it is not ſoluble (like the latter) in cauſtic alkalies; and becauſe the ſalt which it forms with the ſulphuric acid, inſtead of being ſoluble, as it is when glucine conſtitutes the baſe, is on the contrary ſcarcely at all ſoluble; and, laſtly, becauſe it is precipitated from its ſolutions in the acids by the pruſſiate of pot-aſh, which does not take place with glucine.

The pieces of Gadolinite treated by Citizens Vauquelin and Hauy, for the purpoſe of aſcertaining the nature and characters of that ſubſtance, were preſented to them by Meſſrs. Abildgaard, Manthey, and Neergaard.

Soc. Philom. No. 44, *An.* 9.

Note relative to the new Earth denominated Yttria. By M. KLAPROTH.

THIS earth, which contains Gadolinite, not only poſſeſſes the common property of glucine, of forming ſalts of a ſweet taſte, but it alſo readily diſſolves in a ſolution of carbonate of ammonia. It nevertheleſs has peculiar characters, which, diſtinguiſhing it from the earths, renders it in ſome degree ſimilar to the metallic oxides. With the ſulphuric and acetic acids, it forms cryſtallized ſalts, of a pale roſe colour: it is precipitated from its combinations by tannin and by Pruſſian alkali.

The proportion of Yttria in the Gadolinite, ſtated by Ekeberg, is not very exact. The cauſe of this may probably be attributed to the method he employed to ſeparate the Yttria from ferruginous ſubſtances, which induced him erroneouſly to imagine that the ſulphate of Yttria is not decompoſed by fire. This decompoſition, however, does take place, but not ſo readily as that of the ſulphate of iron. M. Klaproth has aſcertained by his analyſes that Gadolinite is compoſed of 21,15. parts of ſilex, 18. of oxide of iron, 59,75. of Yttria, and 0.50. of alumine. This ſmall quantity of aluminous earth is probably merely accidental, and perhaps proceeds from ſome particles of feld-ſpath accompanying the Gadolinite.

Note on fluated Alumine and Phoſphate of Copper.

M. KLAPROTH has completed the analyſes of the fluated alumine, named *Chryolite of Greenland*. It conſiſted of 23½ parts of alumine, 36 of ſoda, and 40½ of fluoric acid and water of cryſtallization. He conſiders the diſcovery of ſoda in ſtony foſſils as particularly remarkable. It is at leaſt the firſt inſtance of ſo great a quantity of this alkali being found in a ſtone, or rather in an inſoluble ſalt.

Cit. Vauquelin has repeated the analyſes of fluated alumine. The reſult was, alumine 21, ſoda 32, fluoric acid and water of cryſtallization 47. This chemiſt obſerves, that in the

the alumine were diſcovered five parts of a ſubſtance having an earthy appearance, but which does not form alum, and is apparently inſoluble in the acids. He conceives this ſubſtance may be ſilex.

M. Klaproth has alſo diſcovered a native phoſphate of copper, and is at preſent employed in analyſing this new combination of the phoſphoric acid.

⁂ *The two laſt Articles were communicated by Letter from Klaproth to Vauquelin, Bulletin des Sciences, No.* 45, *Frimaire,* IX.

On a new Manner of preparing the Acetic Acid. By CIT. BADOLLIER, *Apothecary at Chartres.*

THE proceſs of Cit. Badollier conſiſts in diſtilling a mixture of equal parts of ſulphate of copper and acetite of lead, on the ſand bath, in a glaſs retort adapted to a receiver.

To examine the produce, Cit. Badollier made uſe of the pneumato-chemical apparatus. During the operation, he obtained only a ſmall quantity of elaſtic fluid, which he found to have proceeded from the dilated air contained in the veſſels. This air, tried by the uſual teſts, afforded no indication of carbonic acid. As he preſumed that his acetic acid might contain a portion of ſulphuric acid, he tried it by a ſolution of the muriate of barytes, which did not cauſe any precipitate.

It muſt be obſerved, that the diſtillation is ſpeedily made, and requires a very moderate heat; and the acid thus obtained poſſeſſed no empyreumatic ſmell; that the quantity, as well as quality, were equal to thoſe obtained by the diſtillation of the acetite of copper; and that there was not, as in the old proceſs, a portion of decompoſed acid, communicating a diſagreeable ſmell to the whole product. On account of the time and fuel ſaved, the price of the acid prepared by this new proceſs, is to that obtained by the diſtillation of the acetite of copper, as 1 to 4.

Annales de Chimie, XXXVII. 111.

On the Phoſphoreſcent Properties of the Medullary Subſtance of the Brain and Nerves, extracted from a Memoir of CIT. CABARRIS, *read before the National Inſtitute.*

IT is well known that phoſphorus is derived from animal matter. It is alſo indeed found in the mineral kingdom; but it may be queſtioned whether its origin, like that of calcareous earths, may not always be referred to animal recrements; that at leaſt which is the direct product of theſe recrements, may be conſidered as the immediate effect of

ſenſitive

ſenſitive life, as a reſult of the changes which the animal ſolids and fluids are ſuſceptible of undergoing, or as one of the ſimple ſubſtances which they poſſeſs the peculiar property of aſſimilating. In the bodies of animals which are undergoing decompoſition, phoſphorus appears to enter into a ſlow combuſtion. Without producing real flame, without at leaſt the power of igniting combuſtible bodies in its immediate vicinity, it becomes luminous, and throws out flaſhes of light amid the ſurrounding darkneſs, ſufficiently vivid to afford ſome real foundation for the exiſtence of thoſe phantoms, which people are at once deſirous, and afraid of ſeeing, in the neighbourhood of cemeteries. The brain and its appendices, or rather the nervous ſyſtem in general, appears to be the peculiar reſervoir of phoſphorus; for it is the incipient decompoſition of the cerebral pulp which gives riſe to thoſe phoſphoreſcent lights which, during the darkneſs of the night, are frequently obſerved in anatomical theatres; and it is chiefly around the brain expoſed by being deprived of the natural coverings, or the fragments of nervous matter lying on the tables, that they are perceived. A variety of obſervations have led me to conclude that the quantity of phoſphoreſcent matter developed after death, bears ſome proportion to the activity of the nervous ſyſtem during life. I think I have noticed that the brains of perſons who have died of diſeaſes marked by a ſtrong augmentation of this activity, gave a greater, as well as a more vivid light. Thoſe of maniacs are peculiarly luminous; thoſe of dropſical and leucophlegmatic ſubjects much leſs ſo.

Combuſtion of Metals by Galvaniſm.

PROFESSOR Tromſdorf preſented to the Electoral Society of uſeful Literature at Erfurth, April 8th, a Volta's pile, and exhibited in the preſence of the Prince Biſhop de Conſtanz, ſome experiments, part before known, and part new.

The pile of Profeſſor Tromſdorf was built up of 180 pair of plates, of zinc, copper, and wetted paper.

The ſhock which this pile was capable of giving was very ſtrong, and the ſpark conſiderable.

Profeſſor Tromſdorf, after having ſhewn the production of hidrogen, and oxigen gas, the oxidation and de-oxidation of metals, &c. exhibited the combuſtion of metals, both perfect and imperfect; a fact hitherto unknown to us.

A leaf of fine gold, after having been affixed to the zinc ſide of the pile, took fire, and burnt with a crackling noiſe, when the wire of the copper ſide was brought in contact with it.

A leaf of fine ſilver took fire in a ſimilar manner, and burnt with a green flame; braſs burnt reddiſh brown; laminated copper, emerald-green; zinc, whitiſh-blue; tin, reddiſh-white, &c.

In order to inſure ſucceſs in the exhibition of theſe experiments, Profeſſor Tromſdorf remarks, that the metals ſhould be rolled out very thin, though he does not doubt that a larger pile would be capable of inflaming larger maſſes.

To prove that the accenſion of the perfect metals is a true oxidation, the experiments may be performed in a hollow glaſs ſphere; the oxide will then adhere to the ſides of the glaſs, and may be collected.

Profeſſor Tromſdorf is now engaged in the conſtruction of a larger pile, conſiſting of 5 or 600 pairs of plates.

Algemeine Literatur Zeitung LXXXIV. *July* 16, 1801.

THE combuſtion of metals by galvaniſm has alſo been effected in a ſomewhat different way by Fourcroy in France. An account of this experiment was given in the Moniteur about two months ago; but as the ſet to which I have acceſs appears to be defective for want, as it ſhould appear, of this very Paper, I muſt, for the preſent, give the particulars from recollection. A pile of plates of zinc and ſilver ſix inches, or more in diameter, and conſiſting of no more than ſix plates of each metal, was conſtructed in the uſual manner. It gave little or no perceptible ſhock, but the galvanic ſtream, when paſſed through fine wire, ignited and diſperſed it. The moſt remarkable fact in this exhibition is, that the ſame ſurface formed into a pile with more reiterations; as for example, when the plates were cut into four, and conſtituted twenty-four repetitions, gave ſtrong ſhocks, but did not exhibit the combuſtion of wire. From this curious incident we derive a law of galvaniſm before unknown, which is not only in itſelf very ſtriking, but will tend to improve our knowledge of the electric ſhock, if electricity be the ſame as galvaniſm, or to mark their differences more effectually, if the contrary poſition ſhould hereafter be rendered probable.

Galvanic luminous Signs of the two Electric Powers.

MR. Cruickſhank of Woolwich, has favoured me with an account in converſation, of the flaſh of light attended with noiſe, which he obſerved many months ago, and mentioned to me formerly in a letter, from which an extract is given at page 80 of the preſent volume. The apparatus made uſe of to produce the galvanic current is there deſcribed, namely, two troughs, conſiſting each of 120 pair of plates of zinc and ſilver, ſoldered together all round the edge. If the communication be made from one end of this ſyſtem to the other, by means of a wire having one of its extremities plunged in the fluid, ſo as to communicate with the zinc face, and the other extremity of the wire be applied to the fluid at the end to which

which the ſurfaces of the ſilver plates are oppoſed, a flaſh or bruſh of light, viſible in clear ſun ſhine, is ſeen upon the ſurface of the water at the moment of contact or immerſion; attended with a noiſe exactly ſimilar to that of plunging a red hot wire in water, and accompanied with an ebullition of the ſame kind, as is produced by ſuch an immerſion. The length of the bruſh is about half an inch, and the noiſe may be heard acroſs a room. On the contrary, if the permanent connection, or immerſed end of the wire be at the ſilver end, or that extremity which would ſeparate hidrogen from water, and the application or contact be made to the fluid at the zinc or oxigen end, nothing is ſeen but a ſmall globular ſpeck of light, ſcarcely viſible unleſs the machine be very powerful at the ſame time that there is very little effervеſcence, and no noiſe at all.

From my experiments at page 184 of the 4th Volume of this Journal, which have ſince been repeated and varied by numberleſs operators, it is determined that the zinc or oxigen ſide imparts the plus ſtate of electricity, and the other ſide the minus. The point of the wire was therefore plus when it gave out the luminous bruſh, and minus when the fluid was ſilently conducted. This property of the two electricities was long ago ſhewn by Franklin, and from ſome new facts reſpecting the operation of low points, was applied by myſelf to the conſtruction of an inſtrument called the Diſtinguiſher of Electricity, of which a drawing was publiſhed in my Introduction to Natural Philoſophy about fourteen years ago. The courſe of electricity from a battery, that is to ſay, of a large quantity at a low or moderate intenſity, along the ſurface of fluids or moiſt ſubſtances, was the object of a ſeries of experiments many years ago by Dr. Prieſtley, which he has publiſhed in his Hiſtory of Electricity. Dr. Wollaſton has communicated ſome very effective experiments to the Royal Society, by which the identity of the electrical and galvanic powers appears to be placed beyond controverſy, an account of which will ſoon be laid before the reader.

Mr. Close's Improvements in Wind Instruments.

Fig. 1.

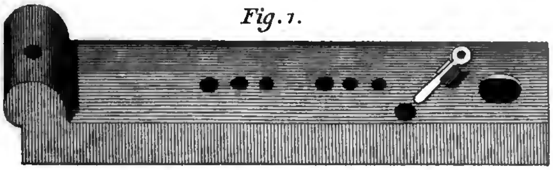


Fig. 3.

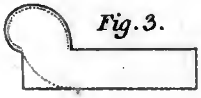


Fig. 2.

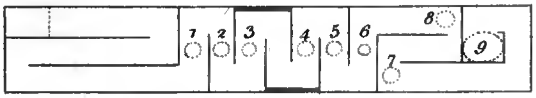


Fig. 4.

Fig. 5.

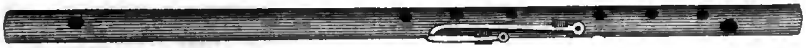


Fig. 6.

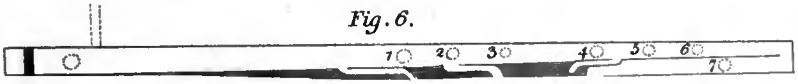


Fig. 7.

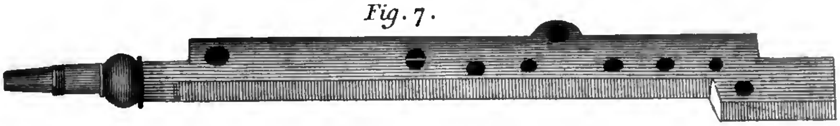


Fig. 8.

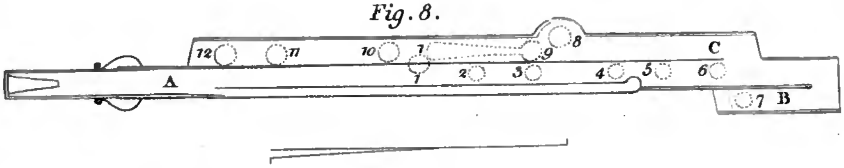


Fig. 9.

Fig. 10.

Mutlow Sc. Russell Court.

Mr. Walker's freezing-Proceſs.

Fig. 1.

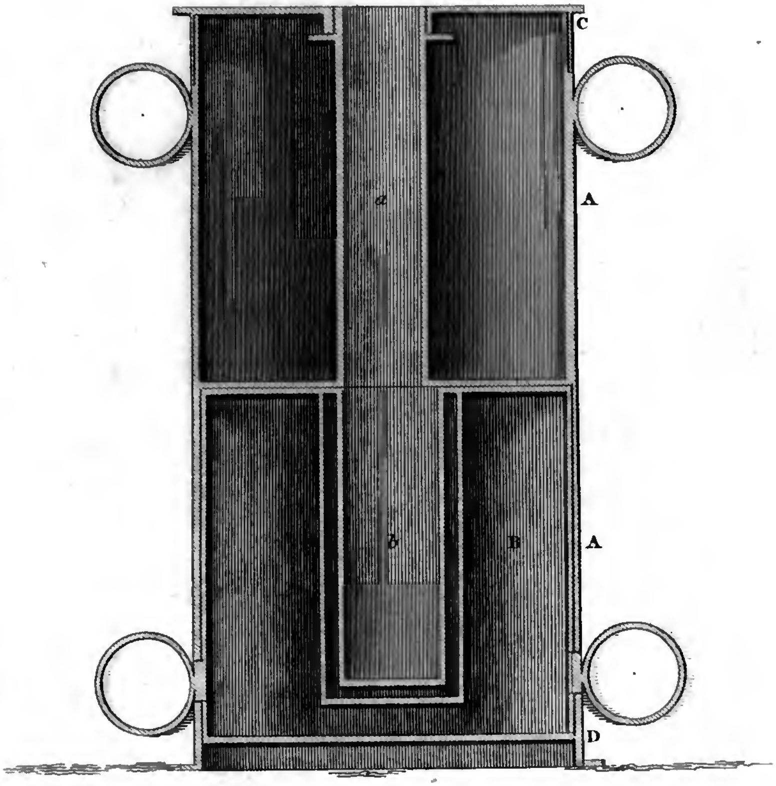
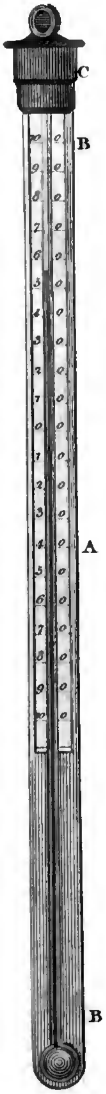


Fig. 2.



Scale of Inches.

Mutlow Sc. Ruſsell Court.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

OCTOBER, 1801.

ARTICLE I.

New Theory of the Conſtitution of mixed Aeriform Fluids, and particularly of the Atmoſphere.

By Mr. JOHN DALTON.

To Mr. NICHOLSON.

SIR,

AT the period when the mechanical properties of the air were firſt aſcertained, by the invention of the air-pump, &c. the atmoſphere was underſtood to be a homogeneous fluid, excluſively poſſeſſing the property of permanent elaſticity. Experiment determined that the elaſtic force of air was accurately as its denſity, in a given temperature. This fact afforded a *datum* from which the law of repulſion of the corpuſcles of air could be calculated. Accordingly Newton demonſtrated, (*Principia Prop.* 23, *B.* 2,) that *an homogeneous elaſtic fluid poſſeſſing a force in proportion to its denſity, is conſtituted of particles repelling each other by a force decreaſing directly as the diſtance of the centres of the particles from each other.* This reſult is, I believe, univerſally allowed as unqueſtionable. Modern chemiſtry however, demonſtrates that the atmoſphere is *not* an homogeneous fluid; it is conſtituted of *ſeveral* elaſtic fluids, poſſeſſing diſtinct properties; but as they all agree in elaſticity, that is, in *their particles repelling each other* by a force decreaſing as their diſtance, it ſeems to have been tacitly admitted by modern philoſophers, that the ſame law of repulſion takes place between two particles of *different* fluids, as does between two particles of *the ſame* fluid. This admiſſion, I conceive, has been a moſt unfortunate one, and the cauſe of much embarraſſment and uncertainty to the chemiſt and philoſopher, which otherwiſe needed not to have taken place.

There may be *four* ſuppoſitions in reſpect to the affections of the particles of one elaſtic fluid towards thoſe of another:

1. Particles of one elaſtic fluid may repel thoſe of another with *the ſame* force as they repel thoſe of their own kind.

2. The particles of one may repel thoſe of another with forces *greater* or *leſs* than that exerted upon thoſe of their own kind.

3. The particles of one may poſſeſs no repulſive (or attractive) power, or be perfectly inelaſtic with regard to the particles of another; and conſequently the mutual action of ſuch fluids, or the action of the particles of one fluid on thoſe of the other, will be ſubject to the laws of inelaſtic bodies.

4. The particles of one may have a chemical affinity, or attraction, for thoſe of another.

Let us now conſider the conſequences reſulting from each of thoſe ſuppoſitions: according to the firſt, if there were two gaſes A and B, the particles of which were of equal ſpecific gravity; then if m meaſures of A were mixed with n meaſures of B, in the pneumatic apparatus, and under atmoſpheric preſſure equal to 30 inches of mercury, the two gaſes would occupy $m + n$ meaſures of ſpace, and would remain in ſuch circumſtances as they were at any time placed; that is, either intermingled or ſeparate: but if A was of leſs ſpecific gravity than B, A would occupy the upper part of the veſſel, and the volume of the compound continue the ſame as before. The preſſure on each particle of the mixture would be the ſame in both caſes, and equal to 30 inches of mercury.

According to the ſecond hypotheſis, if the particles of A had a greater or leſs repulſion to thoſe of B than to their own, ſtill m meaſures of A with n meaſures of B, would occupy $m + n$ meaſures of ſpace, and the preſſure on each particle be the ſame, and equal to 30 inches of mercury. But the fluids could not be mixed, unleſs the repulſive powers of their particles were as the cube roots of the ſpecific gravities of thoſe particles; or, which amounts to the ſame thing, unleſs the two fluids were of equal ſpecific gravities.

On the third hypotheſis, if m meaſures of A be mixed with n meaſures of B, the two will in this caſe likewiſe occupy $m + n$ meaſures of ſpace; but the two fluids, whatever their ſpecific gravities may be, will immediately or in a ſhort time, be intimately diffuſed through each other, in ſuch manner that the denſity of each, conſidered abſtractedly, will be uniform throughout; namely (calling the denſity of the compound, unity) that of A will be $\frac{m}{m+n}$, and that of $B = \frac{n}{m+n}$; becauſe, meeting with no repulſion but what ariſes from particles of *the ſame kind*, they will diffuſe themſelves through any given ſpace, occupied by a very rare medium, in the ſame manner as they would do in a vacuum, each particle being impelled as far as poſſible from its neighbouring particle; only the diffuſion of each may be a little retarded by the other. The preſſure upon any one particle in this caſe will not be as the denſity of the compound, as before, but as the denſity of the particles of its own kind: that is, the preſſure upon a particle of *A* will be equal $\frac{m}{m+n}\cdot 30$ inches of mercury; and that upon a particle of $B = \frac{n}{m+n}\cdot 30$ inches; thoſe preſſures ariſing *ſolely* from the particles of their own kind.

On the fourth hypotheſis, where two elaſtic fluids meet, whoſe particles, inſtead of repelling, attract one another; that is, the particles of *A* attract thoſe of *B*: then, two or more particles coaleſcing, a compound will be formed poſſeſſing peculiar properties. Here, if the compound happen to be an elaſtic fluid in that temperature, *m* meaſures of *A* and *n* meaſures of *B*, will occupy a ſpace leſs than $m + n$ meaſures, and the new fluid be of greater ſpecific gravity than the other two: this at leaſt would moſt probably be the caſe.

To underſtand theſe deductions from the preceding hypotheſes may perhaps require the aid of mathematical reaſoning; but I think they will be moſtly pretty obvious to ſuch as are in ſome degree converſant in theſe ſubjects: I ſhall therefore proceed to the application; that is, to conſider which of thoſe hypotheſes will beſt explain eſtabliſhed facts. When two gaſes of very different ſpecific gravities are confined in the ſame veſſel, for inſtance the oxigenous and hidrogenous, they are ever found to be jointly diffuſed through the whole capacity of the veſſel, diſregarding the laws of hydroſtatics. Again, with reſpect to elaſtic vapours from liquids; aqueous vapour under 212°, cannot poſſibly ſupport a preſſure equal to that of the atmoſphere; vapour from alcohol requires 175°, and that from æther 100 or 105°, in order to be formed at all under a preſſure of 30 inches of mercury; yet we ſee all theſe liquids evaporate daily in the open air, and at low temperatures. It is impoſſible to reconcile the firſt of theſe facts to either the firſt or ſecond hypotheſis; as for the ſecond fact, we attempt to reconcile it to them by another hypotheſis; namely, that one or all of the gaſes conſtituting the atmoſphere, act as a general menſtruum, and diſſolve liquids indiſcriminately in greater or leſs degree. But this explanation is not without ſome difficulties; for the ſpecific gravity of the gaſes is *diminiſhed* by this proceſs, inſtead of being *increaſed*, and heat is abſorbed by the liquid diſſolved, juſt the ſame as if it were changed into a diſtinct elaſtic fluid.

If we adopt the *third* hypotheſis, the explanation of both theſe facts, and indeed of every other, becomes at once eaſy and perfectly intelligible. The intimate mixture and diffuſion of gaſes of whatever ſpecific gravity, is a neceſſary conſequence: the vapour of water, and of every other fluid which does not chemically unite to the azotic and oxigenous gaſes of the atmoſphere, nor to one another, may exiſt as ſo many diſtinct fluids in every temperature of the atmoſphere, and without any regard to its preſſure on the ſurface of the earth, being totally uninfluenced by any other preſſure than that ariſing from the weight of their own particles: in ſhort each vapour, in regard to preſſure, is in the ſame circumſtances as if it were the only elaſtic fluid conſtituting the atmoſphere.

Many other facts which are difficultly explicable upon any other hypotheſis, are readily underſtood upon this. When a portion of atmoſpheric air is ſubjected to a ſolution of

ſulphuret

ſulphuret of pot-aſh, the oxigenous gas in a ſhort time is almoſt wholly abſorbed: now it is not eaſy to conceive how any chemical affinity ſhould operate upon a body at the diſtance perhaps of 12 inches or more. The fact is, the ſulphur abſorbs the oxigen in contact with it; and as the remainder of the gas expands to fill up the vacuity, it is ſucceſſively abſorbed in the ſame way. A ſimilar abſorption takes place, when a quantity of any gas containing aqueous vapour is confined along with ſulphuric acid, or dry, deliqueſcent ſalts; the vapour is abſorbed, and the maſs is diminiſhed in elaſticity, notwithſtanding the ſuppoſed chemical affinity of the gaſes for water.

Lavoiſier's deſcription of the atmoſphere therefore appears to be too limited, when he ſays, "Our atmoſphere is a compound of all the fluids which are ſuſceptible of the vaporous or permanently elaſtic ſtate, in the uſual temperature, *and under the common preſſure:*" this laſt limitation ſhould be omitted. The general atmoſphere ſeems to be a compound of *four* fluids principally, or *four* particular atmoſpheres; *azotic gas*, which at a medium preſſes the earth's ſurface with a force equal to 21.2 inches of mercury; *oxigenous gas*, which is uſually equal to 7.8 inches; *aqueous vapour*, which varies from 1 inch to .1, or leſs, according to climate and ſeaſon; and *carbonic acid gas*, the preſſure of which may perhaps be equal to ½ inch. Thoſe gaſes and vapour preſs *ſeparately* on the ſurface of the earth; and any one of them may be withdrawn, or another added to the number, without materially diſturbing the reſt, or any way affecting their denſity.

The above doctrine neceſſarily requires the force of vapours from any fluid to depend *ſolely upon temperature*, and conſequently to be the ſame in any gas as in an exhauſted receiver. This I had proved to be the fact with ſeveral fluids, before this theory occurred to explain the reaſon. I intend to give ſome account of the experiments relating to this ſubject, and to evaporation in general, in a volume of Memoirs of the Mancheſter's Literary and Philoſophical Society, now in the preſs; together with ſome experiments on the expanſion of gaſes by heat, controverting certain reſults of Guyton and Du Vernois, in the *Annales de Chimie*, Vol. I.

The very important changes in our views of facts in chemiſtry, meteorology, &c. which the principles here advanced will make if adopted; and the ſtriking manner in which they appear to me to reſolve what have juſtly been conſidered as ſome of the greateſt difficulties, muſt be my apology for deſiring theſe obſervations to be communicated as ſoon as convenient.

I am,
With reſpect, &c.
JOHN DALTON.

Mancheſter, Sept. 14, 1801.

II.

On the Opinion or Inference that the Chemical Actions of Light and Heat are the ſame.

By Mr. R. Harrup.

To Mr. NICHOLSON.

SIR,

THE reſult of ſome experiments which I made in 1797 and 1798, on the effects of light upon the mercurial oxides, and which were publiſhed laſt year in the 5th volume of the London Medical Review and Magazine, was,—That the red oxide of mercury, red nitrated mercury, and calomel, are reduced by expoſure to the rays of the ſun.

That this effect takes place when the oxides are incloſed in glaſs tubes hermetically ſealed, and that the two former are intirely reduced, the tubes containing them becoming incruſted with quickſilver, and the latter partially, &c. Struck with the ſimilar effects of light and heat in theſe inſtances, I put the queſtion, if the light which is preſent during the reduction of calcined mercury in the ordinary way, has not ſome ſhare in producing the effect? I did not know at that time, nor indeed till very lately, that any doubts were entertained of the action of light independent of heat; and it was only a few weeks ago, when looking over the 2d volume of your intereſting Journal, that I became acquainted with the opinion of Count Rumford on the ſubject, "That all the viſible changes produced in bodies by expoſure to the action of the ſun's rays, are effected, not by any chemical combination of the matter of light with ſuch bodies, but merely by the *heat* which is generated or excited, by the light which is abſorbed by them," ſeems to me to be a concluſion not warranted by experiment.

Into a very tranſparent glaſs tube I put ten grains of red oxide of mercury, ſpread out thin on a ſlip of window glaſs. To one half the direct rays of the ſun were admitted through the tube, and the light excluded from the other half by means of a caſe of paſteboard, and two ſlices of cork, which fitted the tube exactly. That portion to which the light was admitted began to acquire a deeper colour almoſt inſtantaneouſly; ſome hours after, it had become of a dark dirty brown colour, with ſomewhat of a leaden hue. Upon examination in a ſtrong light, with a lens the whole ſurface was found to be covered with ſmall metallic globules. The effect was intirely ſuperficial, for at the ſmalleſt depth below the ſurface the oxide was without change.

After expoſure for three days no farther change took place, excepting that the colour was become ſomewhat deeper, and the mercury, as it was revived, ſublimed and adhered to the upper ſide of the tube, which was, at leaſt, half an inch diſtant. The portion of the oxide from which the light was excluded had ſuffered no change, excepting a very thin ſtratum of it at the end of the glaſs next the ſlice of cork, which was uſed as a ſtopper.—

This

This part was become quite black, but contained no globules. This was the more ſurprizing, as I had not obſerved any thing ſimilar in my former experiments. By accurate examination it was found, that the rays of light had been admitted to this part of the oxide through the ſlip of window glaſs, which was of a *greeniſh caſt*. Having no priſm by me, I had not an opportunity of aſcertaining the effect of different coloured rays.

As ſome of the Count's experiments ſeem to indicate, that moiſture has ſome concern in producing the changes, and having myſelf frequently obſerved a ſmall quantity of vapour exhale, immediately on expoſure to the rays; I endeavoured to aſcertain the truth by experiment. For this purpoſe I introduced into a very dry glaſs tube, a ſmall quantity of the red oxide made perfectly dry, and encloſed it in ſuch a manner as totally to prevent the acceſs of air and moiſture. By expoſure to the rays the ſame changes took place, and the only difference I could perceive was, that in this the oxide acquired its deepeſt colour in leſs time than in the former experiment; moiſture then, we may conclude, has no effect in the reduction of mercury.

From theſe experiments it appears, that light acts intirely on the ſurface. This alone ſeems to be a material objection to the concluſion of the Count, for we cannot well ſuppoſe, that ſo intenſe a heat as is neceſſary for the reduction of the oxide, ſhould produce its effects merely on the ſurface, and no where elſe. However, there are other objections which appear to be inſuperable. If the effect be produced by the heat alone which the action of light occaſions, it muſt neceſſarily follow, that the metallic oxides in general, when placed in contact with inflammable bodies, will be reduced by mere expoſure to the ſun's rays. To know whether this is actually the caſe, I made choice of the oxides of lead for the ſubject of experiment, on account of their well known property of being eaſily revived by caloric, when mixed with inflammable bodies. Small quantities of minium, and of acetite of lead, were ſeparately expoſed on glaſs to the direct rays of the ſun for ſeveral hours. No change was produced, except that the water of cryſtallization had exhaled from the latter. Next day a ſmall quantity of charcoal was thinly ſprinkled on each, and gently preſſed down. After expoſure to the rays the whole day, not the ſmalleſt change was effected; they were then put into two ſmall cylindrical glaſs veſſels, and alcohol poured over them to the depth of half an inch, and again expoſed on the third day till the ſpirit was intirely evaporated, but without any alteration whatever.

May we not therefore conclude that light acts independent of heat.

I am, SIR,

Your's reſpectfully,

ROBERT HARRUP.

Cobham, Surry, Sept. 1, 1801.

P. S. I cannot neglect this opportunity of giving the reſult of ſome experiments made three years ago, on the production of gas by freezing water, and of which I was reminded at preſent by reading Dr. Prieſtley's communication in the 4th volume of your Journal.

If water from which the air is extricated by long continued boiling, be ſhut up in glaſs veſſels intirely filled with it and then frozen, they will burſt by the expanſion, and numerous air bubbles will be diffuſed throughout the ice. The ſame effect takes place if freſh diſtilled water is uſed. If water, warm from the ſtill, be incloſed in exhauſted bottles which will contain three or four times the quantity, and alternately frozen and thawed a great number of times, a quantity of gaſeous fluid will be produced, equal at leaſt in bulk to the water employed. What the nature of this gas is I have not yet been able to determine, as the quantity I procured was loſt by accident; I only know, that it did not explode by the contact of a lighted taper. During the freezing and thawing ſeveral curious phænomena took place. Upon the whole, the ſubject is highly deſerving attention, and it ſeems ſurprizing that it has not been thoroughly inveſtigated long before this time.

R. H.

III.

Memoir on a Method of Houſe Painting with Milk; *by* ANTONY ALEXIS CADET-DE-VAUX.*

I PUBLISHED in the *Feuille du Cultivateur*, but at a time when the weight of public miſery abſorbed the thoughts of every individual, a proceſs for painting, of ſingular œconomy, which the misfortune of the times compelled me to ſubſtitute, inſtead of painting in diſtemper.

One of my neighbours was repairing his country houſe, in which he employed ſome old wood work that neceſſarily required to be painted; but the univerſal want of every article, and more particularly of money, for it was in the time of the maximum and aſſignats, did not permit him to go to the expence. The amount of the colour would have been four hundred francs. In this ſituation he conſulted me, and fortunately he applied to the proper perſon: for I was employed painting a hot-houſe in my garden, at the time he called to converſe with me. He found me painting without diſtemper and without oil; he expreſſed his uneaſineſs about his four hundred francs, on which head I ſoon removed his anxiety, by aſſuring him that I would reduce the ſum to a tenth part. Part of my hot-houſe had been painted in the morning: he paſſed the palm of his hand over this firſt layer, but I deſired him to rub it with his great coat. It reſiſted like varniſh. He demanded my ſecret; and as I keep no ſecrets of this nature, but on the contrary am deſirous of publiſhing every thing which can be of value in different branches of œconomy, I gave him my proceſs.

As I am not dextrous with the bruſh, I ſent the next day for a painter from Franconville to paint the cieling and floor of my library.

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* Decade Philoſophique, No. 29, An. IX.

My

My paint ſmelled neither of ſize nor of oil. It is a miſerable watery compoſition, ſaid the painter. He ſeemed to do me a favour in condeſcending to uſe it. I had taken care not to ſhew him the experience of the day before. He began with the cieling; I preſented him with another pot of yellow colour for the floor. While the floor was painting the cieling became dry, and gave him an opportunity of judging of the ſolidity of this kind of painting. He immediately ſaw and admitted its ſuperiority over the method in diſtemper; a ſingle coat, or even ſeveral of which will not ſtand the trial of being rubbed with a coarſe woolen cloth, without coming off. He was ſtruck with aſtoniſhment, made his apology, and concluded by aſking for my ſecret, which I immediately gave him, and which he has ſince turned to good account.

At the preſent moment I am about to paint it again, or rather I am deſirous of making another trial to fix the proceſs of this method, more eſpecially that of ſubſtituting it in the place of oil painting.

Let us now proceed to the method itſelf.

Take of ſkimmed milk a pint, which makes two pints of Paris, or nearly two quarts Engliſh; freſh ſlaked lime, ſix ounces, (about $6\frac{1}{2}$ oz. avoirdupois); oil of caraways, or linſeed, or nut, four ounces; Spaniſh white, (whiting) three pounds: put the lime into a ſtone ware veſſel, and pour upon it a ſufficient quantity of milk to make a mixture reſembling thin cream; then add the oil a little at a time, ſtirring it with a ſmall ſpatula; the remainder of the milk is then to be added, and laſtly, the Spaniſh white. Skimmed milk in ſummer is often clotted, but this is a circumſtance of no conſequence to our object; becauſe the contact with the lime ſoon reſtores its fluidity. But it muſt on no account be ſour, becauſe in that caſe it would form with the lime a kind of calcareous acetite, capable of attracting moiſture.

The lime is ſlaked by dipping it in water, out of which it is to be immediately taken, and left to fall in pieces in the air.

The choice of either of theſe oils is indifferent; nevertheleſs for white paint the oil of caraways is to be preferred, becauſe colourleſs. The commoneſt oils may be uſed for painting with the ocres.

The oil when mixed in with the milk and lime diſappears, and is totally diſſolved by the lime, with which it forms a calcareous ſope.

The Spaniſh white is to be crumbled, or greatly ſpread on the ſurface of the fluid, which it gradually imbibes, and at laſt ſinks; at this period it muſt be well ſtirred in. This paint may be coloured like diſtemper (or ſize colour) with levigated charcoal, yellow ochre, &c.

And it is uſed in the ſame manner:

The quantity here preſcribed is ſufficient for the firſt coat of ſix toiſes, or 27 ſquare yards Engliſh.

The price of this quantity amounts to nine ſols, which reduces the price of the ſquare toiſe to one ſol, ſix deniers, prime coſt.

The

The preceding inſtructions have been ſufficient for moſt of thoſe who have made uſe of this painting; but let us reaſon upon it, and diſcuſs the theory which has guided me in the choice and combination of theſe ingredients:

In the firſt place, let us ſpeak of painting in diſtemper. This is done, as every one knows with ſize, to which is added Spaniſh white, or ochre.

Its inconveniencies are numerous.

In the firſt place, it comes off by the ſlighteſt friction; ſo that for the moſt part it is an inconvenience to ſuffer the clothes to come in contact with the walls and paſſages painted in this manner.

It is true that this inconvenience is remedied by uſing ſtronger ſize, and laying on a greater number of coatings; but in this caſe, in the courſe of time, in dry expoſures, the painting peels off, and is detached.

It is no trifling conſideration, that three coatings in diſtemper will coſt thirty ſous, or near ſeven times as much as our paint of three coatings, if that were neceſſary.

The conſequences where the expoſure is moiſt are ſtill more pernicious. The glue or ſize which forms the baſis of diſtemper becomes moiſt, and ferments; like every other animal glue it partly paſſes to the ſtate of acidity; it ceaſes to be glue, and is no longer adheſive; it becomes a fluid and acid water, which ſpeedily acquires the putrid ſtate. It is more particularly at the time of thaw that this effect of moiſture becomes very ſenſible, and then the paint of this deſcription is ſeen to flow in ſtreaks along the walls and wood work. The earth and ochre becoming ſeparated from the gelatinous ſubſtance, are then abandoned to their pulverulent ſtate, and the paint falls off.

This diſpoſition of animal gluten to attract moiſture from the air, and to ferment, explains the reaſon why in proceſs of time painting in diſtemper emits a ſmell, unleſs it be varniſhed, which partly remedies thoſe inconveniencies; but at the ſame time it muſt be recollected, that it adds to the expence.

We muſt alſo remark, that the mixing of the ſize colour requires a fire, and demands the expence of workman's time during part of the morning for its preparation: and that laſtly, this paint will not keep, but frequently changes in the courſe of twenty-four hours, particularly in ſtormy weather.

On the other hand, let us conſider and compare our method of painting.

Skimmed milk has loſt its butyraceous part, but it keeps its cheeſy part or curd, of which milk contains more or leſs, at a medium about one fourth in a ſtate of ſtrong gelatine; conſequently there is about a pound in the Paris pint, or four pounds of milk, which is our proportion. I ſay in the ſtate of gelatine, for the caſeous part not being ſoluble in water is not gelatine, but it is glue.

We have therefore one pound of glue, which deſerves the name of ſtrong glue, in preference to the glues which are ſo called, and this is appropriated to give body to our colours; but this glue of milk has great advantages over the common animal glue.

The

The cheeſy part retains a portion of humidity, a principle which gives it elaſticity. For example, take cheeſe of Gruyeres ; let it be ever ſo dry, it is ſtill elaſtic ; it may be raſped, but cannot be pounded.

This portion of humidity which the cheeſy part retains is ſufficient for its conſtitution. It does not therefore attract the moiſture of the atmoſphere, and the pigment of which it forms the baſe, does not become crumbly in dry elevated well-aired places, neither does it become ſoft in low and humid expoſures.

One of the properties of this paint is, that it may be kept for whole months, and requires neither time nor fire, nor even manipulation ; ten minutes are ſufficient to prepare enough to paint a whole houſe. There probably is formed in this mixture a combination, which in that caſe would add much to the ſolidity of the painting, namely, that of the Spaniſh white with the oil. The putty of the glaziers, which becomes ſo ſolid that it is almoſt impoſſible to detach it, is nothing but this mixture, and ſo likewiſe is the lute of the chemiſts. It may eaſily be conceived, how much this maſtic muſt add to the ſolidity of the colour in which it is diſſolved.

This advantage is attended with other valuable conveniencies. A bed room may be inhabited the very night after it has been painted, becauſe this paint becomes dry in an hour, and the oil which enters into the compoſition loſes its ſmell and character of oil by its combination of lime, which forms a true ſoap ; the ſmell of this colour when freſh laid is ſaponaceous ; the apartment previous to its becoming dry ſmells like a laundry.

A ſingle coat is ſufficient for places which have been before painted. It is only neceſſary to lay on two when the ſpots of greaſe repel the firſt coating ; in this caſe they ſhould be waſhed off with ſtrong lime water or ſoap lees, or ſcraped.

New wood requires two coatings. One coat is ſufficient for a ſtaircaſe, paſſage, or cieling.

The hot-houſe I have ſpoken of has its door hung within : the door-ſill is therefore expoſed to all the injuries of the air ; but notwithſtanding this and the continual friction, the traces of the bruſh are ſtill viſible, and the paint has not undergone the ſlighteſt alteration.

I have ſince given to this method of painting a much greater degree of ſolidity ; for my aim has been not only to ſubſtitute it inſtead of painting in diſtemper, but likewiſe of oil paint.

Milk Paint containing Reſinous Matter.

For out-door work I add to the proportions of the paint,

Slaked lime, - - -	2 ounces
Oil, - - - -	2 ounces
White Burgundy pitch, -	2 ounces.

The

The pitch is to be melted by a gentle heat in the oil, and then added to the ſmooth mixture of milk and lime. In cold weather this mixture ought to be warmed, in order that it may not occaſion too ſpeedy cooling of the pitch, and to facilitate its union with the milk of lime. This paint has ſome analogy with that known by the name of encauſtic.

I have employed this compoſition for outſide ſhutters, formerly painted with oil.

The common method of painting in oil, though thought to be ſo durable, participates ſtrongly in the inconveniencies of painting in diſtemper; that is to ſay, it ſhrinks, cracks, ſcales off, and is ſeparated; with this difference, that the flakes preſerve ſome points of adheſion. But is not the leſs true, that the wood remains uncovered, and expoſed without defence to the action of the air.

For the ſake of neatneſs, it is proper to rub down the ſurface with ſand ſtone, or pumice ſtone, before the firſt coating is laid on.

Time, which deſtroys every thing, will deſtroy this paint; but I do not in theory ſee any reaſon why the adheſion of the reſinous milk paint ſhould break, or that it ſhould crack and ſcale off like the painting in oil; of which the baſe is ceruſe, a preparation of lead, which renders the oil drying, and diſpoſes this kind of paint to crack. This metallic ſubſtance has alſo the inconvenience of becoming coloured in the air by the action of hidrogen gas; ſo that white paint becomes yellow, and is frequently covered with black ſpots by a de-oxidation aſſumed by the oxide of lead. The exhalations of a drain or neceſſary, or the vicinity of a ſtable, are ſufficient completely to blacken painting of lead colour with oil.

There is no lead in the paint of reſinous compoſition with milk, conſequently this inconconvenience is not to be feared. The air cannot change the colour of this painting, as I am convinced by experience in my hot-houſe; and every building of this deſcription is expoſed to the exhalation of fermenting vegetable ſubſtances.

I need not mention the unwholeſome ſmell of oil painting, nor the long time it continues. In country houſes which are not inhabited during winter, it is found to remain even for ſeveral years ſucceſſively. The dangers of the exhalation of this kind of paint are frequent, and but too well known.

Our painting with reſinous compoſition and milk, notwithſtanding the oil and the Burgundy pitch which enter into its compoſition, emits no other ſmell than that of turpentine *; a ſmell which is ſpeedily diſſipated in a day or two, beſides which it is uſed only for outdoor work.

The reaſons are ſtronger in favour of ſubſtituting either of theſe new modes of painting, to that which is called *badigeon* †. This pigment conſiſts of quick-lime, yellow ochre, and water; it laſts very well within-doors, but when expoſed to the air it ſoon diſappears. This mixture has no conſiſtence; its adherence to ſtone is ſo weak, that the rain carries it off, and leaves no trace in two or three years.

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* It is well known that Burgundy pitch is the reſidue of diſtillation of turpentine with water, an operaſion for the purpoſe of ſeparating the volatile oil.

† We have no appropriate name in Engliſh for this method.

This method of painting beſides coſts ten ſols the toiſe; but as we have before obſerved, the painting in diſtemper with milk does not coſt more than one ſol, ſix deniers, to which we muſt add a ſmall matter for the expence of laying it on, as a man can cover a great number of toiſes in a day.

But I ſhould prefer for theſe purpoſes the painting with the reſinous compoſition and milk, uſing the lime and yellow ochre, with or without the addition of Spaniſh white. This paint would laſt twenty or thirty years without alteration; I know of nothing but the nitrification of the walls which could injure it, and this is a ſlow proceſs.

I have ſpoken much to the diſadvantage of painting in oil and in diſtemper, and I have pleaded the cauſe of my new proceſs; but the theory on which I founded my preparation, and the experience I have had, ſpeak ſtrongly in favour of my concluſions.

I ſee only one objection, namely, the difficulty of procuring milk. This is far from being a difficulty, for there is more want of buyers for milk, than milk for the buyers. In the ſeaſon of fruits in France there is ſcarcely any demand for milk, and in many places they are but very indifferently ſkilled in making cheeſe or butter. The ſummer is the ſeaſon for painting. The conſumption of milk at that time will even be advantageous to the farmer, and in no reſpect detrimental to thoſe who conſume it as food. It may alſo be obſerved, that for this purpoſe the milk may be brought from a much greater diſtance than it could for conſumption as food, for if half the lime be added at the place where the milk was produced, it may then be carried without becoming ſour or clotting, in a perfect ſtate to the place of its deſtination. Little objection can be made in the country as to the ſcarcity of milk.

The political objection, that the houſe painters would by this practice be reduced to beggary, would alſo be a great objection with me to the proceſs, if I were not convinced from numberleſs inſtances, that the increaſed conſumption of a cheap thing more than compenſates for the difference of charge by the abundance of employ it gives. Out of a hundred perſons who would paint their apartments at twenty-five francs, there are ſcarcely five who would paint, if the price were fifty crowns. Thoſe who hold their dwellings only for a ſhort term, would adopt this uſeful and wholeſome proceſs when they knew its cheapneſs; ſo that upon the whole we might reaſonably infer, that twice the number of painters might be employed in the workmanſhip, when the materials could be afforded ſo much better and cheaper.

The facility with which this work may be performed, may alſo be of ſome advantage in preventing workmen from giving the law to their employers; for there are many operations of domeſtic employ which are much more difficult than this. The mixture may be brought to a ſtill greater degree of perfection. Fat or tallow may be ſubſtituted inſtead of oil, and wax inſtead of Burgundy pitch.

I therefore invite thoſe chemiſts who devote their exertions to the improvement of the arts, to attend to this method of painting, and fix the proportions; to aſcertain whether

lime

lime acts upon the colouring oxides or ochres; and, laſtly, to render the proceſs generally uſeful. By this union of effort the preſent valuable branch of the arts would be ſimplified, individuals would obtain conſiderable ſavings, our habitations would become more neat and cleanly, and particularly more healthy, as there is nothing which alters the purity of the air more than oil paint.

IV.

On the Mechaniſm of the Eye. By THOMAS YOUNG, *M.D. F.R.S.*[*]

I. IN the year 1793, I had the honour of laying before the Royal Society, ſome obſervations on the faculty by which the eye accommodates itſelf to the perception of objects at different diſtances.[†] The opinion which I then entertained, although it had never been placed exactly in the ſame light, was neither ſo new, nor ſo much forgotten, as was ſuppoſed by myſelf, and by moſt of thoſe with whom I had any intercourſe on the ſubject. Mr. Hunter, who had long before formed a ſimilar opinion, was ſtill leſs aware of having been anticipated in it, and was engaged, at the time of his death, in an inveſtigation of the facts relative to it;[‡] an inveſtigation for which, as far as phyſiology was concerned, he was undoubtedly well qualified. Mr. Home, with the aſſiſtance of Mr. Ramſden, whoſe recent loſs this Society cannot but lament, continued the inquiry which Mr. Hunter had begun; and the reſults of his experiments appeared very ſatisfactorily to confute the hypotheſis of the muſcularity of the cryſtalline lens.[§] I therefore thought it incumbent on me, to take the earlieſt opportunity of teſtifying my perſuaſion of the juſtice of Mr. Home's concluſions, which I accordingly mentioned in a diſſertation publiſhed at Gottingen in 1796,[‖] and alſo in an eſſay preſented laſt year to this Society.[**] About three months ago, I was induced to reſume the ſubject, by peruſing Dr. Porterfield's paper on the internal motions of the eye;[††] and I have very unexpectedly made ſome obſervations, which I think I may venture to ſay, appear to be finally concluſive in favour of my former opinion, as far as that opinion attributed to the lens a power of changing its figure. At the ſame time, I muſt remark, that every perſon who has been engaged in experiments of this nature, will be aware of the extreme delicacy and precaution requiſite, both in conducting them, and in drawing inferences from them; and will alſo readily allow, that no apology is neceſſary for the fallacies which have miſled many others, as well as myſelf, in the application of thoſe experiments to optical and phyſiological determinations.

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[*] Philoſ. Tranſ. 1801.

[†] Phil. Tranſ. for 1793, p. 169.

[‡] Phil. Tranſ. for 1794, p. 21.

[§] Phil. Tranſ. for 1795, p. 21.

[‖] De Corporis humani Viribus conſervatricibus, p. 68.

[**] Phil. Tranſ. for 1800, p. 146.

[††] Edinb. Med. Eſſays, Vol. IV. p. 124.

II.

II. Beſides the inquiry reſpecting the accommodation of the eye to different diſtances, I ſhall have occaſion to notice ſome other particulars relative to its functions; and I ſhall begin with a general conſideration of the ſenſe of viſion. I ſhall then enumerate ſome dioptrical propoſitions ſubſervient to my purpoſes, and deſcribe an iuſtrument for readily aſcertaining the focal diſtance of the eye. On theſe foundations, I ſhall inveſtigate the dimenſions and refractive powers of the human eye in its quieſcent ſtate; and the form and magnitude of the picture which is delineated on the retina. I ſhall next inquire, how great are the changes which the eye admits, and what degree of alteration in its proportions will be neceſſary for theſe changes, on the various ſuppoſitions that are principally deſerving of compariſon. I ſhall proceed to relate a variety of experiments which appear to be the moſt proper to decide on the truth of each of theſe ſuppoſitions, and to examine ſuch arguments as have been brought forwards, againſt the opinion which I ſhall endeavour to maintain; and I ſhall conclude with ſome anatomical illuſtrations of the capacity of the organs of various claſſes of animals, for the functions attributed to them.

III. Of all the external ſenſes, the eye is generally ſuppoſed to be by far the beſt underſtood; yet ſo complicated and ſo diverſified are its powers, that many of them have been hitherto uninveſtigated; and on others, much laborious reſearch has been ſpent in vain. It cannot indeed be denied, that we are capable of explaining the uſe and operation of its different parts, in a far more ſatisfactory and intereſting manner than thoſe of the ear, which is the only organ that can be ſtrictly compared with it; ſince, in ſmelling, taſting, and feeling, the objects to be examined come almoſt unprepared into immediate contact with the extremities of the nerves; and the only difficulty is, in conceiving the nature of the effect produced by them, and its communication to the ſenſorium. But the eye and the ear are merely preparatory organs, calculated for tranſmitting the impreſſions of light and ſound to the retina, and to the termination of the ſoft auditory nerve. In the eye, light is conveyed to the retina, without any change of the nature of its propagation: in the ear, it is very probable, that inſtead of the ſucceſſive motion of different parts of the ſame elaſtic medium, the ſmall bones tranſmit the vibrations of ſound, as paſſive inelaſtic hard bodies, obeying the motions of the air in their whole extent at the ſame inſtant. In the eye, we judge very preciſely of the direction of light, from the part of the retina on which it impinges: in the ear, we have no other criterion than the ſlight difference of motion in the ſmall bones, according to the part of the tympanum on which the ſound, concentrated by different reflections, firſt ſtrikes; hence, the idea of direction is neceſſarily very indiſtinct, and there is no reaſon to ſuppoſe, that different parts of the auditory nerve are excluſively affected by ſounds in different directions. Each ſenſitive point of the retina is capable of receiving diſtinct impreſſions, as well of the colour as of the ſtrength of light; but it is not abſolutely certain, that every part of the auditory nerve is capable of receiving the impreſſion of each of the much greater diverſity of tones that we can diſtinguiſh; although it is extremely probable, that all the different parts of the ſurface expoſed to the fluid of the veſtibule, are more or leſs affected by every ſound, but in different degrees and ſucceſſion,

according

according to the direction and quality of the vibration. Whether or no, ſtrictly ſpeaking, we can hear two ſounds, or ſee two objects, in the ſame inſtant, cannot eaſily be determined: but it is ſufficient, that we can do both, without the intervention of any interval of time perceptible to the mind; and indeed we could form no idea of magnitude, without a comparative, and therefore nearly cotemporary perception of two or more parts of the ſame object. The extent of the field of perfect viſion for each poſition of the eye, is certainly not very great; but it will appear hereafter, that its refractive powers are calculated to take in a moderately diſtinct view of a whole hemiſphere: the ſenſe of hearing is equally perfect in almoſt every direction.

IV. DIOPTRICAL PROPOSITIONS.

Propoſition I. *Phenomenon.*

In all refractions, the ratio of the ſine of the angle of incidence to the ſine of the angle of refraction is conſtant. (Newton's Opt. I. Ax. 5. Smith's Opt. 13. Wood's Opt. 24.)

Scholium 1. We ſhall call it the ratio of m to $m \mp 1$, and $m \mp 1$, n. In refractions out of air into water, $m = 4$ and $n = 3$, very nearly; out of air into glaſs, the ratio is nearly that of 3 to 2.

Scholium 2. According to Barrow, (*Lect. Opt.* ii. 4.) Huygens, Euler, (*Conject. phyſ. circa prop. ſoni et luminis. Opuſc. t. ii.*) and the opinion which I lately ſubmitted to the Royal Society, (Phil. Tranſ. for 1800. p. 128,) the velocity of light is the greater the rarer the medium: according to Newton, (Schol. Prop. 96. l. i. Princip. Prop. 10. p. 3. l. ii. Opt.) and the doctrine more generally received, the reverſe. On both ſuppoſitions, it is always the ſame in the ſame medium, and varies in the ratio of the ſines of the angles. This circumſtance is of uſe in facilitating the computation of ſome very complicated refractions.

Propoſition II. *Phenomenon.*

If between two refracting mediums, a third medium, terminated by parallel ſurfaces, be interpoſed, the whole refraction will remain unchanged. (Newton's Opt. l. i. p. 2. Prop. 3. Smith. r. 399. Wood, 105.)

Corollary. Hence, when the refractions out of two mediums into a third are given, the refraction at the common ſurface of theſe mediums may be thus found. Let the refractions given be as $m : n$, and as $m^1 : n^1$; then the ratio ſought will be that of $m n^1 : m^1 n$. For inſtance, let the three mediums be glaſs, water, and air; then $m = 3$, $n = 2$, $m^1 = 4$, $n^1 = 3$, $m n^1 = 9$, and $m^1 n = 8$. If the ratios be 4 : 3, and 13 : 14, we have $m n^1 : m^1 n :: 39 : 56$; and, dividing by $56 - 39$, we obtain 2·3 and 3·3 for m and $m + 1$, in Schol. 1, Prop. I.

Propoſition III. *Problem.* (Plate XIII. Fig. 1.)

At the vertex of a given triangle (CBA), to place a given refracting ſurface (B), ſo that the incident and refracted rays may coincide with the ſides of the triangle (A B and B C.)

Let the ſides be called d and e; then in the baſe take, next to d (or AB), a portion (AE) equal to $\frac{n d}{n d + m e}$, or (AD $=$) $\frac{m d}{m d + n e}$; draw a line (EB, or DB) to the vertex, and the ſurface muſt be perpendicular to this line, whenever the problem is phyſically poſſible. When e becomes infinite, and parallel to the baſe, take $\frac{n d}{m}$ or $\frac{m d}{n}$ next to d, for the interſection of the radius of curvature.

Propoſition IV. *Theorem.* (Fig. 2.)

In oblique refractions at ſpherical ſurfaces, the line (AI, KL,) joining the conjugate foci (A, I; K, L;) paſſes through the point (G), where a perpendicular from the centre (H) falls on the line (EF), biſecting the chords (BC, BD,) cut off from the incident and refracted rays.

Corollary 1. Let t and u be the coſines of incidence and refraction, the radius being 1, and d and e the reſpective diſtances of the foci of incident and refracted rays; then $e = \frac{m d u u}{m d u - n d t - n t t}$.

Corollary 2. For a plane ſurface, $e = \frac{m d u u}{- n t t}$.

Corollary 3. For parallel rays, $d = \infty$, and $e = \frac{m u u}{m u - n t}$.

Scholium 1. It may be obſerved, that the cauſtic by refraction ſtops ſhort at its cuſp, not geometrically, but phyſically, the total reflection interfering.

Corollary 4. Call $\frac{m u u}{m u - n t}$, b, and $\frac{n t t}{m u - n t}$, c; then $e = \frac{b d}{d - c}$, and $e - b = \frac{b c}{d - c}$; or, in words, the rectangle contained by the focal lengths of parallel rays, paſſing and repaſſing any ſurface in the ſame lines, is equal to the rectangle contained by the differences between theſe lengths and the diſtances of any conjugate foci.

Corollary 5. For perpendicular rays, $e = \frac{m d}{d - n} = m + \frac{m n}{d - n}$; or, if the radius be a, $e = \frac{m a d}{d - n a}$; and if d and e be given to find the radius, $a = \frac{d e}{m d + n e}$.

Corollary 6. For rays perpendicular and parallel, $e = m$, or $e = m a$.

Corollary

Corollary 7. For a double convex lens, neglecting the thickneſs, call the firſt radius g, the ſecond h, and $e = \frac{n d g h}{d g + d h - n g h}$. Hence $n = \frac{d e}{d + e} . \frac{g + h}{g h}$; and, for parallel rays, $e = \frac{n g h}{g + h}$, and $n = e . \frac{g + h}{g h}$. If $g = h = a$, $e = \frac{n a d}{2 d - n a}$; and for parallel rays $e = \frac{n a}{2}$: calling this principal focal length b, $e = \frac{b d}{d - b}$, as in Cor. 4; whence we have the joint focus of two lenſes; alſo, $b = \frac{d e}{d + e}$.

Corollary 8. In a ſphere, $e = m a . \frac{d + a}{2 d - (m - 2) a}$, for the diſtance from the centre, and $b = \frac{m a}{2}$.

Scholium 2. In all theſe caſes, if the rays converge, d muſt be negative. For inſtance, to find the joint focus of two convex, or concave lenſes, the expreſſion becomes, $e = \frac{b d}{b + d}$.

Corollary 9. In Cor. 3, the diviſor becomes ultimately conſtant; and, when the inclination is ſmall, the focus varies as $u u$.

Corollary 10. For parallel rays falling obliquely on a double convex, or double concave lens, of inconſiderable thickneſs, the radius being 1, $e = \frac{n t u}{2 (m u - n t)}$; which varies ultimately as the product of the coſines, or as $\frac{m + n}{n n} t + t^2$.

Scholium 3. In the double convex lens, the thickneſs diminiſhes the effect of the obliquity near the axis; in the double concave, it increaſes it.

Scholium 4. No ſpherical ſurface, excepting one particular caſe, (Wood, 155,) can collect an oblique pencil of rays, even to a phyſical point. The oblique rays which we have hitherto conſidered, are only ſuch as lie in that ſection of the pencil which is made by a plane paſſing through the centre and the radiant point. They continue in this plane, notwithſtanding the refraction, and therefore will not meet the rays of the collateral ſections, till they arrive at the axis. The remark was made by Sir Iſaac Newton, and extended by Dr. Smith, (Smith r. 493, 494;) it appears, however, to have been too little noticed. (Wood, 362.) The geometrical focus thus becomes a line, a circle, an oval, or other figure, according to the form of the pencil, the nature of the ſurface, and the place of the plane receiving the image. Some of the varieties of the focal image of a cylindrical pencil obliquely refracted are ſhown in Fig. 28.

Corollary 11. Hence the line joining the remoter conjugate foci, will always paſs through the centre. The diſtance of the remoter focus of parallel rays will be expreſſed by

$f = \frac{m}{mu - nt}$; and the leaſt circle of aberration will be at the diſtance $\frac{1 + n^3 - 2u^4}{(1 + uu).(mu - nt)}$, dividing the length of aberration in the ratio of the diſtance of its limits from the ſurface. In the caſe of Cor. 10. $f = \frac{n}{2(mu - nt)}$.

Corollary 12. This propoſition extends alſo to reflected rays; and, in that caſe, the line from the centre paſſes through the point of incidence.

Propoſition V. *Problem.*

To find the place and magnitude of the image of a ſmall object, after refraction at any number of ſpherical ſurfaces.

Conſtruction. (Plate XIII. Fig. 3.) From any point (B) in the object (AB), draw lines to (C), the centre of the firſt ſurface, and to (D), the focus of parallel rays coming in a contrary direction: from the interſection of the ſecond line (BD) with the tangent (EF) at the vertex, draw a line (EH) parallel to the axis, and it will cut the firſt line (BC) in (H), the firſt image of the point (B). Proceed with this image as a new object, and repeat the operation for each ſurface; and the laſt point will be in the image required. For calculation, find the place of the image by Cor. 5. Prop. IV. and its magnitude will be to that of the object, as their reſpective diſtances from the centre.

Corollary. If a confuſed image be received on any given plane, its magnitude will be determined by the line drawn from the preceding image through the centre of the laſt ſurface.

Propoſition VI. *Problem.*

To determine the law by which the refraction at a ſpherical ſurface muſt vary, ſo as to collect parallel rays to a perfect focus.

Solution. Let v be the verſed ſine to the radius 1; then, at each point without the axis, n remaining the ſame, m muſt become $\sqrt{mm \mp 2nv}$; and all the rays will be collected in the principal focus.

Corollary. The ſame law will ſerve for a double convex lens, in the caſe of equidiſtant conjugate foci, ſubſtituting n for m.

Propoſition VII. *Problem.*

To find the principal focus of a ſphere, or lens, of which the internal parts are more denſe than the external.

Solution. In order that the focal diſtance may be finite, the denſity of a finite portion about the centre muſt be equable: call the radius of this portion $\frac{1}{l}$, that of the ſphere being unity; let the whole refraction out of the ſurrounding medium into this central part, be as m to

m to n; take $r = \frac{\log. l}{\log. m - \log. n}$, and let the denſity be ſuppoſed to vary every where inverſely as the power $\frac{1}{r}$ of the diſtance from the centre: then the principal focal diſtance from the centre will be $\frac{r-1}{2} \cdot \frac{m}{n l - m}$. When $r = 1$, it becomes $\frac{1}{2(\text{H. L. } m - \text{H. L. } n)}$. For a lens, deduct one-fourth of the difference between its axis and the diameter of the ſphere of which its ſurfaces are portions.

Corollary. If the denſity be ſuppoſed to vary ſuddenly at the ſurface, m muſt expreſs the difference of the refractions at the centre and at the ſurface; and the focal diſtance, thus determined, muſt be diminiſhed according to the refraction at the ſurface.

Propoſition VIII. *Problem.*

To find the nearer focus of parallel rays falling obliquely on a ſphere of variable denſity.

Solution. Let r be as in the laſt propoſition, s the ſine of incidence, t the coſine, and e the diſtance of the focus from the point of immerſion. Then $e = \frac{w - t}{2 - t w}$, w being

$$= \frac{2}{\frac{r+1}{r-1}\,(r-1)s} \cdot (a\text{A} + b\text{B} + c\text{C} + \ldots) + 2a\text{A} + 6b\text{B}s^2 + 10c\text{C}s^4 + \ldots,$$

where $a = \frac{r}{r+1}$, $b = \frac{r}{3r-1}$, $c = \frac{r}{5r-3}$, $\text{A} = 1$, $\text{B} = \frac{1}{2}\text{A}$, $\text{C} = \frac{3}{4}\text{B}$, $\text{D} = \frac{5}{6}\text{C}$. But, when s is large, the latter part of the ſeries converges ſomewhat ſlowly. The former part might be abridged if it were neceſſary: but, ſince the focus in this caſe is always very imperfect, it is of the leſs conſequence to provide an eaſy calculation.

General Scholium. The two firſt propoſitions relate to well known phenomena; the third can hardly be new; the fourth approaches the neareſt to Maclaurin's conſtruction, but is far more ſimple and convenient; the fifth and ſixth have no difficulty; but the two laſt require a long demonſtration. The one is abridged by a property of logarithms; the other is derived from the laws of centripetal forces, on the ſuppoſition of velocities directly as the refractive denſities, correcting the ſeries for the place of the apſis, and making the ſine of incidence variable, to determine the fluxion of the angle of deviation.

V. Dr. Porterfield has employed an experiment, firſt made by Scheiner, to the determination of the focal diſtance of the eye; and has decribed, under the name of an optometer, a very excellent inſtrument, founded on the principle of the phenomenon.*

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* Edinb. Med. Eſſ. Vol. IV. p. 185.

But the apparatus is capable of conſiderable improvement; and I ſhall beg leave to deſcribe an optometer, ſimple in its conſtruction, and equally convenient and accurate in its application.

Let an obſtacle be interpoſed between a radiant point (R, Plate XIII. Fig. 4,) and any refracting ſurface, or lens (CD), and let this obſtacle be perforated at two points (A and B) only. Let the refracted rays be intercepted by a plane, ſo as to form an image on it. Then it is evident, that when this plane (EF) paſſes through the focus of refracted rays, the image formed on it will be a ſingle point. But, if the plane be advanced forwards (to GH), or removed backwards (to IK), the ſmall pencils paſſing through the perforations, will no longer meet in a ſingle point, but will fall on two diſtinct ſpots of the plane (G, H; I, K;) and, in either caſe, form a double image of the object.

Let us now add two more radiating points, (S and T, Fig. 5,) the one nearer to the lens than the firſt point, the other more remote; and, when the plane which receives the images paſſes through the focus of rays coming from the firſt point, the images of the ſecond and third points muſt both be double (*s s*, *t t*;) ſince the plane (EF) is without the focal diſtance of rays coming from the furtheſt point, and within that of rays coming from the neareſt. Upon this principle, Dr. Porterfield's optometer was founded.

But, if the three points be ſuppoſed to be joined by a line, and this line to be ſomewhat inclined to the axis of the lens, each point of the line, except the firſt point (R, Fig. 6,) will have a double image; and each pair of images, being contiguous to thoſe of the neighbouring radiant points, will form with them two continued lines, and the images being more widely ſeparated as the point which they repreſent is further from the firſt radiant point, the lines (*s t*, *s t*,) will converge on each ſide towards (*r*) the image of this point, and there will interſect each other.

The ſame happens when we look at any object through two pin holes, within the limits of the pupil. If the object be at the point of perfect viſion, the image on the retina will be ſingle; but, in every other caſe, the image being double, we ſhall appear to ſee a double object: and, if we look at a line pointed nearly to the eye, it will appear as two lines, croſſing each other in the point of perfect viſion. For this purpoſe, the holes may be converted into ſlits, which render the images nearly as diſtinct, at the ſame time that they admit more light. The number may be increaſed from two to four, or more, whenever particular inveſtigations render it neceſſary.

The optometer may be made of a ſlip of card-paper, or of ivory, about eight inches in length, and one in breadth, divided longitudinally by a black line, which muſt not be too ſtrong. The end of the card muſt be cut as is ſhown in Plate XIV. Fig. 7, in order that it may be turned up, and fixed in an inclined poſition by means of the ſhoulders: or a detached piece, nearly of this form, may be applied to the optometer, as it is here engraved. A hole about half an inch ſquare muſt be made in this part; and the ſides ſo cut as to receive a ſlider of thick paper, with ſlits of different ſizes, from a fortieth to a tenth of an inch in breadth, divided by ſpaces ſomewhat broader; ſo that each obſerver may chooſe that which beſt ſuits the aperture of his pupil. In order to adapt the inſtrument to the uſe of preſbyopic eyes, the other end muſt be furniſhed with a lens of four inches focal length; and a ſcale muſt be made near the line on each ſide of it, divided from one end into inches, and from the other according to the table here calculated from Cor. 7. Prop. IV, by means of which, not only diverging, but alſo parallel and converging rays from the lens are referred to their virtual focus. The inſtrument is eaſily applicable to the purpoſe of aſcertaining the focal length of ſpectacles required for myopic or preſbyopic eyes. Mr. Cary has been ſo good as to furniſh me with the numbers and focal lengths of the glaſſes commonly made; and I have calculated the diſtances at which thoſe numbers muſt be placed on the ſcale of the optometer, ſo that a preſbyopic eye may be enabled to ſee at eight inches diſtance, by uſing the glaſſes of the focal length placed oppoſite to the neareſt croſſing of the lines; and a myopic eye with parallel rays, by uſing the glaſſes indicated by the number that ſtand oppoſite their furtheſt croſſing. To facilitate the obſervation, I have alſo placed theſe numbers oppoſite that point which will be the neareſt croſſing to myopic eyes; but this, upon the arbitrary ſuppoſition of an equal capability of change of focus in every eye, which I muſt confeſs is often far from the truth. It cannot be expected, that every perſon, on the firſt trial, will fix preciſely upon that power which beſt ſuits the defect of his ſight. Few can bring their eyes at pleaſure to the ſtate of full action, or of perfect relaxation; and a power two or three degrees lower than that which is thus aſcertained, will be found ſufficient for ordinary purpoſes. I have alſo added to the ſecond table, ſuch numbers as will point out the ſpectacles neceſſary for a preſbyopic eye, to ſee at twelve and at eighteen inches reſpectively: the middle ſeries will perhaps be the moſt proper for placing the numbers on the ſcale The optometer ſhould be applied to each eye; and, at the time of obſerving, the oppoſite. eye ſhould not be ſhut, but the inſtrument ſhould be ſcreened from its view. The place of interſection may be accurately aſcertained, by means of an index ſliding along the ſcale.

The optometer is repreſented in Plate XIV. Fig. 8 and 9; and the manner in which the lines appear, in Fig. 10.

Table I. *For extending the ſcale by a lens of 4 inches focus.*

4	2.00	11	2.93	30	3.52	200	3.92	—35	4.51	—12	6.00
5	2.22	12	3.00	40	3.64	∞	4.00	—30	4.62	—11	6.29
6	2.40	13	3.06	50	3.70	—200	4.08	—25	4.76	—10	6.67
7	2.55	14	3.11	60	3.75	—100	4.17	—20	5.00	—9.5	6.90
8	2 67	15	3.16	70	3.78	—50	4.35	—15	5.45	—9.0	7.20
9	2.77	20	3.33	80	3.81	—45	4.39	—14	5.60	—8.5	7.56
10	2.86	25	3.45	100	3.85	—40	4.44	—13	5.78	—8.0	8.00

Table II. *For placing the numbers indicating the focal length of convex glaſſes.*

Foc.	VIII.	XII.	XVIII.	Foc.	VIII.	XII.	XVIII.	Foc.	VIII.	XII.	XVIII.
0	8.00	12.00	18.00	20	13.33	30.00	180.00	8	∞	—24.00	—14.40
40	10.00	17.14	32.73	18	14.40	36.00	∞	7	—56.00	—16.80	—11.45
36	10.28	18.00	36.00	16	16.00	48.00	—144.00	6	—24.00	—12.00	— 9.00
30	10.91	20.00	45.00	14	18.67	84.00	— 63.00	5	—13.33	— 8.57	— 5.92
28	11.20	21.00	50.40	12	24.00	∞	— 36.00	4.5	—10.29	— 7.20	— 6.00
26	11.56	22.29	58.50	11	29.33	—132.00	— 28.29	4.0	— 8.00	— 6.00	— 5.14
24	12.00	24.00	72.00	10	40.00	— 60.00	— 22.50	3.5	— 6.22	— 4.94	— 4.34
22	12.77	26.40	99.00	9	72.00	— 36.00	— 18.00	3.0	— 4.80	— 4.00	— 3.6

Table III. *For concave glaſſes.*

Number.	Focus and furtheſt place.	Neareſt place.	Number.	Focus and furtheſt place.	Neareſt place.	Number.	Focus and furtheſt place.	Neareſt place.
0		4.00	7	8	2.67	14	3.00	1.71
1	24	3.43	8	7	2.54	15	2.75	1.63
2	18	3.27	9	6	2.40	16	2.50	1.54
3	16	3.20	10	5	2.22	17	2.25	1.44
4	12	3.00	11	4.5	2.12	18	2.00	1.33
5	10	2.86	12	4.0	2.00	19	1.75	1.22
6	9	2.77	13	3.5	1.87	20	1.50	1.02

VI. Being convinced of the advantage of making every obſervation with as little aſſiſtance as poſſible, I have endeavoured to confine moſt of my experiments to my own eyes; and I ſhall, in general, ground my calculations on the ſuppoſition of an eye nearly ſimilar to my own. I ſhall therefore firſt endeavour to aſcertain all its dimenſions, and all its faculties.

For meaſuring the diameters, I fix a ſmall key on each point of a pair of compaſſes; and I can venture to bring the rings into immediate contact with the ſclerotica. The tranſverſe diameter is externally 98 hundredths of an inch.

To find the axis, I turn the eye as much inwards as poſſible, and preſs one of the keys cloſe to the ſclerotica, at the external angle, till it arrives at the ſpot where the ſpectrum formed by its preſſure coincides with the direction of the viſual axis, and, looking in a glaſs, I bring the other key to the cornea. The optical axis of the eye, making allowance of three hundredths for the coats, is thus found to be 91 hundredths of an inch, from the external ſurface of the cornea to the retina. With an eye leſs prominent, this method might not have ſucceeded.

The vertical diameter, or rather chord, of the cornea, is 45 hundredths: its verſed ſine 11 hundredths. To aſcertain the verſed ſine, I looked with the right eye at the image of the left, in a ſmall ſpeculum held cloſe to the noſe, while the left eye was ſo averted that the margin of the cornea appeared as a ſtraight line, and compared the projection of the cornea with the image of a cancellated ſcale held in a proper direction behind the left eye, and cloſe to the left temple. The horizontal chord of the cornea is nearly 49 hundredths.

Hence

Hence the radius of the cornea is 31 hundredths. It may be thought that I aſſign too great a convexity to the cornea; but I have corrected it by a number of concurrent obſervations, which will be enumerated hereafter.

The eye being directed towards its image, the projection of the margin of the ſclerotica is 22 hundredths from the margin of the cornea, towards the external angle, and 27 towards the internal angle of the eye: ſo that the cornea has an eccentricity of one-fortieth of an inch, with reſpect to the ſection of the eye perpendicular to the viſual axis.

The aperture of the pupil varies from 27 to 13 hundredths; at leaſt this is its apparent ſize, which muſt be ſomewhat diminiſhed, on account of the magnifying power of the cornea, perhaps to 25 and 12. When dilated, it is nearly as eccentric as the cornea; but, when moſt contracted, its centre coincides with the reflection of an image from an object held immediately before the eye; and this image very nearly with the centre of the whole apparent margin of the ſclerotica: ſo that the cornea is perpendicularly interſected by the viſual axis.

My eye, in a ſtate of relaxation, collects to a focus on the retina, thoſe rays which diverge vertically from an object at the diſtance of ten inches from the cornea, and the rays which diverge horizontally from an object of ſeven inches diſtance. For, if I hold the plane of the optometer vertically, the images of the line appear to croſs at ten inches: if horizontally, at ſeven. The difference is expreſſed by a focal length of 23 inches. I have never experienced any inconvenience from this imperfection, nor did I ever diſcover it till I made theſe experiments; and I believe I can examine minute objects with as much accuracy as moſt of thoſe whoſe eyes are differently formed. On mentioning it to Mr. Cary, he informed me, that he had frequently taken notice of a ſimilar circumſtance; that many perſons were obliged to hold a concave glaſs obliquely, in order to ſee with diſtinctneſs, counterbalancing, by the inclination of the glaſs, the too great refractive power of the eye in the direction of that inclination, (Cor. 10. Prop. IV.) and finding but little aſſiſtance from ſpectacles of the ſame focal length. The difference is not in the cornea, for it exiſts when the effect of the cornea is removed by a method to be deſcribed hereafter. The cauſe is, without doubt, the obliquity of the uvea, and of the cryſtalline lens, which is nearly parallel to it, with reſpect to the viſual axis: this obliquity will appear, from the dimenſions already given, to be about 10 degrees. Without entering into a very accurate calculation, the difference obſerved is found (by the ſame corollary) to require an inclination of about 13 degrees; and the remaining three degrees may eaſily be added, by the greater obliquity of the poſterior ſurface of the cryſtalline oppoſite the pupil. There would be no difficulty in fixing the glaſſes of ſpectacles, or the concave eye-glaſs of a teleſcope, in ſuch a poſition as to remedy the defect.

In order to aſcertain the focal diſtance of the lens, we muſt aſſign its probable diſtance from the cornea. Now the verſed ſine of the cornea being 11 hundredths, and the uvea being nearly flat, the anterior ſurface of the lens muſt probably be ſomewhat behind the chord of the cornea; but by a very inconſiderable diſtance, for the uvea has the ſubſtance of a thin membrane, and the lens approaches very near to it: we will therefore call this diſtance 12 hundredths. The axis and proportions of the lens muſt be eſtimated by compariſon with anatomical obſervations; ſince they affect, in a ſmall degree, the determination of its focal diſtance. M. Petit found the axis almoſt always about two lines, or 18 hundredths of an inch. The radius of the anterior ſurface was in the greateſt number 3 lines, but oftener more than leſs. We will ſuppoſe mine to be $3\frac{1}{4}$, or nearly $\frac{3}{10}$ of an inch. The radius of the poſterior ſurface was moſt frequently $2\frac{1}{2}$ lines, or $\frac{2}{9}$ of an inch.* The optical centre will be therefore $\left(\frac{18 \times 30}{30 + 22} =\right)$ about one-tenth of an inch from the anterior ſurface: hence we have 22 hundredths, for the diſtance of the centre from the cornea. Now, taking 10 inches as the diſtance of the radiant point, the focus of the cornea will be 115 hundredths behind the centre of the lens. (Cor. 5. Prop. IV.) But the actual joint focus is (91 — 22 =) 69 behind the centre: hence, diſregarding the thickneſs of the lens, its principal focal diſtance is 173 hundredths. (Cor. 7. Prop. IV.) For its refractive power in the eye, we have (by Cor. 7. Prop. IV.) $n = 13{,}5$, and $m = 14{,}5$. Calculating upon this refractive power, with the conſideration of the thickneſs alſo, we find that it requires a correction, and comes near to the ratio of 14 to 13 for the ſines. It is well known that the refractive powers of the humours are equal to that of water; and, that the thickneſs of the cornea is too equable ro produce any effect on the focal diſtance.

For determining the refractive power of the cryſtalline lens by a direct experiment, I made uſe of a method ſuggeſted to me by Dr. Wollaſton. I found the refractive power of the centre of the recent human cryſtalline to that of water, as 21 to 20. The difference of this ratio from the ratio of 14 to 13, aſcertained from calculation, is probably owing to two circumſtances. The firſt is, that the ſubſtance of the lens being in ſome degree ſoluble in water, a portion of the aqueous fluid within its capſule penetrates after death, ſo as ſomewhat to leſſen the denſity. When dry, the refractive power is little inferior to that of crown glaſs. The ſecond circumſtance is, the unequal denſity of the lens. The ratio of 14 to 13 is founded on the ſuppoſition of an equable denſity: but, the central part being the moſt denſe, the whole acts as a lens of ſmaller dimenſions; and it may be found by Prop. VII. that if the central portion of a ſphere be ſuppoſed of uniform denſity, refracting as 21 to 20, to the diſtance of one-half of the radius, and the denſity of the external parts to decreaſe gradually, and at the ſurface to become equal to that of the ſurrounding medium, the ſphere thus conſtituted, will be equal in focal length to a uniform ſphere of the ſame ſize, with a refraction of 16 to 15 nearly. And the effect will be nearly the ſame, if the central portion be ſuppoſed to be ſmaller than this, but the denſity to be ſomewhat greater at the ſurface than that of the ſurrounding medium, or to vary more rapid externally than internally. On the whole, it is probable that the refractive power of the centre of the human

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* Mem. de l'Acad. de Paris, 1730. p. 6. Ed. Amſt.

cryſtalline, in its living ſtate, is to that of water nearly as 18 to 17; that the water imbibed after death, reduces it to the ratio of 21 to 20; but that, on account of the unequable denſity of the lens, its effect in the eye is equivalent to a refraction of 14 to 13 for its whole ſize. Dr. Wollaſton has aſcertained the refraction out of air, into the centre of the recent cryſtalline of oxen and ſheep, to be nearly as 143 to 100; into the centre of the cryſtalline of fiſh, and into the dried cryſtalline of ſheep, as 152 to 100. Hence, the refraction of the cryſtalline of oxen in water, ſhould be as 15 to 14: but the human cryſtalline, when recent, is decidedly leſs refractive.

(To be continued.)

V.

On the Meaſure of a Ship's Departure by the Re-action of the Fluid through which it paſſes.

By JOHN COOKE, *Eſq.*

To MR. NICHOLSON.

SIR,

July 22, 1801, No. 14, Kildare Street, Dublin.

SINCE the year 1793, when I firſt communicated my ſcheme to you for aſcertaining diſtance at ſea, by the aſcent of water in a tube oppoſed to the ſtream, it occurred to me that departure might be meaſured in the ſame manner, and of ſeveral plans which I conceived for this purpoſe, the following ſeems to be the moſt practicable. It is an inſtrument ſimilar to that deſcribed in the 48th page of the 4th Vol. of your Journal, with theſe alterations:

Firſt, the horizontal and erect parts of the tube muſt be two diſtinct pieces, the former being moveable in an horizontal circle on the extremity of the latter, ſo that the erect part of the tube may be fixed to the veſſel, and made water tight, without impeding the motion of the horizontal part of it.

Secondly, the rod which in the diſtance inſtrument is attached to the lee-way vane, is in this inſtrument attached to the bottom of the horizontal tube, ſo that the orifice of it may be pointed thereby in any horizontal direction, and ſcrewed faſt in that poſition; and the lee-way vane, which is unneceſſary according to this method, is to be removed.

Thirdly, the extremity of the horizontal tube is to be ſo formed, as to be capable of moving up and down on horizontal pivots with two plates, like the fins of a fiſh extending behind it to keep the plane of the orifice in a vertical poſition, notwithſtanding the heeling of the ſhip, which differs from the apparatus annexed to the diſtance inſtrument in this, that the object of the former is to preſent the orifice perpendicularly to the ſtream in all

caſes,

caſes, but in this it is only required to keep the orifice perpendicular to the horizon, though it be preſented to the line of motion in an oblique direction.

If the horizontal tube of ſuch an inſtrument be placed, by means of the directing rod and index, in a true eaſt or weſt direction, according as the ſhip makes eaſting or weſting, (which direction conſequently muſt be altered as often as the ſhip's courſe is altered) the quantity of water diſcharged thereby will be proportionate to the departure, which appears from the following conſiderations:

When the plane of the orifice of this inſtrument coincides with the plane of a meridian, if the ſhip moves in a due eaſt or weſt courſe, the direct impact of the water will occaſion a diſcharge from the orifice, which meaſures the ſpace paſſed over (as was ſhewn in the caſe of the diſtance inſtrument) but in ſailing on a parallel circle this ſpace is the departure.

Again, if the ſhip ſails along a meridian with the orifice of the tube pointed as before, the ſtream will be parallel to the plane of the orifice, and therefore will occaſion no preſſure on it, or diſcharge of water from the aperture, in which caſe alſo the departure is repreſented by the reſult, becauſe a ſhip ſailing on a meridian makes no departure.

And laſtly, if this inſtrument (the orifice being ſtill in the plane of a meridian) be moved along a rhumb-line, the reſiſtance which the loweſt plate of water at the orifice meets, is the ſame as that which the orifice of a ſimilar inſtrument would meet in paſſing over the departure of that rhumb in a direct poſition, and in the ſame time, becauſe the preſſure on each orifice is as the product of the force and number of impinging particles which ſtrikes it; but the force of a ſingle particle on the direct orifice, is to the force of a particle on the oblique orifice in a ratio compoſed of the ratio of their velocities, that is, as *departure to diſtance*, and of the ratio of radius to the ſine of the angle of obliquity, that is, as *diſtance to departure*: therefore theſe forces are in a ratio compounded of the direct and inverſe ratios of the ſame pair of quantities, which is the ratio of equality, and every particle ſtrikes each orifice with equal force.

Again, the number of particles which ſtrikes theſe orifices is the ſame, becauſe they conſtitute two homogeneous columns, which having the ſame baſe, and lying between the ſame parallel planes are equal, whence it follows, that the preſſures on theſe orifices are equal at all times, and conſequently the quantities of water diſcharged through theſe apertures in equal times are equal alſo; but ſince the quantity diſcharged from the direct inſtrument would meaſure the ſpace paſſed over by it, which is the departure, it is evident that the quantity diſcharged from the oblique inſtrument in the ſame time muſt give the departure alſo, and in general that the ſpace given by ſuch an inſtrument in an oblique poſition, is as the ſine of the angle of obliquity; hence it is evident, that if this inſtrument be uſed with its orifice pointed to the north or ſouth, inſtead of the eaſt or weſt, that it will ſhew the difference of latitude, which furniſhes the beſt method of trying the performance of the inſtrument; for if when uſed in this ſhape at ſea, the difference of latitude found thereby ſhould agree with that had from obſervation, it may be ſafely concluded, that it will meaſure departure with the ſame degree of certainty,

The principal advantage which the departure inſtrument ſeems to have over the diſtance inſtrument, is, that the error of lee-way is intirely avoided by it; this may be eaſily conceived, by conſidering that the magnetic needle points out the angle between the meridian and the line of the ſhip's keel, (ſuppoſing no variation) but ſince the line of her motion is ſeldom the line of her keel, this angle is ſeldom the true courſe; but in this method the line of the ſhip's keel does not enter into conſideration, the angle made uſe of is that which is contained between the meridian, ſuppoſed to lie in the plane of the orifice and the direction of the ſtream, which muſt be the ſhip's true line of motion, and conſequently this is the true courſe, without addition or deduction for lee-way.

If the moveable orifice at the extremity of the immerſed tube were removed, it would ſimplify and improve the inſtrument much; the error which it is intended to correct, is that occaſioned by the deviations of the plane of the orifice from perpendicularity, which when moving in an inclined poſition receives an undue degree of preſſure, and miſrepreſents the departure; in this ſituation the preſſure on the orifice is diminiſhed as the ſquare of the ſine of inclination; therefore, if the ſpouting aperture were to be increaſed in area as the ſine of inclination is diminiſhed, the error would be exactly compenſated, becauſe the ſection of the ſpouting ſtream being then inverſely as its velocity, the quantity of water delivered muſt remain the ſame, which may perhaps be effected in the following manner: let a ſliding cover be joined to the ſpouting aperture, which may increaſe or diminiſh it by being moved up or down, and if the aperture be of ſuch a form that its area may be enlarged in the ratio of the elevation of this ſlide, a pendulum moving ſteadily in the plane of the two parts of the tube, may be, ſo connected with the ſlide as to give it a motion proportionate to the ſine of inclination, which would have the deſired effect; but the difficulty in this caſe would be to give the pendulum a ſlow deliberate motion: however, if its vibrations were to be regulated by the reſiſtance of air contained in a compreſſible caſe, with ſmall holes for its eſcape and return, all ſudden vacillations would be checked thereby, and the pendulum would obey gradually every motion of the ſhip which might require this correction.

If you think that this Paper merits inſertion in your Journal, it is at your diſpoſal.

I am, SIR,
Your much obliged,
humble Servant,

JOHN COOKE.

VI.

Report made to the Claſs of Phyſical and Mathematical Sciences of the Inſtitute of France, by Citizens Guyton *and* Vauquelin, *on a Memoir of Citizen Thenard, entitled, "Obſervations on the Combination of the Tartareous Acid, with the Saliſiable Baſes, and the Properties of the Salts thus produced."* *

It has long been known that the tartareous acid has a ſtrong tendency to form triple combinations; among which the ſalt of Seignette, martial tartar, tartar emetic, and various ſalts of this nature uſed in medicine are inſtances; but Citizen Thenard has greatly added to the number of theſe combinations, by others which have hitherto been unknown.

Triple Salts, Alcaline and Earthy.

The author was led to theſe diſcoveries by mixing ſolutions of lime, barites, and ſtrontian, with the ſolution of the acidulous tartrite of pot-aſh. He remarks, that the firſt portions of either of theſe earths produced no precipitation in the ſolution of tartrite of pot-aſh, and that no change is ſeen in the fluid till the moment when the exceſs of the acid of the cream of tartar approaches its term of ſaturation; and that if this point be not exceeded, there will be formed on the ſides of the veſſel tranſparent cryſtals, in which analyſis has ſhewn him the preſence of tartareous acid, pot-aſh, and the earth in queſtion.

This firſt obſervation led him to preſume, that the tartareous acid may be ſuſceptible of many other combinations of this kind, and the experiments we are about to relate were the conſequences.

1. A combination is formed between the tartrite of pot-aſh and that of lime, from which reſults a ſalt with a double baſe ſoluble in water. This fact explains why no precipitate is afforded, when lime water is added to the tartrite of pot-aſh, unleſs the quantity be ſuch as to exceed what is neceſſary to form the triple ſalt.

This ſalt may be obtained in the cryſtalline form, if at the moment when the precipitation begins to manifeſt itſelf the fluid be left to repoſe. A ſhort time afterwards it becomes clear, and depoſits on the ſurface of the glaſs cryſtals, of which Citizen Thenard was not able to determine the figure.

2. Analogous combinations take place between the tartrite of pot-aſh, barites, and ſtrontian, each taken ſeparately. This is ſhewn by the limpidity which the fluid preſerves, ſo long as the maſs of the earths added does not exceed certain limits. Now as the author remarks, this phænomenon cannot take place but by the effect of a triple combination, becauſe the ſimple and neutral union of the tartareous acid with either of theſe earths, falls down as ſoon as it is formed.

3. The tartrite of magneſia has likewiſe a certain affinity with the tartrite of pot-aſh, by virtue of which they contract an intimate union. The ſalt reſulting does not cryſtallize; it

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* Annales de Chimie, XXXVIII. 30.

it attracts the humidity of the air and becomes viſcid by heat. Pot-aſh ſeparates the magneſia, which proves contrary to the opinion of Bergmann, that this alkali has a ſtronger attraction for the tartareous acid, than for that earth. It is more eaſily prepared by heating magneſia, recently precipitated from its ſolution, by a cauſtic alkali and ſtill in the humid ſtate, with the ſolution of cream of tartar.

4. From the union of alumen with the acidulous tartrite of pot-aſh, a ſaline ſubſtance reſults which is not cryſtallizable, nor precipitated by the alkalies, whether pure or carbonated. To aſcertain the cauſe of this ſingular effect, Citizen Thenard combined, by means of a gentle heat, gelatinous alumen to tartrite of pot-aſh, and though a conſiderable quantity of the earth was diſſolved, there was not any trace of pot-aſh having ſeparated.

This experiment ſhews the reaſon why pot aſh does not occaſion a depoſition in the tartrite of alumen in combination with pot-aſh, namely, that in this caſe the vegetable ſalt which is formed retains in ſolution the alumen ſeparated from the tartareous acid.

The ſalt of Seignette preſented the ſame properties.

5. He alſo mentions the ammoniacal tartrite of pot-aſh, which cryſtallizes in fine tranſparent cryſtals, decompoſable after a time by expoſure to the air; this ſalt is already known.

Concerning the Alkaline, Earthy, and Metallic Acidulous Tartrites.

Citizen Thenard in this ſection ſhews, that the tartrites of ſoda, of magneſia, and of copper, unite with a ſuper-abundance of their acid, and by that means produce acidulous ſalts, leſs ſoluble than the neutral tartrites. This property was before obſerved in the tartrites of pot-aſh, of ſoda, and of ammonia.

If to a ſolution of tartrite of copper, which is very ſoluble, and difficultly cryſtallizable, there be poured a ſufficient quantity of pure tartareous acid, a new combination is effected of ſparing ſolubility, and of which almoſt the whole falls down under the form of a white blueiſh powder, which ſoda, pot-aſh, and the carbonate of pot-aſh diſſolve completely without any ſeparation of the copper.

The water in which the acidulous tartrite of copper has been precipitated, does not become blue by ammonia, which proves that no part of the ſalt has remained ſuſpended, and conſequently that it is of very ſparing ſolubility.

Order of Attractions of Earthy and Alkaline Subſtances, with Regard to the Tartareous Acid.

After having compoſed the triple ſalts here ſpoken of, and exhibited their principal properties, Citizen Thenard determines by comparative experiments, the degrees of affinity which the alkalies and the earths have for the tartareous acid. According to him lime muſt occupy the firſt rank, and barites the ſecond, and the others in the following order, ſtrontian, pot-aſh, ſoda, ammonia, magneſia, and alumen.

Metallic Alkaline Tartrites.

Citizen Thenard then paſſes to the examination of the metallic alkaline tartrites, among which he principally diſtinguiſhes thoſe of mangáneſe, zinc, tin, copper, lead, mercury, and

and ſilver. In order to prepare them he makes uſe of different methods, according to the nature of the metal. Thoſe which have the property of decompoſing water, can be employed in the metallic ſtate, others in the ſtate of oxide, and ſome diſſolved in the acids.

Zinc and iron are in the firſt claſs—copper, tin, and lead in the ſecond—mercury and ſilver in the third.

The alkaline tartrites of manganeſe, of iron, of zinc, and of tin, are very ſoluble, and difficultly cryſtallizable. Their taſte participates more or leſs of the metallic oxides which they contain. None of them is decompoſed, or at leaſt precipitated, either by cauſtic alkali, or the alkaline carbonates; but they are all decompoſed by ſulphurated hidrogen, hidro-ſulphuret, and the gallic acid. There is however an exception with regard to the ſalt of manganeſe and ſulphurated hidrogen.

The alkaline tartrite of copper is diſtinguiſhed from the other tartrites by its ſaccharine taſte, and the great quantity of metal it contains. That of lead is not ſoluble. It is not decompoſed by the alkalis, and what is remarkable, it is not decompoſible even by the ſulphates.

The tartrites of pot-aſh and of ſilver, of pot-aſh, and of mercury, do not follow the general rule; they are decompoſed by the alkalies, the alkaline carbonates, ſulphurated hidrogen, the hidro-ſulphurates, the ſulphates, and the muriates.

Though tartar emetic, or antimoniated tartarite of pot-aſh, has long been a ſubject of inquiry by many chemiſts, and a number of intereſting properties have been diſcovered in conſequence, Citizen Thenard directed by new ideas, has made ſome additional diſcoveries. He ſhews for example, that a ſolution of this ſalt, which is decompoſed and precipitated by the alkalies and the carbonates, ceaſes to be decompoſed if a ſufficient quantity of tartrite of pot-aſh be added, in order that this ſalt may retain the oxide of antimony in ſolution.

It likewiſe entered into the plan of this chemiſt, to ſubmit the tartar emetic to a more accurate analyſis than has hitherto been made, and this analyſis has required that of various other ſalts, particularly the nitrate of pot-aſh or ſalt-petre, the proportions of the elements of which eſſentially intereſt many of the arts.

In order to obtain this reſult, he expoſed 100 parts of cryſtalliſed tranſparent emetic tartar to a gentle heat, in order to drive off merely the water of cryſtallization. It was reduced by this operation to 92 parts. After having diſſolved theſe 92 parts in water, he precipitated them by ſulphurated hidrogen, which afforded 50 parts of golden ſulphuret, in which there are, according to other experiments before made by him, 38 parts of oxide of antimony, ſuch as it exiſts in the emetic tartar. In order to aſcertain the quantity of tartareous acid, he mixed the acetite of lead with ſuperabundance of acid, in the liquor ſeparated from the golden ſulphur, and he obtained one hundred parts of tartrite of lead, which contain, according to his analyſis, 66 parts of oxide of lead, and 34 of tartareous acid.

Having formed by theſe ſimple methods the proportions of water and tartareous acid, and of oxide of antimony in emetic tartar, there remained nothing more for Citizen Thenard to determine than the proportion of pot-aſh, which would have been eaſy for him to have come at by ſubtracting the quantities already known from the ſum total employed; but he choſe rather to have recourſe to experiment, and by that means to verify his former operations. He therefore took 100 new parts of emetic, calcined them in a crucible till the tartareous acid muſt have been decompoſed, and afterwards treated the reſidue with weak nitric acid. He preferred this acid to all the others, becauſe it does not diſſolve the oxide of antimony, and the ley afforded him by evaporation, 30 parts of nitrate of pot-aſh. He might have deduced the quantity of pot-aſh contained in theſe 30 parts of nitre from the analyſis given by many chemiſts, but the reſults of their analyſis being very different, he could not know which to depend on. He therefore choſe to reſume this operation himſelf, and he found that this ſalt is formed of 53 parts of pot-aſh, and 47 of nitric acid, and accordingly that 30 parts of nitre contained 16 parts of pot-aſh. Whence we may eſtabliſh the following proportions for 100 parts of emetic; namely, antimony 38, tartareous acid 34, pot-aſh 16, water 8, and loſs 4.

The acidulous tartrite of pot-aſh being formed according to the analyſis made by Citizen Thenard of 57 parts of tartareous acid, 33 of pot-aſh, and 7 of water, and the tartareous acid dividing itſelf in the fabrication of tartar emetic, between the pot-aſh and the oxide of antimony, we may conclude, that 100 parts of tartar emetic contained 34 parts of tartrite of pot-aſh, 54 of tartrite of antimony, and 8 of water, becauſe 100 parts of neutral tartrite of pot-aſh contained 48 parts of acid, 43 of pot-aſh, and 7 of water.

Citizen Thenard having obtained by analyſis 57 parts of tartareous acid, 33 of pot-aſh, and 7 of water from the acidulous tartrite of pot-aſh; and having compared the proportions of the tartrite of pot-aſh and antimony which conſtitute emetic tartar, he found that there is in cream of tartar more tartrite of pot-aſh than is neceſſary to ſaturate the tartrite of antimony; that this exceſs of ſalt remains in the mother water of emetic tartar, and that in order to obtain this laſt ſalt in a ſtate of purity, the cryſtals obtained from the firſt evaporation only muſt be collected.

CONCLUSION.

The experiments contained in the memoir, of which the Commiſſaries have given this account are numerous, intereſting, and made as to them appears, with all the accuracy which chemical methods are ſuſceptible of. They prove that moſt of the tartrites can combine with each other, and form triple ſalts which have particular properties. There are ſome in fact which have for their baſis two alkalies, others an alkali and an earth, others an alkali and a metal, and others an earth and a metal. What is remarkable is, that moſt of theſe baſes which are ſeparated by the alkalis from their ſimple combinations with the tartareous acid, are not ſeparated when in the ſtate of triple ſalt.

Beſides

Beſides theſe intereſting facts, this memoir contains other analyſes which are very well made, of ſubſtances of conſequence to ſeveral arts and to medicine, as to the accuracy of their proportions.

The commiſſaries are therefore of opinion that the claſs ſhould order it to be printed in the volumes of the Savans Etrangers; and that Citizen Thenard would render a great ſervice to chemiſtry by continuing to employ himſelf on the ſame plan upon theſe complex combinations of which different natural bodies are ſuſceptible; this important object having been hitherto too much neglected by chemiſts.

VII.

On the Production of artificial Cold by Means of Muriate of Lime. By Mr. RICHARD WALKER. *(Philoſ. Tranſ. 1801.)*

(Concluded from page 227.)

PLATE XII. Fig. 1, repreſents the ſection of an apparatus, conſiſting of two veſſels; viz. AA, is a veſſel 4 inches in diameter, and 4 inches high, (omitting its ſtand, by which it reſts over another veſſel, preſently to be deſcribed,) having a tube (of one piece with it) *a*, $\frac{7}{8}$ and $\frac{1}{2}$ in diameter, and 4 inches deep. This tube has a horizontal rim or ſhoulder, at $\frac{2}{8}$ of an inch from the top,* and is open at both ends; the lower one being cloſed occaſionally, in the manner hereafter to be deſcribed; and the top of the tube, when the apparatus is of glaſs, by a ſtopple; or, if conſtructed of tin, by a ſliding cover, fixed on the lid C.

C, is the cover or lid of this veſſel, fitting over it quite cloſe, and having a collar to fit over the tube, likewiſe, down to its ſhoulder.

B, repreſents a ſecond veſſel upon which the former fits cloſely, but not tight. This veſſel is 3 inches and $\frac{6}{8}$ in height, having a conical cup (of one piece with it) *b*, 1 inch and $\frac{1}{8}$ diameter at the top, and 1 inch in diameter at the bottom, and three inches in depth: this cup is incloſed, at the diſtance of $\frac{1}{8}$ of an inch, by a thin partition *c c*, † of one piece likewiſe with the veſſel.

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* This veſſel, when uſed, is to be filled up to this rim or ſhoulder *only*; that ſerving as a guard to prevent the frigorific mixture from getting into the tube: hence, the capacity of the veſſel ending here, its height, and that of the tube, may be conſidered as 3 inches and $\frac{6}{8}$.

† Having aſcertained by experiment, that a ſtratum of air of this thickneſs did not prevent (during the length of time which is required to freeze the water, and reduce the ice to powder in the tube *a*) the materials in the cup *b* from receiving the temperature required, yet was nevertheleſs ſufficient to impede the action of the mixture on the materials, when mixed in the cup *b*, during the *ſhort time* required to take its temperature, or to freeze the quickſilver, I adopted this method, in preference to letting out the frigorific mixture from the veſſel B, immediately before mixing the laſt materials, as formerly.

D, is the cover or lid of this veſſel, fitting over it water-tight,* forming a bottom to the veſſel, and having a rim $\frac{2}{8}$ of an inch deep, as a ſtand to inſulate it from the table; the whole apparatus appearing to form, when together, one cylindrical veſſel, 8 inches high, and 4 inches wide.† The veſſels A and B contain each 1 pint and $\frac{1}{4}$; and the tube *a*, and cup *b*, 1 ounce and $\frac{1}{2}$ each.

N. B. The drawing, with the ſcale annexed, gives the ſection of this apparatus, of exactly the dimenſions mentioned. The inſtrument deſcribed in Phil. Tranſ. for 1795, page 288, to be uſed with this apparatus, ſhould be ſo long as juſt to paſs through the bottom of the tube *a*, viz. $\frac{2}{8}$ of an inch below it.

Fig. 2, repreſents the *ſpirit thermometer* made uſe of in the experiments: it conſiſts of three parts, viz. A is the thermometer, having its ſcale-board (made of box-wood) of a *ſemi-cylindrical* form, being flat in front, and round at the back, in order that it may be adapted to the cylindrical tube B B, in which it ſlides eaſily up and down, centrally; and may be occaſionally taken out of it C, is a braſs ferrule, cemented to, and forming one piece with, the tube, having a top or cover, which ſcrews off and on. The ſcale extends from 100 below 0, to 100 above 0; the ſcale upwards being carried ſo far only as to allow of the unavoidable expanſion to which the ſpirit may be ſubjected by atmoſpheric heat.

The thermometer (contrived by myſelf, and very accurately and neatly executed by Mr. Nairne, philoſophical inſtrument-maker, in London,) is exactly *twice* the dimenſions of the repreſentation in the drawing, (as the ſcale,) and is graduated to ſingle degrees; it has a common caſe, to make it portable.

N. B. A thermometer of the ſize deſcribed, is equally fit for an apparatus on a larger or ſmaller ſcale than that repreſented in Fig. 1.

The apparatus is uſed thus. The two veſſels being taken apart, 1ſt. A circular piece of writing-paper is cemented‡ over the bottom of the tube *a*. 2d. A frigorific mixture § is made

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* If this cover does not fit water tight, it may be made ſo, by the intervention of a thin bladder previouſly ſoaked in warm water.

† For the purpoſe of aſcertaining the proper proportion of the materials to be mixed at different temperatures, and other preparatory matters, I uſed an apparatus of the ſame conſtruction as Fig. 3, Tab. XXIII. Phil. Tranſ. for 1795, but differing in having two cubes inſtead of one. The dimenſions of this apparatus, being adapted to the ſame ſcale as the former, are thus: the veſſel is $5\frac{1}{2}$ inches high, and $4\frac{1}{2}$ inches in diameter; the tubes are each $\frac{2}{8}$ of an inch in diameter, and 5 inches deep. The materials, being prepared ſeparately in this veſſel, were afterwards mixed in a wine-glaſs.

By means of this apparatus, in one inſtance, muriate of lime put into one tube, in a liquid ſtate, and water in the other, were both conſolidated by cold, then ground to powder, and afterwards mixed; but the ſalt did not grind well, and it moreover eroded and ruſted the inſtrument.

‡ I uſe mucilage of gum arabic, or butter, if the other be not at hand; but it is ſufficient merely to dip the paper in water, and apply it; the effect of the freezing mixture quickly cementing it.

§ A frigorific mixture, according to the intention, may be ſelected from the table in page 135 of this paper; or, in defect of ice, from the table in page 279 of Phil. Tranſ. for 1795. The mixture I uſe for this made in the veſſel B, and the veſſel covered by its lid D, then ſet upright, and four drams of muriate of lime put into the cup *b*. 3d. A ſimilar frigorific mixture is made in the veſſel A A, which is cloſed with its lid C. 4th. This veſſel being placed over the other, as repreſented, three drams of water are to be poured gently, through a funnel, into the tube *a*, and the aperture cloſed. 5th. When the water is become perfectly ſolid ice,* the grinding inſtrument is to be put in, and, after ſuffering it to remain a ſhort time to be cooled, the ice is to be ground gently to fine powder, (an aſſiſtant holding the apparatus firm,) and the inſtrument continued quite through the aperture at the bottom of the tube. 6th. The whole of the ice-powder is then to be forced into the cup of the lower veſſel, and ſtirred about in it a little. 7th. The upper veſſel being removed, and ſet aſide, the muriate of lime and ice are to be thoroughly mixed, and a ſmall tube, containing the quickſilver to be frozen, ſtirred about in the mixture; or the bulb of a ſpirit-thermometer, to take its temperature, which, if the experiment be conducted properly, will indicate, even if the experiment be made in ſummer, a cold of —50°. †

The apparatus, as repreſented in the drawing, is upon as fit a ſcale as may be required for common experiments; by attention, however, to the proportions given, one of any ſize may be procured.

this purpoſe, is that conſiſting of the ſolution of three different ſalts in water; and, in order to aſcertain what proportions may be neceſſary to fill any ſized veſſel, I ſhall give the proportional quantities for a veſſel containing in meaſure a *wine pint*, which are as follows: of muriate of ammonia 3 ounces, nitrate of potaſh 3 ounces, ſulphate of ſoda 4 ounces and ½, and water 10 ounces: having procured the ſalts ſeparately in fine powder, I put theſe firſt into the veſſel, and then fill up (without meaſuring) the veſſel with water.

A mixture of this kind, made in the ſummer, when the temperature of the air is + 70°, will cool the materials to + 20°; and, if the ſalts and water are cooled to near + 50°, previouſly to mixing, by immerſion in cold water, to + 10°. My uſual method is, (without taking the uſual precaution of cooling the ſalts,) to add the water much reduced in temperature, by pumping off a bucket or more firſt, by which the materials, conſiſting of muriate of lime and ice, are cooled to + 15° before mixing.

In winter, the experiment may be conducted by adding ſnow, at the temperature of the air, to muriate of lime, (cooled to a low temperature,) in the veſſel B. In ſummer, by adding ice-powder, cooled to a low temperature, in the veſſel A, to muriate of lime cooled to + 50°, by water, (inſtead of a frigorific mixture,) in the veſſel B.

The temperature of the ſprings, or of well-water, it is well known, is in this climate nearly + 50° throughout the year.

* At this period, I ſhake the apparatus, in order to expedite the ſolution of the ſalts, and to diffuſe the effects of the freezing mixtures; or, if neceſſary, *renew* them.

† Muriate of lime and ice-powder, mixed at the temperature of + 20°, give a cold of — 48°; if mixed at + 15, of — 51°; and, at + 10°, a cold of — 54°.

The freezing point of quickſilver is — 39°; but that metal requires a temperature of — 45°, to aſſume its perfectly ſolid ſtate.

I have repeatedly frozen quickſilver in the middle of ſummer, by mixing together muriate of lime and ice-powder, at + 20°; and likewiſe, by mixing together nitrous acid and ice-powder, at + 8°.

Muriate of lime produces no effect upon tin or japanned veſſels; hence the apparatus is beſt made of common block-tin; obſerving that the tube and cup be made of the *thinneſt* tinned iron, and the whole beſides of the ſame ſubſtance, but conſiderably *thicker*. *

The tube and cup ſhould be very ſmooth within-ſide, and perfectly central; the tube having as little ſeam as poſſible, that the grinding inſtrument be not obſtructed.

The grinding inſtrument acts beſt when the edge, inſtead of being quite horizontal, is a little inclined from each ſhoulder, towards the centre.

In order to keep this paper within tolerable limits, I have carefully avoided a repetition of all matters mentioned in my former papers on this ſubject; I muſt therefore refer to thoſe, eſpecially that "On the beſt Methods of producing artificial Cold," † for the particular mode of conducting experiments on cold; this being eſſentially the ſame in principle, whatever be the materials made uſe of to effect it. Hence, the apparatus juſt deſcribed is applicable to the uſe of the *mineral acids*, ‡ as well as to that of *muriate of lime*; recollecting that it is neceſſary to ſubſtitute glaſs for tin, when the former are uſed; or to give the inſide of the cup, or veſſel containing it, a coating of wax, to defend the tin from their action.

Having given an account of Pr. Lowit's experiments on the power of muriate of lime for producing artificial cold, and added ſuch obſervations of my own as reſulted from them, I ſhall conclude by exhibiting a general view of the different frigorific mixtures compoſed of chemical ſubſtances with *ice*, as I have before done, (Phil. Tranſ. for 1795, page 279,) of thoſe in which the uſe of ice is diſpenſed with.

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* The beſt method of conſtructing veſſels for the purpoſe of excluding heat, is obviouſly to have them made of the beſt non-conducting ſubſtance, lined within with the beſt conducting ſubſtance; hence theſe veſſels (the tube excepted) might be fitter for the purpoſe, if made of wood lined with tin.

My general rule for conſtructing the apparatus, is to allow *four* times, by meaſure, the water to be frozen and reduced to powder, in the capacity of the tube that is to contain it; and *three* times the weight (by meaſure) of the muriate of lime, to the cup in which the muriate of lime is to be cooled, and the ice-powder afterwards added to it: and, when nitrous acid is uſed, (inſtead of muriate of lime,) *four* times its weight; and about four times the diameter of the veſſels to that of the tube.

The preparatory mixtures, that is, ſuch as are uſed for cooling the materials previouſly to mixing, are beſt made of *ice* and ſalts; theſe retaining their temperature longer than thoſe conſiſting of ſolutions of ſalts in water or acid; but, in either caſe, if neceſſary, they may be occaſionally renewed, after the water is become ſolid.

† Phil. Tranſ. for 1795, p. 270.

‡ Nitrous acid, and vitriolic acid, may at any time be immediately procured from a chemiſt's ſhop; whereas the muriate of lime, not being uſed for any other purpoſe, muſt commonly be prepared for this *alone*; hence it may not unfrequently happen that the former, on this account, may be preferred.

CLASS I.

Acids and ſalts.	Ice.	Temp. of mat. before mixing	Temp. or cold. produced
Muriate of ſoda 1,	Snow 2,	- -	—5°
——— ——— 2, Muriate of ammonia 1,	——— 5,	- -	—12°
——— ——— 10, ——— ——— 5, Nitrate of pot-aſh 5,	——— 24,	- -	—18°
——— ——— 5, Nitrate of ammonia 5,	——— 12,	- -	—25°

CLASS II.

Diluted vitriolic acid 2,*	Snow † 3,	+32°	—23°
Concentrated muriatic acid 5,	——— 8,	+32°	—27°
Concentrated nitrous acid 4,‡	——— 7,	+32°	—30°
Muriate of lime 5, ‖	——— 4,	+32°	—40°
——— ——— 3,**	——— 2,	+32°	—50°
Cauſtic vegetable alkali 4,	——— 3,	+32°	—51°

The above table is divided into two claſſes. The firſt claſs conſiſts of mixtures of ſalts and ice, in which the temperature of mixing is of no conſequence, the effect produced being the ſame at any temperature of the air: the ſalts ſhould be in the ſtate of powder. Ice pounded ſmall may be ſubſtituted, with equal effect, for ſnow.

The ſecond claſs conſiſts of ſuch mixtures as will produce an effect *greater*, the colder the temperature is at which the materials are mixed, but in a *diminiſhing* ratio; ceaſing entirely at that degree of cold at which the compoſition itſelf freezes.†† The ſalts ſhould be in the ſtate of fine powder.

N. B. The figures after the ſalts, or acids, and ice, expreſſes the proportions, *by weight*, to be uſed.

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[*] Concentrated vitriolic acid, diluted with half its weight of ſnow, or diſtilled water, and cooled.

[†] Snow that is freſh, dry, and uncompreſſed, or ſuch as has never been ſubject to the effects of a temperature leſs than freezing; or, when ſuch is not to be procured, ice reduced to powder, in the manner deſcribed in Phil. Tranſ. for 1795, p. 271, may be ſubſtituted in its ſtead, with equal effect.

[‡] Concentrated *fuming* nitrous acid alone; or concentrated *pale* nitrous acid, diluted with one-fifth its weight of ſnow, or diſtilled water, and cooled.

[‖] Of the ſtrength of 1,490, at 80° of heat.

[**] Of the ſtrength of 1,450, at 80° of heat.

[††] The materials may be cooled, previouſly to mixing, when required, by a frigorific mixture taken from the table: for this purpoſe, either of the mixtures in Claſs I. are convenient; particularly the firſt, conſiſting of muriate of ſoda and ſnow.

In

In the above table, the ordinary effect of ſnow, or ice-powder, is given; but, if the latter be prepared (ground) with a ſharp inſtrument, uſing light preſſure, the effect will be ſomewhat greater, the ice being then reduced to an impalpable powder: hence, ice powder, thus obtained from a block of ice, may at any time be ſubſtituted for ſnow.

Cold is produced by mixing various other chemical ſubſtances with ice: in the above table, ſuch only are retained as produce that effect in a remarkable degree.

As the new nomenclature is now generally adopted, I have uſed it in this paper.

POSTSRIPT.

In the courſe of my former papers on the ſubject of cold, I have had occaſion to make, incidentally, ſome remarks on the power of water, under certain circumſtances, in reſiſting an extraordinary degree of cold without freezing; likewiſe on the particular kind of agitation which induces water, cooled below its freezing point, to cryſtallize or become ice. † As theſe are ſubjects which have likewiſe engaged the attention of others, I ſhall take the liberty of barely mentioning a fact, having relation to thoſe points, which has lately occurred to me.

It is a remarkable circumſtance reſpecting artificial freezing, that the ice thus procured in the uſual way, (viz. by immerſing the water to be frozen, in a convenient veſſel, in a frigorific mixture,) will always be more or leſs opake, never tranſparent: this I had conſtantly remarked, without much attending to it; however, having in the courſe of laſt ſummer been induced to try the effect of an ice-ſpeculum for producing heat, it became neceſſary that the ice, which in this inſtance was ſubſtituted for glaſs, ſhould be perfectly tranſparent. After varying the proceſs in every poſſible way I could think of, by *immerſing the water* to be frozen, without effect, I at laſt ſucceeded completely, by forming a coating of ice, of ſufficient thickneſs, on the outſide of a veſſel containing the frigorific mixture; the bottom of this veſſel, which was made concave for this particular purpoſe, being immerſed for a ſufficient length of time in a ſhallow pan of water.

Hence ariſes the means, before unknown to me, of obtaining ice, either in an opake, or perfectly tranſparent ſtate; moreover, water, as I have experienced lately, conſtantly forms a coating of ice on the outſide of a veſſel containing a frigorific mixture, ſo ſoon as it is cooled to + 32°.

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[*] Phil. Tranſ. for 1788, page 401.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Philoſophical Tranſactions of the Royal Society of London for the Year 1801. *Part the Firſt. Quarto,* 240 *Pages, with fifteen Plates, and a Meteorological Journal.* London, ſold by Elmſly.

THIS part contains;—1. The Croonian Lecture. On the Irritability of Nerves, by Everard Home, Eſq. F. R. S. 2. The Bakerian Lecture. On the Mechaniſm of the Eye, by Thomas Young, M. D. F. R. S. (Philoſ. Journal V. 253.) 3. On the neceſſary Truth of certain Concluſions obtained by means of imaginary Quantities. By Robert Woodhouſe, A. M. Fellow of Caius College. 4. On the Production of artificial Cold, by means of Muriate of Lime. By Mr. Richard Walker. (See Phil. Journal V. 222, 272.) 5. Account of a monſtrous Lamb, by Mr. Anthony Carliſle. 6. An Anatomical Deſcription of a male Rhinoceros. By Mr. H. Leigh Thomas, Surgeon. 7. Demonſtration of a Theorem, by which ſuch Portions of the Solidity of a Sphere are aſſigned as admit an Algebraic Expreſſion. By Robert Woodhouſe, A. M. Fellow of Caius College, Cambridge. 8. Account of the Diſcovery of Silver in Herland Copper Mine. By the Rev. Malachi Hitchins. 9. Account of an Elephant's Tuſk, in which the Iron Head of a Spear was found imbedded. By Mr. Charles Combe. 10. Deſcription of the Arſeniates of Copper and of Iron from the County of Cornwall. By the Count de Bournon. 11. Analyſis of the Arſeniates of Copper and of Iron, deſcribed in the preceding Paper; likewiſe an Analyſis of the red octahedral Copper Ore of Cornwall; with Remarks on ſome particular Modes of Analyſis. By Richard Chenevix, Eſq. M. R. I. A. Appendix. Meteorological Journal kept at the Apartments of the Royal Society, by order of the Preſident and Council.

NATIONAL INSTITUTE OF FRANCE.

Extract from the Notice of the Works of the Claſs of Mathematical and Phyſical Sciences, of the National Inſtitute of France during the third Trimeſtre of the Ninth Year.

Natural Philoſophy.—New Galvanic Experiments.—The combuſtion of metals by this power as related at page 239 of our laſt Number, was diſcovered by Fourcroy, Vauquelin, and Thenard, and not by Fourcroy alone as there ſtated.

Chemiſtry.—On the Reduction of the Oxides of Zinc by Charcoal.—The newly diſcovered combination of oxigen with carbon in the form of an oxide, upon which Mr. Cruickſhank has publiſhed ſuch an excellent courſe of experiments in this Journal, has likewiſe been the ſubject of attention with Dr. Woodhouſe of America, and the French chemiſts Deſormes, Berthollet, Clement, Guyton, Fourcroy, Thenard, Haſſenfratz. As this ſubjeſt will form the baſis of more ample communication and reſearch, I ſhall not here attempt to give any more conciſe abridgement of the communications to the Inſtitute.

Mineralogy.—Concerning arſeniated Copper in Plates. This mineral from Cornwall, mentioned by ſome German chemiſts, but little known in France, has been deſcribed by Lelievre, and analyſed by Vauquelin. It cryſtallizes in brilliant hexagonal plates, tranſparent, olive-green, which decrepitate in the flame of a candle, to which they give a green colour: it is very difficultly fuſible by the blow-pipe, emits a ſmell of garlic, colours borax green with reddiſh zones, and is ſoluble with effloreſcence in the nitric acid, to which it gives a ſlight green colour.

Geology.—On the Eruption of Mount Veſuvius in the ſecond Republican Year.—One of the moſt important points to be determined in the hiſtory of volcanos, is the degree of heat neceſſary to give fluidity to the lavas. Is this fire ſimilar in intenſity to that which produces glaſs? or is the fluidity owing to ſome other cauſe? Dolomieu has been long employed on this queſtion. The eruption of Veſuvius in the ſecond year afforded him the means of aſcertaining this degree of heat almoſt thermometrically. He found that this heat is not greater than fuſes ſilver, but leſs than is required to fuſe copper. The metals capable of being oxided at a leſs heat than that which is required to fuſe them, have been oxided in the centre of the moſt voluminous maſſes. Lead was converted into teſſular galena with large ſcales, glaſs into Reaumur's porcelain, &c.

On the Belemnites.—Citizen Sage, who deſcribed ſeveral new ſpecies of this foſſil in the Journal de Phyſique, communicates in this article ſome others, which at that time he had not ſeen.

Meteorology.—Agreement between the Variations of the Atmoſphere over a great Extent of Country.—The labours of Citizen Lamark, to determine whether the variations of the atmoſphere be governed by any periodical law are well known. On the preſent occaſion he has endeavoured to aſcertain whether theſe variations extend to great diſtances. For without this condition, every method of predicting the atmoſpheric changes muſt undoubtedly be merely local; and different rules would be required, even for the greater variations, at each different place. His preſent inquiries are confined to the barometrical changes. He traced twenty-ſix parallel lines upon paper, to repreſent the ſtations or heights at which the barometrical column ſtands in our climate. Other perpendicular lines repreſent the days in ſucceſſion. By this ingenious contrivance (which was adopted very early in the laſt century) he was able to exhibit a curve denoting the barometrical variations for any given courſe of time. But the inquiries of Lamark demanded, that he ſhould trace in the ſame picture the lines for different places. One of theſe anſwers to the obſervatory at Paris; a ſecond to the reſidence of Citizen Pictot of Geneva; a third to that of C. Thulis at Marſeilles; and he found that theſe lines aſcend and deſcend generally together, and differ ſcarcely at all, but according to the height of the place of obſervation, or the extent of the variations, but not their direction.

Zoology.—

Zoology.—*On the monoculous Flea.*—By the vulgar name of water-flea is known a ſmall cruſtaceous animal, very abundant in ſtagnant waters, which has ſometimes given riſe to the ſtory of ſhowers of blood, becauſe in the ſpring its ova with which it is filled, give it a red colour, and the waters containing many of theſe animals do then in fact appear as if they contained blood. The moſt ſkilful naturaliſts, Swammerdam, De Geer, Schœffer, and Otto-Frederick Muller, have ſucceſſively ſtudied this animal, but nature is inexhauſtible in her ſmalleſt productions. Citizen Jurine, aſſociate of the Inſtitute of Geneva, has diſcovered in this inſect a number of curious circumſtances which had eſcaped thoſe learned men. He has deſcribed at large its internal and external organization, its ſexual intercourſe, &c. The moſt ſingular fact he has diſcovered is, that the female, which has received the male, tranſmits the influence to its female deſcendants, ſo that they all produce eggs, without recourſe to the male to the ſixth generation, after which their young periſh in the hatching. Another ſpecies has carried this influence of a ſingle copulation to the fifteenth generation. It is known that fleas have afforded ſimilar obſervations to Bennet. Theſe generations without conjunction are leſs abundant, and ſucceed each other leſs rapidly than thoſe in which the communication of the male has taken place.

Botany.—Deſcription of the new, or ſcarcely known plants in the garden of Citizen Cels, by C. Ventenat. Of the magnificent work here deſcribed, four livraiſons have already appeared, each containing ten plants.

Hiſtory of the Oaks of America.—It is ſomewhat ſingular, that the ſpecies of the oak which are ſo few in Europe, ſhould be ſo diverſified under the ſame latitude in America. C. Michaux has given a hiſtory of theſe numerous ſpecies, which is the more intereſting, as it is probable that ſome of them may be of much utility when tranſplanted into Europe.

Anatomy.—*On the Symphiſis of the Pubis.*—Citizen Tenon, who has long been occupied on this part of the anatomy of the human ſubject, has aſcertained that the os pubis does not ſeparate but in ſuch pulvis' as have a double cartilage, in which caſe the ligaments only are lengthened, and that this ſtructure is the leaſt frequent.

Technology.—New method of waſhing linen. For the account of this proceſs of Chaptal, *vide* our laſt Number, page 233.

Paper making.—C. Seguin, who has been five years employed on the art of manufacturing paper, has made ſeveral uſeful diſcoveries to expedite the progreſs. The manufacture of paper from ſtraw diſcovered ſo long ago in Germany, and lately carried into effect in this country, is among the improvements of C. Seguin. But he has not communicated any of his methods to the Inſtitute.

Philos Journal Vol.V Pl.XIII. facing p. 280

Fig. 1.

Fig. 2.

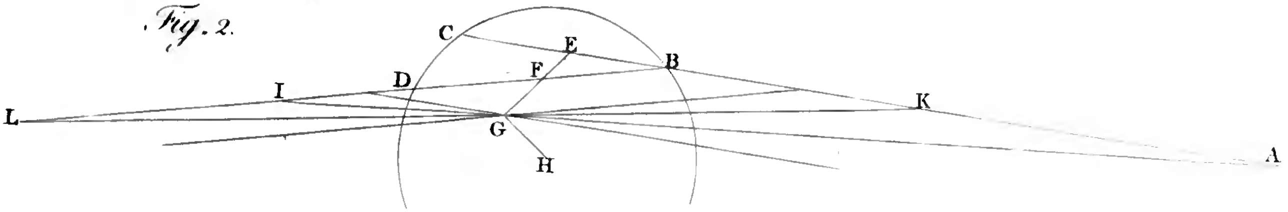

Fig. 3.

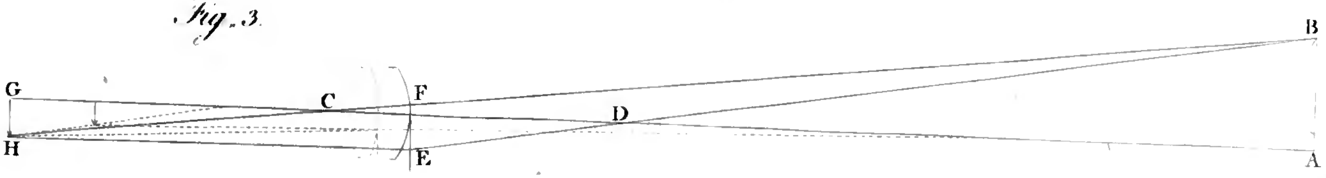

Fig. 4.

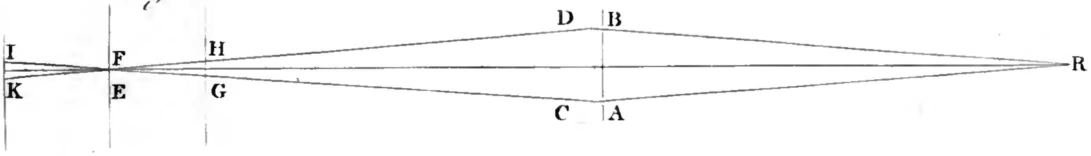

Fig. 5.

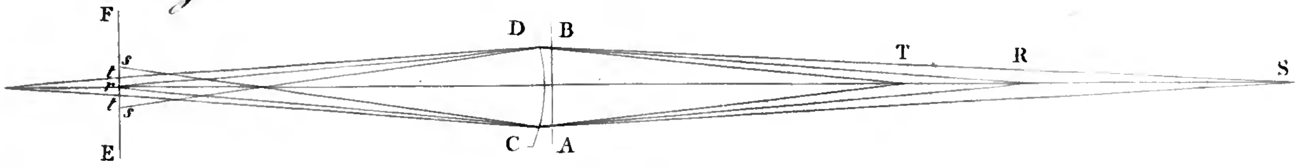

Fig. 6

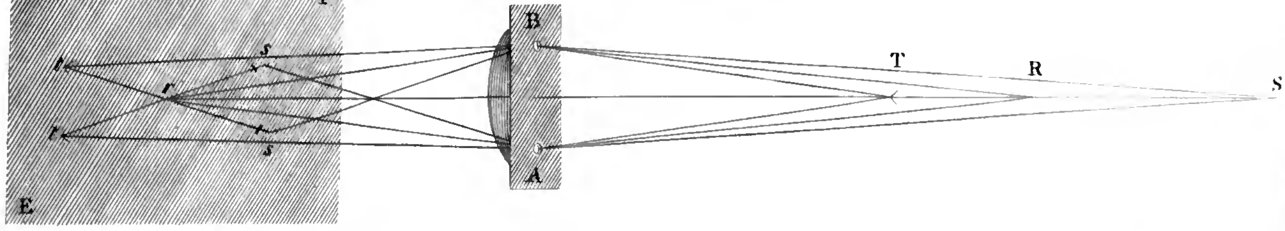

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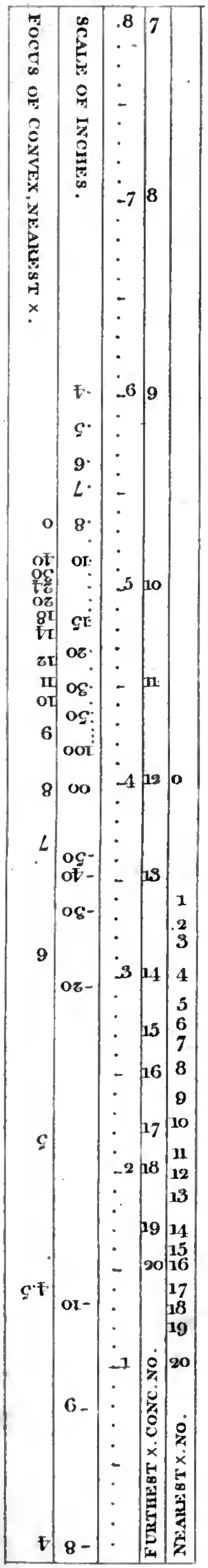
Fig. 8.



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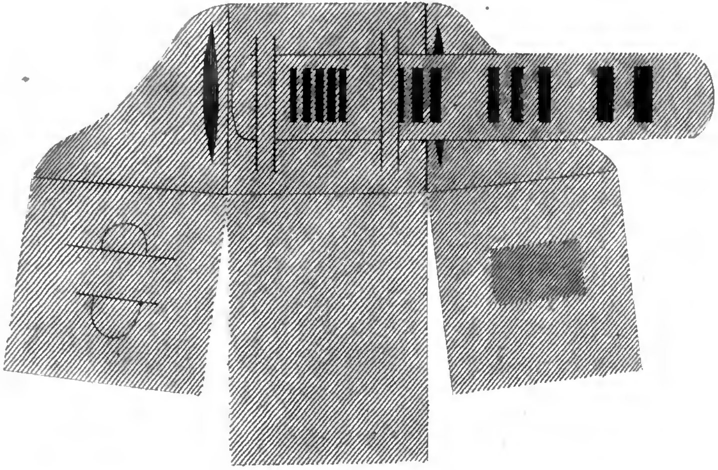


Fig. 11.

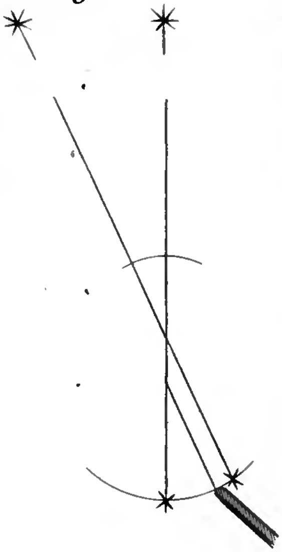


Fig. 9.

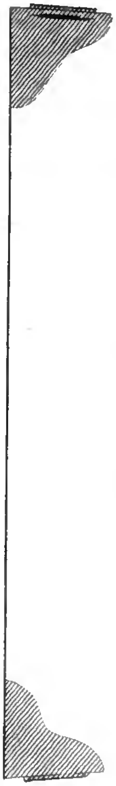


Fig. 10.

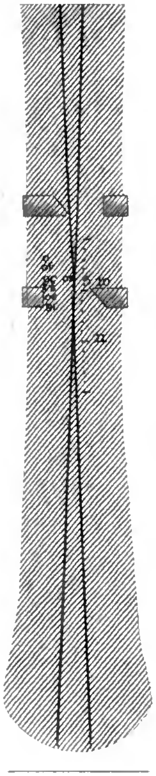


Fig. 12.

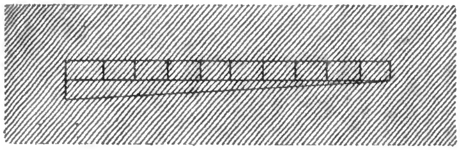


Fig. 13.

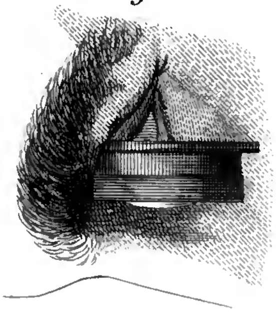


Fig. 14.

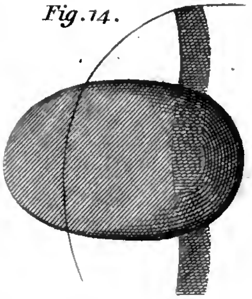
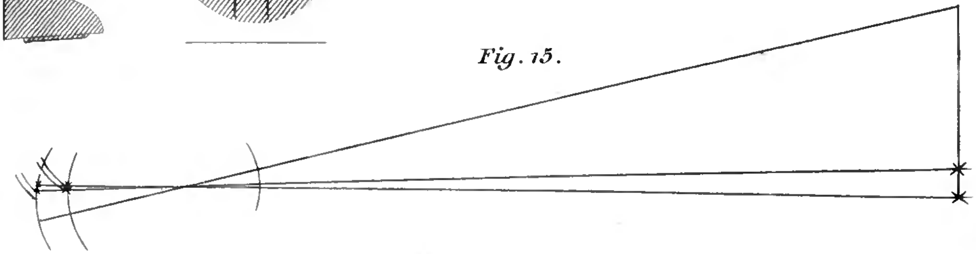


Fig. 15.



Mutlow Sc. Russell Court.

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

NOVEMBER, 1801.

ARTICLE I.

Outlines of Obſervations relating to Nitrous Oxide, or Dephlogiſticated Nitrous Air. By Mr. DAVY, *Director of the Chemical Laboratory, and Lecturer on Chemiſtry in the Royal Inſtitution.* *

I.

SECT. 1. The aëriform fluid, that conſtitutes the ſubject of the following obſervations, was called by its diſcoverer, Dr. Prieſtley, dephlogiſticated nitrous air. The Dutch chemiſts, who, ſome time ago, inveſtigated certain of its properties, named it gaſeous oxide of azote. Lately, in this country, it has been denominated nitrous oxide, which, according to the principles of the French Nomenclature, ſignifies nitrogen in the firſt degree of oxigenation. This term cannot well be confounded with nitrous gas; † and it bears the neceſſary analogy to words denoting other combinations of oxigen and nitrogen.

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* Extracted from "Reſearches, Chemical and Philoſophical, concerning Nitrous Oxide, 1800, Johnſon," with additions, and communicated by the author.

† Nitrous gas, according to the *ſtrict* principles of the French Nomenclature, ought to be called *nitric* oxide.

Sect. 2. A ſhort and general account of the production and properties of nitrous oxide will not, perhaps, be wholly unacceptable to the chemical world; particularly as the moſt remarkable agencies of this ſubſtance are but little known out of Great Britain; and they are connected with curious phyſiological enquiries.

II. *On the Production of Nitrous Oxide.*

Sect. 1. As yet nitrous oxide has not been compoſed immediately from its elements; and there is no reaſon to believe that it is produced in any of the operations of nature. But it may be procured in a number of artificial proceſſes, during the decompoſition of nitrous gas and nitric acid.

Sect. 2. Nitrous gas may be converted into nitrous oxide in two modes. Firſt, by the ſimple abſtraction of a portion of its oxigen by bodies poſſeſſing a ſtrong affinity for that principle, ſuch as alkaline ſulphites, muriate of tin, and ſolphurets. Secondly, by the combination of a body with portions both of its oxigen and nitrogen, ſuch as hidrogen, when in a peculiar ſtate of combination, or in a naſcent form.

a. The alkaline ſulphites convert nitrous gas into nitrous oxide with much greater rapidity than any other ſubſtances.

At temperature 46°, Fahrenheit, 16 cubic inches of nitrous gas were converted in leſs than an hour into 7,8 cubic inches of nitrous oxide, by 100 grains of pulverized ſulphite of pot-aſh containing its water of cryſtallization. During this proceſs, the ſulphite became partially converted into ſulphate; but no *ſenſible* increaſe of temperature was produced. No water was decompoſed, nor was any free nitrogen evolved.

Muriate of tin, and the ſulphurets,* whether ſolid or in aqueous ſolution, act ſlowly upon nitrous gas, and become oxigenated, whilſt they convert it into nitrous oxide. The diminution of the nitrous gas, in theſe proceſſes, is the ſame as in the proceſs with ſulphite of pot-aſh.

b. When nitrous gas and ſulphurated hidrogen gas are mingled together, they ſlowly decompoſe each other, with great diminution of volume. In this operation, nitrous oxide is formed,† ſulphur is depoſited, and ſigns of the production of ammoniac and water are perceived. There is every reaſon to believe that the hidrogen of the ſulphurated hidrogen is the great agent in effecting this decompoſition, by combining with portions of both the oxigen and nitrogen of the nitrous gas, to produce water and ammoniac. For no perceptible quantity of acid is formed in the experiment; and the ſulphur depoſited appears to be in its common ſtate. This opinion indeed is further proved, by the phenomena connected with the decompoſition of nitrous gas by naſcent hidrogen.

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* Dr. Prieſtley firſt noticed the converſion of nitrous gas into nitrous oxide by ſulphurets. To the Dutch chemiſts we owe the diſcovery of the action of muriate of tin on nitrous gas. Journal de Phyſique, XLIII. part II. page 326.

† Mr. Kirwan firſt obſerved the peculiar action of ſulphurated hidrogen on nitrous gas. Phil. Tranſ. Vol. LXXVI. page 133.

When

When nitrous gas is expoſed to naſcent hidrogen, whether it be procured by the action of metals upon water, in common caſes, or in Galvanic experiments, ammoniac and nitrous oxide are uniformly produced; and the volume of the nitrous gas diminiſhes nearly to the ſame extent, as in the proceſs with ſulphurated hidrogen, *i.e.* to about one-third.

Sect. 3. Nitrous oxide may be procured from nitric acid, when this ſubſtance is decompoſed by certain metallic bodies; and likewiſe during the mutual decompoſition of nitric acid and ammoniac at certain temperatures.

a. During the action of zinc, iron, or tin, upon diluted nitric acid, certain quantities of nitrous oxide are produced, but mingled with nitrous gas and nitrogen. In theſe proceſſes, as has been long known, ammoniac is formed; and conſequently water is decompoſed: a circumſtance moſt probably connected with the production of the nitrous oxide; for the quantity of the gas is leſs in proportion as the acid is more concentrated.

b. Nitrous oxide, perfectly pure, may be obtained in an eaſy manner, and at a cheap rate, from the decompoſition of nitrate of ammoniac. This ſalt, when deprived of as much of its water of cryſtallization as poſſible, becomes fluid at the temperature of 320°, Fahrenheit, and immediately begins to be converted into water and nitrous oxide.*

One hundred grains of ſalt, perfectly decompoſed at any temperature between 320° and 620°, produces about 57 grains of gas, and 43 of fluid. In experiments on this decompoſition the heat muſt be carefully applied and regulated; as at temperatures above 800°, nitrate of ammoniac detonates, and becomes changed into nitrous gas, nitric acid, water, and nitrogen.

III. *On the Combinations of Nitrous Oxide.*

Sect. 1. *Pure* water is capable of holding in ſolution, at the common temperatures of the atmoſphere, a quantity of nitrous oxide equal to about half its volume.† Water, impregnated with nitrous oxide, has a ſweetiſh taſte, which is hardly perceptible, except to delicate organs: and it becomes effervеſcent on agitation. It is poſſeſſed of no action in vegetable colours; and effects no perceptible change in metallic ſolutions.

Nitrous oxide is expelled unaltered from water at temperatures equal to about 212°. It is likewiſe incapable of remaining in combination with it, when the preſſure of the atmoſphere is removed.

Nitrous oxide is expelled from water by muriatic acid, carbonic acid, and ſulphurated hidrogen; but it has a ſtronger affinity for that fluid than nitrous gas, oxigen, or hidrogen.

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* To Mr. Berthollet we owe the diſcovery of the products evolved during the ſlow decompoſition of nitrate of ammoniac. See Mem. de Paris, 1785.

† Dr. Prieſtley's Experiments and Obſervations, Vol. II. page 81.

Sect. 2. Ether, alcohol, and the volatile and fixed oils, diſſolve nitrous oxide in larger quantities than water at common temperatures; but without undergoing any material alteration in their properties. The gas is incapable of remaining in combination with them at the temperature of their ebullition, or under the exhauſted receiver.

Sect. 3. Nitrous oxide has as yet been combined with no ſolid bodies, except the fixed alkalies; and even the affinities of water, alcohol, ether, &c. for this gas appear to be diminiſhed when they hold in ſolution neutral ſalts and other ſubſtances. The ſulphurets, the ſulphites, and the different bodies that act upon nitrous gas, both dry and in ſolution, effect no alteration in the compoſition of nitrous oxide.

Sect. 4. The muriatic, and the ſulphureous acid gaſes, expand a little when mingled with nitrous oxide; but they are readily ſeparated from it by water. No other aëriform fluids appear to be poſſeſſed of the leaſt power of action upon this gas at common temperatures.

Sect. 5. None of the alkaline bodies, except pot-aſh and ſoda, have as yet been combined with nitrous oxide; and even theſe ſubſtances are capable of uniting with it, only when it is in the naſcent ſtate.

a. When nitrous oxide in a free ſtate is expoſed to the dry alkalies, or alkaline earths, at common temperatures, it is neither abſorbed nor acted upon. When it is placed in contact with ſolutions of them in water, a ſmall quantity of it is diſſolved: this phenomenon, however, appears to depend, in a great meaſure, on the water of the ſolution; for the gas may be expelled from it unaltered at the temperature of its ebullition.

b. To form the combination of pot-aſh, or ſoda, and nitrous oxide, the alkalies, after being mingled in the dry ſtate with alkaline ſulphites, muſt be expoſed, together with them, to nitrous gas. The nitrous oxide, as faſt as it is formed by the action of the ſulphite on the nitrous gas, is abſorbed by the free alkali; and thus a permanent union is effected between the two bodies.

c. The compound of nitrous oxide and pot-aſh is very ſoluble in water, but apparently inſoluble in alcohol. Its taſte is not very different from that of cauſtic pot-aſh, perhaps a little more pungent. When acted upon by the acids it gives out nitrous oxide. This gas is likewiſe diſengaged from it in a pure form by the application of a heat above 400°, Fahrenheit. It produces ſcintillations when thrown upon red hot charcoal.

d. The properties of the combination of ſoda and nitrous oxide are not materially different from thoſe of the compound of pot-aſh and nitrous oxide.

IV. *Of the Action of Heat and Electricity on Nitrous Oxide.*

Sect 1. Nitrous oxide undergoes no change in its compoſition at temperatures below thoſe of ignition, though expoſed to them for ever ſo great a length of time. When, however, it is paſſed through a porcelain tube, heated red, or when it is acted on for a long while by the electric ſpark, a new arrangement of its principles takes place, and it becomes converted into nitrous acid, and a gas analogous to atmoſpheric air, but containing a little leſs oxigen.*

The nitrous acid formed in this proceſs is very ſmall in quantity, but the unabſorbable gas equals about ,8 of the original volume of the nitrous oxide.

V. *On the Decompoſition of Nitrous Oxide by Combuſtible Bodies.*

Sect. 1. The changes produced in nitrous oxide by heat, are probably intimately connected with the phenomena of its decompoſition by the inflammable bodies, which in general require, for their combuſtion in this gas, much higher temperatures than thoſe at which they burn in atmoſpheric air or oxigen. When they are intenſely heated, they act upon it, and become oxigenated, nitrogen being evolved, and much heat and light generated.

During the combuſtion of ſolid and fluid bodies producing flame in nitrous oxide, nitrous acid is formed, moſt likely in conſequence of a new arrangement of principles produced by the ignition of a part of the gas not in contact with the burning ſubſtance. Likewiſe when nitrous oxide in exceſs is decompoſed by inflammable gaſes, nitrous acid, nitrogen, and a little oxigen, are produced, probably from the ſame cauſe.

a. Pyrophorus, which inflames in nitrous gas and in atmoſpheric air, at or even below 40°, Fahrenheit, requires for its combuſtion in nitrous oxide a temperature nearly equal to that of ignition.

b. Phoſphorus may be fuſed and even ſublimed in nitrous oxide without effecting any change in it; but when it is introduced into it in a ſtate of active inflammation, it burns with intenſely vivid light.

c. Sulphur does not act in nitrous oxide when introduced into it, burning with the feeble blue flame; but if in a ſtate of vivid inflammation, it decompoſes it with the production of a beautiful roſe-coloured light.

d. Iron and charcoal, when heated white, burn in nitrous oxide with much greater vividneſs than in the atmoſphere.

e. The lighted taper, introduced into nitrous oxide, burns at firſt as in oxigen; but afterwards with a flame, white in the centre, and blue at the circumference; a phenomenon apparently depending upon the nitrous acid formed in the proceſs.

f. Hidrogen, and the compound inflammable gaſes, when mingled in certain proportions with nitrous oxide, and acted on by the electric ſpark, explode, with the production of much light and heat.†

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* Dr. Prieſtley firſt obſerved, that nitrous oxide, after being acted upon by the electric ſpark, became immiſcible with water; but he did not notice the nitrous acid produced in the proceſs: neither did the Dutch chemiſts, who repeated his experiments.

† The Society of Chemiſts at Amſterdam thought that hidrogen was the only body capable of decompoſing nitrous oxide. Journal de Phyſique, tom. XLIII. part II. page 331. From their experiments it appears probable that in many caſes they did not raiſe the temperatures of other inflammable bodies ſufficiently high for this purpoſe.

Sect. 2. When nitrous acid is formed in proceſſes of combuſtion in nitrous oxide, ſo as to remain in the reſidual gas, the quantity of free nitrogen evolved never equals more than $\frac{5}{6}$ of the volume of the nitrous oxide decompoſed, though when this ſubſtance is not produced, 10 of nitrous oxide give out about 11 of nitrogen.

Sect. 3. The heat produced by even the ſlow combuſtion of bodies in nitrous oxide, is in all caſes ſufficient to keep up the proceſs for a certain time, after the original heating cauſe is removed: but in proportion as the volume of nitrogen increaſes, the inflammation diminiſhes in vividneſs, and it uniformly ceaſes before the whole of the gas is decompoſed. Charcoal and iron ceaſe to burn in nitrous oxide when it is mingled with an equal volume of nitrogen; yet the combuſtion of phoſphorus will continue (though feebly) in a gas conſiſting of 4 nitrogen and 1 nitrous oxide.

VI. *On the Analyſis of Nitrous Oxide.*

Sect. 1. *a.* One hundred cubic inches of nitrous oxide weigh at temperature 55°, Fahrenheit, and atmoſpheric preſſure 30. about 50.2 grains.

b. Four cubic inches of this gas, when perfectly decompoſed by the combuſtion of charcoal, are converted into about 2.2 cubic inches of carbonic acid, and 4.2 cubic inches of nitrogen; no nitrous acid being produced in the proceſs.

c. Likewiſe 2 cubic inches of nitrous oxide require, for their complete decompoſition, about 2.2 cubic inches of hidrogen, and evolve during the inflammation, a quantity of nitrogen, nearly equal to it in volume.

b. Comparing theſe facts with other facts relating to the production of nitrous oxide, we may conclude that 100 parts of this gas are compoſed of about 37 parts oxigen and 63 parts nitrogen,* exiſting in a much more condenſed ſtate than when in their ſimple forms.

c. The oxigen appears to be combined with the nitrogen much more intimately in nitrous oxide than in nitrous gas; a phenomenon that may be eaſily accounted for, when we conſider the general law of ſaturation. †

VII. *On the Action of Nitrous Oxide on Living Beings.*

Sect. 1. Nitrous oxide when taken into the mouth taſtes faintly ſweetiſh. Its odour is weak, but agreeable. It is reſpirable, and produces very peculiar effects, when made to act on the ſyſtem by means of the lungs.

The ſenſations it occaſions are in general analogous to thoſe connected with intoxication from fermented liquors.

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[*] This eſtimation is very analogous to that of the Dutch chemiſts. Journal de Phyſique, tom. XLIII. part II. page 331.

[†] The affinity of one body for another is inverſely as its ſaturation with it.

a. Healthy perſons after breathing it accurately for a minute and a half, or two minutes, are uſually exhilarated, and affected by ſtrong pleaſurable ſenſations, the circulation is quickened, the countenance becomes darker, and a ſenſe of warmth is induced.

b. On unhealthy perſons the effects are leſs pleaſant, and in certain caſes it has induced hyſterical affections.

c. Nitrous oxide after being breathed for more than three minutes, produces violent excitement, which generally ends in momentary loſs of ſenſation.

d. As far as experiments have yet gone, there is no reaſon to believe that any notable debility follows, even its moſt violent agency.

Sect. 2. Warm blooded animals when permanently immerſed in nitrous oxide, live longer in it than in any other gaſes, except ſuch as contain free oxigen; but cold blooded animals are quickly deſtroyed by it.

a. The ſmaller quadrupeds die in pure nitrous oxide in three or four minutes; they abſorb a little of the gas, and their death ſeems to be occaſioned by exceſſive ſtimulation.

b. When examined after death their internal parts are found to have undergone very peculiar changes.

The animal fibre is very inirritable; the blood both in the arteries and veins is dark and purple, and the lungs are marked with large purple ſpots.

Sect. 3. When nitrous oxide is expoſed to fluid venous blood, whether in the veſſels or out of them, it is rapidly abſorbed, apparently no portion of it is decompoſed, and the colour of the blood becomes rather more inclined to purple.

II.

Obſervations on the ſuppoſed Magnetic Property of Nickel, by RICHARD CHEVENIX, *Eſq. F. R. S. and M. R. I. A. Communicated by the Author.*

THE properties of magnetiſm and polarity, firſt obſerved in iron, were, for a long time, the attributes of it alone. But the experiments of later philoſophers had given reaſon to ſuppoſe, that nickel and cobalt participate of both. The celebrated Bergman, in his diſſertation upon the former of theſe two metals, after lamenting the inſufficiency of all his attempts to deprive it of the laſt portions of iron with which it was contaminated, concludes the paragraph with a doubt, whether magnetiſm may not be a property of nickel, as well as of iron. To this the authority of Arvidſon may be added; and hence chymiſts have ſuppoſed, that magnetiſm is not a property of iron excluſively. In Italy, within theſe five years, as I have been informed, compaſſes for marine purpoſes have been made of nickel and of cobalt; thus being preferable to iron, as better reſiſting the action of the air. I know it was the opinion of the French chymiſts, that magnetic needles, made of either

either of thoſe metals perfectly pure, anſwered the purpoſe as well as thoſe made of the uſual ſubſtance. In the ſummer of 1798 I was witneſs to ſome experiments, performed at the *Ecole des mines*, to ſhew that ſeveral pieces of both, which had been reduced to the metallic ſtate, in the laboratory of that eſtabliſhment, and which were magnetic, were totally exempt from iron. The experiments were conducted by Monſ. Vauquelin, and were as follows: ſome magnetic bits of each of thoſe metals were diſſolved in an acid, and iron was, in vain, looked for by the delicate teſts of an infuſion of gall nuts, and a ſolution of pruſſiate of pot-aſh. Hence it was reaſonable to conclude that no iron was preſent, and that nickel and cobalt really are magnetic of themſelves, and independently of alloy.

If I have obtained nickel in its metallic ſtate, and in no way attractable, it was not in performing experiments with this direct view, for ſo fully was I convinced of the accuracy of thoſe I had witneſſed in Paris in 1798, that I entertained no doubt upon the ſubject. I had an acid ſolution of impure oxide of nickel, which I precipitated by ammonia, for the purpoſe of procuring ſome pure nickel which I happened to want. I filtered immediately, and ſuffered the ammoniacal ſolution to ſtand ſome days in a well ſtopped bottle. The liquor remained of the uſual colour, but a blueiſh precipitate, which likewiſe I ſeparated by the filtre, had been formed at the bottom. The alkaline liquor, which had paſſed clear, was evaporated; and the oxide of nickel, which fell as the ammonia was volatilized, was collected, waſhed, dried, and treated with a proper flux, in a Heſſian crucible, and in a forge capable of giving a ſtrong heat. By this proceſs I obtained a reſult which was nickel, ſo unattractable, that preſented as near as a ſteady hand could hold it, to a very delicate magnet, ſupported on a cork floating on water or on mercury, no viſible influence took place upon the magnet. A bit of the nickel, diſſolved in an acid, exhibited, as may be well ſuppoſed, no traces of iron.

Its ſpecific gravity, taken by the Count de Bournon, at a balance ſenſible to the $\frac{1}{200}$ of a grain, and at the temperature of 60, was 7,3806, a ſpecific gravity much inferior to that which Bergman had found, 9,0000: and even to that given by Monſ. de Morveau, 7,8070; hence it would appear that the preſence of iron tends to increaſe its denſity. Its fuſibility I could not aſcertain, having no pyrometer. Its malleability is certainly greater than that of zinc. Its colour, like ſome of the alloys of ſilver and copper, of a very pale red. Its hardneſs conſiderable, and its fracture grainy and uneven. Several of my chymical friends in this country have witneſſed the inefficacy of this nickel in attracting the magnetic needle; and Profeſſor Pictet of Geneva was ſo good as to take ſome of it to the chymiſts in France, to convince them of the error which they are under, with regard to this property in nickel.

It does not appear from the reſults of the experiments to which, as I mentioned, I was a witneſs in 1798, that there was any inaccuracy in the mode of operating; but merely, that magnetiſm is a re-agent more powerful to detect minute portions of iron, when in its metallic ſtate, than is either a ſolution of gall nuts, or of pruſſiate of pot-aſh, to diſcover its oxide when held in ſolution by acid menſtrua. Indeed the magnet will, in advantageous circumſtances, be influenced by the ſmalleſt particle of metallic iron adhering to the ſurface of any body, without regard to the actual quantity of the ſubſtance to which it is attached; while the efficacy of chymical teſts is, in ſome meaſure, bounded by the proportion of heterogeneous elements. In one caſe the action is independent and abſolute; in the other, relative and limited. Thus a lump of copper, of whatever ſize, being ſtruck with an iron hammer upon an anvil, ſometimes acquires the power of influencing the magnet; but I believe it would be rather difficult to diſcover, by reagents, the iron that confers on it that power; and which perhaps does not amount to a millionth part of the whole maſs.

As far as I have obſerved, the following cautions alone are neceſſary in thus treating the oxide of nickel by ammonia.

1. All oxides of iron, except the red, are ſoluble, more or leſs, in ammonia; therefore the iron muſt, by the means of nitric acid, be brought to its full ſtate of oxidation, before precipitation and rediſſolution.

2. The ammonia muſt be perfectly cauſtic; for carbonate of ammonia diſſolves red oxide of iron. And,

3. It may be expedient to allow ſome precipitate of nickel to be formed at the bottom, before the ammoniacal liquor is drawn off for evaporation; as, by that time, we may be ſure, that if the two firſt conditions have been obſerved, all the oxide of iron has previouſly fallen to the bottom.

Although I have not directly performed a ſimilar operation upon cobalt, or obtained that metal unattractable, I have little doubt but that ſuch a quality is intirely owing to iron; and that the property of magnetiſm muſt be again reſtricted to its former limits.

III.

On the Mechaniſm of the Eye. By THOMAS YOUNG, *M.D. F.R.S.*

(Continued from page 265.)

THESE conſiderations will explain the inconſiſtency of different obſervations on the refractive power of the cryſtalline; and, in particular, how the refraction which I formerly calculated, from meaſuring the focal length of the lens,* is ſo much greater than that which is determined by other means. But, for direct experiments, Dr. Wollaſton's method is exceedingly accurate.

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* Phil. Tranſ. for 1793, p. 174.

When I look at a minute lucid point, ſuch as the image of a candle in a ſmall concave ſpeculum, it appears as a radiated ſtar, as a croſs, or as an unequal line, and never as a perfect point, unleſs I apply a concave lens inclined at a proper angle, to correct the unequal refraction of my eye. If I bring the point very near, it ſpreads into a ſurface nearly circular, and almoſt equably illuminated, except ſome faint lines, nearly in a radiating direction. For this purpoſe, the beſt image is a candle, or a ſmall ſpeculum, viewed through a minute lens at ſome little diſtance, or ſeen by reflection in a larger lens. If any preſſure has been applied to the eye, ſuch as that of the finger keeping it ſhut, the ſight is often confuſed for a ſhort time after the removal of the finger, and the image is in this caſe ſpotty or curdled. The radiating lines are probably occaſioned by ſome ſlight inequalities in the ſurface of the lens, which is very ſuperficially furrowed in the direction of its fibres: the curdled appearance will be explained hereafter. When the point is further removed, the image becomes evidently oval, the vertical diameter being longeſt, and the lines a little more diſtinct than before, the light being ſtrongeſt in the neighbourhood of the centre; but immediately at the centre there is a darker ſpot, owing to ſuch a ſlight depreſſion at the vertex as is often obſervable in examining the lens after death. The ſituation of the rays is conſtant, though not regular; the moſt conſpicuous are ſeven or eight in number; ſometimes about twenty fainter ones may be counted. Removing the point a little further, the image becomes a ſhort vertical line; the rays that diverged horizontally being perfectly collected, while the vertical rays are ſtill ſeparate. In the next ſtage, which is the moſt perfect focus, the line ſpreads in the middle, and approaches nearly to a ſquare, with projecting angles, but is marked with ſome darker lines towards the diagonals. The ſquare then flattens into a rhombus, and the rhombus into a horizontal line unequally bright. At every greater diſtance, the line lengthens, and acquires alſo breadth, by radiations ſhooting out from it, but does not become a uniform ſurface, the central part remaining always conſiderably brighteſt, in conſequence of the ſame flattening of the vertex which before made it fainter. Some of theſe figures bear a conſiderable analogy to the images derived from the refraction of oblique rays, (Schol. 4. Prop. IV.) and ſtill more ſtrongly reſemble a combination of two of them in oppoſite directions; ſo as to leave no doubt, but that both ſurfaces of the lens are oblique to the viſual axis, and co-operate in diſtorting the focal point. This may alſo be verified, by obſerving the image delineated by a common glaſs lens, when inclined to the incident rays. (See Plate XV. Fig. 28—40.)

The viſual axis being fixed in any direction, I can at the ſame time ſee a luminous object placed laterally at a conſiderable diſtance from it; but in various directions the angle is very different. Upwards it extends to 50 degrees, inwards to 60, downwards to 70, and outwards to 90 degrees. Theſe internal limits of the field of view nearly correſpond with the external limits formed by the different parts of the face, when the eye is directed forwards and ſomewhat downwards, which is its moſt natural poſition; although the internal limits are a little more extenſive than the external; and both are well calculated for enabling us to perceive the moſt readily, ſuch objects as are the moſt likely to concern us.

Dr. Wollaſton's eye has a larger field of view, both vertically and horizontally, but nearly in the ſame proportions, except that it extends further upwards. It is well known, that the retina advances further forwards towards the internal angle of the eye, than towards the external angle; but upwards and downwards its extent is nearly equal, and is indeed every way greater than the limits of the field of view, even if allowance is made for the refraction of the cornea only. The ſenſible portion ſeems to coincide more nearly with the painted choroid of quadrupeds: but the whole extent of perfect viſion is little more than 10 degrees; or, more ſtrictly ſpeaking, the imperfection begins within a degree or two of the viſual axis, and at the diſtance of 5 or 6 degrees becomes nearly ſtationary, until, at a ſtill greater diſtance, viſion is wholly extinguiſhed. The imperfection is partly owing to the unavoidable aberration of oblique rays, but principally to the inſenſibility of the retina: for, if the image of the ſun itſelf be received on a part of the retina remote from the axis, the impreſſion will not be ſufficiently ſtrong to form a permanent ſpectrum, although an object of very moderate brightneſs will produce this effect when directly viewed. It would probably have been inconſiſtent with the economy of nature, to beſtow a larger ſhare of ſenſibility on the retina. The optic nerve is at preſent very large; and the delicacy of the organ renders it, even at preſent, very ſuſceptible of injury from ſlight irritation, and very liable to inflammatory affections; and, in order to make the ſight ſo perfect as it is, it was neceſſary to confine that perfection within narrow limits. The motion of the eye has a range of about 55 degrees in every direction; ſo that the field of perfect viſion, in ſucceſſion, is by this motion extended to 110 degrees.

But the whole of the retina is of ſuch a form as to receive the moſt perfect image, on every part of its ſurface, that the ſtate of each refracted pencil will admit; and the varying denſity of the cryſtalline renders that ſtate more capable of delineating ſuch a picture, than any other imaginable contrivance could have done. To illuſtrate this, I have conſtructed a diagram, repreſenting the ſucceſſive images of a diſtant object filling the whole extent of view, as they would be formed by the ſucceſſive refractions of the different ſurfaces. Taking the ſcale of my own eye, I am obliged to ſubſtitute, for a ſeries of objects at any indefinitely great diſtance, a circle of 10 inches radius; and it is moſt convenient to conſider only thoſe rays which paſs through the anterior vertex of the lens; ſince the actual centre of each pencil muſt be in the ray which paſſes through the centre of the pupil, and the ſhort diſtance of the vertex of the lens from this point, will always tend to correct the unequal refraction of oblique rays. The firſt curve (Plate XV. Fig. 16.) is the image formed by the furtheſt interſection of rays refracted at the cornea; the ſecond, the image formed by the neareſt interſection; the diſtance between theſe, ſhows the degree of confuſion in the image; and the third curve, its brighteſt part. Such muſt be the form of the image which the cornea tends to delineate in an eye deprived of the cryſtalline lens; nor can any external remedy properly correct the imperfection of lateral viſion. The next three curves ſhow the images formed after the refraction at the anterior ſurface of the lens, diſtinguiſhed in the ſame manner; and the three following, the reſult of all the ſucceſſive

refractions.

refractions. The tenth curve is a repetition of the ninth, with a ſlight correction near the axis, at F, where, from the breadth of the pupil, ſome perpendicular rays muſt fall. By comparing this with the eleventh, which is the form of the retina, it will appear that nothing more is wanting for their perfect coincidence, than a moderate diminution of denſity in the lateral parts of the lens. If the law, by which this denſity varies, were more accurately aſcertained, its effect on the image might be eſtimated by means of the eighth propoſition; and probably the image, thus corrected, would approach very nearly to the form of the twelfth curve.

To find the place of the entrance of the optic nerve, I fix two candles at ten inches diſtance, retire ſixteen feet, and direct my eye to a point four feet to the right or left of the middle of the ſpace between them: they are then loſt in a confuſed ſpot of light; but any inclination of the eye brings one or the other of them into the field of view. In Bernoulli's eye, a greater deviation was required for the direction of the axis;* and the obſcured part appeared to be of greater extent. From the experiment here related, the diſtance of the centre of the optic nerve from the viſual axis is found (by Prop. V.) to be 16 hundredths of an inch; and the diameter of the moſt inſenſible part of the retina, one-thirtieth of an inch. In order to aſcertain the diſtance of the optic nerve from the point oppoſite to the pupil, I took the ſclerotica of the human eye, divided it into ſegments, from the centre of the cornea towards the optic nerve, and extended it on a plane. I then meaſured the longeſt and ſhorteſt diſtances from the cornea to the perforation made by the nerve, and their difference was exactly one-fifth of an inch. To this we muſt add a fiftieth, on account of the eccentricity of the pupil in the uvea, which in the eye that I meaſured was not great, and the diſtance of the centre of the nerve from the point oppoſite the pupil will be 11 hundredths. Hence it appears, that the viſual axis is five hundredths, or one-twentieth of an inch, further from the optic nerve than the point oppoſite the pupil. It is poſſible that this diſtance may be different in different eyes: in mine, the obliquity of the lens, and the eccentricity of the pupil with reſpect to it, will tend to throw a direct ray upon it, without much inclination of the whole eye; and it is not improbable, that the eye is alſo turned ſlightly outwards, if looking at any object before it, although the inclination is too ſmall to be ſubjected to meaſurement.

It muſt alſo be obſerved, that it is very difficult to aſcertain the proportions of the eye ſo exactly as to determine, with certainty, the ſize of an image on the retina; the ſituation, curvature, and conſtitution of the lens, make ſo material a difference in the reſult, that there may poſſibly be an error of almoſt one-tenth of the whole. In order, therefore, to obtain ſome confirmation from experiment, I placed two candles at a ſmall diſtance from each other, turned the eye inwards, and applied the ring of a key ſo as to produce a ſpectrum, of which the edge coincided with the inner candle; then, fixing my eye on the outward one, I found that the ſpectrum advanced over two-ſevenths of the diſtance between them.

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* Comm. Petrop. I. p. 314.

Hence,

Hence, the ſame portion of the retina that ſubtended an angle of ſeven parts at the centre of motion of the eye, ſubtended an angle of five at the ſuppoſed interſection of the principal rays; (Plate XIV. Fig. 11.) and the diſtance of this interſection from the retina was 637 thouſandths. This nearly correſponds with the former calculation; nor can the diſtance of the centre of the optic nerve from the point of moſt perfect viſion be, on any ſuppoſition, much leſs than that which is here aſſigned. And, in the eyes of quadrupeds, the moſt ſtrongly painted part of the choroid is further from the nerve than the real axis of the eye.

I have endeavoured to expreſs in four figures, the form of every part of my eye, as nearly as I have been able to aſcertain it; the firſt (Pl. XV. Fig. 17.) is a vertical ſection; the ſecond (Fig. 18.) a horizontal ſection; the third and fourth are front views, in different ſtates of the pupil. (Fig. 19 and 20.)

Conſidering how little inconvenience is experienced from ſo material an inequality in the refraction of the lens as I have deſcribed, we have no reaſon to expect a very accurate proviſion for correcting the aberration of the lateral rays. But, as far as can be aſcertained by the optometer, the aberration ariſing from figure is completely corrected; ſince four or more images of the ſame line appear to meet exactly in the ſame point, which they would not do if the lateral rays were materially more refracted than the rays near the axis. The figure of the ſurfaces is ſometimes, and perhaps always, more or leſs hyperbolical* or elliptical: in the interior laminæ indeed, the ſolid angle of the margin is ſomewhat rounded off; but the weaker refractive power of the external parts, muſt greatly tend to correct the aberration ariſing from the too great curvature towards the margin of the diſc. Had the refractive power been uniform, it might have collected the lateral rays of a direct pencil nearly as well; but it would have been leſs adapted to oblique pencils of rays; and the eye muſt alſo have been encumbered with a maſs of much greater denſity than is now required, even for the central parts: and, if the whole lens had been ſmaller, it would alſo have admitted too little light. It is poſſible too, that Mr. Ramſden's obſervation,† on the advantage of having no reflecting ſurface, may be well-founded: but it has not been demonſtrated, that leſs light is loſt in paſſing through a medium of variable denſity, than in a ſudden tranſition from one part of that medium to another; nor are we yet ſufficiently acquainted with the cauſe of this reflection, to be enabled to reaſon ſatisfactorily on the ſubject. But, neither this gradation, nor any other proviſion, has the effect of rendering the eye perfectly achromatic. Dr. Jurin had remarked this, long ago,‡ from obſerving the colour bordering the image of an object ſeen indiſtinctly. Dr. Wollaſton pointed out to me on the optometer, the red and blue appearance of the oppoſite internal angles of the croſſing lines; and mentioned, at the ſame time, a very elegant experiment for proving the diſperſive power of the eye. He looks through a priſm at a ſmall lucid point, which of courſe

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* Petit Mém, de l'Acad. 1725, p. 20.

† Phil. Tranſ. for 1795, p. 2.

‡ Smith, e. 96.

becomes

becomes a linear ſpectrum. But the eye cannot ſo adapt itſelf as to make the whole ſpectrum appear a line; for, if the focus be adapted to collect the red rays to a point, the blue will be too much refracted, and expand into a ſurface; and the reverſe will happen if the eye be adapted to the blue rays; ſo that, in either caſe, the line will be ſeen as a triangular ſpace. The obſervation is confirmed, by placing a ſmall concave ſpeculum in different parts of a priſmatic ſpectrum, and aſcertaining the utmoſt diſtances at which the eye can collect the rays of different colours to a focus. By theſe means I find, that the red rays, from a point at 12 inches diſtance, are as much refracted as white or yellow light at 11. The difference is equal to the refraction of a lens 132 inches in focus. But the aberration of the red rays in a lens of crown glaſs, of equal mean refractive power with the eye, would be equivalent to the effect of a lens 44 inches in focus. If, therefore, we can depend upon this calculation, the diſperſive power of the eye collectively, is one-third of the diſperſive power of crown glaſs, at an equal angle of deviation. I cannot obſerve much aberration in the violet rays. This may be, in part, owing to their faintneſs; but yet I think their aberration muſt be leſs than that of the red rays. I believe it was Mr. Ramſden's opinion, that ſince the ſeparation of coloured rays is only obſerved where there is a ſudden change of denſity, ſuch a body as the lens, of a denſity gradually varying, would have no effect whatever in ſeparating the rays of different colours. If this hypotheſis ſhould appear to be well-founded, we muſt attribute the whole diſperſion to the aqueous humour; and its diſperſive power will be half that of crown glaſs, at the ſame deviation. But we have an inſtance, in the atmoſphere, of a very gradual change of denſity; and yet Mr. Gilpin informs me, that the ſtars, when near the horizon, appear very evidently coloured. At a more favourable ſeaſon of the year, it would not be difficult to aſcertain, by means of the optometer, the diſperſive power of the eye, and of its different parts, with greater accuracy than by the experiment here related. Had the diſperſive power of the whole eye been equal to that of flint glaſs, the diſtances of perfect viſion would have varied from 12 inches to 7 for different rays, in the ſame ſtate of the mean refractive powers.

VII. The faculty of accommodating the eye to various diſtances, appears to exiſt in very different degrees in different individuals. The ſhorteſt diſtance of perfect viſion in my eye, is 26 tenths of an inch for horizontal, and 29 for vertical rays. This power is equivalent to the addition of a lens of 4 inches focus. Dr. Wollaſton can ſee at 7 inches, and with converging rays; the difference anſwering to 6 inches focal length. Mr. Abernethy has perfect viſion from 3 inches to 30, or a power equal to that of a lens $3\frac{1}{3}$ inches in focus. A young lady of my acquaintance can ſee at 2 inches and at 4; the difference being equivalent to 4 inches focus. A middle aged lady at 3 and at 4; the power of accommodation being only equal to the effect of a lens of 12 inches focus. In general, I have reaſon to think, that the faculty diminiſhes in ſome degree, as perſons advance in life; but ſome alſo of a middle age appear to poſſeſs it in a very ſmall degree. I ſhall take the range of my own eye, as being probably about the medium, and inquire what changes will be neceſſary in order to produce it; whether we ſuppoſe the radius of the cornea to be diminiſhed, or the diſtance of the lens from the retina to be increaſed, or theſe two cauſes to act conjointly, or the figure of the lens itſelf to undergo an alteration.

1. We have calculated, that when the eye is in a ſtate of relaxation, the refraction of the cornea is ſuch as to collect rays diverging from a point ten inches diſtant, to a focus at the diſtance of $13\frac{2}{3}$ tenths. In order that it may bring to the ſame focus, rays diverging from a point diſtant 29 tenths, we find (by Cor. 5, Prop. IV.) that its radius muſt be diminiſhed from 31 to 25 hundredths, or very nearly in the ratio of five to four.

2. Suppoſing the change from perfect viſion at ten inches to 29 tenths, to be effected by a removal of the retina to a greater diſtance from the lens, this will require, (by the ſame Corollary,) an elongation of 135 thouſandths, or more than one-ſeventh of the diameter of the eye. In Mr. Abernethy's eye, an elongation of 17 hundredths, or more than one-ſixth, is requiſite.

3. If the radius of the cornea be diminiſhed one-ſixteenth, or to 29 hundredths, the eye muſt at the ſame time be elongated 97 thouſandths, or about one-ninth of its diameter.

4. Suppoſing the cryſtalline lens to change its form; if it became a ſphere, its diameter would be 28 hundredths, and, its anterior ſurface retaining its ſituation, the eye would have perfect viſion at the diſtance of an inch and a half. (Cor. 5 and 8, Prop. IV.) This is more than double the actual change. But it is impoſſible to determine preciſely how great an alteration of form is neceſſary, without aſcertaining the nature of the curves into which its ſurfaces may be changed. If it were always a ſpheroid more or leſs oblate, the focal length of each ſurface would vary inverſely as the ſquare of the axis: but, if the ſurfaces became, from ſpherical, portions of hyperbolic conoids, or of oblong ſpheroids, or changed from more obtuſe to more acute figures of this kind, the focal length would vary more rapidly. Diſregarding the elongation of the axis, and ſuppoſing the curvature of each ſurface to be changed proportionally, the radius of the anterior muſt become about 21, and that of the poſterior 15 hundredths.

VIII. I ſhall now proceed to inquire, which of theſe changes takes place in nature; and I ſhall begin with a relation of experiments made in order to aſcertain the curvature of the cornea in all circumſtances.

The method deſcribed in Mr. Home's Croonian Lecture for 1795,* appears to be far preferable to the apparatus of the preceding year:† for a difference in the diſtance of two images ſeen in the cornea, would be far greater, and more conſpicuous, than a change of its prominency, and far leſs liable to be diſturbed by accidental cauſes. It is nearly, and perhaps totally impoſſible to change the focus of the eye, without ſome motion of its axis. The eyes ſympathize perfectly with each other; and the change of focus is almoſt inſeparable from a change of the relative ſituation of the optic axes; ſo much, that if I direct both my eyes at an object beyond their furtheſt focus, I cannot avoid bringing that focus a little

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* Phil. Tranſ. for 1796, p. 2.

† Phil. Tranſ. for 1795, p. 13.

nearer.

nearer: while one axis moves, it is not eaſy to keep the other perfectly at reſt; and it is not impoſſible, that a change in the proportions of ſome eyes, may render a ſlight alteration of the poſition of the axis abſolutely neceſſary. Theſe conſiderations may partly explain the trifling difference in the place of the cornea that was obſerved in 1794. It appears that the experiments of 1795 were made with conſiderable accuracy, and no doubt with excellent inſtruments; and their failing to aſcertain the exiſtence of any change, induced Mr. Home and Mr. Ramſden to abandon, in great meaſure, the opinion which ſuggeſted them, and to ſuppoſe, that a change of the cornea produces only one-third of the effect. Dr. Olbers of Bremen, who in the year 1780 publiſhed a moſt elaborate diſſertation on the internal changes of the eye,* which he lately preſented to the Royal Society, had been equally unſucceſsful in his attempts to meaſure this change of the cornea, at the ſame time that his opinion was in favour of its exiſtence.

Room was however ſtill left for a repetition of the experiments; and I began with an apparatus nearly reſembling that which Mr. Home has deſcribed. I had an excellent achromatic microſcope, made by Mr. Ramſden for my friend Mr. John Ellis, of five inches focal length, magnifying about 20 times. To this I adapted a cancellated micrometer, in the focus of the eye not employed in looking through the microſcope: it was a large card, divided by horizontal and vertical lines into fortieths of an inch. When the image in the microſcope was compared with this ſcale, care was taken to place the head ſo that the relative motion of the images on the micrometer, cauſed by the unſteadineſs of the optic axis, ſhould always be in the direction of the horizontal lines, and that there could be no error, from this motion, in the dimenſions of the image taken vertically. I placed two candles ſo as to exhibit images in a vertical poſition in the eye of Mr. Konig, who had the goodneſs to aſſiſt me; and, having brought them into the field of the microſcope, where they occupied 35 of the ſmall diviſions, I deſired him to fix his eye on objects at different diſtances in the ſame direction; but I could not perceive the leaſt variation in the diſtance of the images.

Finding a conſiderable difficulty in a proper adjuſtment of the microſcope, and being able to depend on my naked eye in meaſuring diſtances, without an error of one 500th of an inch, I determined to make a ſimilar experiment without any magnifying power. I conſtructed a divided eye-glaſs of two portions of a lens, ſo ſmall, that they paſſed between two images reflected from my own eye; and, looking in a glaſs, I brought the apparent places of the images to coincide, and then made the change for viewing nearer objects; but the images ſtill coincided. Neither could I obſerve any change in the images reflected from the other eye, where they could be viewed with greater convenience, as they did not interfere with the eye-glaſs. But, not being at that time aware of the perfect ſympathy of the eyes, I thought it moſt certain to confine my obſervation to the one with which I ſaw. I

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* De Oculi Mutationibus internis. Gotting. 1780. 4°.

muſt remark that, by a little habit, I have acquired a very ready command over the accommodation of my eye, ſo as to be able to view an object with attention, without adjuſting my eye to its diſtance.

I alſo ſtretched two threads, a little inclined to each other, acroſs a ring, and divided them by ſpots of ink into equal ſpaces. I then fixed a ring, applied my eye cloſe behind it, and placed two candles in proper ſituations before me, and a third on one ſide, to illuminate the threads. Then, ſetting a ſmall looking-glaſs, firſt at four inches diſtance, and next at two, I looked at the images reflected in it, and obſerved at what part of the threads they exactly reached acroſs in each caſe; and with the ſame reſult as before.

I next fixed the cancellated micrometer at a proper diſtance, illuminated it ſtrongly, and viewed it through a pin-hole, by which means it became diſtinct in every ſtate of the eye; and, looking with the other eye into a ſmall glaſs, I compared the image with the micrometer in the manner already deſcribed. I then changed the focal diſtance of the eye, ſo that the lucid points appeared to ſpread into ſurfaces, from being too remote for perfect viſion; and I noted on the ſcale, the diſtance of their centres; but that diſtance was invariable.

Laſtly, I drew a diagonal ſcale, with a diamond, on a looking-glaſs, (Plate XIV. Fig. 12.) and brought the images into contact with the lines of the ſcale. Then, ſince the image of the eye occupies on the ſurface of a glaſs half its real dimenſions, at whatever diſtance it is viewed, its true ſize is always double the meaſure thus obtained. I illuminated the glaſs ſtrongly, and made a perforation in a narrow ſlip of black card, which I held between the images; and was thus enabled to compare them with the ſcale, although their apparent diſtance was double that of the ſcale. I viewed them in all ſtates of the eye; but I could perceive no variation in the interval between them.

The ſufficiency of theſe methods may be thus demonſtrated. Make a preſſure along the edge of the upper eyelid with any ſmall cylinder, for inſtance a pencil, and the optometer will ſhow that the focus of horizontal rays is a little elongated, while that of vertical rays is ſhortened; an effect which can only be owing to a change of curvature in the cornea. Not only the apparatus here deſcribed, but even the eye unaſſiſted, will be capable of diſcovering a conſiderable change in the images reflected from the cornea, although the change be much ſmaller than that which is requiſite for the accommodation of the eye to different diſtances. On the whole, I cannot heſitate to conclude, that if the radius of the cornea were diminiſhed but one-twentieth, the change would be very readily perceptible by ſome of the experiments related; and the whole alteration of the eye requires one-fifth.

But a much more accurate and deciſive experiment remains. I take out of a ſmall botanical microſcope, a double convex lens, of eight-tenths radius and focal diſtance, fixed in a ſocket one-fifth of an inch in depth; ſecuring its edges with wax, I drop into it a little water, nearly cold, till it is three-fourths full, and then apply it to my eye, ſo that the cornea enters half way into the ſocket, and is every where in contact with the water. (Plate XIV. Fig. 13.) My eye immediately becomes preſbyopic, and the refractive power of of the lens, which is reduced by the water to a focal length of about 16 tenths, (Cor. 5, Prop. IV.) is not ſufficient to ſupply the place of the cornea, rendered inefficacious by the intervention of the water; but the addition of another lens, of five inches and a half focus, reſtores my eye to its natural ſtate, and ſomewhat more. I then apply the optometer, and I find the ſame inequality in the horizontal and vertical refractions as without the water; and I have, in both directions, a power of accommodation equivalent to a focal length of four inches, as before. As firſt ſight indeed, the accommodation appears to be ſomewhat leſs, and only able to bring the eye from the ſtate fitted for parallel rays to a focus at five inches diſtance; and this made me once imagine, that the cornea might have ſome ſlight effect in the natural ſtate; but, conſidering that the artificial cornea was about a tenth of an inch before the place of the natural cornea, I calculated the effect of this difference, and found it exactly ſufficient to account for the diminution of the range of viſion. I cannot aſcertain the diſtance of the glaſs lens from the cornea to the hundredth of an inch; but the error cannot be much greater, and it may be on either ſide.

After this, it is almoſt neceſſary to apologize for having ſtated the former experiments; but, in ſo delicate a ſubject, we cannot have too great a variety of concurring evidence.

IX. Having ſatisfied myſelf that the cornea is not concerned in the accommodation of the eye, my next object was to inquire if any alteration in the length of its axis could be diſcovered; for this appeared to be the only poſſible alternative: and, conſidering that ſuch a change muſt amount to one-ſeventh of the diameter of the eye, I flattered myſelf with the expectation of ſubmitting it to meaſurement. Now, if the axis of the eye were elongated one-ſeventh, its tranſverſe diameter muſt be diminiſhed one-fourteenth, and the ſemi-diameter would be ſhortened a thirtieth of an inch.

I therefore placed two candles ſo that when the eye was turned inwards, and directed towards its own image in a glaſs, the light reflected from one of the candles by the ſclerotica appeared upon its external margin, ſo as to define it diſtinctly by a bright line; and the image of the other candle was ſeen in the centre of the cornea. I then applied the double eye-glaſs, and the ſcale of the looking-glaſs, in the manner already deſcribed; but neither of them indicated any diminution of the diſtance, when the focal length of the eye was changed.

Another teſt, and a much more delicate one, was the application of the ring of a key at the external angle, when the eye was turned as much inwards as poſſible, and confined at the ſame time by a ſtrong oval iron ring, preſſed againſt it at the internal angle. The key was forced in as far as the ſenſibility of the integuments would admit, and was wedged, by a moderate preſſure, between the eye and the bone. In this ſituation, the phantom cauſed by the preſſure extended within the field of perfect viſion, and was very accurately defined; nor did it, as I formerly imagined, by any means prevent a diſtinct perception of the objects actually ſeen in that direction; and a ſtraight line coming within the field of this oval phantom, appeared ſomewhat inflected towards its centre; (Plate XIV. Fig. 14.) a diſ-a diſtortion eaſily underſtood by conſidering the effect of the preſſure on the form of the retina. Suppoſing now, the diſtance between the key and the iron ring to have been, as it really was, invariable, the elongation of the eye muſt have been either totally or very nearly prevented; and, inſtead of an increaſe of the length of the eye's axis, the oval ſpot cauſed by the preſſure would have ſpread over a ſpace at leaſt ten times as large as the moſt ſenſible part of the retina. But no ſuch circumſtance took place: the power of accommodation was as extenſive as ever; and there was no perceptible change, either in the ſize or in the figure of the oval ſpot.

Again, ſince the rays which paſs through the centre of the pupil, or rather the anterior vertex of the lens, may, as already obſerved, be conſidered as delineating the image; and, ſince the divergence of theſe rays with reſpect to each other, is but little affected by the refraction of the lens, they may ſtill be ſaid to diverge from the centre of the pupil; and the image of a given object on the retina muſt be very conſiderably enlarged, by the removal of the retina to a greater diſtance from the pupil and lens. (Cor. Prop. V*.) To aſcertain the real magnitude of the image with accuracy, is not ſo eaſy as it at firſt ſight appears; but, beſides the experiment laſt related, which might be employed as an argument to this purpoſe, there are two other methods of eſtimating it. The firſt is too hazardous to be of much uſe; but, with proper precautions, it may be attempted. I fix my eye on a braſs circle placed in the rays of the ſun, and, after ſome time, remove it to the cancellated micrometer; then, changing the focus of my eye, while the micrometer remains at a given diſtance, I endeavour to diſcover whether there is any difference in the apparent magnitude of the ſpectrum on the ſcale; but I can diſcern none. I have not inſiſted on the attempt; eſpecially as I have not been able to make the ſpectrum diſtinct enough without inconvenience; and no light is ſufficiently ſtrong to cauſe a permanent impreſſion on any part of the retina remote from the viſual axis. I therefore had recourſe to another experiment. I placed two candles ſo as exactly to anſwer to the extent of the termination of the optic nerve, and, marking accurately the point to which my eye was directed, I made the utmoſt change in its focal length; expecting that, if there were any elongation of the axis, the external candle would appear to recede outwards upon the viſible ſpace. (Plate XIV. Fig. 15.) But this did not happen; the apparent place of the obſcure part was preciſely the ſame as before. I will not undertake to ſay, that I could have obſerved a very minute difference either way: but I am perſuaded, that I ſhould have diſcovered an alteration of leſs than a tenth part of the whole.

It may be inquired if no change in the magnitude of the image is to be expected on any other ſuppoſition; and it will appear to be poſſible, that the changes of curvature may be

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* This Corollary ſhould ſtand thus. "If a confuſed image be received on any given plane, it will be neceſſary, in order to determine its magnitude, to advert to the aperture admitting the rays. If the aperture be ſuppoſed to be infinitely ſmall, it may be conſidered as a radiant point, in order to find the direction of the emergent rays."

ſo adapted, that the magnitude of the confuſed image may remain perfectly conſtant. Indeed, to calculate from the dimenſions which we have hitherto uſed, it would be expected that the image ſhould be diminiſhed about one-fortieth, by the utmoſt increaſe of the convexity of the lens. But the whole depends on the ſituation of the refracting ſurfaces, and the reſpective increaſe of their curvature, which, on account of the variable denſity of the lens, can ſcarcely be eſtimated with ſufficient accuracy. Had the pupil been placed before the cornea, the magnitude of the image muſt, on any ſuppoſition, have been very variable: at preſent, this inconvenience is avoided by the ſituation of the pupil; ſo that we have here an additional inſtance of the perfection of this admirable organ.

From the experiments related, it appears to be highly improbable that any material change in the length of the axis actually takes place; and it is almoſt impoſſible to conceive by what power ſuch a change could be effected. The ſtraight muſcles, with the adipoſe ſubſtance lying under them, would certainly, when acting independently of the ſocket, tend to flatten the eye: for, ſince their contraction would neceſſarily leſſen the circumference or ſuperficies of the maſs that they contain, and round off all its prominences, their attachment about the nerve and the anterior part of the eye muſt therefore be brought nearer together. (Plate XV. Fig. 21, 22.) Dr. Olbers compares the muſcles and the eye to a cone, of which the ſides are protruded, and would by contraction be brought into a ſtraight line. But this would require a force to preſerve the cornea as a fixed point, at a given diſtance from the origin of the muſcles; a force which certainly does not exiſt. In the natural ſituation of the viſual axis, the orbit being conical, the eye might be ſomewhat lengthened, although irregularly, by being forced further into it; but, when turned towards either ſide, the ſame action would rather ſhorten its axis; nor is there any thing about the human eye that could ſupply its place. In quadrupeds, the oblique muſcles are wider than in man; and in many ſituations might aſſiſt in the effect. Indeed a portion of the orbicular muſcle of the globe is attached ſo near to the nerve, that it might alſo cooperate in the action: and I have no reaſon to doubt the accuracy of Dr. Olbers, who ſtates, that he effected a conſiderable elongation, by tying threads to the muſcles, in the eyes of hogs and of calves; yet he does not ſay in what poſition the axis was fixed; and the flaccidity of the eye after death might render ſuch a change very eaſy as would be impoſſible in a living eye. Dr. Olbers alſo mentions an obſervation of Profeſſor Wriſberg, on the eye of a man whom he believed to be deſtitute of the power of accommodation in his life-time, and whom he found, after death, to have wanted one or more of the muſcles: but this want of accommodation was not at all accurately aſcertained. I meaſured, in the human eye, the diſtance of the attachment of the inferior oblique muſcle from the inſertion of the nerve: it was one-fifth of an inch; and from the centre of viſion not a tenth of an inch; ſo that, although the oblique muſcles do in ſome poſitions nearly form a part of a great circle round the eye, their action would be more fitted to flatten than to elongate it. We have therefore reaſon to agree with Winſlow, in attributing to them the

the office of helping to ſupport the eye on that ſide where the bones are moſt deficient: they ſeem alſo well calculated to prevent its being drawn too much backwards by the action of the ſtraight muſcles. And, even if there were no difficulty in ſuppoſing the muſcles to elongate the eye in every poſition, yet at leaſt ſome ſmall difference would be expected in the extent of the change, when the eye is in different ſituations, at an interval of more than a right angle from each other; but the optometer ſhews that there is none.

Dr. Hofack alleges that he was able, by making a preſſure on the eye, to accommodate it to a nearer object:* it does not appear that he made uſe of very accurate means of aſcertaining the fact; but, if ſuch an effect took place, the cauſe muſt have been an inflection of the cornea.

It is unneceſſary to dwell on the opinion which ſuppoſes a joint operation, of changes in the curvature of the cornea and in the length of the axis. This opinion had derived very great reſpectability, from the moſt ingenious and elegant manner in which Dr. Olbers had treated it, and from being the laſt reſult of the inveſtigation of Mr. Home and Mr. Ramſden. But either of the ſeries of experiments which have been related, appears to be ſufficient to confute it.

X. It now remains to inquire into the pretenſions of the cryſtalline lens to the power of altering the focal length of the eye. The grand objection to the efficacy of a change of figure in the lens, was derived from the experiments in which thoſe who have been deprived of it have appeared to poſſeſs the faculty of accommodation.

My friend Mr. Ware, convinced as he was of the neatneſs and accuracy of the experiments related in the Croonian Lecture for 1795, yet could not ſtill help imagining, from the obvious advantage all his patients found, after the extraction of the lens, in uſing two kinds of ſpectacles, that there muſt, in ſuch caſes, be a deficiency in that faculty. This circumſtance, combined with a conſideration of the directions very judiciouſly given by Dr. Porterfield, for aſcertaining the point in queſtion, firſt made me wiſh to repeat the experiments upon various individuals, and with the inſtrument which I have above deſcribed as an improvement of Dr. Porterfield's optometer: and I muſt here acknowledge my great obligation to Mr. Ware, for the readineſs and liberality with which he introduced me to ſuch of his numerous patients as he thought moſt likely to furniſh a ſatisfactory determination. It is unneceſſary to enumerate every particular experiment; but the univerſal reſult is, contrary to the expectation with which I entered on the inquiry, that in an eye deprived of the cryſtalline lens, the actual focal diſtance is totally unchangeable. This will appear from a ſelection of the moſt deciſive obſervations.

1. Mr. R. can read at four inches and at ſix only, with the ſame glaſs. He ſaw the double lines meeting at three inches, and always at the ſame point; but the cornea was ſomewhat irregularly prominent, and his viſion not very diſtinct; nor had I, at the time I ſaw him, a convenient apparatus.

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* Phil. Tranſ. for 1794, p. 212.

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I afterwards provided a ſmall optometer, with a lens of leſs than two inches focus, adding a ſeries of letters, not in alphabetical order, and projected into ſuch a form as to be moſt legible at a ſmall inclination. The exceſs of the magnifying power had the advantage of making the lines more divergent, and their croſſing more conſpicuous; and the letters ſerved for more readily naming the diſtance of the interſection, and, at the ſame time, for judging of the extent of the power of diſtinguiſhing objects too near or too remote for perfect viſion. (Plate XV. Fig. 23.)

2. Mr. J. had not an eye very proper for the experiment; but he appeared to diſtinguiſh the letters at 2½ inches, and at leſs than an inch. This at firſt perſuaded me, that he muſt have a power of changing the focal diſtance: but I afterwards recollected that he had withdrawn his eye conſiderably, to look at the nearer letters, and had alſo partly cloſed his eyelids, no doubt contracting at the ſame time the aperture of the pupil; an action which, even in a perfect eye, always accompanies the change of focus. The ſlider was not applied.

3. Miſs H. a young lady of about twenty, had a very narrow pupil, and I had not an opportunity of trying the ſmall optometer: but, when ſhe once ſaw an object double through the ſlits, no exertion could make it appear ſingle at the ſame diſtance. She uſed for diſtant objects a glaſs of 4½ inches focus; with this ſhe could read as far off as 12 inches, and as near as five: for nearer objects ſhe added another of equal focus, and could then read at 7 inches, and at 2½.

4. Hanſon, a carpenter, aged 63, had a cataract extracted a few years ſince from one eye: the pupil was clear and large, and he ſaw well to work with a lens of 2⅜ inches focus; and could read at 8 and at 15 inches, but moſt conveniently at 11. With the ſame glaſs, the lines of the optometer appeared always to meet at 11 inches; but he could not perceive that they croſſed, the line being too ſtrong, and the interſection too diſtant. The experiment was afterwards repeated with the ſmall optometer: he read the letters from 2 to 3 inches; but the interſection was always at 2½ inches. He now fully underſtood the circumſtances that were to be noticed, and ſaw the croſſing with perfect diſtinctneſs: at one time, he ſaid it was a tenth of an inch nearer; but I obſerved that he had removed his eye two or three tenths from the glaſs, a circumſtance which accounted for this ſmall difference.

5. Notwithſtanding Hanſon's age, I conſider him as a very fair ſubject for the experiment. But a ſtill more unexceptionable eye was that of Mrs. Maberly. She is about 30, and had the cryſtalline of both eyes extracted a few years ſince, but ſees beſt with her right. She walks without glaſſes; and, with the aſſiſtance of a lens of about four inches focus, can read and work with eaſe. She could diſtinguiſh the letters of the ſmall optometer from an inch to 2½ inches; but the interſection was invariably at the ſame point, about 19 tenths of an inch diſtant. A portion of the capſule is ſtretched acroſs the pupil, and cauſes her to ſee remote objects double, when without her glaſſes; nor can ſhe, by any

any exertion, bring the two images nearer together, although the exertion makes them more diſtinct, no doubt by contracting the pupil. The experiment with the optometer was conducted, in the preſence of Mr. Ware, with patience and perſeverance; nor was any opinion given to make her report partial.

Conſidering the difficulty of finding an eye perfectly ſuitable for the experiments, theſe proofs may be deemed tolerably ſatisfactory. But, ſince one poſitive argument will counterbalance many negative ones, provided it be equally grounded on fact, it becomes neceſſary to inquire into the competency of the evidence employed to aſcertain the power of accommodation attributed, in the Croonian Lecture for 1794, to the eye of Benjamin Clerk. And it appears, that the diſtinction long ſince very properly made by Dr. Jurin, between diſtinct viſion and perfect viſion, will readily explain away the whole of that evidence.

It is obvious that viſion may be made diſtinct to any given extent, by means of an aperture ſufficiently ſmall, provided at the ſame time, that a ſufficient quantity of light be left, while the refractive powers of the eye remain unchanged. And it is remarkable, that in thoſe experiments, when the compariſon with the perfect eye was made, the aperture of the imperfect eye only was very conſiderably reduced. Benjamin Clerk, with an aperture of $\frac{3}{40}$ of an inch, could read with the ſame glaſs at $1\frac{7}{8}$ inch, and at 7 inches.[*] With an equal aperture, I can read at $1\frac{1}{2}$ inch and at 30 inches: and I can retain the ſtate of perfect relaxation, and read with the ſame aperture at $2\frac{1}{4}$ inches; and this is as great a difference as was obſerved in Benjamin's Clerk's eye. It is alſo a fact of no ſmall importance, that Sir Henry Englefield was much aſtoniſhed, as well as the other obſervers, at the accuracy with which the man's eye was adjuſted to the ſame diſtance, in the repeated trials that were made with it.[†] This circumſtance alone makes it highly probable, that its perfect viſion was confined within very narrow limits.

(To be continued.)

IV.

Deſcription of a Support applicable to Balances of all Dimenſions, and proper to render the Operations performed with theſe Inſtruments more expeditious and convenient without affecting their Preciſion.[‡] *By* R. Prony, *of the National Inſtitute of France.*

SOME experiments which I was engaged in during the courſe of laſt winter obliged me to contrive a ſupport, to which the beam of any balance whatever might be applied without diſtinction, whether provided with its frame of ſupport or not, and which, without de-

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[*] Phil. Tranſ. for 1795, p. 9.

[†] Phil. Tranſ. for 1795, p. 8.

[‡] Annales de Chimie, XXXVI. 50.

priving it in any reſpect of its preciſion, ſhould afford a convenient means of raiſing and lowering it at pleaſure. It is well known how troubleſome it is to weigh by balances held in the hand, and this trouble is frequently by no means the ſmalleſt inconvenience attending that method.

Several artiſts have invented ſupports, convenient in their uſe, and ingenious in their contrivance; but each of theſe is adapted only to a ſingle balance, and increaſes the price in ſuch a manner as to put it out of the convenience of moſt philoſophers and artiſts. I hope I ſhall therefore give them pleaſure by publiſhing, in conſequence of the invitation of ſeveral celebrated chemiſts, the deſcription of a ſupport, which, to the advantage of being univerſal, adds that of cheapneſs and eaſy execution, whether in wood or in metal.

It was my intention to have added to this deſcription ſome views reſpecting the theory and conſtruction of balances, from which I apprehend that artiſts acquainted with the elements of geography and calculation might derive ſome advantage in the improvement of machines for weighing; but as this part of my work is more extended than I at firſt thought, I ſhall publiſh it ſeparately.

A triangular foot of braſs, A *a*, *a*, *a*,, (Plate XVI. Fig. 1 & 2) has its three extremities, *a*, *a''* *a'''*, ſolidly ſcrewed to a table or horizontal plane. In the part A of this foot is ſcrewed a cylindrical ſtem A B, of any convenient length. It may be proper to have two of different lengths, in order to exchange them when very large balances are uſed. Thoſe which I have uſed are of half a metre and a metre in length.

A vertical pulley is placed at the top of the rod A B, ſo that the ſame vertical plane paſſes through the axis of the beam and the horizontal axis of the pulley. The ſupport C D of this pulley has at its lower part a cylindrical bore, into which the upper extremity of the rod A B enters with an eaſy friction; and a thumb-ſcrew E ſerves by its preſſure to keep the pulley in its fixed poſition.

Another pulley P is fixed below the rod A B, ſo that the tangent line to the pullies P *p* is parallel to the axis of the rod A B.

A ſtring K *t p* H G P F, at the end of which is ſuſpended, beyond the edge of the table K, a ſmall weight, *k* paſſes through a hole *t*, made in the foot *a''*, thence over the pullies *p* and P, and is faſtened at F to the upper part of a piece *m m'* *n q*, which is forked, and to which are ſuſpended, as I ſhall proceed to explain, the balance, the weights, and the objects to be weighed. F *m* is a button, which, being ſcrewed to the top of the fork, receives the ſtring through a hole, in which it remains by virtue of a knot made in its extremity.

The tail, or lower part of this fork, is priſmatic in its part *m' n*; and this priſmatic part enters into a notch *f f* made in the extremity N of the horizontal piece N O, in which it can freely ſlide upwards and downwards through a limited ſpace, its aſcent being ſtopped by the enlargement of the fork below, and above by the enlargement occaſioned by the ſeparation of the two branches of the fork.

The piece N O is perforated, and may be ſlided up and down the ſtem A B, and alſo moved horizontally to the right and left. When it has the proper height, it is fixed, by means of the preſſing ſcrew V, that is to ſay, 1ſt. when its height is ſuch that the ſtop *m'* reſts on the edge of the notch *f f*, or when N O, being at its loweſt limit, the diſhes of the balance are in contact with the table or horizontal plane, ſo that it may be raiſed afterwards through the whole height *f n*; and, 2dly, that the ſtring F F ſhall be in the ſame vertical plane with H G.

The notch in N muſt be ſo placed that the axis of the priſmatic part of the tail of the fork and the ſtring F F″ ſhall be always in the ſame vertical line parallel to the axis of the ſtem A B.

Theſe arrangements being underſtood, let us imagine the two branches *n q* of the fork to be drilled through with holes of different ſizes, to receive the horizontal pins *g g* of different diameters, and we ſhall poſſeſs an apparatus quite adequate to weighing in the air with balances which have their beams ſupported between the uſual frame and rings.

Whatever, in fact, may be the ſize of the balance intended to be uſed, the extremity of its clip, or frame, is to be introduced between the fork *n q*, and a pin of ſuch a ſize as to enter very freely into the hole at the top of the clip muſt be paſſed through; after which the piece O N is to be placed, ſo that the conditions before mentioned with regard to this piece ſhall be obtained; and in this poſition it is to be fixed with the thumb-ſcrew V. This operation being made, the diſhes of the balance are to be loaded, which, being in contact with the table or horizontal plane, cannot move. The ſmall ball *k* being then taken in the hand, the ſtring muſt be drawn ſo that the balance ſhall riſe very ſlowly. If the ſcales be not in equilibrium, the beam muſt be ſuffered to deſcend, and the proceſs of weighing be completed by altering the load as uſual.

A counterpoiſe Q, ſuſpended on the ſtring F G, forms an equilibrium to the weight of the balance. By means of this precaution, the reſult of all the efforts which ſupport the pulley P paſſes in all caſes through the axis of the rod A B, which, conſequently, will have no diſpoſition to bend or incline.

But if it be propoſed to uſe an hydroſtatical balance, a ſmall table O′ N, Fig. 3, is to be adapted to the rod A B, which, by means of a cylindrical perforation in O′, can ſlide up and down the rod, and be fixed at any required height by the thumb-ſcrew V′. Another piece, or little table, K′ K, is placed upon V N, ſo that the holes T correſpond to the centre of the diſhes, below which are placed the hooks intended to hold the body ſuſpended in water, and K′ K′ is fixed upon V′ N′ by means of the ſcrew V′.

This adjuſtment being prepared, the piece N O is to be placed on the table K′ K′, ſo that, 1ſt. the whole height of the balance ſhall be comprehended between this piece and the ſmall table, and the diſhes ſhall be contact with the ſame, their centers correſponding with the openings made in T; 2d. that K K′ ſhall be ſufficiently raiſed to admit of the veſſels W W of water being placed beneath, ſo as conveniently to immerſe the body intended to be weighed in one of theſe veſſels.

Theſe

Theſe bodies are uſually ſuſpended by a very thin metallic wire; but, by placing two veſſels as I have done, and ſuſpending to each of the baſons a wire of metal of the ſame diameter, one to ſupport the body, and the other to be ſimply immerſed in the water, the magnitude of the diameter will have no influence on the accuracy of the experiment. In fact, if we ſuppoſe the machine to be ſo adjuſted that at the commencement of the operation the diſhes ſhall be in equilibrio (which is eaſily effected by varying the height of the water in the veſſels), theſe two wires will continue to form an equilibrium, when the beam F F″ being raiſed ſtill preſerves its horizontal poſition. Whence it follows that if to one of the wires there be ſuſpended an immerſed body, and in the oppoſite baſon there be placed out of the water a weight capable of maintaining the equilibrium, that equilibrium will continue, whatever may be the elevation or depreſſion of the whole balance, provided it continues horizontal. For the lengths of the wires raiſed out of the water being equal, the difference between the ſpecific gravities of the water and the metal will have the ſame influence on the two extremities of the beam. It is evident that the ſame advantage could not be had by employing only one wire; namely, that which ſupports the ſuſpended body, and that the equilibrium correſponding with a certain elevation, and the horizontal poſition of the beam, will not agree with other elevations and the ſame horizontal poſition.

It muſt be obſerved that my proceſs not only compenſates for the exceſs of ſpecific gravity of the wires beyond that of the water, but alſo for the adheſion of the fluid and that portion which hangs round the wires, and is raiſed with them.

The whole of the preceding account is applicable to balances ſuſpended in the uſual clip, or forked piece, terminating in rings for ſupporting the fulcrum. But in order to render my ſupport abſolutely univerſal, it muſt be adapted to a beam alone, the knife edges of which ſhall find a proper place to act upon: and in this I have ſucceeded, by contriving a forked piece, which hangs from the fork *n q*, and is capable of receiving the knife edges of any beam whatever. In the drawing this piece is properly placed in the machine, and its conſtruction is as follows:

A piece S *s*′ (fig. 2.) is pierced at *s*′ by a tapped hole, into which the ſcrew *d d* enters to half its length. Another hole made in S, perpendicular to the former, receives the pin *g g*, (fig. 1.) which ſuſpends the whole lower apparatus, and anſwers to the hole in the upper part of the fork of common balances.

Two other vertical pieces *r r*, Fig. 4, have at their upper part holes not tapped, in which the ſcrew *d d* may freely move. Theſe upper ends are to be placed at an arbitrary diſtance, where they are retained, and fixed by four ſcrew-nuts *u*, each piece being thus confined between two ſcrews. A cylindrical rod *h h* paſſes through the lower extremities of theſe pieces *r r*, and is fixed by preſſing ſcrews; ſo that the upper and lower extremities of thoſe pieces are invariably at the ſame diſtance.

Each of theſe pieces *r r* has on its face, which is perpendicular to the direction of *d d* and *h h*, a long opening *f f*, and a circular perforation X, (fig. 1. and 5.) the lower part of which is armed with a ſmall face of poliſhed ſteel, upon which the edge of the fulcrum reſts;

reſts; in the upper part of the openings *f f*, a rule *e′ e′* is introduced by tight friction, which, together with the pieces *d d* and *h h*, give to the frame ſuch a degree of ſolidity, that there is not the leaſt ſhake or play in any of its parts. That part of its opening which is not occupied by *e′ e′* muſt be rather longer than the longeſt index of the beams intended to be uſed.

The uſe of the frame or fork of ſupport, here deſcribed, is extremely ſimple. The beam is to be placed between the two branches *r r*, which are to be ſeparated till the knife edges are oppoſite the circular openings X, when they are again to be brought together, ſo as to include that fulcrum, leaving a little play between the ſupports, in order that the vibration may be perfectly free. The paralleliſm of the pieces *r r*, and the perfect ſolidity of their connection, is then to be effected by means of the ſcrew-nuts, the ſmall cylindric rod *h h*, and the rule *e e*. When theſe diſpoſitions are made, the whole apparatus is to be ſuſpended by the pin *g g*, in the fork *s*, and the balance is fit for uſe. The equilibrium, or horizontal poſition of the beam, is known by the poſition of the index *y y* in the middle of the opening *e e*; or, to render this eſtimate more exact, I have ſuſpended to the rule *e e* a plumb-line *e i*, by which the ſlighteſt deviations from the perpendicular may be ſeen.

Such is the deſcription and uſe of my ſupport, which in practice has perfectly anſwered my expectation. The inſtrument I poſſeſs is entirely of braſs, made by Citizen Dumotier, an artiſt well known for the conſtruction of philoſophical inſtruments. If the fork, and ſome of the nicer parts, be made of metal, the reſt may be advantageouſly formed of wood.

V.

On the Combinations of the Metals with Sulphur. By the CITIZEN VAUQUELIN.

THE combinations of the metals with ſulphur may be divided into three orders, namely,

1. The metals and ſulphur. Theſe are combinations, which properly ſpeaking, ought to be called metallic ſulphurets, or ſulphurated metals; nature and art afford many ſpecimens of theſe.

2. Metallic oxides and ſulphur. Theſe ought to be called ſulphurated metallic oxides, or oxided metallic ſulphurets. They are alſo very common in the native ſtate.

3. Metallic oxides with ſulphur and hidrogen. Theſe triple combinations are named hidro-ſulphurated metallic oxides.

If the predominating ſubſtance in theſe different combinations be taken as the baſis of the nomenclature, it would be neceſſary to reverſe the order of the common denominations; for in the ſimple ſulphurets, the metal always predominates as to its maſs; the ſame is alſo the caſe in the ſulphurated oxides; and in the hidro-ſulphurets the predominance is firſt in the metals, then in the ſulphur, and laſtly, in the hidrogen.

Rr 2 So

So that inſtead of metallic ſulphurets, oxided metallic ſulphurets, and metallic hidroſulphurets; we ought to ſay ſulphurated metals, ſulphurated metallic oxides, and ſulphurohidrogenated metallic oxides.

But uſage has prevailed in chemiſtry, whether from facility of pronunciation, or the greater ſmoothneſs of the terms. However this is of no great importance, and if the elements of bodies be clearly underſtood from the terms, it will be ſufficient.

Concerning the action of the acids upon the metallic ſulphurets.

SULPHURIC ACID.

Whenever the ſum of the affinities of oxigen for the metallic ſubſtance, and of the oxide thus formed for the ſulphuric acid, is greater than that of the metal for ſulphur and of oxigen for the ſulphureous acid, there will conſtantly be a decompoſitian of the metallic ſulphuret, and the reſult will be a metallic ſulphate and ſulphureous acid. The whole of theſe conditions are not always neceſſary: but each concurs in the accompliſhment of the new combinations which are formed, and accelerates the operation. There are nevertheleſs caſes, in which for want of one of theſe conditions the bodies do not act at all upon each other.

The ſulphuric acid for example, decompoſes the natural ſulphurated oxide of iron or martial pyrites very well; but the muriatic acid cannot decompoſe it, becauſe the ſulphur in this caſe is not attracted by any body, whereas in the former it is attracted by the ſulphuric acid, which it converts into ſulphureous acid; for though I have found by many facts, that the iron in martial pyrites is oxided nearly to the point in which it is eaſily ſoluble in acids, it cannot however be decompoſed without forming a great quantity of ſulphuric acid.

This opinion is confirmed by the conditions neceſſary for completing the operation. In fact, the complete decompoſition of pyrites cannot be effected without heat, and a great quantity of concentrated ſulphuric acid. For it is evident here, that acid is not only required for the ſaturation of the oxide of iron, but alſo for that of the ſulphur. I could likewiſe urge in ſupport of the ſame opinion, the greater affinity of the muriatic acid for the oxide of iron, which nevertheleſs it is incapable of ſeparating from the ſulphur under theſe circumſtances. It may ſeem ſtrange that the nitric acid, of which the affinity to iron is leſs ſtrong, ſhould nevertheleſs decompoſe the martial pyrites with ſo much facility; but we ought not to deceive ourſelves by ſuppoſing, that it is by a ſtronger affinity that this acid takes the oxide of iron from ſulphur; for it is on the contrary, by affording a new quantity of oxigen to the iron that the affinity of the metal for the ſulphur becomes ſingularly diminiſhed. In reality, ſulphur does not unite with iron oxided to the maximum, and when by certain peculiar proceſſes theſe two ſubſtances are combined, the effect always happens from a ſeparation of part of the oxigen.

Though the muriatic acid cannot decompoſe the ſulphurated oxide of iron, it decompoſes on the contrary the ſulphuret of iron, in which the metal is not oxigenated, with the greateſt facility. But in this caſe three forces act at once—namely, that of the iron on the

the oxigen of the water, that of the ſulphur on the hidrogen of the ſame ſubſtance, and laſtly, that of the metallic oxide formed, for the muriatic acid. In this circumſtance ſulphurated hidrogen gas is conſtantly diſengaged.

The ſulphuric acid which does not decompoſe the ſulphurated oxide of iron, unleſs it be concentrated and boiling, decompoſes the ſulphuret of iron without heat, and the more readily when it is diluted with a certain quantity of water. The reaſon of this evidently requires no explanation.

Though a great quantity of water oppoſes the action of the acids on the metallic ſulphurets in general, nevertheleſs it is leſs noxious with reſpect to that of iron than with regard to the others, becauſe the ſulphurated hidrogen does not decompoſe the ſolutions of iron in the acids.

Concerning the Sulphuret of Lead.

In the natural ſulphuret of lead or galena, lead exiſts in the metallic ſtate; for which reaſon, when this mineral ſubſtance is brought into contact with the muriatic acid, ſulphurated hidrogen is formed even in the cold. It appears even when the ſulphur is united to the oxide of lead; this metal loſes its oxigen in order to unite with the ſulphur, for we obtain a matter perfectly ſimilar to natural galena. The manner in which the reduction of lead is here effected may be eaſily underſtood. A portion of ſulphur combines with the oxigen and forms ſulphuric acid, which the heat volatilizes, and another portion unites with the metallic lead. The reduction of lead and the formation of galena take place nearly in the ſame manner, when a ſolution of this metal in an acid is mixed with a ſolution of ſulphurated hidrogen, or of any hidro-ſulphuret whatever. There is nevertheleſs this difference, namely, that in this caſe the hidrogen takes the oxigen from the lead, and forms water at the ſame time that the ſulphur and the metal, which meet in a ſtate of extreme diviſion, combine without heat in the midſt of the fluid. There is alſo another difference, that the galena formed by this proceſs contains rather more ſulphur than the natural compound, as is proved by the ſmall quantity of ſulphur which ſeparates from this ſubſtance at a very gentle heat, and then leaves it abſolutely in the ſame ſtate as common galena.

From theſe facts it follows that the muriatic acid, and probably all other acids act on the oxide of lead by their maſs and their concentration; that when theſe two forces, or even one of them is deſtroyed, or ſimply weakened, the ſulphurated hidrogen deprives them of the lead; but whatever may be the quantity and concentration of the acid united with the lead or other metals, the decompoſition of the ſalt will take place if an hidroſulphuret be employed in ſufficient quantity.

It is not ſingular that the affinities of the acids for the metallic oxides ſhould be weakened by water, nor that their action on theſe ſubſtances ſhould be more energetic by virtue of their maſſes. Antimony, mercury, biſmuth, and lead itſelf, afford ſtriking examples of this.

It is known, in fact, that when a large quantity of water is added to their concentrated ſolutions, very conſiderable precipitates are formed, which have been miſtakenly ſuppoſed to be pure metallic oxides, but which are really ſalts, containing a portion of acid ſtrongly adherent, that cannot be completely ſeparated but by means of an alkali.

So that we may conſider the combinations formed by theſe metals, and probably many others alſo with the acids, as neutral ſalts diſſolved by a ſuper-abundance of acid, which is taken from them by the water, and conſequently the ſalts fall down truly neutral and inſoluble.

From the manner in which the muriatic acid acts on the metallic ſulphurets, it follows, that the more the metals which compoſe them abſorb of oxigen to unite with this acid, the leſs there remains of ſulphur after the ſolution, and *vice verſa;* but if theſe metals abſorb little oxigen, if they contain much ſulphur, and if alſo their affinity for oxigen be inconſiderable, the totality of the metal will never be ſeparated from the ſulphur by the muriatic acid, becauſe in proportion as this combuſtible body becomes more abundant, it oppoſes by its maſs the ſolution of the metal, and hence it is that certain metallic ſulphurets which are not attacked by the muriatic acid, yield to that force, after a portion of their ſulphur has been ſeparated by diſtillation.

The mode of action of the nitric acid on the metallic ſulphurets is very different, and though the ſulphur is not in this caſe attracted by any ſubſtance, nevertheleſs it is completely ſeparated from the metal, and falls down perfectly pure if the operation have been properly performed; but though the combination of the nitric acid with the metal is not aſſiſted by hidrogen, as happens with regard to the muriatic acid, yet this firſt acid yields oxigen to the metal more eaſily than water; and it is by virtue of this affinity of oxigen for the metal and the oxide which is formed for the undecompoſed acid, that the ſolution is effected. So that in all the inſtances when metallic ſulphurets are decompoſed by the nitric acid, there is conſtantly a formation of nitrous gas, and never of ſulphurated hidrogen.

It appears in general, that ſulphur has a ſtronger affinity with the metals than with their oxides, and that this affinity diminiſhes in proportion to the quantity of oxigen they abſorb. There are even ſome which do not unite with ſulphur but in the complete metallic ſtate. Thus for example, by triturating the brown oxide of lead with ſulphur, a portion of the ſulphur takes fire, and the other portion combines with the lead thus reduced to the metallic ſtate.

It appears alſo that zinc does not unite with ſulphur when it is completely metallic, or at leaſt that nature has not yet preſented inſtances of this, nor has art ſucceeded in combining theſe two ſubſtances. That which is known in mineralogy by the name of Blende, or ſulphurated zinc, has been miſtakenly regarded by ſome mineralogiſts as a combination of zinc with ſulphur. It is on the contrary a ſulphurated oxide of zinc, which very frequently contains hidrogen; for the blendes are almoſt always ſoluble in acids, and produce ſulphurated

rated hidrogen gas. A tranſparent blende which acts in this manner with the acids, muſt therefore be conſidered as the true hidro-ſulphurated oxide of zinc, or ſulphuro-hidrogenated oxide. There may exiſt oxides of zinc ſimply ſulphurated, and there would then be two ſpecies of ores very different, one ſoluble in the ſulphuric and muriatic acids without effervesſcence, and the other on the contrary affording a greater or leſs quantity of ſulphurated hidrogen gas.

Metallic mercury does not unite very intimately with ſulphur. Mineral ethiops appear rather to be a mixture than a true combination, but when this oxided metal is combined with ſulphur, as for example in cinnabar, it adheres ſo ſtrongly that no ſubſtance can directly decompoſe it, and the ſeparation can never be effected but by altering one or other of theſe principles. It is either neceſſary to burn the ſulphur, or dis-oxide the mercury. Cinnabar owes its fine red colour to the great quantity of oxigen which the mercury contains.

VI.

Notice reſpecting the extenſive Logarithmic and Trigonometrical Tables, calculated under the Direction of CITIZEN PRONY, *Member of the National Inſtitute, &c. and of the Report concerning the ſame. By Citizens La Grange, Laplace, and Delambre.*

WHEN in the year 2 of the Republic, C. Prony was charged to calculate the new tables of ſines, tangents, &c. and their logarithms as required for the decimal diviſion of the quadrant, he was engaged *not only to compoſe tables which ſhould leave nothing to be deſired with regard to accuracy, but to make them the moſt extended and moſt ſtriking monument of calculation which had ever been executed, or even imagined.*

Cit. Prony applied to the execution of this grand enterprize ſuggeſted by Citizens Carnot, Prieur de la Côte d'Or, Brunet de Montpellier, which required to be finiſhed in a ſhort time, the principle of the diviſion of labour by means of which the perfection of workmanſhip, with the ſaving of expence and time, are obtained in the arts. This diviſion, which renders the moſt unſkilful hands profitable in manual works, permits the uſeful employ of men of very limited powers in operations, which on the whole are delicate, but abound with ſimple and minute details. It is much to be wiſhed, that this expedient might continue to be uſed for the perfection and extent of great ſcientific works, of which the plan cannot be underſtood but by men of a ſuperior order, but of which the execution prodigiouſly exceeds their phyſical power. Such for example, are the numerical developement of formulæ, which give the perturbation ariſing from the mutual attraction of different bodies of the planetary ſyſtem, and the reduction of the aſtronomical obſervations proper to determine the elements of the orbits of theſe bodies.

Citizen Prony divided his aſſiſtants into three ſections.

The firſt was compoſed of five or ſix mathematicians of very great merit, who were employed on the analytical part of the work, and in general on the application of the method of differences to the formation of tables, the calculation of various fundamental numbers, &c. Citizen Legendre, who for ſome time belonged to this ſection, gave very elegant formulæ for determining the ſucceſſive differences of the ſines *a priori*.

The ſecond ſection conſiſted of calculators to whom the analyſis was familiar. They directly calculated the differences of the ſeveral orders neceſſary to form by ſucceſſive additions, according to the method of interpolation, the values of the trigonometric lines departing from a given firſt value through the whole interval, wherein the differences of the moſt elevated order might, without ſenſible error, be conſidered as conſtant.

The filling of theſe intervals was reduced to ſimple addition performed by the third ſection, the papers of which were afterwards tranſmitted for examination by the ſecond.

The calculators of each ſection formed two diviſions, each of which performed the whole work without communication with the other. The compariſon of the reſults thus obtained afforded the verification.

By theſe means two manuſcript copies of the whole of the table was formed, compoſed of ſeventeen volumes large folio, and containing,

1. An introduction, conſiſting of an expoſition of the analytical formulæ, the uſe of the trigonometrical table, and a great number of particular and auxiliary tables.

2. The natural ſines for each for 10,000th part of the quadrant, calculated to twenty-five places of decimals, with ſeven or eight columns of differences, to be publiſhed with twenty-two decimals, and five columns of differences.

3. The logarithms of the ſines for each 100,000th of the quadrant, calculated to fourteen decimals with five columns of differences.

4. The logarithms of the ratios of the ſines to the arcs for the firſt five thouſand 100,000th part of the quadrant, calculated to fourteen decimals, with three columns of differences.

5. The logarithms of the tangents correſponding with the logarithm of the ſines.

6. The logarithms of the ratios of the tangents to the arcs calculated like thoſe of the fourth article.

7. Logarithms of numbers from 1 to 100,000, calculated to nineteen places of decimals.

8. The logarithms of 100,000 to 200,000, calculated to twenty-four decimals, in order to be publiſhed to twelve decimals, and three columns of differences.

This explanation ſhews how far ſuperior this work is to the *Opus Palatinum de Triangulis*, began by Rheticus, the diſciple of Copernicus, and compleated in 1596 by Otho, the diſciple of Rheticus, or to the Theſaurus Mathematicus, publiſhed by Pitiſcus in 1613, and the laſt tables of Vlacq. This was the judgment of the Commiſſaries, which the Claſs of the Phyſical and Mathematical Societies of the National Inſtitute nominated to give an account of of this work. It is much to be wiſhed, that the government would reſume the impreſſion of this immenſe work, which was ſuſpended at the fall of the aſſignats.

To the notice, of which we have given an account, Cit. Prony has added a bibliographic anecdote reſpecting the *Opus Palatinum*, very intereſting to thoſe who ſeek rare books. They will learn with pleaſure, that there are a few copies of this book, in which 86 pages have been reprinted, containing the co-tangents and co-ſecants of the ſeven firſt degrees, of which the laſt figures were uncertain in the firſt copies, and which Pitiſcus could not correct, but by carrying the ſines of theſe degrees to twenty-two decimals, which had been calculated only to fifteen.

This note is followed by the report of Citizens Lagrange, Laplace, and de Lambre, reſpecting theſe tables, in which we find various intereſting details on the conſtruction of trigonometrical tables in general, and the methods employed by one of the Commiſſaries to verify thoſe which were ſubmitted to them.

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VII.

Obſervations relative to the Means of increaſing the Quantities of Heat obtained in the Combuſtion of Fuel. By COUNT RUMFORD [*].

It is a fact which has been long known, that clays, and ſeveral other incombuſtible ſubſtances, when mixed with ſea coal, in certain proportions, cauſe the coal to give out more heat in its combuſtion, than it can be made to produce when it is burned pure or unmixed; but the cauſe of this increaſe of heat does not appear to have been yet inveſtigated with that attention which ſo extraordinary and important a circumſtance ſeems to demand.

Daily experience teaches us that all bodies,—thoſe which are incombuſtible,—as well as thoſe which are combuſtible, and actually burning, throw off in all directions heat, or rather calorific (heat-making) rays, which generate heat wherever they are ſtopped, or abſorbed; but common obſervation was hardly ſufficient to ſhew any perceptible difference between the quantities of calorific rays thrown off by different bodies, when heated to the ſame temperature, or expoſed in the ſame fire; although the quantities ſo thrown off, might be, and probably are, very different.

It has lately been aſcertained, that when the ſides and back of an open chimney fireplace, in which coals are burned, are compoſed of fire-bricks, and heated red-hot, they throw off into the room incomparably more heat than all the coals that could poſſibly be put into the grate, even ſuppoſing them to burn with the greateſt poſſible degree of brightneſs.

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[*] Journal of the Royal Inſtitution, p. 28.

neſs. Hence it appears that a red-hot burning coal does not ſend off near ſo many calorific rays as a piece of redhot brick or ſtone, of the ſame form and dimenſions; and this intereſting diſcovery will enable us to make very important improvements in the conſtruction of our fire-places, and alſo in the management of our fires.

The fuel inſtead of being employed to heat the room *directly*, or by the direct rays from the fire, ſhould be ſo diſpoſed, or placed, as *to heat the back and ſides of the grate;* which muſt always be conſtructed of fire-brick, or fire-ſtone, and *never of iron, or of any other metal.* Few coals, therefore, when properly placed, make a much better fire than a larger quantity; and ſhallow grates, when they are conſtructed of proper materials, throw more heat into a room, and with a much leſs conſumption of fuel, than deep grates; for a large maſs of coals in the grate arreſts the rays which proceed from the back and ſides of the grate, and prevents their coming into the room; or, as fires are generally managed, it prevents the back and ſides of the grate from ever being ſufficiently heated to aſſiſt much in heating the room, even though they be conſtructed of good materials, and large quantities of coals be conſumed in them.

It is poſſible, however, by a ſimple contrivance, to make a good and an economical fire in almoſt any grate, though it would always be adviſable to conſtruct fire-places on good principles, or to improve them by judicious alterations, rather than to depend on the uſe of additional inventions for correcting their defects.

To make a good fire in a bad grate, the bottom of the grate muſt be firſt covered with a ſingle layer of balls, made of good fire-bricks, or artificial fire-ſtone, well burnt, each ball being perfectly globular, and about 2½ or 2¾ inches in diameter. On this layer of balls the fire is to be kindled, and, in filling the grate, more balls are to be added with the coals that are laid on; care muſt, however, be taken in this operation to mix the coals and the balls well together, otherwiſe, if a number of the balls ſhould get together in a heap, they will cool, not being kept red hot by the combuſtion of the ſurrounding fuel, and the fire will appear dull in that part; but if no more than a due proportion of the balls are uſed, and if they are properly mixed with the coals, they will all, except it be thoſe perhaps at the bottom of the grate, become red-hot, and the fire will not only be very beautiful, but it will ſend off a vaſt quantity of radiant heat into the room; and will continue to give out heat for a great length of time. It is the opinion of ſeveral perſons who have for a conſiderable time practiſed this method of making their fires, that more than one third of the fuel uſually conſumed may be ſaved by this ſimple contrivance. It is very probable that, with careful and judicious management, the ſaving would amount to one half, or fifty per cent.

As theſe balls, made in moulds, and burnt in a kiln, would coſt very little, and as a ſet of them would laſt a long time, probably ſeveral years, the ſaving of expence in heating rooms by chimney fires with bad grates, in this way, is obvious; but ſtill, it ſhould be remembered that a ſaving quite as great may be made by altering the grate, and making it a good fire-place.

In

In uſing theſe balls, care muſt be taken to prevent their accumulating at the bottom of the grate: as the coals go on to conſume, the balls mixed with them will naturally ſettle down towards the bottom of the grate, and the tongs muſt be uſed occaſionally to lift them up; and, as the fire grows low, it will be proper to remove a part of them, and not to replace them in the grate till more coals are introduced: a little experience will ſhew how a fire made in this manner can be managed to the greateſt advantage, and with the leaſt trouble.

Balls made of pieces of any kind of well burnt hard brick, though not equally durable with fire-brick, will anſwer very well, provided they be made perfectly round; but if they are not quite globular their flat ſides will get together, and by obſtructing the free paſſage of the air amongſt them, and amongſt the coals, will prevent the fire from burning clear and bright.

The beſt compoſition for making theſe balls, when they are formed in moulds, and afterwards dried and burnt in a kiln, is pounded crucibles mixed up with moiſtened Sturbridge clay; but good balls may be made with any very hard burnt common bricks, reduced to a coarſe powder, and mixed with Sturbridge clay, or even with common clay. The balls ſhould always be made ſo large as not to paſs through between the front bars of a grate.

Theſe balls have one advantage, which is peculiar to them, and which might perhaps recommend the uſe of them to the curious, even in fire-places conſtructed on the beſt principles; they cauſe the cinders to be conſumed almoſt intirely; and even the very aſhes may be burnt, or made to diſappear, if care be taken to throw them repeatedly upon the fire when it burns with an intenſe heat. It is not difficult to account for this effect in a ſatisfactory manner, and in accounting for it we ſhall explain a circumſtance on which it is probable that the great increaſe of the heat of an open fire, where theſe balls are uſed, may, in ſome meaſure, depend. The ſmall particles of coal, and of cinder, which, in a common fire, fall through the bottom of the grate, and eſcape combuſtion, when theſe balls are uſed can hardly fail to fall and lodge on ſome of them; and, as they are intenſely hot, theſe ſmall bodies which alight upon them in their fall, are ſoon heated red hot, and diſpoſed to take fire and burn; and, as freſh air from below the grate is continually making its way upwards amongſt the balls, every circumſtance is highly favourable to the rapid and complete combuſtion of theſe ſmall inflammable bodies. But if theſe ſmall pieces of coal and cinder ſhould, in their fall, happen to alight upon the metallic bars which form the bottom of the grate; as theſe bars are conductors of heat, and, on account of that circumſtance, as well as of their ſituation, *below* the fire, never can be made very hot, any ſmall particle of fuel that happens to come into contact with them, not only cannot take fire, but would ceaſe to burn, ſhould it arrive in a ſtate of actual combuſtion.

Theſe facts are very important, and well deſerving of the attention of thoſe who may derive advantage from the improvement of fire places, and the economy of fuel.

There are ſome circumſtances which ſtrongly indicate that an admixture of incombuſtible bodies with fuel, and eſpecially with coal, cauſe an increaſe of the heat, even when the fuel is burnt in a cloſed fire-place, No fire-place can well be contrived more completely cloſed than thoſe of the iron ſtoves in common uſe in the Netherlands; but in theſe ſtoves, which are heated by coal fires, a large proportion of wet clay is always coarſely mixed with the coals before they are introduced into the fire-place. If this practice had not been found to be uſeful it would certainly never have obtained generally; nor would it have been continued, as it has been, for more than two hundred years.

The combination of different ſubſtances, combuſtible, and incombuſtible, to form, artificially, various kinds of cheap and pleaſant fuel, particularly adapted for the different proceſſes in which the fuel is employed, is a ſubject well worthy of the attention of enterprizing and ingenious men. How much excellent fuel, for inſtance, might be made with proper additions, and proper management, of the mountains of refuſe coal duſt that lie uſeleſs at the mouths of coal pits; and how much would it contribute to cleanlineſs and elegance if the uſe of improved coke, or of hard and light fire balls, could be generally introduced in our houſes and kitchens, inſtead of crude, black, powdery, dirty, ſea coal. Of the great economy that would reſult from ſuch a change there cannot be the ſmalleſt doubt.

It is a melancholy truth, but, at the ſame time, a moſt undiſputable fact, that, while the induſtry and ingenuity of millions are employed, with unceaſing activity, in inventing, improving, and varying thoſe ſuperfluities which wealth and luxury introduce into ſociety, no attention whatever is paid to the improvement of thoſe common neceſſaries of life on which the ſubſiſtence of all, and the comforts and enjoyments of the great majority of mankind, abſolutely depend.

Much will be done for the benefit of ſociety, if means can be deviſed to call the attention of the active and benevolent to this long neglected, but moſt intereſting, ſubject.

The Royal Inſtitution ſeems to be well calculated to facilitate and expedite the accompliſhment of this important object. Indeed it is more than probable that this, preciſely, is the object which was principally had in view in the foundation and arrangement of that eſtabliſhment.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Diſeaſes of horned Cattle and Sheep.

W. P. Whyte, Eſq. of Worceſter, has drawn the outline of a plan for aſcertaining and defining the different diſeaſes of horned cattle and ſheep, their cauſes, and means of prevention or cure. He intends to publiſh a proſpectus ſpeedily; but in the mean time his outline, which is of ſome length, appears in that uſeful work "the Annals of Agriculture" The preſent propoſals are as follow:

1. That a tour ſhall be made at different times through all the principal grazing diſtricts in that part of Great Britain called England, ſimilar to thoſe of Arthur Young, Eſq. or Mr. Marſhall, the writer of the valuable volumes on the Rural Economy of the different Counties, for the purpoſe of aſcertaining and defining the different diſeaſes incident to horned cattle and ſheep, the various cauſes whence they ariſe, and the means of preventing and remedying them.

2. That every particular diſeaſe (ſo far at leaſt as ſuch tour ſhall afford opportunity) ſhall undergo a ſcientific and full inveſtigation, with a view to the different ends propoſed.

3. That minutes of every material occurrence and tranſaction relative to the objects in view ſhall be regularly taken and preſerved, with a view to future publication; in particular, that theſe ſhall record the nature of particular diſeaſes, the diſtricts where they are moſt prevalent, the real, apparent, or probable cauſes of them, the provincial opinions on the ſubject, the ſymptoms of ſuch diſeaſes, the means which have been hitherto employed with intent to remedy them, the ſucceſs or otherwiſe of ſuch means, the morbid anatomical appearances after death, the certain or probable means of prevention, &c.

4. That the reſult of the whole ſhall be publiſhed in octavo volumes, either in diſtinct parts once in every ſix or twelve months, or, arranged in ſyſtematical form, at the concluſion of the tour, as may be thought adviſable by the Secretary to the Board and the Author, upon ſuch terms as ſhall furniſh them to the ſubſcribers at a moderate expence.

5. That the Board ſhall afford their ſanction to the undertaking by a letter of recommendation from the Preſident; which the Touriſt ſhall be at liberty to carry with him, and ſhow occaſionally.

6. That the expence of the undertaking and the remuneration to the Touriſt ſhall be borne and defrayed by a public ſubſcription and the money to ariſe from publication of the minutes.

7. That ſubſcriptions ſhall be received (probably) by Arthur Young, Eſq. F. R. S. Secretary to the Board.

8. That the Royal Family and Nobility of this kingdom, Foreign Potentates and Nobility, and Public Bodies and Societies eſtabliſhed by royal charter, be invited to ſubſcribe at pleaſure.

9. That the Gentry, Land Owners, Breeders, Graziers, and all others ſhall become ſubſcribers upon the payment of   to the Receiver.

10. That a ſufficient number of copies of the minutes ſhall be handſomely printed on a ſuperfine royal quarto paper, and hot preſſed, and delivered *gratis* to all ſubſcribers of any ſum, being five guineas more than the ordinary ſubſcription.

11. That ſo ſoon as   pounds are ſubſcribed, the Touriſt will proceed in the undertaking.

On

On the New Alcali called Pneum of Dr. Hahneman.

Meſſrs. Klaproth, Karſtein, and Hermſtadt having publiſhed in the Literary Gazette of Jenu, that the reſult of their examination of Mr. Hahneman's new alcali was, that it is merely the acid of borax ſuperſaturated with ſoda, that is to common borax. The Doctor publiſhed his anſwer addreſſed to thoſe chemiſts, as follows:

"I may be deceived, but I am not capable of deceiving. I was at work upon crude borax; pot-aſh dropped into a ſolution not yet diſpoſed to cryſtallize, afforded an abundant precipitate in the form of powder. But as authors aſſure us that borax becomes uncryſtallizable by the addition of alcali, it is not ſurprizing that I ſhould have conſidered this precipitate as a new and particular ſubſtance. The re-agents alſo ſhewed phenomena different from thoſe preſented by common borax. But in order to clear it as I thought from all adherent borax, I ſaturated it exactly with acetous acid, and mixed the concentrated ley with equal parts of ſpirit of wine. After the ſeparation of what remained inſoluble, I ſuppoſed myſelf to be in poſſeſſion of the alcaline part, combined with the acetous acid diſſolved in the ſpirit of wine, and I ſeparated this acid by adding one of the three alcalies. The ſalt thus precipitated and waſhed, appeared to me to poſſeſs a ſufficient number of properties different from thoſe of common borax, to be conſidered as a peculiar alcali. The general notion of chemiſts of an unſaturated exceſs of alcali in borax led me into error; for how could this exceſs exiſt in my borax of ſoda, ſince the mineral acids added to my borax to dryneſs eſcaped by heat? How could the exceſs of ſoda exiſt in my borax in a cauſtic ſtate? This exceſs either is not ſoda, or it is not a real exceſs, it is adopted by chemiſts without foundation. This laſt is in fact the caſe. The acid of borax neutralizes the ſoda in ſuch a manner, and extends its neutralizing power in ſuch a manner on the part which has hitherto appeared to us to be in exceſs, that the carbonic acid and the mineral acids ſcarcely enter into any intimate combination with this exceſs."

Journal de Phyſique, Meſſidor An. 9.

Facts and Diſcoveries reſpecting Galvaniſm.

In a letter from Berlin inſerted in the Magazine Encyclopedique I. 1801. pag. 110, dated April laſt, ſeveral experiments are related by Helvige, a Swediſh major, Bourguet, profeſſor of chemiſtry, Hermann, profeſſor of natural philoſophy, and Dr. Grapengieſſer. Theſe philoſophers repeated the experiments made by Volta, together with thoſe which we added in this country. Their pile conſiſted of gold, and zinc with cloth moiſtened with ſalt water. Other metals were uſed, namely, ſilver and copper, inſtead of the gold, without affording any determinate ground for preference, as it appeared to theſe operators. They found that the galvanic ſpark is obtained with much greater facility, when one of the two conductors terminates in very fine pointed iron wire, and the other in a knob or button. This diſcovery preſented itſelf when the gold leaf of the electrometer of Bennet was made part of the circuit; and ſince making that obſervation they conſtantly armed the extremities of theit conductors with gold leaf; by which means they obtained with eaſe a ſtrong ſpark, and uſed the ſame method with the greateſt ſucceſs in ſetting fire to phoſphorus, ſublimed ſulphur, fulminating gas, and gunpowder, the latter of which is not eaſily fired by the electric fluid. It was remarked, that in thoſe experiments the portion of gold leaf which covered the extremity of the conductors, was itſelf fuſed and reduced into a globule more or leſs perfect, according to the force of the ſpark.

They repeated my experiment with the condenſer, and found agreeably to what I had obſerved, that the ſilver is in the negative ſtate, and the zinc poſitive. By the intervention of the condenſer they charged a jar, but could not obtain the ſlighteſt galvanic commotion. But by means of two condenſers (by the proceſs of doubling I ſuppoſe) they repeated upon the Electrophore the experiment of producing the figures of Lichtenberg, with the ſame varieties and forms which would have been produced by the electric fluid.

Dr. Grapengeiſſer has applied the galvanic energy in the courſe of medical practice, and obſerves, that the ſimilitude of this action with that of the electric fluid cannot be miſtaken: but theſe philoſophers think, that a great diſſimilitude, or variation in theſe two powers, was indicated by an experiment they made of inſulating the whole apparatus, together with the tube in which the proceſs of decompoſing water was going forward. In this ſituation they electrified the whole apparatus by the common machine, and obtained very conſiderable ſparks, without producing any change in the phænomena between the wires in the tube. Hence they think that thoſe powers may be imagined to be independent of each other. Like many philoſophers in this country, as well as elſewhere, they are diſpoſed to think that the galvanic power depends upon the tranſition of oxigen; but it may be obſerved, that there are many unequivocal facts, which ſeem to prove that the common electricity alſo is never excited, but by the condenſation or developement of the ſame principle.

Galvanic Flaſh.

Mr. Cruickſhank has favoured me with the remark, that a miſtake appears in the uſe of the words zinc-face and ſilver face, at page 240 of our Journal for September, where an account is given of the luminous ſigns of galvaniſm correſponding with the two electricities. I here relate the object in his own words: *the bruſh or flaſh always appears at the* "*extremity of the wire, which if immerſed in water would diſengage hidrogen, or what has been* "*generally called the ſilver wire, whereas the zinc, or oxigen wire, exhibit only a faint luminous* "*point.*" And as he perfectly agrees in the remarks made at the page laſt quoted, reſpecting the plus and minus electricities, he is diſpoſed to ſuſpect, that there may be ſome miſtake in the original experiments, which were made to aſcertain the ſtate as to plus and minus of the extremities of the pile. I will repeat thoſe experiments, in order that the facts may clear up this matter, and ſhew whether there be any ſtriking difference between the galvanic and electric lights.

Effect of Galvaniſm in a Caſe of Paralyſis of the Muſcles of the Left Side of the Face.

By CITIZEN HALLE *.

A man whoſe muſcles of the left ſide of the face, and the interior muſcles of the globe of the eye on the ſame ſide were paralyſed in conſequence of a cold, had been electrified ſeveral times. He experienced no ſenſation nor contraction when the affected part received the ſpark, and there was ſcarcely the ſlighteſt contraction in the zygomaticus major when the electric ſhock was applied. This man was ſubjected to the galvanic action of a pile of fifty ſetts, by forming a communication between the different points of the diſeaſed ſide of the face, and the two extremities of the pile by means of metallic excitators.

At the moment of contact all the muſcles of the face became contracted. The patient felt pain with a very diſagreeable ſenſation of heat, the eye became convulſed, tears flowed involuntarily, and redneſs with ſwelling was perceived on the different parts which were touched.

Theſe experiments, which appear to afford ſome means of comparing the effects of galvaniſm with electricity, were repeated ſeveral ſucceſſive days at the ſchool of medicine, and on the 26th of Prarial Citizen Halle gave an account of the ſame to the National Inſtitute. He obſerved, that the muſcles remained contracted for ſome minutes after the galvanic commotion, and that the left eye even followed the motion of the right.

In this application of galvaniſm to the human body, Citizen Halle obſerved ſome very ſingular anomalies. The pile was often very long before it communicated its effect; ſometimes it ceaſed to produce any effect for ſeveral ſeconds together. In both theſe caſes it ſeemed as if the fluid met ſome obſtacle in its courſe. For in theſe circumſtances it was neceſſary to moiſten the ſkin, to rub it, and to change the relative poſition of the rings, in order to make the communication. In general it was obſerved, that in order that the ſenſation ſhould be ſpeedy, it was not enough that the ſkin ſhould be wetted, but that it muſt be, as it were, ſoaked and ſoftened with the water. He himſelf, as well as ſeveral other perſons, made trial of the ſenſation which the galvaniſm produces. It reſembles in ſome reſpects that of the action of ſeveral needles forced at the ſame time into the ſkin. It is a pungent pain, accompanied with a ſenſe of heat and a ſlight metallic taſte, when the exciters were applied near the ſalivary glands.

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* Communicated to the National Inſtitute of France, and inſerted in the Bulletin des Sciences, No. 51, An. IX.

Fig. 16.

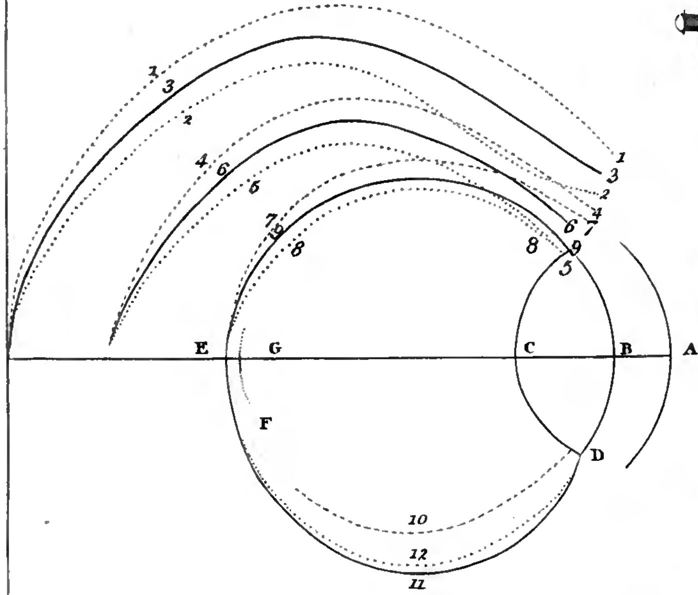


Fig. 17.

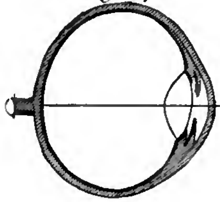
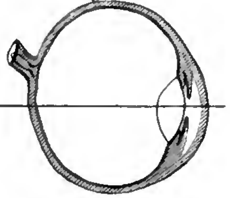


Fig. 18.



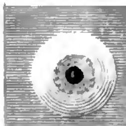


Fig. 19.

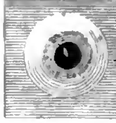


Fig. 20.

Fig. 21.

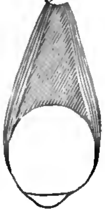


Fig. 22.

Fig. 25.

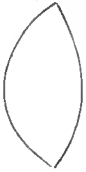


Fig. 23.

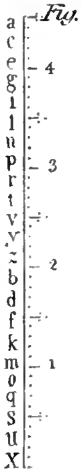


Fig. 24.

Fig. 27.

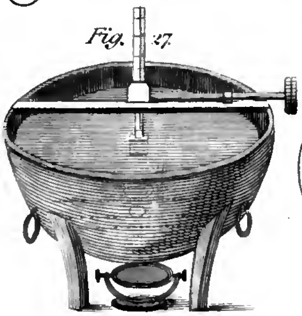
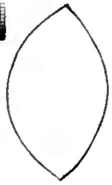


Fig. 26.



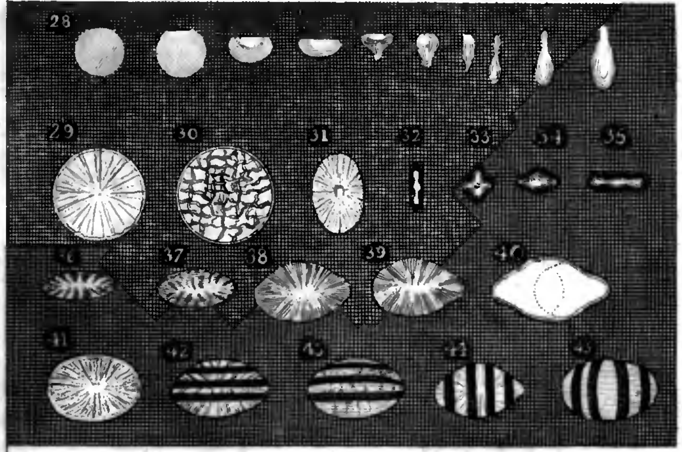


Fig. 52.

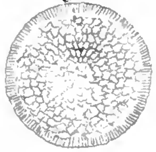


Fig. 53.

Fig. 54.

Fig. 55.

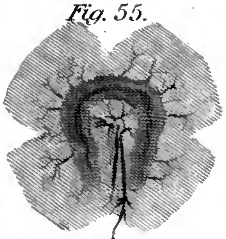


Fig. 46.

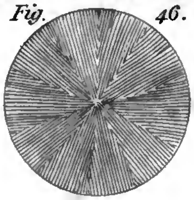


Fig. 47.

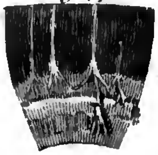


Fig. 48.

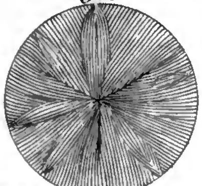


Fig. 56.

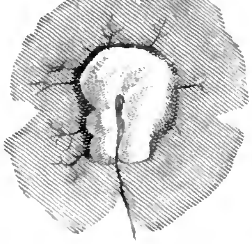


Fig. 57.

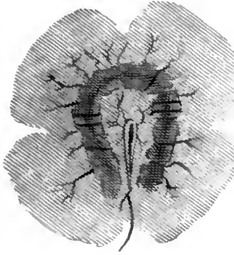


Fig. 49.

Fig. 50.

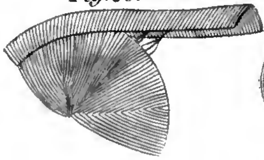
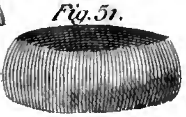


Fig. 51.



Nutlow Sculp.t

Prony's universal Support for a Balance.

Fig. 2.

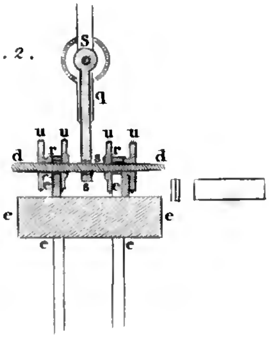


Fig. 4.

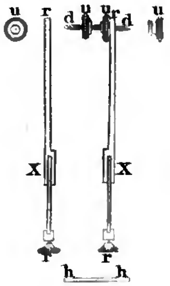


Fig. 3.

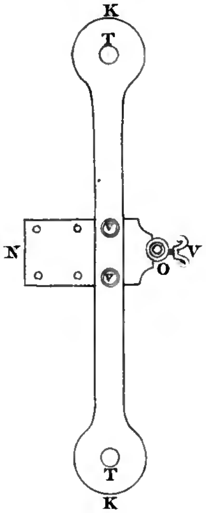


Fig. 1.

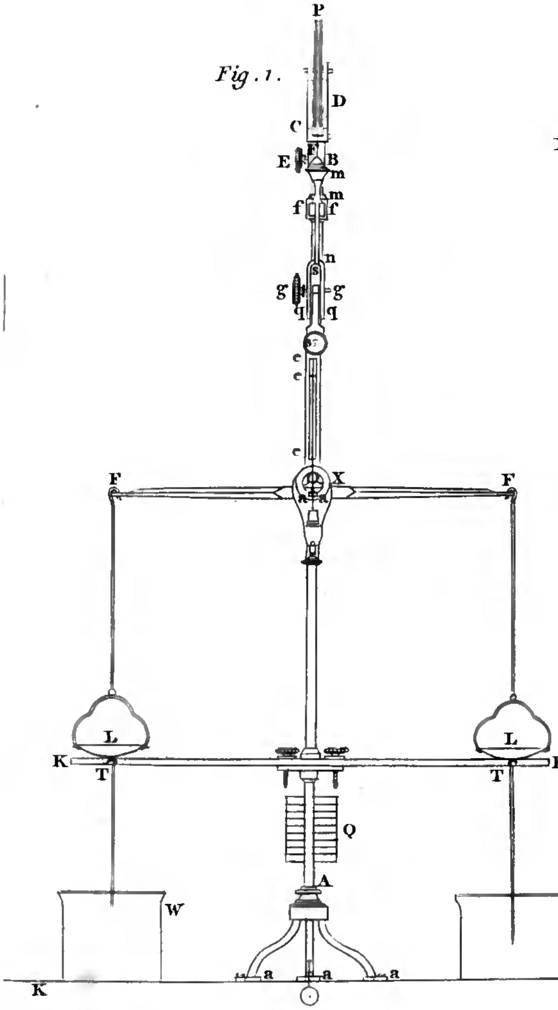
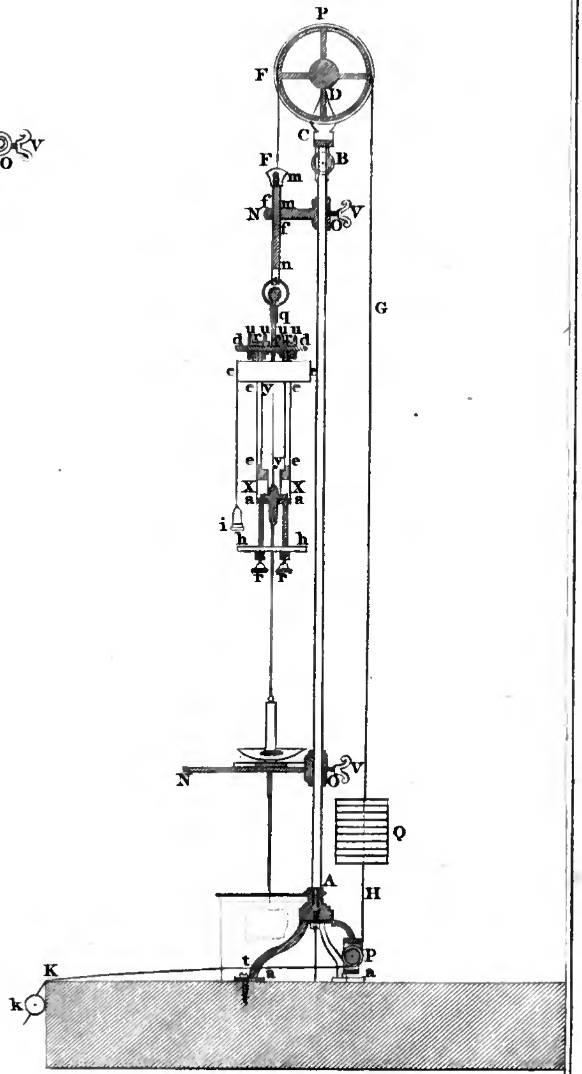


Fig. 5.



Mutlow Sc. Russell Court.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

DECEMBER, 1801.

ARTICLE I.

Obſervations on the Baſaltic Coaſt of Ireland. In a Letter from Wm. Richardson, *D.D. to the Honourable* Geo. Knox, *M. P. Communicated by Mr. Knox.*

DEAR SIR,

WHEN you left Portruſh in Auguſt laſt, you lamented that you could not accompany me on two expeditions I was then preparing for, one to explore our coaſt in ſearch of the Whynn Dykes (we call them Gaws) which iſſuing from it at right angles proceed nearly due northward, and croſs moſt of the Hebrides, cutting through all ſubſtances indifferently.

I found four at Bengore Head, in addition to the two at the Giant's Cauſeway we examined together, and whoſe internal priſmatic conſtruction, and other circumſtances aſtoniſhed you ſo much; I found alſo four at Fairhead, making ten in all, every one of which that I could get acceſs to, differed in ſome material points from all the others.

Mr. Mills (Phil. Tranſ. 1790,) mentions ſeveral of theſe Whynn Dykes he met with in the Scotch iſlands, but he does not ſeem to have examined them with attention, or noticed their differences, if they really differ from each other as ours do.

As ſoon as I am able to explore the coaſt from Dunscurich to Ballintay, where poſſibly I may find more, I will (according to my promiſe) give you an accurate account of of theſe wonderful walls, which ſeem to me utterly irreconcileable to any of the theories invented by coſmogoniſts.

In this Letter I will confine myſelf to the mighty promontory of FAIRHEAD, which forms the north-eaſt point of our iſland, and a line drawn from it to Cape Clear in Kerry meaſures its greateſt dimenſion.

Before I proceed to particulars, I muſt dwell a little on its magnificence, both becauſe you yourſelf, when admiring from the water the vaſt baſaltic precipices of BENGORE, (370 feet high) ſeemed to doubt me, when I told you that at FAIRHEAD they were far more ſtupendous; and alſo becauſe Mr. PENNANT aſſerts, that the baſaltic pillars at STAFFA *far exceed the Iriſh in grandeur.*

It has of late been much the faſhion to prefer STAFFA to any thing our baſaltic coaſt can boaſt of, with what foundation will ſoon appear.

In point of grandeur, menſuration affords a good teſt, and fortunately we have the heights of the pillars and precipices at STAFFA accurately taken by Sir Joſeph Banks, and I make myſelf anſwerable for the meaſures I ſtate at FAIRHEAD.

In Staffa the longeſt pillar or range of pillars is 55 feet; and the higheſt point of the precipice and iſland 126; (this laſt aſſertion is from recollection) whereas at Fairhead the height of the face of the columnar precipice, that is, the height of each pillar in a range of near an Engliſh mile, varies only from 252 to 250 feet, and from the angle (ſee view of Faiſhead taken at four miles diſtance W. S. W.) to the level of the ſea, the perpendicular height of the precipitous declivity is at leaſt 300 feet, making together the height of the promontory 550 feet.

After comparing theſe meaſures, it is mortifying to obſerve the animation with which Sir Joſeph Banks deſcribes the magnificence of Staffa, while Fairhead remains unnoticed.

I apprehend it is by far the moſt ſuperb colonade of baſaltic pillars yet diſcovered, though Mr. KIRWAN, on the authority of Mr. Charpentier ſays, the pillars at STOLPE are 300 feet high, but the Encyclopedie tells us, theſe ſame pillars at STOLPE are but 17 or 18, and Gentlemen (particularly my friend Archdeacon Lovel) who have viſited STOLPE, aſſure me that the latter dimenſions ſeem to them the true ones.

The vaſt perpendicular precipice which lines the upper part of Fairhead promontory, is twice cut into by very acute angles; the weſternmoſt of theſe is as it were biſected by a mighty wall, compoſed of pillars near 150 feet long each, while the eaſternmoſt affords a path by which I deſcended to the baſes of the pillars; as I ſtood at the opening of this angle the ſcene about me was the moſt ſtupendous I ever beheld; on my right and left hand enormous walls reaching almoſt to the ſky, intirely columnar, the diameter of each pillar exceeding two feet, but quite out of proportion to their enormous height.

The view below me was terrific, for I ſtood on the top of a ſecond ſtratum of perpendicular pillars, probably 80 feet long each, with the declivity from the bottom of theſe pillars to the ſea, ſteep and precipitous, not much leſs than 300 feet.

The

The views from the ſummit of this promontory are highly gratifying to thoſe who delight in the ſublime, but it is by coaſting its baſe alone that the naturaliſt will obtain entertainment or information; this however is an expedition of difficulty and uncertainty, from the rapidity of the tide that runs cloſe to this headland, and the violent ſurf which generally beats againſt its baſe; twice I reached the north-weſt point of this formidable head without daring to proceed, the third time I was more fortunate, and amply repaid for my perſeverance.

While at Fairhead nature ſeems to have exerted her utmoſt powers in diſplaying a magnificence unrivalled any where elſe, at the ſame time ſhe appears in the conſtruction of this promontory to have adopted different principles from thoſe ſhe has hitherto been governed by in the formation of the reſt of our baſaltic country.

I had always obſerved that our ſtrata were in the ſame place, all ſteadily parallel to each other, and I ſhewed you that this rule obtained in all the precipices we rowed along together; but at Fairhead the caſe is different, for there the columnar ſtrata are inclined to each other in a conſiderable angle.

I alſo ſhewed you, that in every ſeparate ſtratum the material was uniform through its whole extent, the principle of conſtruction, and the degree of perfection with which it was executed, the ſame.

At Fairhead on the contrary, the material is ſenſibly changed, in paſſing from the weſt to the eaſt ſide, the perfection in the workmanſhip of the pillars conſiderably abated, and a new principle of internal conſtruction introduced.

A moſt extraordinary circumſtance alſo occurs here, which I have not met with in any other of the columnar groups with which our baſaltic country abounds, nor do I find any thing ſimilar mentioned by the writers on theſe ſubjects.

In viewing Fairhead from any diſtance, many of its pillars appear to be of a moſt enormous ſize, but upon a near approach thoſe gigantic columns from ten to twenty feet diameter, are found to be formed by an accumulation of the ſmaller ones (from two to three feet) which compoſe the promontory, and adhere together with ſuch force, that when the mighty pillar tumbles down (as ſometimes happens) it breaks in pieces by the violence of a fall 250 feet perpendicular, without ſeparating from each other, the ſmaller pillars of which it is compoſed.

The ſteep declivity is covered quite down to the water edge with enormous fragments, or fruſta of theſe pillars, they are always cylindrical, and to look at their ſides they reſemble an aſſemblage of great beams of timber conſolidated together, while their baſes (generally planes, and at right angles with their axes) exhibit a teſſelated pavement, exactly like the ſurface of the Giant's Cauſeway, but the polygons are ſomewhat larger, and approach more to equilateral figures.

Sometimes theſe ſmaller pillars are continued quite up to the top, of their original ſize, at other times when half way up they merge into, and form greater pillars, which are continued to the ſummit, at other times again they merge into the ſolid rock.

It alſo frequently happens that the lower half of columns, both great and ſmall, has fallen down, leaving the upper half apparently unſupported, yet firm in its place; this laſt circumſtance, however, is common in all our baſaltic precipices.

This ſuſpenſion of the upper parts of pillars, and ſtill more the adheſion of their ſides, which I have juſt now ſhewn to be ſo ſtrong as not to yield to the ſhock of a mighty fall, prove, that at leaſt in this place there are no intervals between the pillars.

From analogy I have long ſuſpected this to be the caſe, every variety of our baſalt is of priſmatic conſtruction, and in *all*, the priſms adhere ſo cloſely together as to be ſeparable by the crow or ſledge alone, the columnar is the only one that has intervals between its priſms, and theſe I have little doubt proceed from external cauſes, but it is impoſſible to penetrate ſo far into a range of pillars as to bring the point to certainty; it is by catching at accidental circumſtances that we are enabled to throw any light upon it.

As I coaſted along the foot of the promontory, the magnificence of the ſcene above me baffled all deſcription, the mighty wall which ſtruck me ſo forcibly when on the top of the cliff, was now an inſignificant object, and the angle through which I had deſcended to the foot of the precipice, was diminiſhed almoſt to nothing.

A little to the eaſtward of this angle (called the Grey Man's Bath) we came oppoſite to the place where a conſiderable part of the precipice had fallen down about 25 years ago; its length ſeemed to be near a furlong, and its depth into the face of the rock muſt have exceeded 150 feet; the materials of this immenſe maſs are ſcattered in vaſt fragments piled upon one another from the foot of the perpendicular precipice, quite down to the water edge, forming by their accumulations a wild variety of caverns, arches, and paſſages beneath the ruins.

The face of rocks which is now laid bare, and which before the diſruption was far in the interior, is by no means ſo columnar as the reſt of the ſurface; ſometimes the vaſt pillars alone are viſible, ſometimes the rocks ſeem quite plain, and in one place the lines ſeparating three or four pillars after aſcending about 150 feet, faintly marked, make a ſlight curve for a few feet, and then merge in the ſolid rock.

All this looks as if the columnar conſtruction did not reach far into the interior of our precipices; but excluſive of the unfairneſs of drawing a general concluſion from this ſolitary inſtance, I muſt admit I could not get ſo near the baſe of the perpendicular face, as to make any accurate obſervations; beſides, the material ſeems to undergo a conſiderable change in the paſſage from the weſt to the eaſt ſide of Fairhead, to have aſſumed a darker colour, and from many of the fragments looks as if compoſed of ſmall trapezoidal priſms, like the Gaw, or Whynn Dyke we examined together at PORT SPAGNA BENGORE, the internal conſtruction of whoſe maſſive horizontal priſms was diſcloſed to us by the nervous arm, and weighty ſledge of your friend Mr. Crampton, without whoſe powerful aſſiſtance we ourſelves would in vain have attempted to diſcover it.

I have not met with any writer who notices this curious circumſtance attending many of our baſalts, to wit, that excluſive of their external priſmatic form, they have, as it were, a ſubordinate

ſubordinate internal principle of conſtruction, the great priſms breaking into ſmaller; columnar baſalt has rarely this property, I have found it only in the pillars I ſhewed you at Ballylagan) and at the eaſt ſide of Fairhead, but all the gaws or dykes ſeem to be conſtructed upon this principle, and the two ſtrata which alternate ſo frequently at Portruſh poſſeſs the ſame property in an high degree, and one of theſe you and your friend Mr. Chenevix have proved by analyſing it, that it is pure baſalt.

To return to Fairhead, immediately below the Grey Man's Bath, a new ſtratum of columnar baſalt begins to appear, and contrary to what I have obſerved every where elſe, not parallel to the one above it (which ſeems nearly horizontal) but dipping to the ſouth-eaſt, of courſe as we advance in this direction theſe ſtrata diverge from each other, and the increaſing ſpace between them is fitted up with new materials, to wit, alternate ſtrata of free ſtone and coal.

Theſe ſtrata are all parallel to the lower columnar ſtratum; the one immediately reſting upon it is coal, this is the vein that was laſt wrought at MURTOGH, and the adit to the mine runs in along the heads of the baſalt pillars.

On tracing theſe alternate ſtrata in a weſtern direction, they are found to terminate ſucceſſively againſt the baſes of the upper columnar ſtratum, always making with it an angle equal to that of the inclination of the two great ſtrata.

Here we have probably the ſtrongeſt inſtances that have yet occurred any where of the contacts of coal with columnar baſalt, for while the loweſt ſtratum of coal reſts with its whole ſurface on the heads of baſalt pillars, every ſtratum of coal above it comes by regular ſucceſſion into contact with the baſes of the pillars of the upper columnar ſtratum. I muſt confeſs I did not examine theſe contacts, the acclivity was ſo ſteep, and ſo ſtrewed with enormous maſſes of rock as to be inacceſſible at leaſt to me.

You muſt obſerve that the ſtrata at FAIRHEAD totally differ both in materials and arrangement, from the ſtrata compoſing BENGORE HEAD, which we counted together, and found to be 16 in number; in truth no two of our vaſt precipices, whether hanging over the ſea, or in the interior of the country, perfectly (or I may ſay nearly) reſemble each other in theſe circumſtances.

This variety in the ſtratification of our baſalt country, muſt be a ſubject of much curioſity, particularly to geologiſts, and was no doubt conſidered as ſuch by Mr. WHITEHURST and MILLS, when they promiſed in their title pages an account of our ſtrata, yet in the body of their works forgot the topic.

If you can find leiſure for another excurſion ſo far north next ſummer, our ſtrata will afford us an ample fund of entertainment, both in trying the theories of coſmogoniſts by the teſt of theſe ſtrata, and in conjecturing from their interruptions and other circumſtances, what changes our globe has undergone ſince its creation.

I am, SIR,
Your very humble Servant,
WILLIAM RICHARDSON, D. D.
Late Fellow of Trinity College, Dublin.

Clonfeele Houſe, MOY, *Oct.* 22, 1801.

Obſervations

II.

Obſervations on a Miſt which prevailed at Maeſtricht on the 14th Nivoſe, in the Year 8, by CIT. PAISSÉ, *with Remarks by* CIT. PARMENTIER*.

IT is of great importance in the accurate ſciences to pay attention to all the circumſtances upon which obſervations can be founded. If this part of natural phiſoſophy were cultivated with the care it deſerves, there is no doubt but we ſhould be much more intimately acquainted with the influence of meteors on the health of men, which it ſo evidently affects. Thoſe fogs which are almoſt periodically renewed, preſent to the miniſters of health an extenſive field of obſervation, from which it ſhould appear, that the moſt uſeful conſequences reſpecting the origin of diſeaſes may be deduced.

The miſt which ſorms the ſubject of the preſent communication was ſo peculiar in its appearance and effects, that I have concluded that it might be of advantage to give a detailed account of the ſame.

On the morning of the 11th Nivoſe Reaumur's thermometer had deſcended to 13½° below O, where it remained for moſt part of the day, with no greater variation than ½ a degree. At ſeven in the evening it was conſtant at the ſame temperature, and the following day at ſeven in the morning it had riſen two degrees above the freezing point, and at eleven o'clock the thaw had unequivocally taken place at the temperature of 5 degrees. At two in the evening the thermometer marked 6½ degrees, the thaw continuing; about five in the evening a ſlight miſt appeared, which continued till half paſt ſeven; at eight it was intirely diſſipated, and the moon ſhone bright, at which time the thermometer deſcended again within four degrees of the freezing point; at nine its elevation did not exceed one degree, when the ſnow began to harden. On the 13th at ſeven in the morning the thermometer ſtood at ſeven degrees above O; at nine, eight degrees, and at noon it roſe to the ninth degree, and the ſun which appeared from time to time in the courſe of the day had conſiderable force. The thermometer kept at the ſame elevation, and the thaw continued its progreſs at eight in the evening.

On the 14th day of the month the horizon was covered with a miſt of inconſiderable denſity. It appeared like an evaporation from the earth ariſing about a metre above the ſurface. At nine it roſe to the height of ſeveral metres without becoming leſs denſe; at eleven it became more conſiderable; and at noon it was ſo thick that two perſons at a very ſmall diſtance aſunder could not ſee each other without much difficulty. At three in the evening its denſity was ſtill greater, and its ſmell ſo fetid as ſtrongly to impede reſpiration. The thermometer during this time underwent no greater change than half a degree, not paſſing beyond the ninth degree. The ſmell and acrid effects of this meteor were ſo diſ-

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* Annales de Chimie, XXXIII. 217.

agreeable,

agreeable, that I thought for a time that this property was communicated by the vapours of the bituminous combuſtible which is uſed in this country, and I did not give up my opinion till I had found myſelf as ſtrongly affected with it in the open country as in the town. My eyes were irritated ſo as to flow with involuntary tears. I returned home after experiencing the greateſt difficulties, and obſerving that the fog in the ſtreets was much more elevated than in the open country. I obſerved, as Citizen Fourcroy did on a former occaſion at Paris, that it was more eaſy to find the way in the narrow ſtreets, than in ſuch as were wider, in the former of which the fog deſcended with much leſs rapidity.

III.

On the Mechaniſm of the Eye. By THOMAS YOUNG, *M.D. F.R.S.*

(Continued from page 303.*)*

HITHERTO I have endeavoured to ſhew the inconveniences attending other ſuppoſitions, and to remove the objections to the opinion of an internal change of the figure of the lens. I ſhall now ſtate two experiments, which, in the firſt place, come very near to a mathematical demonſtration of the exiſtence of ſuch a change, and, in the ſecond, explain in great meaſure its origin, and the manner in which it is effected.

I have already deſcribed the appearance of the imperfect image of a minute point at different diſtances from the eye, in a ſtate of relaxation. For the preſent purpoſe, I will only repeat, that if the point is beyond the furtheſt focal diſtance of the eye, it aſſumes that appearance which is generally deſcribed by the name of a ſtar, the central part being conſiderably the brighteſt. (Plate XV. Fig. 36—39.) But, when the focal diſtance of the eye is ſhortened, the imperfect image is of courſe enlarged; and, beſides the neceſſary conſequence, the light is alſo very differently diſtributed; the central part beomes faint, and the margin ſtrongly illuminated, ſo as to have almoſt the appearance of an oval ring. (Fig. 41.) If I apply the ſlider of the optometer, the ſhadows of the ſlits, while the eye is relaxed, are perfectly ſtraight, dividing the oval either way into parallel ſegments: (Fig. 42, 44.) but, when the accomodation takes place, they immediately become curved, and the more ſo the further they are from the centre of the image, to which their concavity is directed. (Fig. 43, 45.) If the point be brought much within the focal diſtance, the change of the eye will increaſe the illumination of the centre, at the expenſe of the margin. The ſame appearances are equally obſervable, when the effect of the cornea is removed by immerſion in water; and the only imaginable way of accounting for the diverſity, is to ſuppoſe the central parts of the lens to acquire a greater degree of curvature than the marginal parts. If the refraction of the lens remained the ſame, it is abſolutely impoſſible that any change of the diſtance of the retina ſhould produce a curvature in thoſe ſhadows, which, in the relaxed ſtate of the eye, are found to be in all parts ſtraight; and, that neither the form nor the relative ſituation of the cornea is concerned, appears from the application of water already mentioned.

The truth of this explanation is fully confirmed by the optometer. When I look through four narrow ſlits, without exertion, the lines always appear to meet in one point: but, when I make the interſection approach me, the two outer lines meet conſiderably beyond the inner ones, and the two lines of the ſame ſide croſs each other at a ſtill greater diſtance. (Plate XV. Fig. 24.)

The experiment will not ſucceed with every eye; nor can it be expected that ſuch an imperfection ſhould be univerſal: but one caſe is ſufficient to eſtabliſh the argument, even if no other were found. I do not however doubt, that in thoſe who have a large pupil, the aberration may be very frequently obſervable. In Dr. Wollaſton's eye, the diverſity of appearance is imperceptible; but Mr. Konig deſcribed the interſection exactly as they appear to me, although he had received no hint of what I had obſerved. The lateral refraction is the moſt eaſily aſcertained, by ſubſtituting for the ſlits a tapering piece of card, ſo as to cover all the central parts of the pupil, and thus determining the neareſt croſſing of the ſhadows tranſmitted through the marginal parts only. When the furtheſt interſection was at 38, I could bring it to 22 parts with two narrow ſlits; but with the tapered card only to 29. From theſe data we may determine pretty nearly, into what form the lens muſt be changed, ſuppoſing both the ſurfaces to undergo proportional alterations of curvature, and taking for granted the dimenſions already laid down: for, from the lateral aberration thus given, we may find (by Prop. III.) the ſubtangents at about one-tenth of an inch from the axis; and the radius of curvature at each vertex, is already determined to be about 21 and 15 hundredths of an inch. Hence the anterior ſurface muſt be a portion of a hyperboloid, of which the greater axis is about 50; and the poſterior ſurface will be nearly parabolical. In this manner the change will be effected, without any diminution of the tranſverſe diameter of the lens. The elongation of its axis will not exceed the fiftieth of an inch; and, on the ſuppoſition with which we ſet out, the protruſion will be chiefly at the poſterior vertex. The form of the lens thus will be nearly that of Plate XV. Fig. 26; the relaxed ſtate being nearly as repreſented in Fig. 25. Should, however, the rigidity of the internal parts, or any other conſiderations, render it convenient to ſuppoſe the anterior ſurface more changed, it would ſtill have room, without interfering with the uvea; or it might even force the uvea a little forwards, without any viſible alteration of the external appearance of the eye.

From this inveſtigation of the change of the figure of the lens, it appears that the action which I formerly attributed to the external coats, cannot afford an explanation of the phenomenon. The neceſſary effect of ſuch an action would be, to produce a figure approaching to that of an oblate ſpheroid; and, to ſay nothing of the inconvenience attending a diminution of the diameter of the lens, the lateral refraction would be much more increaſed than the central; nor would the ſlight change of denſity, at an equal diſtance from the axis,

axis, be at all equivalent to the increaſe of curvature: we muſt therefore ſuppoſe ſome different mode of action in the power producing the change. Now, whether we call the lens a muſcle or not, it ſeems demonſtrable, that ſuch a change of figure takes place as can be produced by no external cauſe; and we may at leaſt illuſtrate it by a compariſon with the uſual action of muſcular fibres. A muſcle never contracts, without at the ſame time ſwelling laterally, and it is of no conſequence which of the effects we conſider as primary. I was induced, by an occaſional opacity, to give the name of membranous tendons to the radiations from the centre of the lens; but, on a more accurate examination, nothing really analogous to tendon can be diſcovered. And, if it were ſuppoſed that the parts next the axis were throughout of a tendinous, and therefore unchangeable nature, the contraction muſt be principally effected by the lateral parts of the fibres; ſo that the coats would become thicker towards the margin, by their contraction, while the general alteration of form would require them to be thinner; and there would be a contrariety in the actions of the various parts. But, if we compare the central parts of each ſurface to the belly of the muſcle, there is no difficulty in conceiving their thickneſs to be immediately increaſed, and to produce an immediate elongation of the axis, and an increaſe of the central curvature; while the lateral parts co-operate more or leſs, according to their diſtance from the centre, and in different individuals in ſomewhat different proportions. On this ſuppoſition, we have no longer any difficulty in artributing a power of change to the cryſtalline of fiſhes. M. Petit, in a great number of obſervations, uniformly found the lens of fiſhes more or leſs flattened: but, even if it were not, a ſlight extenſion of the lateral part of the ſuperficial fibres would allow thoſe ſofter coats to become thicker at each vertex, and to form the whole lens into a ſpheroid ſomewhat oblong; and here, the lens being the only agent in refraction, a leſs alteration than in other animals would be ſufficient. It is alſo worthy of inquiry, whether the ſtate of contraction may not immediately add to the refractive power. According to the old experiment, by which Dr. Goddard attempted to ſhow that muſcles become more denſe as they contract, ſuch an effect might naturally be expected. That experiment is, however, very indeciſive, and the opinion is indeed generally exploded, but perhaps too haſtily; and whoever ſhall aſcertain the exiſtence or non-exiſtence of ſuch a condenſation, will render eſſential ſervice to phyſiology in general.

Dr. Pemberton, in the year 1719, firſt ſyſtematically diſcuſſed the opinion of the muſcularity of the cryſtalline lens *. He referred to Leeuwenhoek's microſcopical obſervations; but he ſo overwhelmed his ſubject with intricate calculations, that few have attempted to develope it: and he grounded the whole on an experiment borrowed from Barrow, which with me has totally failed; and I cannot but agree with Dr. Olbers in the remark, that it is eaſier to confute him than to underſtand him. He argued for a partial change of the figure of the lens; and perhaps the opinion was more juſt than the reaſons

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* De Facultate Oculi qua ad diverſas Rerum diſtantias ſe accommodat. L. B. 1719. Ap. Hall. Diſp. Anat. IV. p. 301.

adduced for its ſupport. Lobé, or rather Albinus[*], decidedly favours a ſimilar theory; and ſuggeſts the analogy of the lens to the muſcular parts of pellucid animals, in which even the beſt microſcopes can diſcover no fibres. Camper alſo mentions the hypotheſis with conſiderable approbation[†]. Profeſſor Reil publiſhed, in 1793, a Diſſertation on the Structure of the Lens; and, in a ſubſequent paper, annexed to the tranſlation of my former Eſſay in Profeſſor Gren's Journal[‡], he diſcuſſed the queſtion of its muſcularity. I regret that I have not now an opportunity of referring to this publication; but I do not recollect that Profeſſor Reil's objections are different from thoſe which I have already noticed.

Conſidering the ſympathy of the cryſtalline lens with the uvea, and the delicate nature of the change of its figure, there is little reaſon to expect that any artificial ſtimulus would be more ſucceſsful in exciting a contractive action in the lens, than it has hitherto been in the uvea; much leſs would that contraction be viſible without art. Soon after Mr. Hunter's death, I purſued the experiment which he had ſuggeſted, for aſcertaining how far ſuch a contraction might be obſervable. My apparatus (Plate XV. Fig. 27.) was executed by Mr. Jones. It conſiſted of a wooden veſſel blacked within, which was to be filled with cool, and then with warmer water: a plane ſpeculum was placed under it; a perforation in the bottom was filled with a plate of glaſs; proper rings were fixed for the reception of the lens, or of the whole eye, and alſo wires for tranſmitting electricity: above theſe, a piece of ground and painted glaſs, for receiving the image, was ſupported by a bracket, which moved by a pivot, in connection with a ſcale divided into fiftieths of an inch. With this apparatus I made ſome experiments, aſſiſted by Mr. Wilkinſon, whoſe reſidence was near a ſlaughter-houſe: but we could obtain, by this method, no ſatisfactory evidence of the change; nor was our expectation much diſappointed. I underſtand alſo, that another member of this Society was equally unſucceſsful, in attempting to produce a conſpicuous change in the lens by electricity.

XI. In man and in the moſt common quadrupeds, the ſtructure of the lens is nearly ſimilar. The number of radiations is of little conſequence; but I find that in the human cryſtalline there are ten on each ſide, (Plate XV. Fig. 46.) not three, as I once, from a haſty obſervation, concluded[§]. Thoſe who find any difficulty in diſcovering the fibres, muſt have a ſight very ill adapted to microſcopical reſearches. I have laboured with the moſt obſtinate perſeverance to trace nerves into the lens, and I have ſometimes imagined that I had ſucceeded; but I cannot poſitively go further than to ſtate my full conviction of their exiſtence, and of the precipitancy of thoſe who have abſolutely denied it. The long nerves, which are very conſpicuous between the choroid and ſclerotic coats, divide each into two,

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[*] De quibuſdam Oculi Partibus, L. B. 1746. Ap. Hall. Diſp. Anat. IV. p. 301.

[†] De Oculo Humano. L. B. 1742. Ap. Hall. Diſp. Anat. VII. 2. p. 108, 109.

[‡] 1794. p. 352, 354.

[§] De Corp. Hum. Vir. Conſ. p. 68.

three, or more branches, at the ſpot where the ciliary zone begins, and ſeem indeed to furniſh the choroid with ſome fine filaments at the ſame place. The branches often reunite, with a ſlight protuberance, that ſcarcely deſerves the name of a ganglion: here they are tied down, and mixed with the hard whitiſh-brown membrane that covers the compact ſpongy ſubſtance, in which the veſſels of the ciliary proceſſes anaſtomoſe and ſubdivide. (Plate XV. Fig. 47.) The quantity of the nerves which proceeds to the iris, appears to be conſiderably ſmaller than that which arrives at the place of diviſion: hence there can be little doubt that the diviſion is calculated to ſupply the lens with ſome minute branches; and it is not improbable, from the appearance of the parts, that ſome fibres may paſs to the cornea; although it might more naturally be expected, that the tunica conjunctiva would be ſupplied from without. But the ſubdiviſions which probably paſs to the lens, enter immediately into a mixture of ligamentous ſubſtance, and of a tough browniſh membrane; and I have not hitherto been able to develope them. Perhaps animals may be found in which this ſubſtance is of a different nature; and I do not deſpair that, with the aſſiſtance of injections, for more readily diſtinguiſhing the blood veſſels, it may ſtill be poſſible to trace them in quadrupeds. Our inability to diſcover them, is ſcarcely an argument againſt their exiſtence: they muſt naturally be delicate and tranſparent; and we have an inſtance, in the cornea, of conſiderable ſenſibility, where no nerve has yet been traced. The capſule adheres to the ciliary ſubſtance, and the lens to the capſule, principally in two or three points; but I confeſs, I have not been able to obſerve that theſe points are exactly oppoſite to the trunks of nerves; ſo that, probably, the adheſion is chiefly cauſed by thoſe veſſels which are ſometimes ſeen paſſing to the capſule in injected eyes. We may, however, diſcover ramifications from ſome of theſe points, upon and within the ſubſtance of the lens, (Plate XV. Fig. 48.) generally following a direction near to that of the fibres, and ſometimes proceeding from a point oppoſite to one of the radiating lines of the ſame ſurface. But the principal veſſels of the lens appear to be derived from the central artery, by two or three branches at ſome little diſtance from the poſterior vertex; which I conceive to be the cauſe of the frequent adheſion of a portion of a cataract to the capſule, about this point: they follow nearly the courſe of the radiations, and then of the fibres; but there is often a ſuperficial ſubdiviſion of one of the radii, at the ſpot where one of them enters. The veſſels coming from the choroid appear principally to ſupply a ſubſtance, hitherto unobſerved, which fills up the marginal part of the capſule of the cryſtalline, in the form of a thin zone, and makes a ſlight elevation, viſible even through the capſule, (Fig. 49—51.) It conſiſts of coarſer fibres than the lens, but in a direction nearly ſimilar; they are often intermixed with ſmall globules. In ſome animals, the margin of the zone is crenated, eſpecially behind, where it is ſhorter: this is obſervable in the partridge; and, in the ſame bird, the whole ſurface of the lens is ſeen to be covered with points, or rather globules, arranged in regular lines, (Plate XV. Fig. 52.) ſo as to have ſomewhat the appearance of a honeycomb, but towards the vertex leſs uniformly diſpoſed. This regularity is a ſufficient proof that there could be no optical deception in the appear-

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ance; although it requires a good microſcope to diſcover it diſtinctly: but the zone may be eaſily peeled off under water, and hardened in ſpirits. Its uſe is uncertain; but it may poſſibly ſecrete the liquid of the cryſtalline; and it as much deſerves the name of a gland, as the greater part of the ſubſtances uſually ſo denominated. In peeling it off, I have very diſtinctly obſerved ramifications, which were paſſing through it into the lens; (Plate XV. Fig. 50.) and indeed it is not at all difficult to detect the veſſels connecting the margin of the lens with its capſule; and it is ſurpriſing that M. Petit ſhould have doubted of their exiſtence. I have not yet clearly diſcerned this cryſtalline gland in the human eye; but I infer the exiſtence of ſomething ſimilar to the globules, from the ſpotted appearance of the image of a lucid point already mentioned; for which I can no otherwiſe account, than by attributing it to a derangement of theſe particles, produced by the external force, and to an unequal impreſſion made by them on the ſurface of the lens.

In birds and in fiſhes, the fibres of the cryſtalline radiate equally, becoming finer as they approach the vertex, till they are loſt in a uniform ſubſtance, of the ſame degree of firmneſs, which appears to be perforated in the centre by a blood veſſel. (Plate XV. Fig. 53.) In quadrupeds, the fibres at their angular meeting are certainly not continued, as Leeuwenhock imagined, acroſs the line of diviſion; but there does not appear to be any diſſimilar ſubſtance interpoſed between them, except that very minute trunks of veſſels often mark that line. But, ſince the whole maſs of the lens, as far as it is moveable, is probably endued with a power of changing its figure, there is no need of any ſtrength of union, or place of attachment, for the fibres, ſince the motion meets with little or no reſiſtance. Every common muſcle, as ſoon as its contraction ceaſes, returns to its natural form, even without the aſſiſtance of an antagoniſt; and the lens itſelf, when taken out of the eye, in its capſule, has elaſticity enough to re-aſſume its proper figure, on the removal of a force that has compreſſed it. The capſule is highly elaſtic; and, ſince it is laterally fixed to the ciliary zone, it muſt co-operate in reſtoring the lens to its flatteſt form. If it be inquired, why the lens is not capable of becoming leſs convex, as well as more ſo, it may be anſwered, that the lateral parts have probably little contractive power; and, if they had more, they would have no room to increaſe the ſize of the diſc, which they muſt do, in order to ſhorten the axis; and the parts about the axis have no fibres ſo arranged as to ſhorten it by their own contraction.

I conſider myſelf as being partly repaid for the labour loſt in ſearch of the nerves of the lens, by having acquired a more accurate conception of the nature and ſituation of the ciliary ſubſtance. It had already been obſerved, that in the hare and in the wolf, the ciliary proceſſes are not attached to the capſule of the lens; and if by the ciliary proceſſes we underſtand thoſe filaments which are ſeen detached after tearing away the capſule, and conſiſt of ramifying veſſels, the obſervation is equally true of the common quadrupeds, and I will venture to ſay, of the human eye*. Perhaps this remark has been made by others,

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* Vid. Hall. Phyſiol. V. p. 432. et Duverney, ibi citat.

but the circumſtance is not generally underſtood. It is ſo difficult to obtain a diſtinct view of theſe bodies, undiſturbed, that I am partly indebted to accident, for having been undeceived reſpecting them: but, having once made the obſervation, I have learnt to ſhew it in an unqueſtionable manner. I remove the poſterior hemiſphere of the ſclerotica, or ſomewhat more, and alſo as much as poſſible of the vitreous humour, introduce the point of a pair of ſciſſors into the capſule, turn out the lens, and cut off the greater part of the poſterior portion of the capſule, and of the reſt of the vitreous humour. I next diſſect the choroid and uvea from the ſclerotica; and, dividing the anterior part of the capſule into ſegments from its centre, I turn them back upon the ciliary zone. The ciliary proceſſes then appear, covered with their pigment, and perfectly diſtinct both from the capſule and from the uvea; (Plate XV. Fig. 54.) and the ſurface of the capſule is ſeen ſhining, and evidently natural, cloſe to the baſe of theſe ſubſtances. I do not deny that the ſeparation between the uvea and the proceſſes, extends ſomewhat further back than the ſeparation between the proceſſes and the capſule; but the difference is inconſiderable, and, in the calf, does not amount to above half the length of the detached part. The appearance of the proceſſes is wholly irreconcileable with muſcularity; and their being conſidered as muſcles attached to the capſule, is therefore doubly inadmiſſible. Their lateral union with the capſule, commences at the baſe of their poſterior ſmooth ſurface, and is continued nearly to the point where they are more intimately united with the termination of the uvea; ſo that, however this portion of the baſe of the proceſſes were diſpoſed to contract, it would be much too ſhort to produce any ſenſible effect. What their uſe may be, cannot eaſily be determined: if it were neceſſary to have any peculiar organs for ſecretion, we might call them glands, for the percolation of the aqueous humour; but there is no reaſon to think them requiſite for this purpoſe.

The marſupium nigrum of birds, and the horſe-ſhoe-like appearance of the choroid of fiſhes, are two ſubſtances which have ſometimes, with equal injuſtice, been termed muſcular. All the apparent fibres of the marſupium nigrum are, as Haller had very truly aſſerted, merely duplicatures of a membrane, which, when its ends are cut off, may eaſily be unfolded under the microſcope, with the aſſiſtance of a fine hair pencil, ſo as to leave no longer any ſuſpicion of a muſcular texture. The experiment related by Mr. Home *, can ſcarcely be deemed a very ſtrong argument for attributing to this ſubſtance a faculty which its appearance ſo little authoriſes us to expect in it. The red ſubſtance in the choroid of fiſhes, (Plate XV. Fig. 55.) is more capable of deceiving the obſerver; its colour gives it ſome little pretenſion, and I began to examine it with a prepoſſeſſion in favour of its muſcular nature. But, when we recollect the general colour of the muſcles of fiſhes, the conſideration of its redneſs will no longer have any weight. Stripped of the membrane which looſely covers its internal ſurface, (Fig. 56.) it ſeems to have tranſverſe diviſions, ſomewhat reſembling thoſe of muſcles, and to terminate in a manner ſomewhat ſimilar;

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* Phil. Tranſ. for 1796. p. 18.

(Fig,

(Fig. 57.) but, when viewed in a microſcope, the tranſverſe diviſions appear to be cracks, and the whole maſs is evidently of a uniform texture, without the leaſt fibrous appearance; and, if a particle of any kind of muſcle is compared with it, the contraſt becomes very ſtriking. Beſides, it is fixed down, throughout its extent, to the poſterior lamina of the choroid, and has no attachment capable of directing its effect; to ſay nothing of the difficulty of conceiving what that effect could be. Its uſe muſt remain, in common with that of many other parts of the animal frame, entirely concealed from our curioſity.

The bony ſcales of the eyes of birds, which were long ago deſcribed in the Philoſophical Tranſactions by Mr. Ranby *, and by Mr. Warren †, afterwards in two excellent Memoirs of M. Petit on the eye of the turkey and of the owl ‡, and lately by Mr. Pierce Smith §, and Mr. Home ‖, can, on any ſuppoſition, have but little concern in the accommodation of the eye to different diſtances: they rather ſeem to be neceſſary for the protection of that organ, large and prominent as it is, and unſupported by any ſtrength in the orbit, againſt the various accidents to which the mode of life and rapid motion of thoſe animals muſt expoſe it; and they are much leſs liable to fracture than an entire bony ring of the ſame thickneſs would have been. The marſupium nigrum appears to be intended to aſſiſt in giving ſtrength to the eye, to prevent any change in the place of the lens by external force: it is ſo ſituated as to intercept but little light, and that little is principally what would have fallen on the inſertion of the optic nerve; and it ſeems to be too firmly tied to the lens, even to admit any conſiderable elongation of the axis of the eye, although it certainly would not impede a protruſion of the cornea.

With reſpect to the eyes of inſects, an obſervation of Poupart deſerves to be repeated here. He remarks, that the eye of the libellula is hollow; that it communicates with an air-veſſel placed longitudinally in the trunk of the body; and that it is capable of being inflated from this body: he ſuppoſes that the inſect is provided with this apparatus, in order for the accommodation of its eye to the perception of objects at different diſtances **. I have not yet had an opportunity of examining the eye of the libellula; but there is no difficulty in ſuppoſing that the means of producing the change of the refractive powers of the eye, may be, in different claſſes of animals, as diverſified as their habits, and the general conformation of their organs.

I beg leave to correct here an obſervation in my former paper, relative to the faint lateral radiations, which I ſuppoſed to proceed from the margin of the iris ††. I find, on further examination, that they are occaſioned by reflections from the eye-laſhes.

XII. I ſhall now finally recapitulate the principal objects and reſults of the inveſtigation which I have taken the liberty of detailing ſo fully to the Royal Society. Firſt, the deter-

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* Phil. Tranſ. Vol. XXXIII. p. 223. Abr. Vol. VII. p. 435.

† Phil. Tranſ. Vol. XXXIV. p. 113. Abr. Vol. VII. p. 437.

‡ Mém. de l'Acad. 1735, p. 163. 1736, p. 166. Ed. Amſt.

§ Phil. Tranſ. for 1795, p. 263. ‖ Phil. Tranſ. for 1796, p. 14.

** Phil. Tranſ. Vol. XXII. p. 673. Abr. II. p. 762.

†† Phil. Tranſ. for 1793, p. 178.

mination of the refractive power of a variable medium, and its application to the conſtitution of the cryſtalline lens. Secondly, the conſtruction of an inſtrument for aſcertaining, upon inſpection, the exact focal diſtance of every eye, and the remedy for its imperfections. Thirdly, to ſhew the accurate adjuſtment of every part of the eye, for ſeeing with diſtinctneſs the greateſt poſſible extent of objects at the ſame inſtant. Fourthly, to meaſure the collective diſperſion of coloured rays in the eye. Fifthly, by immerging the eye in water, to demonſtrate that its accommodation does not depend on any change in the curvature of the cornea. Sixthly, by confining the eye at the extremities of its axis, to prove that no material alteration of its length can take place. Seventhly, to examine what inference can be drawn from the experiments hitherto made on perſons deprived of the lens; to purſue the inquiry, on the principles ſuggeſted by Dr. Porterfield; and to confirm his opinion of the utter inability of ſuch perſons to change the refractive ſtate of the organ. Eighthly, to deduce, from the aberration of the lateral rays, a deciſive argument in favour of a change in the figure of the cryſtalline; to aſcertain, from the quantity of this aberration, the form into which the lens appears to be thrown in my own eye, and the mode by which the change muſt be produced in that of every other perſon. And I flatter myſelf, that I ſhall not be deemed too precipitate, in denominating this ſeries of experiments ſatisfactorily demonſtrative.

EXPLANATION OF THE FIGURES.

Plate XIII. Fig. 1. See page 256. Prop. III.

Fig. 2. See page 256. Prop. IV.

Fig. 3. See page 258. Prop. V.

Fig. 4—6. Relating to the optometer. See page 259.

Plate XIV. Fig. 7. The form of the ends of the optometer, when made of card. The apertures in the ſhoulders are for holding a lens: the ſquare ends turn under, and are faſtened together.

Fig. 8. The ſcale of the optometer. The middle line is divided, from the lower end, into inches. The next column ſhews the number of a concave lens requiſite for a ſhort-ſighted eye; by looking through the ſlider and obſerving the number oppoſite to which the interſection appears when moſt remote. By obſerving the place of apparent interſection when neareſt, the number requiſite will be found in the other column, provided that the eye have the average power of accommodation. At the other end, the middle line is graduated for extending the ſcale of inches by means of a lens four inches in focus; the negative numbers implying that ſuch rays as proceed from them are made to converge towards a point on the other ſide of the lens. The other column ſhews the focal length of convex glaſſes required by thoſe eyes to which the interſection appears, when neareſt, oppoſite the reſpective places of the numbers.

Fig. 9. A ſide view of the optometer, half its ſize.

Fig. 10. The appearance of the lines through the ſlider.

Fig.

Fig. 11. Method of meaſuring the magnitude of an image on the retina. See page 293.

Fig. 12. Diagonal ſcale drawn on a looking-glaſs.

Fig. 13. The method of applying a lens with water to the cornea.

Fig. 14. The appearance of a ſpectrum occaſioned by preſſure; and the inflection of ſtraight lines ſeen within the limits of the ſpectrum.

Fig. 15. An illuſtration of the enlargement of the image, which would be the conſequence of an elongation of the eye: the images of the candles which, in one inſtance, fall on the inſertion of the nerve, falling, in the other inſtance, beyond it.

Plate XV. Fig. 16. The ſucceſſive forms of the image of a large diſtant object, as it would be delineated by each refractive ſurface in the eye; to ſhew how that form at laſt coincides with the retina. E G is the diſtance between the foci of horizontal and vertical rays in my eye.

Plate XV. Fig. 17. Vertical ſection of my right eye, ſeen from without; twice the natural ſize.

Fig. 18. Horizontal ſection, ſeen from above.

Fig. 19. Front view of my left eye when the pupil is contracted; of the natural ſize.

Fig. 20. The ſame view when the pupil is dilated.

Fig. 21. Outline of the eye and its ſtraight muſcles when at reſt.

Fig. 22. Change of figure which would be the conſequence of the action of thoſe muſcles upon the eye, and upon the adipoſe ſubſtance behind it.

Fig. 23. Scale of the ſmall optometer.

Fig. 24. Appearance of four images of a line ſeen by my eye when its focus is ſhorteſt.

Fig. 25. Outline of the lens when relaxed; from a compariſon of M. Petit's meaſures with the phenomena of my own eye, and on the ſuppoſition that it is found in a relaxed ſtate after death.

Fig. 26. Outline of the lens ſufficiently changed to produce the ſhorteſt focal diſtance.

Fig. 27. Apparatus for aſcertaining the focal length of the lens in water.

Plate XV. Fig. 28. Various forms of the image depicted by a cylindrical pencil of rays obliquely refracted by a ſpherical ſurface, when received on planes at diſtances progreſſively greater.

Fig. 29. Image of a minute lucid object held very near to my eye.

Fig. 30. The ſame appearance when the eye has been rubbed.

Fig. 31—37. Different forms of the image of a lucid point at greater and greater diſtances; the moſt perfect focus being like Fig. 33, but much ſmaller.

Fig. 38. Image of a very remote point ſeen by my right eye.

Fig. 39. Image of a remote point ſeen by my left eye; being more obtuſe at one end, probably from a leſs obliquity of the poſterior ſurface of the cryſtalline lens.

Fig. 40. Combination of two figures ſimilar to the fifth variety of Fig. 28; to imitate Fig. 38.

Fig. 41. Appearance of a diſtant lucid point when the eye is adapted to a very near object.

Fig. 42, 44. Shadow of parallel wires in the image of a diſtant point, when the eye is relaxed.

Fig. 43, 45. The ſame ſhadows rendered curved by a change in the figure of the cryſtalline lens.

Fig. 46. The order of the fibres of the human cryſtalline.

Fig. 47. The diviſion of the nerves at the ciliary zone; the ſclerotica being removed. One of the nerves of the uvea is ſeen paſſing forwards and ſubdividing. From the calf.

Fig. 48. Ramifications from the margin of the cryſtalline lens.

Fig. 49. The zone of the cryſtalline faintly ſeen through the capſule.

Fig. 50. The zone raiſed from its ſituation, with the ramifications paſſing through it into the lens.

Fig. 51. The zone of the cryſtalline detached.

Plate XV. Fig. 52. The crenated zone, and the globules regularly arranged on the cryſtalline of the partridge.

Fig. 53. The order of the fibres in the lens of birds and fiſhes.

Fig. 54. The ſegments of the capſule of the cryſtalline turned back, to ſhew the detached ciliary proceſſes. From the calf.

Fig. 55. Part of the choroid of the cod-fiſh, with its red ſubſtance. The central artery hangs looſe from the inſertion of the nerve.

Fig. 56. The membrane covering this ſubſtance internally, raiſed by the blow-pipe.

Fig. 57. The appearance of the red ſubſtance, after the removal of the membrane.

IV.

Experiments on the Chemical Production and Agency of Electricity. By WILLIAM HYDE WOLLASTON, *M. D. F. R. S.*[*]

NOTWITHSTANDING the power of Mr. Volta's electric pile is now known to be proportional to the diſpoſition of one of the metals to be oxidated by the fluid interpoſed, a doubt has been entertained by many perſons, whether this power ariſes from the chemical action of the fluid on the metal, or, on the contrary, whether the oxidation itſelf may not be occaſioned by electricity, ſet in motion by the contact of metals that have different conducting powers.

That the oxidation of the metal is the primary cauſe of the electric phenomena obſerved, is, I think, to be inferred from the following experiments, which exhibit the galvanic proceſs reduced to its moſt ſimple ſtate.

Experiment 1. If a piece of zinc and a piece of ſilver have each one extremity immerſed in the ſame veſſel, containing ſulphuric or muriatic acid diluted with a large quantity of water,

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[*] Phil. Tranſ. for 1801, p. 427.

water, the zinc is diſſolved, and yields hidrogen gas, by decompoſition of the water: the ſilver, not being acted upon has no power of decompoſing water; but, whenever the zinc and ſilver are made to touch, or any metallic communication is made between them, hidrogen gas is alſo formed at the ſurface of the ſilver.

Any other metal beſide zinc, which by aſſiſtance of the acid employed is capable of decompoſing water, will ſucceed equally, if the other wire conſiſts of a metal on which the acid has no effect.

Experiment 2. If zinc, iron or copper, are employed with gold in dilute nitric acid, nitrous gas is formed; in the ſame manner, and under the ſame circumſtances, as the hidrogen gas in the former experiment.

Experiment 3. Experiments analogous to the former, and equally ſimple, may alſo be made with many metallic ſolutions. If, for inſtance, the ſolution contains copper, it will be precipitated by a piece of iron, and appear on its ſurface. Upon ſilver merely immerſed in the ſame ſolution, no ſuch effect is produced; but as ſoon as the two metals are brought into contact, the ſilver receives a coating of copper.

In the explanation of theſe experiments, it is neceſſary to advert to a point eſtabliſhed by means of the electric pile.

We know that when water is placed in a circuit of conductors of electricity, between the two extremities of a pile, if the power is ſufficient to oxidate one of the wires of communication, the wire connected with the oppoſite extremity affords hidrogen gas.

Since the extrication of hidrogen, in this inſtance, is ſeen to depend on electricity, it is probable, that in other inſtances, electricity may be alſo requiſite for its converſion into gas. It would appear, therefore, that in the ſolution of a metal, electricity is evolved during the action of the acid upon it; and that the formation of hidrogen gas, even in that caſe depends on a tranſition of electricity between the fluid and the metal.

We ſee, moreover, in the firſt experiment, that the zinc, without contact of any other metal, has the power of decompoſing water; and we can have no reaſon to ſuppoſe that the contact of the ſilver produces any new power, but that it ſerves merely as a conductor of electricity, and thereby occaſions the formation of hidrogen gas.

In the third experiment alſo, the iron by itſelf has the power of precipitating copper, by means, I preſume, of electricity evolved during its ſolution; and here likewiſe the ſilver, by conducting that electrity, acquires the power of precipitating the copper in its metallic ſtate.

The explanation here given receives additional confirmation from comparative experiments which I have made with common electricity; for it will be ſeen, that the ſame transfer of chemical power, and the ſame apparent reverſion of the uſual order of chemical affinities in the precipitation of copper by ſilver, may be effected by a common electrical machine.

The machine with which the following experiments were conducted, conſiſts of a cylinder ſeven inches in diameter, with a conductor on each ſide, 16 inches long, and 3½ inches diameter,

diameter, each furniſhed with a ſliding electrometer, to regulate the ſtrength of the ſpark received from them.

Experiment 4. Having a wire of fine ſilver $\frac{1}{120}$ of an inch in diameter, I coated the middle of it for two or three inches, with ſealing wax, and by cutting through in the middle of the wax, expoſed a ſection of the wire. The two coated extremities of the wire thus divided, were immerſed in a ſolution of ſulphate of copper, placed in an electric circuit between the two conductors; and ſparks, taken at $\frac{1}{10}$ of an inch diſtance, were paſſed by means of them through the ſolution. After 100 turns of the machine, the wire which communicated with (what is called) the negative conductor, had a precipitate formed on its ſurface, which, upon being burniſhed, was evidently copper; but the oppoſite wire had no ſuch coating.

Upon reverſing the direction of the current of electricity, the order of the phenomena was of courſe reverſed; the copper being ſhortly re-diſſolved by aſſiſtance of the oxidating power of poſitive electricity, and a ſimilar precipitate formed on the oppoſite wire.

Experiment 5. A ſimilar experiment made with gold wires $\frac{1}{100}$ of an inch diameter, in a ſolution of corroſive ſublimate, had the ſame ſucceſs.

The chemical agency, therefore, of common electricity, is thus proved to be the ſame with the power excited by chemical means; but, ſince a difference has been obſerved in the comparative facility with which the pile of Volta decompoſes water, and produces other effects of oxidation and de-oxidation of bodies expoſed to its action, I have been at ſome pains to remove this difficulty, and can at leaſt produce a very cloſe imitation of the galvanic phenomena, by common electricity.

It has been thought neceſſary to employ powerful machines, and large Leyden jars, for the decompoſition of water; but when I conſidered that the decompoſition muſt depend on duly proportioning the ſtrength of the charge of electricity to the quantity of water, and that the quantity expoſed to its action at the ſurface of communication depends on the extent of that ſurface, I hoped that, by reducing the ſurface of communication, the decompoſition of water might be effected by ſmaller machines, and with leſs powerful excitation, than have hitherto been uſed for that purpoſe; and, in this hope, I have not been diſappointed.

Experiment 6. Having procured a ſmall wire of fine gold, and given it as fine a point as I could, I inſerted it into a capillary glaſs tube; and after heating the tube, ſo as to make it adhere to the point and cover it in every part, I gradually ground it down, till, with a pocket lens, I could diſcern that the point of the gold was expoſed.

The ſucceſs of this method exceeding my expectations, I coated ſeveral wires in the ſame manner, and found, that when ſparks from the conductors before-mentioned were made to paſs through water, by means of a point ſo guarded, a ſpark paſſing to the diſtance of $\frac{1}{8}$ of an inch would decompoſe water, when the point expoſed did not exceed $\frac{1}{700}$ of an inch in diameter. With another point, which I eſtimated at $\frac{1}{1500}$, a ſucceſſion of ſparks $\frac{1}{20}$ of an inch in length, afforded a current of ſmall bubbles of air.

I have ſince found, that the ſame apparatus will decompoſe water, with a wire $\frac{1}{40}$ of an inch diameter, coated in the manner before deſcribed, if the ſpark from the prime conductor paſſes to the diſtance of $\frac{4}{10}$ of an inch of air.

Experiment 7. In order to try how far the ſtrength of the electric ſpark might be reduced by proportional diminution of the extremity of the wire, I paſſed a ſolution of gold in *aqua regia* through a capillary tube, and, by heating the tube, expelled the acid. There remained a thin film of gold, lining the inner ſurface of the tube, which, by melting the tube, was converted into a very fine thread of gold, through the ſubſtance of the glaſs.

When the extremity of this thread was made the medium of communication through water, I found that the mere current of electricity would occaſion a ſtream of very ſmall bubbles to riſe from the extremity of the gold, although the wire, by which it communicated with the poſitive or negative conductor, was placed in abſolute contact with them. Hence it appears, that decompoſition of water may take place by common electricity, as well as by the electric pile, although no diſcernible ſparks are produced.

The appearance of two currents of air may alſo be imitated, by occaſioning the electricity to paſs by fine points of communication on both ſides of the water; but, in fact, the reſemblance is not complete; for, in every way in which I have tried it, I obſerved that each wire gave both oxigen and hidrogen gas, inſtead of their being formed ſeparately, as by the electric pile.

I am inclined to attribute the difference in this reſpect to the greater intenſity with which it is neceſſary to employ common electricity; for, that poſitive and negative electricity, ſo excited, have each the ſame chemical power as they are obſerved to have in the electric pile, may be aſcertained by other means.

In the precipitation of copper by ſilver, an inſtance of de-oxidation (or phlogiſtication) by negative electricity has been mentioned; the oxidating power of poſitive electricity may be alſo proved, by its effect on vegetable blue colours.

Experiment 8. Having coloured a card with a ſtrong infuſion of litmus, I paſſed a current of electric ſparks along it, by means of two fine gold points, touching it at the diſtance of an inch from each other. The effect, as in other caſes, depending on the ſmallneſs of the quantity of water, was moſt diſcernible when the card was nearly dry. In this ſtate a very few turns of the machine were ſufficient to occaſion a redneſs at the poſitive wire, very manifeſt to the naked eye. The negative wire, being afterwards placed on the ſame ſpot, ſoon reſtored it to its original blue colour.

By Mr. Volta's apparatus the ſame effects are produced in a much leſs time.

Beſide the ſimilarity which has thus been traced between the effects of electricity excited by the common machine, and thoſe obſerved from the electric pile, I think it appears alſo probable, that they originate from the ſame ſource.

With regard to the latter, its power is now known to depend on oxidation; ſo alſo does the excitement in the former appear very much to depend on the ſame proceſs; for, *Experiment* 9. I have found that, by uſing an amalgam of ſilver or of platina, which are not liable to be oxidated, I could obtain no electricity. An amalgam of tin, on the contrary affords a good degree of excitement. Zinc acts ſtill better; but the beſt amalgam is made with both tin and zinc, a mixture which is more eaſily oxidated than either metal ſeparately.

Experiment 10. But, as a farther trial whether oxidation aſſiſts in the production of electricity, I mounted a ſmall cylinder, with its cuſhion and conductor, in a veſſel ſo contrived, that I could at pleaſure change the contained air.

After trying the degree of excitement in common air, I ſubſtituted carbonic gas, and found that the excitement was immediately deſtroyed, but that it returned upon re-admiſſion of atmoſpheric air.

In conformity to this hypotheſis, we find that the metal oxidated is, in each caſe, in a ſimilar ſtate of electricity; for the cuſhion of the machine, by oxidation of the amalgam adhering to it, becomes negative; and in the ſame manner, zinc, oxidated by the accumulated power of an electric pile, or ſimply by action of an acid, is alſo negative.

This ſimilarity in the means by which both electricity and galvaniſm appear to be excited, in addition to the reſemblance that has been traced between their effects, ſhews that they are both eſſentially the ſame, and confirms an opinion that has already been advanced by others, that all the differences diſcoverable in the effects of the latter, may be owing to its being leſs intenſe, but produced in much larger quantity.

V.

An Account of ſome Galvanic Combinations, formed by the Arrangement of ſingle Metallic Plates and Fluids, analogous to the new Galvanic Apparatus of Mr. Volta. By Mr. HUMPHRY DAVY, *Lecturer on Chemiſtry in the Royal Inſtitution* *.

1. ALL the galvanic combinations analogous to the new apparatus of Mr. Volta, which have been heretofore deſcribed by experimentaliſts, conſiſt (as far as my knowledge extends) of ſeries containing at leaſt two metallic ſubſtances, or one metal and charcoal, and a ſtratum of fluid. And it has been generally ſuppoſed, that their agencies are, in ſome meaſure, connected with the different powers of the metals to conduct electricity. But I have found that an accumulation of Galvanic influence, exactly ſimilar to the accumulation in the common pile, may be produced by the arrangement of ſingle metallic plates, or arcs, with different ſtrata of fluids.

The train of reaſoning which led to the diſcovery of this fact, was produced by the obſervation of ſome phænomena relating to the connection of chemical changes with the evolution of Galvanic power.

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* Philoſophical Tranſactions, 1801.

It appeared, in ſeveral experiments, that ſeries of double metallic plates, incapable of acting as Galvanic combinations, when arranged in the proper order, with portions of water, were readily made to produce Galvanic effects, by being alternated with acids, or other fluids capable of oxidating one only of the metals of the ſeries. Thus, double plates, compoſed of ſilver and gold, (metals which have been ſuppoſed to differ very little in their powers of conducting electrity,) produced Galvanic action, when placed in contact, in the common order, with cloths moiſtened in diluted nitric acid. And copper and ſilver acted powerfully with nitrate of mercury.

Theſe facts induced me to ſuppoſe, that the alternation of two metallic bodies with fluids, was eſſential to the production of accumulated Galvanic influence, only ſo far as it furniſhed two conducting ſurfaces of different degrees of oxidability; and that this production would take place, if ſingle metallic plates could be connected together by different fluids, in ſuch a manner that one of their ſurfaces only ſhould undergo oxidation, the arrangement being regular.

On this ſuppoſition, I made a number of experiments on different arrangements of ſingle metals and fluids; and, after many various proceſſes, I was enabled to aſcertain, that many of theſe arrangements could be made active, not only when oxidations, but likewiſe when other chemical changes were going on in ſome of their parts.

In deſcribing the different Galvanic combinations formed by ſingle metallic plates and fluids, I ſhall divide them into three claſſes, following, in the arrangement, the order of time with regard to diſcovery.

II. The firſt and moſt feeble claſs is compoſed, whenever ſingle metallic plates, or arcs, are arranged in ſuch a manner that two of their ſurfaces, or ends oppoſite to each other, are in contact with different fluids, one capable, and the other incapable, of oxidating the metal. In this caſe, if the ſeries are numerous, and in regular alternation, Galvanic influence will be accumulated, analogous, in all its effects, to the influence of the common pile.

Tin, zinc, and ſome other eaſily oxidable metals, act moſt powerfully in this claſs of combinations.

If pieces of poliſhed tin, about an inch ſquare, and $\frac{1}{20}$ of an inch thick, be connected with woollen cloths of the ſame ſize, (moiſtened, ſome in water, and ſome in diluted nitrous acid,) in the following order, tin, acid, water, and ſo on, till twenty ſeries are put together, a feeble Galvanic battery will be formed, capable of acting weakly on the organs of ſenſe, and of ſlowly producing the common appearances in water; the wire from the oxidating ſurface of the plates evolving hidrogen; and the wire from the non-oxidating ſurface (when of ſilver) depoſiting oxide.

In all caſes, when the batteries of the firſt claſs are erected perpendicularly, the cloth moiſtened in acid muſt be placed under the cloth moiſtened in water; and, in this arrangement, as the acid is ſpecifically heavier than water, little or no mixture of the fluids will take place.

When

When zinc is employed, on account of its rapid oxidation in water containing atmoſpheric air, three cloths ſhould be uſed; the firſt moiſtened in weak ſolution of ſulphuret of pot-aſh, (which is poſſeſſed of no power of action upon zinc, and which prevents it from acting upon the water;) the ſecond moiſtened in a ſolution of ſulphate of pot-aſh, of greater ſpecific gravity than the ſolution of ſulphuret; and the third wetted in an oxidating fluid ſpecifically heavier than either of the ſolutions. In this caſe, if the order be as follows, zinc, oxidating ſolution, ſolution of ſulphate of pot-aſh, ſolution of ſulphuret of pot-aſh, very little mixture of the fluids, or chemical action between them, will take place: and an alternation of twelve ſeries of this kind, forms a battery capable of producing ſenſible effects.

III. The ſecond claſs of GALVANIC combinations with ſingle plates is formed, when plates, or arcs, compoſed of a metallic ſubſtance capable of acting upon ſulphurated hidrogen, or upon ſulphurets diſſolved in water, are formed into ſeries, with portions of a ſolution of ſulphuret of pot-aſh, and water, in ſuch a manner that one ſide of every plate, or arc, is in contact with water, whilſt the oppoſite ſide is acted on by the ſolution of ſulphuret. Under theſe circumſtances, when the alternation is regular, and the number of ſeries ſufficiently great, Galvanic power is evolved; and water, placed in the circuit with ſilver wires, is acted on; oxide being depoſited on the wire connected with the ſide of the plate undergoing chemical alteration, whilſt hidrogen is evolved from the ſide in contact with water,

Silver, copper, and lead, are each capable of forming this combination. Plates made from either of thoſe metals, may be arranged with cloths, (moiſtened, ſome in water, and others in ſolution of ſulphuret of pot-aſh,) in the following order, metal, cloth moiſtened in ſulphuret of pot-aſh, cloth moiſtened in water, and ſo on.

Eight ſeries will produce ſenſible effects; and the wire from the top of the pile produces oxide.

Copper is more active, in this claſs of batteries, than ſilver: and ſilver more active than lead.

IV. The third and moſt powerful claſs of Galvanic batteries, conſtructed with fluids and ſingle metals, is formed, when metallic ſubſtances oxidable in acids, and capable of acting on ſolutions of ſulphurets, are connected, as plates, with oxidating fluids and ſolutions of ſulphuret of pot-aſh, in ſuch a manner that the oppoſite ſides of every plate may be undergoing different chemical changes; the mode of alternation being regular.

The ſame metals that act in the ſecond claſs, may be uſed in the third claſs; and the order of their powers is ſimilar. The pile may be erected in the ſame manner as the pile with zinc in the firſt claſs; the cloths moiſtened in acid being ſeparated from thoſe moiſtened in ſolution of ſulphuret, by a third cloth, ſoaked in ſolution of ſulphate of pot-aſh.

Three plates of copper, or ſilver, arranged in this manner, in the juſt order, produce ſenſible effects; and twelve or thirteen ſeries are capable of giving weak ſhocks, and of rapidly producing gas and oxide in water; the wire connected with the oxidating end of the

the apparatus evolving hidrogen; and the wire attached to the end acting on the ſulphuret, depoſiting oxide when compoſed of ſilver, and generating oxigen when of gold.

V. In all the ſingle metallic piles conſtructed with cloths, the action is very tranſient: the decompoſition of the acids, and of the ſulphurets, is generally completed in a few minutes; and, in conſequence, the Galvanic influence ceaſes to be evolved. The arrangement of all the different ſeries may, however, (by means of an apparatus conſtructed after the ideas of Count Rumford,) be made in ſuch a manner as to give conſiderable permanency to their effects. This apparatus is a box, covered with cement incapable of conducting electricity, and compoſed of three pieces of mahogany, each containing grooves capable of receiving the edges of the different plates proper for compoſing the ſeries. One half of theſe plates muſt be compoſed of horn, or glaſs, and the other half of metallic ſubſtances; and the conductors of electricity, and the non-conductors, muſt be alternately cemented into the grooves, ſo as to form water-tight cells.

When the apparatus is uſed, theſe cells are filled, in the Galvanic order, with different ſolutions, according to the claſs of the combination; and connected in pairs with each other, by ſlips of moiſtened cloth, carried over the non-conducting plates.

A combination of fifty copper-plates, arranged in this manner, with weak ſolutions of nitrous acid, or nitrate of ammoniac, and ſulphuret of pot-aſh, gives pretty ſtrong ſhocks, rapidly evolves gas from water, and affects the condenſing electrometer.

It does not loſe its power of action for many hours; and, when this power is loſt, it may be reſtored by the addition of ſmall quantities of concentrated ſolutions of the proper chemical agents to the fluids in the different cells.

From two experiments made on copper and ſilver, it would appear, that the ſingle metallic batteries act equally well, when the metals made uſe of are ſlightly alloyed, and when they are in a ſtate of purity.

VI.

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

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

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[*] Iriſh Academy, 1801.

Having

Having therefore, in the uſual manner, aſcertained what quantity of that ingredient remained untouched, I was forced to ſeek the reſt of it in the liquor, which had waſhed the various precipitates. To obtain it, I poured a ſolution of nitrate of barytes into thoſe waſhings, when all the other ſubſtances had been carefully ſeparated, and was thereby enabled to precipitate, in a ſtate of purity, the ſulphate of barytes, formed by that earth, and by the portion of ſulphur originally acidified, in the firſt treatment of the ore by nitric acid. To come at the knowledge of the proportion of ſulphur, contained in a given quantity of ſulphate of barytes, I had recourſe, in the firſt inſtance, to the quantity of ſulphur, ſaid by Lavoiſier to be contained in ſulphuric acid, and, in the 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 oxigen; and again, in adopting the proportions of the latter, we have 33 per cent. of acid, on ſulphate of barytes. But if 100 contain 71 of ſulphur, 33 muſt contain 23.43. Conſequently, for every 100 parts of ſulphate of barytes, I was to allow 23.43 of ſulphur. But, by the reſults of my analyſes, I had ſuch quantities of ſulphate of barytes, as induced me to doubt the accuracy of one or other of the ſtatements, by which I eſtimated the quantity of real ſulphur contained in the ore.

No perſon is better acquainted than our celebrated Preſident, with the many difficulties that occur in the analyſis of ſalts in general; particularly with regard to the quantity of real acid they may contain. It has been a work of trouble to the ableſt 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 are already known; or by obtaining, in a ſtate perfectly free from water, the acid reſulting from ſuch combuſtion. To the former method, the general objections againſt all analyſes of ſalts muſt apply. The latter is ſtill more defective. It is by no means certain, that we have ever yet obtained any acid, in a ſtate of perfect ſiccity, unleſs we except the phoſphoric and the arſenic; for even the cryſtallized vegetable acids retain a portion of water in their cryſtallization. It is not that I abſolutely deny our having obtained them ſo; but I ſay merely, that we have no proof. It would indeed be ſetting narrow bounds to the perfection of nature, to aſſert, that no combuſtible body could, when ſaturated with oxigen, aſſume of itſelf, the ſtate of liquidity; or that the oxide of the particular ſubſtance, called hidrogen, 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 of error, therefore, remained open in this branch of the calculation, which gave the proportion of 23.43 of ſulphur, as that contained in 100 parts of ſulphate of barytes.

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

To aſcertain this point, I operated in the following manner: I prepared ſome lime, as pure, I believe, as 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 pot-aſh, 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.

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 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

Lime	57
Sulphuric acid	43
	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 (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 intire 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 oxigen will form 100 of real ſulphuric acid.

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 fire, 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.

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 oxigen gas.

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

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

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

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

Sulphur - - - -	55.56
Oxigen - - - - - -	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

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

Having determined with accuracy the proportion of acid in any inſoluble ſulphate, it is eaſy to proceed to the determination of that contained in any other ſulphate. The docimaſtic art or analytic 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, each will ſerve as a proof of the validity of the other; and perhaps demonſtrate, that ſulphuric acid, without the intervention of water, may enjoy liquidity, at the temperature and preſſure which act upon our globe.

VII.

Deſcription of an Apparatus for Diſtilling, in which the Effects of Abſorption are prevented without admitting the external Air. In a Letter from Mr. A. S. Burkitt.

To Mr. NICHOLSON.

SIR,

I HAVE taken the liberty of incloſing a drawing of an apparatus, to prevent the abſorption which ſo frequently happens in diſtilling acids, in particular the nitrous; for which I have moſtly uſed it with the deſired effect.

Should it prove original, and meet your attention, you will confer a favour by giving it a place in your valuable Journal.

I am, SIR,

Your moſt obedient humble Servant,

Fleet Street. Nov. 20, 1801. A. S. BURKITT.

Plate XVII. repreſents the apparatus; where A is a retort, B, the uſual receiver, having a neck to receive the retort, and another from which the recurved tube F proceeds to the bottom of a ſecond receiver C; and from this laſt veſſel a communication is made in the well known manner with the apparatus of Woulfe D. The principal ſingularity and advantage of my apparatus conſiſts in the valve E, conſtructed by placing a plate, or plano-convex lens upon the mouth of a ſmaller tube, inſerted by grinding within F like the valve in in North's apparatus, but with more water way; and this advantage I find in practice to be ſuperior both in effect and cheapneſs to the ingenious tube of ſafety of Walter. That contrivance, ſo familiar to chemiſts, conſiſts of a perpendicular tube with a ſmall baſon at top, and another reſervoir or ſwell at bottom, in an aſcending part of the ſame tube; and its action when duly charged with mercury is, 1. When the internal preſſure increaſes, the mercury riſes, till the whole column is ſupported, after which, upon any farther increaſe, the intire maſs of mercury is ſuddenly blown into the upper reſervoir, and gas is at once let out, till the ſpring is reduced nearly to that of the atmoſphere; and 2. If there be abſorption, the external air in all caſes is very little reſiſted, but is ſuffered to enter the veſſels.

In my apparatus the preſſure during the tranſmiſſion of elaſtic fluid through the valve is conſtant; being equal to the re-action of the liquids in the veſſels C and D, and whatever other external force may be added by the well known means: and whenever by diminution of the fire, or other cauſes, the ſpring of the gaſes in B ſhall become leſs than the preſſure of the external air, that ſpace will become partly vacuous; but the valve will prevent any return of the fluid from C, ſo that the only effect will be, that the materials in A will give out their product ſomewhat more rapidly from the diminiſhed re-action.

A. S. B.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Diſcovery of Comets.

In the journal of the 8th of prairial the Notary Caigné announced, that he had 600 fr. to pay to any one who ſhould diſcover a comet. On the morning of the 23d of Meſſidor, C. Pons, keeper of the obſervatory at Marſeilles, diſcovered one, which three able aſtronomers of Paris alſo obſerved the ſame evening. The board of longitude, being requeſted to adjudge the prize, conſidered C. Pons as having a degree of priority, to which the three aſtronomers of Paris aſſented, and the prize was awarded to him. I am ſoliciting the government to offer a ſimilar prize, to accelerate the progreſs of our knowledge of comets, the only branch of aſtronomy which is not in an advanced ſtate. It is ſo eaſy, however, to find comets, that any one, without being an aſtronomer, may diſcover them when he pleaſes.

LALANDE.

Mémoires d'Agriculture, &c. Memoirs of Agriculture, and Rural and Domeſtic Economy, publiſhed by the Agricultural Society of the Department of the Seine, and printed by Order of the Prefect of the Department. Vol. I. Paris, 8vo. 447 p. 1 Plate. Price 4 fr.

At

AT the head of this volume are the order of the Prefect for printing it, the rules of the ſociety, and a liſt of its ordinary and correſponding members, with an account of the ſociety's labours; after which are the following memoirs.—Inſtructions for preſerving Wheat from Smut, by C. C. Cadet-de-Vaux, Parmentier, Saint.Genis, and Yvart.—Report of the Experiments of C. Houdart, Junr. on preparing and economiſing ſeed—On the means of draining various lands by ſimple and not expenſive proceſſes, by C. Chaſſiron. This memoir, which has been publiſhed ſeparately, is preceded by ſome obſervations on the general ſyſtem of inland navigation in France, and followed by a comparative table of the coal-mines worked in each department, and of thoſe which require only navigable canals and rivers to enable them to be worked. The former are fifty-one in number; the latter, ſixty. Their importance to thoſe of the French manufactures that require fuel, our mechanic arts, and our agriculture, muſt be obvious, at a time when we are perhaps not ſufficiently aware of the want of wood, with which we are threatened, from the great devaſtation of the foreſts in moſt parts of the republic; a ſubject which has been treated of by C. Laſterie in a ſeparate memoir in this volume.—On the preciſe Signification of the terms *Agriculture* and *Rural Economy*, by C. Cels.—Some Reflections on the ſuppoſed number of Sheep in France, by C. Delong. The intention of this memoir is to excite inquiry concerning this important ſubject.—The advantages of nurſeries on eſtates of a certain extent, in facilitating ſuch annual plantations as may be ſuited to them, by C. Villele.—An intereſting Account of the ſucceſsful Amputation of the fore leg of a Cow, which had been fractured, by C. Chaumontel.—Experiments, Inquiries, and Obſervations on Elms, by C. Boucher; to which C. Denorgelles has added his Experiments for obtaining Alcohol from the ſap of Vegetables.—On the product of different ſorts of wheat that are cultivated, and the melioration of them; or the advantages that would reſult from inquiring what ſorts of wheat yield the moſt bread; by C. Chancey.—On Rural Edifices, by C. Garnier-Deſchênes.—On the Manner in which the Mountains in the Cevennes are fertilized, by C. Chaptal.—On the Means of rendering fit for uſe Wells that have been abandoned in conſequence of the Mephitiſation of the Soil, by C. Cadet-de-Vaux.—Obſervations on the working of Wines, by C. Creuſé-Latouche.—On the Cultivation of the Sugar-Cane in the Caribbee Iſlands, and particularly of that of Otaheite, by C. Moreau-St. Mery.—On the Cultivation of the Sugar-Cane, by C. Coſſigny.—On the Cinnamon-Tree of French Guiana, by C. le Blond.—The volume concludes with accounts of the lives of Cretté de Palluel, and T. Francis de Grace. *Mag. Encycl.*

Hiſtoire Naturelle des Mineraux, &c. The Natural Hiſtory of Minerals; by E. M. L. Patrin, Aſſociate of the National Inſtitute; containing a Deſcription of Minerals, and of their native Situation [Gite], *the Theory of their Formation, their Connection with Geology, or the Hiſtory of the Earth, an Account of their Properties and Uſes, their Chemical Analyſis, &c.* 5 Vols. large 18mo, about 350 p. each, with 40 Plates. Price 12 fr. 50 c. plain; 18 fr. 50 c. with coloured plates; on fine paper, 5 fr. 50 c. more.

C. PATRIN

C. PATRIN has made himſelf known by ſeveral memoirs in the *Journal de Phyſique*, in 1788 and 1791, on the mines of Siberia, which he ſpent eight years in examining from the boundaries of Europe to the frontiers of China, and whence he has brought home a collection of minerals as valuable as it is ample. The object of this tour was to collect information concerning the ſtructure of mountains in general, the formation of metallic veins, the relation that different mineral ſubſtances bear to each other, and the hiſtory of the terreſtrial globe itſelf. It is particularly in this important point of view, that the author has treated of the hiſtory of minerals. This too was the object propoſed by the illuſtrious Buffon; but that great man wanted the aid of obſervation, which genius cannot ſupply; and what he has ſaid on the ſubject is built wholly on ſyſtems formed in the cloſet, and now conſidered only as ſplendid chimeras. To theſe different notions of general theory, the author has added all the particulars that he judged of moſt importance reſpecting the properties of the different mineral ſubſtances, and their uſes in phyſic or the arts: every thing, in ſhort, that can render them ſerviceable to man. He has carefully added the analyſes made of them by the moſt celebrated modern chemiſts, which were altogether omitted by Buffon; though, indeed, had they not, what he could have given would have been of little uſe at preſent, from the amazing ſuperiority the chemical operator has attained ſince his time. In this work is an eſſay on volcanoes, in which C. P. advances a new theory of theſe grand phenomena: and one thing ſhews, that the author has ſeen the operations of nature contemplated as they actually are; his having pointed out ſeveral diſcoveries as poſſible, which have been really made ſince his memoir was read at the Inſtitute.

END OF THE FIFTH VOLUME.

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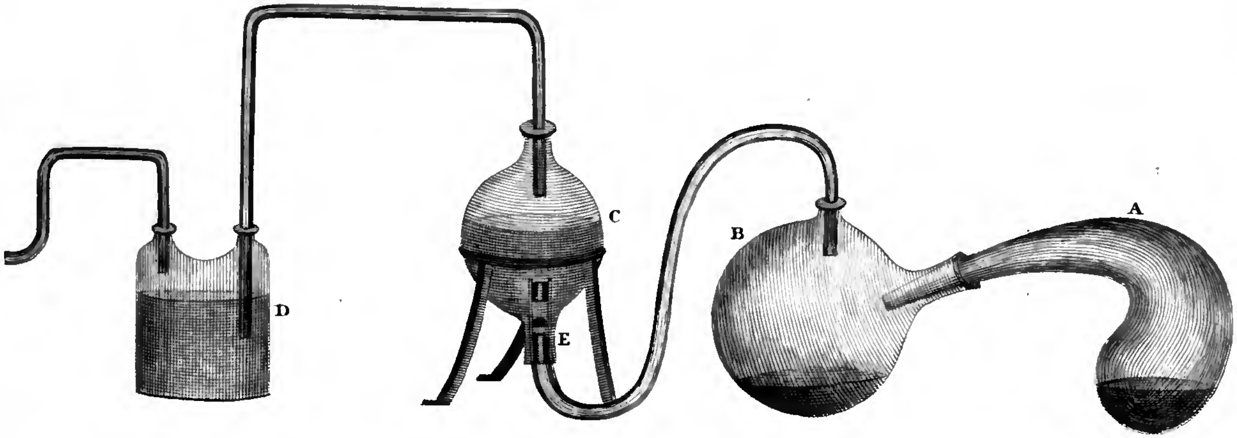
the end of the Vol.

Mr. Burkitt's arrangement of distillatory Veſsels
to prevent the effects of absorption.



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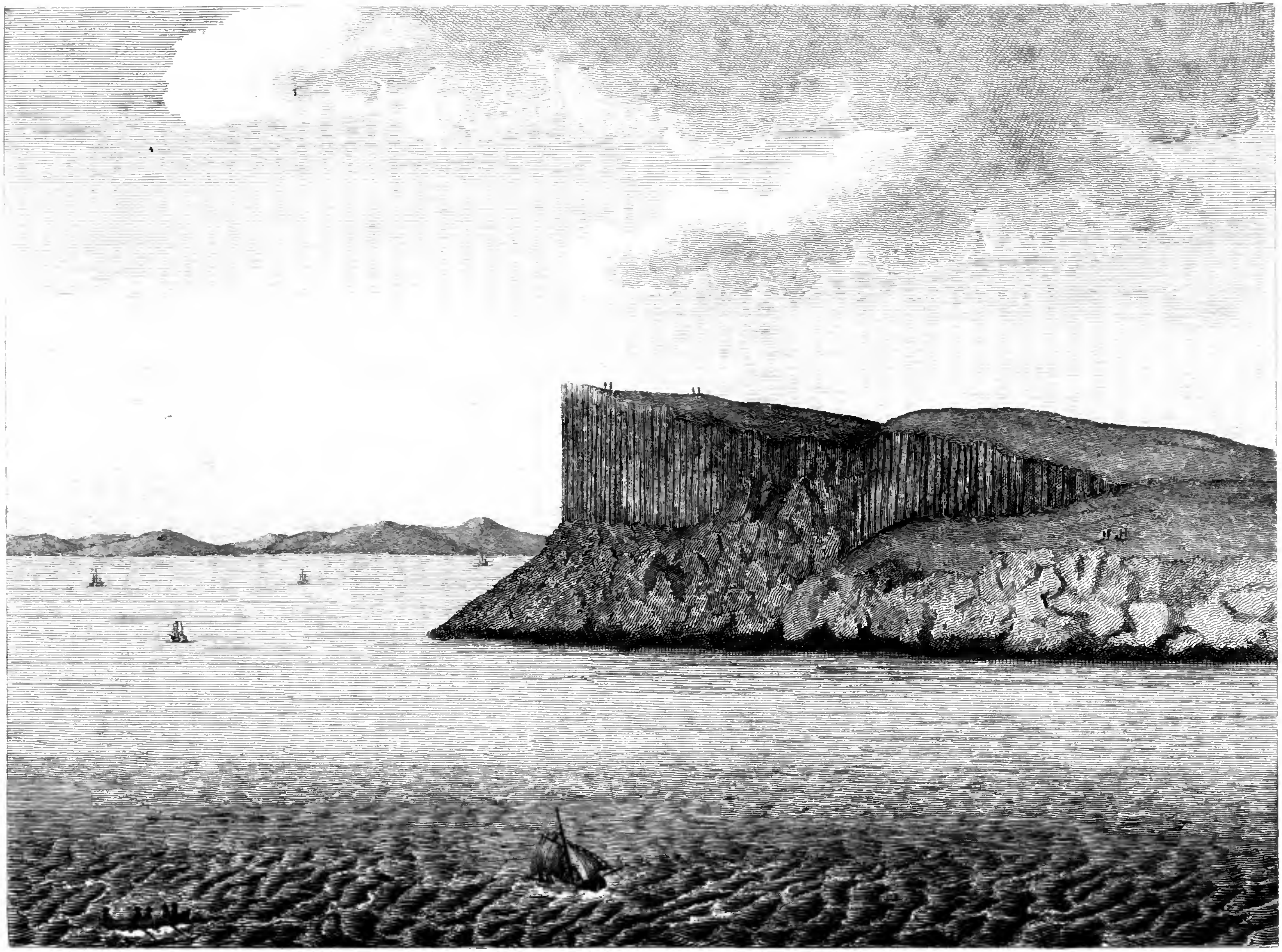

Mutlow & Kay sc.

View of the basaltic Columns of Fairhead, on the Coast of Ireland.

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